An Information Theory Interpretation of Relativistic Phenomena

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Author's Declaration

I, Gareth Boardman, do hereby declare that:

> This dissertation entitled **"An Information Theory Interpretation of Relativistic Phenomena,"** contains no material which has been accepted for the award of any other degree or diploma at this or any other academic institution.
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An Information Theory Interpretation of Relativistic Phenomena

Abstract:

It is well recognized that two of the foundational theories of modern physics, relativity and quantum mechanics, are inconsistent. Efforts to overcome this through intensive pursuit of unifying theoretical frameworks or improvements in experimental precision as pursued by Robertson-Mansouri-Sexl and Standard-Model-Extension test theory protocols, have so far proven unsuccessful. Nonetheless, empirical evidence deriving from investigations inspired by the twentieth-century "EPR" polemic, can now claim to have identified 'locality' and 'separability' as notions integral to special relativity theory inconsistent with quantum mechanics and experimental evidence supporting it.

Following intimations of J.S. Bell, L. Smolin, Maudlin and others, the history and philosophy of science are engaged as research tools to uncover the origins of the assumptions underlying such incompatibility. In particular, known but prevalent fallacies and the deprecation of processes as described by Whitehead, together with recent insights into the functional distinctions between representative models and explanatory theories, are engaged. Analysis of the evolution of optical and electromagnetic theorizing is then suggested to demonstrate that the choice of a mechanically compatible model of electromagnetic radiation, as has conventionally underwritten development of the light-speed postulate of special relativity, is inappropriate. Pursuing options made available within a *multiple models idealization* of optical and electromagnetic phenomena, replacement of the sponsoring model by a non-local and non-separable wavefront process of radiation of the form advanced by Fresnel and Kirchhoff on the basis of Huygens' principle, suggests the possibility of reformulation of relativity theory without incurring such conflicts.

However, choice of a wavefront electrodynamic model including the extinction theorem of Ewald and Oseen, equally suggests possible theory reformulation without necessitating a four-dimensional construct of space and time or postulating an absolute constancy of light-speed in vacuum. Such a constancy can in turn be considered as demonstrating an innate limitation on empirical observation resulting from the loss of relative velocity information during electromagnetic transmission due to the memorylessness of the Markov propagation process of refraction together with information flow discontinuity resulting from dielectric boundary extinction events. Such characteristics are shown to be applicable to electromagnetic radiation of all frequencies.

Further, from a wavefront reformulated hypothesis of inertial relative motion, it appears possible to demonstrate that the Galilean principle of relativity can be preserved. However, Lorentz invariance would then require reinterpretation wherein the Lorentz transform group would be deemed to quantify the degree of information distortion inherently experienced by any empirical enquiry into electrodynamics involving relative motion.

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Introduction and Overview

Together with Quantum Mechanics and Thermodynamics, Special Relativity is acknowledged as one of the foundational theories of twenty-first century science, which now enjoys the most accurate empirical results ever obtained. Nonetheless, in what constitutes a generally understated crisis in the history of science, it is now well recognized that, taken together, Quantum Mechanics and Special Relativity are inconsistent. Efforts to address this disparity through the intensive pursuit of unifying theoretical frameworks, such as "string" hypotheses, have recently been assessed by some as having failed to meet with early expectations, and held by others as not worthy of further support.

However, some recent empirical results, deriving from an original challenge to quantum theory's claim to completeness and mounted in 1935 by Podolsky in collaboration with Rosen and Einstein, are ironically now found to be in contention with assertions implicit in special relativity. Pursuing an approach to experimental realization of the "Inequality theorem" advanced by J. S. Bell as a potential resolution to the on-going debate, often denoted the "EPR" paradox, theoretical constructs foundational to both theories continue to be rigorously examined. Although interpretation of intensive investigations into concepts such as causality and superluminal information transport remain in dispute, it can now be claimed that notions such as "locality" and "separability," as concepts integral to special relativity theory, have been demonstrated inconsistent with quantum mechanics and the experimental evidence supporting it.

For some physicists and philosophers of science, such a revelation has suggested that special relativity may be conceptually incorrect or perhaps that it only represents an approximation of reality over the range of scale normally encountered. In response, specialist laboratories now pursue improved precision for kinematical tests of local Lorentz invariance as prescribed by the Robertson-Mansouri-Sexl test theory and dynamical tests as decreed by the Standard-Model-Extension protocol that attempts experimental investigations at both very high energies and the extremely small "Planck scale." To date, and despite impressive improvements in precision, relativity theory maintains a robust empirical compliance and no violations of Lorentz invariance have been detected.

Faced with the dilemma of a demonstrable inconsistency yet the diminishing prospects of a resolution through theoretical unification or the discovery of new principles through experimentation, some theorists such as Maudlin and Smolin, are finding agreement with an early conjecture by Bell that: "It may well be that real synthesis of quantum and relativity theories requires not just technical developments but radical conceptual renewal." (Bell 1984, p. 172) Following Bell's intimation and giving credence to claims made by such as Holton for the study of the history of science, and further acknowledging the discerning and constructive role claimed for the philosophy of science by such as Whitehead, the history and philosophy of science are engaged as research tools with the intent of uncovering the origins and assumptions underlying the now evident exhibition of theoretical disparity.

With respect to the philosophy of science, particular consideration is given to Whitehead's claim that known but prevalent fallacies potentially distort balanced and sound theory construction. Of concern in Whitehead's view was the "fallacy of misplaced concreteness," wherein abstractions, such as mathematical formulae, might find themselves reified and then be incorrectly assigned properties of substantiality. Additionally, the "fallacy of simple location" found recognition as restricting the allowable range of physical phenomena to that of objects, individually locatable and separable, and potentially demeaning the equally valid inclusion of physical processes.

Additionally, an understanding of the functional role played by scientific models as an integral step in the scientific method, and bearing directly on the process of theory construction, is explored. Of importance, modeling construction and possible model properties such as idealization, abstraction and simplification are clarified. The practice of de-idealization and concretization of preliminary physical models in preparation for the formulation of testable hypotheses, is also treated. The eventual constuction of empirically successful theories is held to occur through recursive and realistic model modifications tested by experiment on the basis of hypothetical prediction. Predictive failure is held to impugn the adequacy of the predisposing model leading to possible theory disconfirmation.

Although the complexity and convolutions of optical history are held by some to be too extensive for useful academic analysis except on the basis of carefully edited events, application of the history and philosophy of science, and appropriating Whitehead's observations together with a recognition of the role played by representational strategies, can be seen to demonstrate a noticeable order in the developmental drama. Early optical speculations are often readily depicted as contentions between an objectification of abstract geometrical constructs or local discrete entities as against the modeling of optical phenomena by continua and processes. Further delineated through experimentation and the discourse of science, an antagonistic dichotomy between an often theologically sponsored atomism as against a wave-based process is found to simplify a summary of optical theorizing under the mechanistic hegemony that introduced the era of modern science.

Two historical periods are particularly examined as being of import to the modern theoretical dilemma. A re-examination of Newton's prism experiments and his subsequent theorizing utilizing a corpuscular model of light is compared with the continuum modeling developed by his contemporary, Huygens. A contrast of philosophies of science, the place accorded to modeling and hypothesis as necessary elements in the scientific method, is explored. The ultimate disadvantage experienced by the process approach to Huygens' principle that lasted over a century, is thought to accord with Whitehead's suppositions.

Much that can be recognized as important deriving from analysis of this episode is then found repeated in the later context of the development of special relativity. In contrast with the more conventional view that assumes the continuum modeling of Maxwell's field electromagnetism as sponsoring theoretical development of special relativity's "second" or "light-speed" postulate, a careful examination of source documentation reveals the predisposing model to again be Newton's atomist modeling of light. Motivation for this choice of a discrete particle modeling of radiation is traced to a perceived need to assign realism and a primary principle status to Planck's discovery of the quantum of energy in 1900. Such a sponsoring model is then recognized as having lent the notions of locality and separability to relativity theory construction as attributes by which electrodynamics could be assumed logically compatible with mechanics. Continuum and wave-like modeling, in particular the wavefront model of light as later advanced by Fresnel and Kirchhoff on the basis of Huygens' principle, is again found to have been disadvantaged.

Following a model-theory relationship as advocated by such as McMullin and Box, a model of light supporting theoretical development, but found to sponsor a proven disparity of modern theories, is recognized as being in need of either modification or replacement. Pursuing options made apparent by Weisberg's claim that incompatible and seemingly mutually exclusive depictions of light, such as waves, particles, spherical wavefronts and linear rays, are to be recognized as equally valid and useful models non-competitively embraced within a *multiple models idealization* of optical phenomena, the wavefront model of optical radiation, previously denied a supporting role in theory construction, is suggested as more appropriate.

However, although a predisposing wavefront model can be viewed as potentially supporting a relativity hypothesis reformulation without introducing the concepts of locality and separability to theory reconstruction, such a model suggests a radical difference in modes of propagation wherein propagation characteristics of light are incommensurate with those of inertial mechanics. In particular, a Huygens-based model reinterprets the "speed of light" to be the scalar electromagnetic radial inflation rate of an expanding spherical wavefront, wherein the now precisely quantified value c is defined as no more than the environment dependent expansion rate of the light complex in free space. Extension of wavefront modeling to include an interface with matter is then recognized in the extinction theorem advanced by Ewald and Oseen, wherein a light ray impinging on a dielectric boundary, and encountering a sharp change in refractive index, is extinguished and replaced by a refracted ray propagating at a rate determined exclusively by characteristics of the dielectric medium. Of equal importance, examination of Huygens' principle, in respect of the Chapman-Kolmogorov equation, as the equation of motion of Markov processes, demonstrates that refractive wavefront propagation is memoryless, wherein its velocity history is lost, instant by instant.

On the basis of both extinction of primary sources and the memorylessness of wavefront refraction, it might then be argued that electrodynamic velocity information, originating in a relatively moving inertial frame of reference, would be systematically lost when passed through any dielectric or optical element in a measurement frame of reference, and to be continually replaced with velocity information deriving solely from environmental constraints imposed by the local measurement environment. Such a distortion involved in the information flow from source to interception, is shown to be equally applicable to all electromagnetic frequencies. In practice, the pertinent information loss would be that of the relative velocity of the source frame. Such a loss would yield experimental results mathematically expressed as a Lorentz-styled transform group yet incorrectly suggesting agreement with a conventional interpretation of special relativity wherein the vacuum velocity of light in the relatively moving frame remains constant.

On such a reinterpretation, relativistic phenomena might then be viewed as an inconvenient but systematic artefact of information distortion to which any attempted physical measurement of wave-like phenomena in relative motion, and employing Markovian propagation processes, is inherently vulnerable. In respect of this, the mathematical occurrence of a Lorentzian-styled transformation formulation might then be deemed indicative of the existence of such a transmission information loss and to quantify the degree of distortion of measured parameters as would be encountered by all currently employed measurement strategies. Additionally, a more constructive reformulation of a relativity hypothesis might then proceed on the edicts of a classical Galilean interpretation of the relativity principle without necessitating the conjecture of a formal relationship such as that of a Minkowski spacetime with its cardinal review of classical spatial and temporal dimensions. On this view, a length dimension would not physically contract due to relative motion, but information about the dimension would be distorted due to refractive propagation, falsely appearing in measurement to be diminished in its direction of motion. Equally, an interval of time applicable to a system in relative inertial motion, would also falsely appear in measurement as having taken longer to transpire or as being dilated. On such a view, the so-called 'proper length' of a physical object would be its only physical length pertinent to an exact science. Equivalently, an absolute time, as determined for time as measured in the local laboratory reference frame, would be the only definition of time necessary for the pursuit of an exact science.

An Information Theory Interpretation of Relativistic Phenomena

Part 1

Chapter 1 - Conflict in the foundations of science.

According to prominent string-theorist and recent Nobel Laureate[1], David J. Gross, all is not well in the early twenty-first century world of theoretical physics. Concluding the proceedings of the prestigious 23rd Solvay Conference on the quantum structure of space and time held in Brussels during December 2005, Gross was to sound a deflationary note, admitting that, at least in respect of string theory for which he had long been a forceful advocate, “we don’t know what we are talking about,” and suggested that science may be missing something “profound.” (New Scientist 2005; Smolin 2006, p. xv) Such a pessimistic review contrasts with the exuberance and optimism that attended the emergence of prototype string hypotheses[2] in the late 1970s and early ‘80s. Initial appraisals raised expectations of a unifying theory that might correctly describe both the fundamental forces, including gravity, and all known elementary particles, thus resolving our deepest metaphysical questions about the structure and constitution of the physical world. However, such a unification carried with it a further prospect. Two of the foundational formulations[3] of 20th century science – relativity and quantum theory – had long been known to be incompatible. Co-existence despite perceived inconsistencies had not, however, crippled scientific practice. Although both sprang as scientific revolutions from the first decade of the twentieth century, each had developed somewhat independently. Seldom did their domains of application overlap and cause for conflict was minimal, with quantum theory addressing issues at the relatively ‘very small’ end of the physical spectrum where gravitational effects could usually be ignored, and relativity dealing with the ‘very large’ where quantum phenomena might equally be viewed as negligible. (Hawking 1989, p. 13; Smolin 2006, p. 5) However, difficulties increasingly arose as their realms expanded and overlapped, precipitating what some assess as a crisis in the 1970s. (Lemonick 2006) It was for the resolution of just such an impasse that the unifying potential of an emerging string theory also appeared to hold great promise. Against this backdrop, the Gross review, which typified the current status as “utter confusion,” was particularly disheartening. Three decades of work by teams of dedicated and talented physicists amounting to one of the most concerted, and certainly one of the more expensive, theoretical programs in the history of science, had been assessed as a failure.

[1] Nobel prize in physics 2004, "for the discovery of asymptotic freedom in the theory of the strong interaction,” jointly with H. David Politzer and Frank Wilczek. (Nobel Foundation 2010b)

[2] In recent usage, ‘string theory’ has become an umbrella term encompassing developments and extensions of its source ideas to include such formulations as ‘Superstring Theory” and “M-Theory.”

[3] “Relativity (both the Special and General) theories, quantum mechanics, and thermodynamics are the three major theories on which modern physics is based.” (Cresser 2003)

Nonetheless, for many involved in the program the Gross review was premature – the huge investment in time and expertise together with the need for eventual success was simply too great and too important to be allowed to fail. For others, maintaining the program constituted the only valid strategy since no unproblematic alternatives were conceded.

Gross did not however, stand alone. Oxford University's Emeritus Rouse Ball Professor of Mathematics, Roger Penrose, has maintained a tradition of his predecessors such as Edmund Halley and Baden Powell, in offering public lectures designed to narrate "*the advantages attending the prosecution of science*," and inform " *the state of public opinion respecting its claims*." (Gillow & Schlackow 2007) Within this context, it was of particular import, that, in a series of lectures delivered at Princeton University, Penrose (2003) had also declared his belief that physics was missing something fundamental. Expanding on his claim, Penrose went on to deride the more recent conceptions advanced by quantum theorists and to disdainfully dismiss string theory as no more than a "fashion" (Kruglinski 2009)

Further, publishing his controversial work "*The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next"* in 2006, theoretical physicist, Lee Smolin, hardened the Gross stance, claiming that theoretical physics had now palpably failed in its quest to understand nature at its deepest levels through the agency of a unifying theoretical framework, resulting yet again in "a crisis in physics". (Smolin 2006, p. xiii) Recalling that for more than two centuries, science had enjoyed rapid and substantially uninterrupted growth in its attempt to understand the fundamental laws of nature, he noted that today "despite our best efforts, what we know for certain about these laws is no more than we knew back in the 1970s." (p. viii) A major contributor to Smolin's disillusionment is his assessment of the almost monopolistic and consequently obstructive role played by current string theories – no longer classifiable as a single theory since "it appears to come in an infinite[4] number of versions." (p. xiv) Further, drawing on his own professional career experience, partly spent pursuing string theory, he now questions the right of the current string approach to be denoted "theory" rather than conjecture or even to continue to be accepted as good science. Also writing in 2006, quantum field theorist and Columbia University mathematician, Peter Woit independently published his equally controversial review: "*Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law,*" with much of its literary mission coinciding with Smolin's uncomplimentary appraisal. Woit had commenced voicing a dissident note as early as 2001

[4] Smolin claims that, even restricting the 'landscape' of string theories to those that have some agreement with observable facts, "we are left with as many as 10^{500} distinct string theories." (Smolin 2006, p. xiv)

complaining that string theory's "experimental situation is best described with Pauli's phrase 'it's not even wrong.'"[5] (Woit 2001, p. 6)

Another Nobel laureate and prominent particle physicist, Gerardus 't Hooft[6], in some agreement with Smolin and Woit, is also no longer "prepared to call string theory a 'theory,' rather a 'model,' or not even that: just a hunch." (Smolin, p. xv) Enshrined in the philosophy of modern science is the injunction that candidates for admission into the corpus of accepted scientific theory must demonstrate not only confirmation of their predictions by successful verification through meaningful experimentation but also a complementary vulnerability to possible falsification. For Woit, string theory's experimental situation "leads one to question whether string theory really is a scientific theory at all," since "it's a theory that cannot be falsified by any conceivable experimental result." (Woit 2001, p. 6) A similar conclusion is evident in Smolin's overview, which claims failure on both counts:

> The current crisis in particle physics springs from the fact that the theories that have gone beyond the standard model in the last thirty years fall into two categories. Some were falsifiable, and they were falsified. The rest [at least a dozen new approaches including string theories] are untested – either because they make no clean predictions or because the predictions they do make are not testable … by current – or even currently conceivable – experiments. (Smolin 2006, pp. xiii, xiv)

Beyond expressions of disappointment that the string approach has not, and may have proven itself incapable of, realizing its early expectations to provide a unifying theory of the fundamental forces and elementary particles, is the added disappointment of its consequent failure to resolve the increasingly pressing issue of incompatibility between fundamental theories. Nonetheless, both relativity and quantum theory continue to be equally acknowledged as foundations of the same modern scientific edifice. For its part, special relativity underpins modern physical constructs extending from the standard model of particle physics[7] to general relativity and astronomy. (Wolf et al. 2006, p. 2) Consequently, for philosophers of science such as Tim Maudlin of

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[5] The phrase was first coined by early theoretical physicist Wolfgang Pauli when commenting on a student's work of dubious value. It has subsequently been adopted to characterize ideas that fail to reference anything substantial or to indicate that a scientific conjecture, irrespective of its apparent basis in scientific formalism, mathematical elegance, or the sincerity of its advocacy, provides neither verification through predictions nor opportunity for attempted falsification.

[6] Nobel prize in physics 1999, "for elucidating the quantum structure of electroweak interactions in physics," jointly with Martinus J. G. Veltman (Nobel Foundation 2010a)

[7] The Standard Model in turn incorporates the theory of electromagnetic interactions together with the theories of both weak and strong interactions.

Rutgers University, the "final reconciliation of quantum theory and Relativity is a theoretical problem of the first magnitude." (Maudlin 2002, p. 1). 'Relativity,' as here cited, stands for two allied but distinct formulations which also reflect two separate stages of historical development in the early decades of last century. In principle, the 'special' or 'restricted' theory addresses issues of space, time, energy, momentum, and linear motion in a more idealized inertial world devoid of accelerations[8] wherein the speed of light in vacuum is accorded the unique role as a universal constant in its space-time configuration. On the other hand, the 'general' theory subsumes special relativity as its limiting case and extends its domain of applicability to include non-inertial phenomena such as gravity and rotations. Both the special and general formulations of relativity are conjointly and independently cited in charges of incompatibility with quantum theory. The import of the dilemma, however, is seldom brought to the notice of the general public, although it has found brief mention in popularized science such as Stephen Hawking's "*A Brief History of Time.*"

> Today scientists describe the universe in terms of two basic partial theories … the general theory of relativity describes the force of gravity and the large-scale structure of the universe … Quantum mechanics, on the other hand, deals with phenomena on extremely small scales … Unfortunately, however, these two theories are known to be inconsistent with each other – they cannot both be correct. (Hawking 1989, pp. 12-13)

Such tension between quantum mechanics and relativity theory has been characterized as manifesting in two classes of problems. One class has been summarized as being "linked with some structural problems of quantum relativistic theories, like the definition of a position operator that fulfills 'basic' requirements." (Scarani et al. 2000, p. 1) The more experimentally amenable and historically investigated impediment to theoretical accord has been denoted by Scarani as "the search for a covariant description of the measurement process." This issue has manifested as "the impossibility of a [classically constrained] causal description of the [quantum mechanical wave-function] collapse in an [Einstein-Podolsky-Rosen type] experiment that would be valid in all [reference] frames," and has as a consequence brought into debate such long-standing classical notions as "causality" and "locality," with alternative resolutions having the potential to radically alter fundamentals of our contemporary scientific worldview. For both practicing physicists[9] and other

[8] Although the domain of applicability is conservatively so restricted, "calculus can be used to apply SR in accelerated systems, as can the more advanced mathematics of differential geometry …" (Roberts & Schleif 2007)

[9] Occasionally, physicists in the course of explaining empirical results find it necessary to reiterate the fact: "According to relativity theory it's meaningless to say which measurement happened first. Some observers will say measurement A happened first while others will say measurement B came first, and

philosophers of science alike, some facets of the inter-theoretical contention are more simply stated:

> That special relativity predicts that there is generally no physical matter of fact concerning the temporal order of events provides a straightforward way of showing the incompatibility of the standard formulation of quantum mechanics and relativity. (Barrett 2003, p. 1213)

> Perhaps the deepest level at which the incompatibility between the two …most fundamental theories in physics … is manifest is in the fact that general relativity is local whereas quantum mechanics is non-local. This is reflected in the formulation of general relativity geometrically in *space-time*, whereas quantum mechanics is formulated in *Hilbert spaces* whose elements (state vectors or wave functions) have non local properties in space-time. (Anandan 2002, p. 2)

Background to the problem

Historically, fidelity to detail might more properly trace the seeds of theoretical disparity to that moment when physicist Max Planck first announced his discovery of the quantization of energy in 1900. The revolutionary character of a quantized conception lay not only in its seemingly confrontational departure from classical notions of continua but with what some perceived as the intimation of a new atomism applicable not only to energy and discrete events, but embedded in the very substructure of spatial phenomena such as fields. Such was the well-spring of an issue which early engaged the emerging younger physicist, Albert Einstein, in animated controversy with the more matured veteran Max Planck, through many of the early years of the new century. Marrying the novelty of the quantum to the otherwise intractable phenomenon of the photoelectric effect, Einstein entered the arena of emerging theorists and inaugurated what was to become a high-profile scientific career, later to be honored with a Nobel prize. Central to the Einstein approach of the time was to be a more pervasive quantum-styled atomism, not merely that of the chemical atom still rejected by positivists such as Ernst Mach, but an innate corpuscularity as the fundamental character of radiation and recalling archaic conjectures Isaac Newton had once used to characterize light. To Planck's dismay:

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they will both be equally valid viewpoints … [which] seems to bring into question some of our most fundamental views about causality. (Felder 1999, pp. 9, 10) "Relativity teaches us that if two space-time events are separated so that they cannot be connected by any signal traveling at *c* or less, then different observers will disagree as to which of the two events came first." (Steinberg 2000)

> According to the latest considerations of Mr. Einstein, it would be necessary to conceive the free radiation of vacuum, and thus the light waves themselves, as atomistically constituted, and hence to give up Maxwell's equations. (Planck et al. 1909, p. 395)

Seemingly irreconcilable concepts of the essential constitution of radiation were, then, the first grounds upon which contention between an emerging quantum-based approach and a hard-won classical formulation were to become evident. Neither Planck's quantum of action nor Maxwell's electromagnetic continuum would capitulate, with each consolidating its status through ever increasing empirical ratification. With time, resolution of an intolerable inconsistency was broached by synthesis and presented as a "wave-particle duality." The continuing potential for antagonism, nonetheless still implicit in the formula, was addressed by invoking the edict of complementarity. In essence and in practice, this principle, as Niels Bohr proposed it,[10] claimed that although an object may exist with multiple seemingly contradictory and thus mutually exclusive properties, experiment can only give rise to the exhibition of one such property at any one time. For measurements of radiation, different protocols would reveal it as having either a wave-like or a particle-like property, but never simultaneously. (Bohr 1966, pp. 1233-1234) Nevertheless, for some, such a proffered solution amounted to little more than a restatement of the problem in the form of a strategic compromise or convenient artifice rather than enjoining effective explanation. Later, and flowing from the advent of the laser in the 1960's, new terminology, such as "photonics" gained employment to engage with new fields of research and optical application such as information processing and telecommunications, traditionally the domains of electronics. In this more recent incarnation, such optical terminology is deemed to denote that photons, although the quantum unit of radiant energy, are neither exactly particle nor wave but exhibit characteristics of both.

The "EPR" polemic

Although for both the educated layman and academic non-specialist alike, the apparent contradiction of equally valid but incompatible exhibitions of basic phenomena may have appeared settled, a further cause for controversy arose in 1935. It was specifically the quantum-based approach, now ripening into contemporary quantum theory, that was subjected to inquisition primarily incited by Boris Podolsky at the Institute for Advanced Study in Princeton. Ironically it was to be Albert Einstein, lauded

[10] At the International Physical Congress held in September 1927, at Como, Bohr "advocated a point of view conveniently termed 'complementarity,' suited to embrace the characteristic features of individuality of quantum phenomena, and at the same time to clarify the peculiar aspects of the observational problem in the field of experience." (Bohr 1966, pp. 1233-1234)

the champion of the quantum-styled solution, who, with Nathan Rosen, was to be drawn into collaboration with Podolsky. Together they called upon a maturing quantum theory to vindicate its claim to be a complete description of reality and challenged it to clarify its apparently non-classical approach to such fundamentals as causality and locality. (Einstein, Podolsky & Rosen 1935) Subsequently recognized as a significant document in the history and philosophy of science, and often cited as the "EPR Paradox," their paper was to instigate vigorous debate and extensive experimentation ongoing to the present time. For his own part, Einstein's eventual participation in the EPR challenge was elicited by more than one predisposing concern. As Barrett explains:

> Einstein certainly held the collapse dynamics [formulation of quantum mechanics] to be philosophically objectionable, but he also had straightforward physical worries concerning the relationship between the standard formulation of quantum mechanics and relativity. As early as 1927, Einstein complained that the collapse dynamics and the essential use of configuration space to represent the states of entangled systems in quantum mechanics implied, to his mind, "a contradiction with the postulate of relativity." (Barrett 2003, p. 1210)

The standard wave-function collapse formulation of quantum mechanics,[11] fielded by physicist Paul Dirac and mathematician John von Neumann in the early 1930s, attempted to unify such disparate theoretical proposals as Louis De Broglie's wave mechanics of matter, (de Broglie 1929), Werner Heisenberg's uncertainty (or 'indeterminacy') principle[12] (Heisenberg 1956, 1966), Erwin Schrödinger's wave equation (Schroedinger 1966) and Max Born's statistical interpretations (Born 1964, 1966), and still retains strong support by modern physicists although contested by many alternatives. (Barrett 2003, p. 1207) Central to collapse formulations is that, left to itself, an inertial system will evolve in a mode both linear and deterministic. However, any attempt to evaluate a property of the system by any measurement process[13] must of necessity interact with, and consequently disturb, the system. Such an interactive intrusion causes the system, initially describable in terms of a wave function

[11] The standard model is to be distinguished from the "Copenhagen" interpretation of QM, which primarily represents the first general attempt by Bohr, Heisenberg, Born and other quantum pioneers, to understand the world of atoms using a quantum mechanical approach. "The term is rather a label introduced by people opposing Bohr's idea of complementarity, to identify what they saw as the common features behind the Bohr-Heisenberg interpretation as it emerged in the late 1920s." (Faye 2008)
[12] The principle requires that a certain uncertainty in any pair of conjugate variables – such as location and momentum - is ultimately irreducible and obeys an inverse relation such that the more precisely one variable is determined, the less precisely its conjugate variable can be known.
[13] According to Maudlin, "the most useful way to characterize interpretations of quantum theory is by their strategy for solving the measurement problem." (Maudlin 2000, p. 875)

superposition with indeterminate observables,[14] to "instantaneously and nonlinearly jump to an eigenstate of the observable being measured (a state where the system has a determinate value of the property being measured)" (Barrett, p. 1207; Dirac 1958, pp. 3, 36) Although quantum theory dictates a limited number of possible eigenstates as solutions, the one assumed is randomly selected at any individual event, but nonetheless equally conforms to a definite probability calculable for assuming that state. (Dirac 1958, p. 6) Also entailed in the 'fine print' of the standard formula, and also ensuing from Born's statistical interpretation of quantum mechanical states, is the claim of 'state completeness' which insists that the standard quantum-mechanical state of a system provides a complete physical description of the system. (Barrett, p. 1208) For Einstein however, resistance to Born's statistical interpretation gave rise to his often quoted retort that "God does not play at dice"[15] and in written response, Einstein remarked that,

> … my highly esteemed colleagues Born, Pauli, Heitler, Bohr, and Margenau … are all firmly convinced that the riddle of the double nature of all corpuscles (corpuscular and undulatory character) has in essence found its final solution in the statistical quantum theory … I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this [theory] operates with an incomplete description of physical systems. (Einstein 1949c, pp. 633-688)

It was predominantly this contention, that the description of reality as then dispensed by quantum theory was incomplete, that fuelled the EPR debate. The case was primarily prosecuted by a thought experiment exploiting the observation that an edict of quantum mechanics dictates that not all the classically definable observables of a system can be quantified simultaneously, either experimentally by improved precision, or in principle.[16] (Einstein, Podolsky & Rosen 1935, pp. 777-778) Discussion detailing their supporting argument then examined two initially interacting systems after their spatial

[14] Dirac distinguished between objective properties in a classical sense and observables in the quantum sense. "We call a real dynamical variable whose eigenstates form a complete set an *observable*. Thus any quantity that can be measured is an observable." (Dirac 1958, p. 37)

[15] "On his side, Einstein mockingly asked us whether we could really believe that the providential authorities took recourse to dice-playing ('… *ob der liebe Gott würfelt'*)" (Bohr 1966, p. 1240) (Pais 1982, p. 440)

[16] In general, "it is shown in quantum mechanics that, if the operators corresponding to two physical quantities, say A and B, do not commute, that is if $AB \neq BA$, then the precise knowledge of one of them precludes such a knowledge of the other. Furthermore, any attempt to determine the latter experimentally will alter the state of the system in such a way as to destroy the knowledge of the first." (Einstein, Podolsky & Rosen 1935, p. 778) Sets of observables which give qualitatively different, but nonetheless complete descriptions of a quantum mechanical system are said to be sets of "good quantum numbers" and also are denoted "maximal sets of commuting observables." Observables from different sets are denoted "non-commuting observables." Position and momentum are continuous non-commuting observables whereas the quantum mechanical 'spin' along each of three orthogonal axes is a set of discrete mutually non-commuting observables. (Blanton & Chase 1993)

separation and at a time when "we suppose that there is no longer any interaction between the parts," and claimed that a QM derived analysis of the final state could be demonstrated to be paradoxical in a way "no reasonable definition of reality could be expected to permit." (ibid., p. 780) A phenomenon, which had already been encountered[17] and which was later termed "quantum entanglement," portrayed the spatially separated systems as nonetheless still described by the self-same Schrödinger wave equation as had described the system from which they derived, and that thus still causally connected them. (Maudlin 2002, p. 162) Further, measurements of observables in the subsequently separated systems honored the quantum edicts of the parent superposition as remaining applicable – a measurement of one observable in one system would determine the measurement of the corresponding observable of the other system even where the systems had migrated outside of each others light cones and were deemed relativistically space-like separated. In part and more simplistically presented, the EPR claim to paradox[18] sought to exploit the QM prohibition of simultaneous knowledge of more than one of a pair of mutually non-commuting observables. In EPR defined terms, measurement of one non-commuting observable of system I must determine exact, and physically real knowledge of the corresponding observable for its remote entangled counterpart in system II although such knowledge was gained without physical intrusion or disturbance of system II.[19] The measurement of a complementary non-commuting observable in system I should then equally permit determination its counterpart in system II, thus revealing exact, and real, determinations of both mutually non-commuting observables simultaneously but contrary to the edicts of QM. (Pais 1982, pp. 455-456) Any attempted EPR remedy, however, had to overcome an apparently formidable difficulty. Exhibition of the entangled state appeared insensitive to the extent of spatial separation. Apparently, measurements in one system could enforce seemingly 'instantaneous' effects in a system so far spatially separated that information transmitted by the event, if deemed necessary as cause of the remote effect, would violate the universal velocity constraint imposed by the classical Einstein-Minkowski space-time formalism of special relativity. In pursuing their claim to have exposed a theoretical incompleteness in the received formalism of QM, the EPR authors "took it for granted … that the apparent nonlocality of quantum mechanics must be

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[17] The phenomenon was first reported in 1926 by Erwin Schrödinger who later coined it 'entangled' in his initial response to the EPR paper: "By [their] interaction the two representatives [the quantum states] have become entangled." (Schrödinger 1935, p. 555)

[18] As Pais notes: "The content of this paper has been referred to … as the Einstein-Podolsky-Rosen paradox. It should be stressed that this paper contains neither paradox nor any flaw of logic. It simply concludes that objective reality [as Einstein defined it] is incompatible with the assumption that quantum mechanics is complete." (Pais 1982, p. 456)

[19] The EPR authors held it as a necessary requirement that for a complete theory, "every element of the physical reality must have a counterpart in the physical theory." (Einstein, Podolsky & Rosen 1935, p. 777)

apparent only, that it must be some kind of mathematical anomaly or notational infelicity or, at any rate, that it must be a disposable artifact of the algorithm." (Albert & Galchen 2009, p. 29) Provisional resolution then postulated the existence of additional as yet unknown or, as elsewhere termed, "hidden," variables of the quantum theory which might logically account for the inconsistency and restore a more classically desirable realism to its physical explanation. Such conjectured EPR variables could then not only render QM conventionally complete as a realist theory but also "restore to the theory causality and locality" in terms compliant to relativistic doctrine. (Bell 1964, p. 195)

Rather than an embarrassment to be explained away, the entanglement phenomenon has more recently flowered as a novel and exploitable resource, giving birth to practical applications of quantum information exchange[20] such as quantum computing,[21] (Monroe & Wineland 2008) cryptography, (Bub 2004) and quantum key distribution systems, (Gisin 2009) 'dense' and 'superdense coding',[22] (Bennett & Wiesner 1992; Mattle et al. 1996) and the potential for exotics such as 'quantum teleportation,' (Bennett et al. 1993) and even a 'quantum telephone exchange.' (Bose 1999) More recently, the nonlocal correlations of quantum entanglement have been utilized to create a secure random number generator in a rigorous but classically impossible form "and possible in quantum systems only if certified by a Bell inequality violation." (Pironio et al. 2010, pp. 1021-1024) However, quantum entanglement and particularly the nonlocal effects it appeared to propose were to polarize the earlier physics community. Whereas Schrödinger praised it as "*the* characteristic trait of quantum physics, the one that enforces its entire departure from classical lines of thought" (Penrose 1998, p. 1927; Schrödinger 1935, p. 555) Einstein's opinion became antagonistic, deriding what he later called *spukhafte Fernwirkungen* – 'spooky actions at a distance,' (Buhrman, Cleve & Van Dam 2001, p. 1829) and demanding fundamental tenets of classical locality and separability remain sacrosanct:

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[20] Quantum information, as physical information held in the 'state' of a quantum system, is to be distinguished from classical digital, or discrete, information. Quantum information can be quantitatively evaluated in 'qbits,' employing the "Von Neumann" entropy as an analogue of the classical "Shannon" entropy, conceived by Claude E. Shannon in 1948, in which the 'bit' (binary digit) is the unit.

[21] "The first small-scale device to perform all the functions required in large-scale ion-based quantum processing [has been claimed by] researchers based at the US National Institute of Standards and Technology (NIST) in Boulder, Colorado [and] hailed … as an important breakthrough in quantum computing." (Dacey 2009)

[22] Whereas transmission of more than one 'bit' (binary digit) of information classically involves more than one two-state element, Mattle *et al*. demonstrated the transmission of one of three messages, i.e. a 'trit' (equivalent to ≈ 1.58 classical bits) by manipulating only one of two entangled states, and proved an increase in information channel efficiency by transmitting ASCII characters in five trits instead of the conventional 8 bits. (Mattle et al. 1996) "Superdense" coding further improves information channel efficiency by doubling the classical limit, with two bits encoded on one 'qbit' (e.g. one spin-1/2 quantum particle) with the aid of quantum entanglement. (Bennett & Wiesner 1992)

> But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former. (Einstein 1949a, p. 85)

> An essential aspect of … things in physics is that they lay claim, at a certain time, to an existence independent of one another, provided these objects 'are situated in different parts of space'. Unless one makes this kind of assumption … physical thinking in the familiar sense would not be possible. It is also hard to see any way of formulating and testing the laws of physics unless one makes a clear distinction of this kind …
> The following idea characterizes the relative independence of objects far apart in space (A and B): external influence on A has no direct influence on B; this is known as the 'principle of contiguity' … If this axiom were to be completely abolished, the idea of the existence of (quasi-) enclosed systems, and thereby the postulation of laws which can be checked empirically in the accepted sense, would become impossible. (Einstein, Born & Born 1971, pp. 170-171)

Elsewhere, Einstein more clearly distinguished between what might better be described as the principle of separability proper, as that requirement by virtue of which distinct statements are possible about the independent real states of separated objects. Locality then dictates a constraint falling within its purview, wherein the real state of one system must remain unaffected by changes occurring in a separated system. (Timpson & Brown 2003, p. 7) In the context of discussing "that we should try to hold on to [such concepts of] physical reality," Einstein made the distinction, and his objections to quantum theory, more accessible:

> When a system in physics extends over the parts of space A *and* B, then that which exists in B should somehow exist independently of that which exists in A. That which really exists in B should therefore not depend on what kind of measurement is carried out in part of space A; it should also be independent of whether or not any measurement at all is carried out in space A. If one adheres to this programme, one can hardly consider the quantum-theoretical description as a complete representation of the physically real. (Einstein, Born & Born 1971, p. 164)

Bell's theorem and the empirical 'inequality' test

Although an extensive literature eventually developed claiming to demonstrate the inconsistency of local realistic theories with the statistical predictions of quantum mechanics, it was to be 1964 before theoretical physicist John Stewart Bell of the European Laboratory for Particle Physics (CERN), proposed an approach generally acclaimed as capable of resolving the impasse. (Bell 1964; Clauser et al. 1969) Bell's strategy was in turn inspired in part by a re-interpretation of quantum theory proposed by physicist David Bohm in which he had pursued a 'hidden variables' concept. (Bohm 1952a, 1952b) Although the EPR paper had spread its enquiry to include issues of realism and theoretical completeness, for Bell, the predominant, and more experimentally tractable, problem was clear:

> It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty. (Bell 1964, p. 195)

Creating a formalism that has become known as "Bell's Theorem," the notion of additional 'hidden' variables required by a local realism as advanced as an EPR solution, was mathematically systematized and "shown to be incompatible with the statistical predictions of quantum mechanics … [for any] theory which reproduces exactly the quantum mechanical predictions." (Bell 1964, p. 195) In practice, Bell demonstrated that, if EPR-styled supplementary parameters, or 'hidden' variables, are present, statistically averaged results collected from an appropriate measurement regimen must comply with an algebraic inequality derived from the theorem, now termed "Bell's Inequality," and thus, at least in principle, permit adjudication by experiment.[23] On the other hand, although not proving or even directly addressing claims made for quantum theory, empirical violation of that inequality would lend support to the non-classical correlations purported for quantum entanglement and reinforce its apparent tension with relativistic locality. In his conclusion Bell claimed that, "for at least one quantum mechanical state, the 'singlet' state … the statistical predictions of quantum mechanics are incompatible with separable predetermination," (ibid., p. 199) and are thus inconsistent with EPR separability and locality assumptions. However, missing from Bell's proposal was a means of practical implementation, and although no methodology was suggested, Bell claimed that his theorem "had the advantage that it requires little imagination to envisage the measurements involved actually being made." (ibid., p. 199) Further, to be valid, Bell insisted that any proposed experimental procedures must include a provision noted by David Bohm and Yakir Aharonov (1957, p. 1070) wherein experimental settings should be changed during the flight of test particles. Writing in 1969, John Clauser and his associates generalized

[23] Evaluation of Bell's expression for the class of local realist theories must be ≤ 2, whereas the quantum mechanical expectation should be experimentally distinguishable at $2\sqrt{2}$. (≈ 2.828)

Bell's theorem - the Clauser-Horne-Shimony-Holt (CHSH) inequalities[24] (Clauser et al. 1969) – in a more experimentally realizable form that subsequently fostered a range of experimental approaches during the next decade. [25] Included were "polarization correlation measurements of light emitted from calcium or mercury atoms … electron-positron (positronium) annihilation and spin angular momentum correlation of pairs of protons …" (Robinson 1983, pp. 40-41) Although pioneering experimental strategies as used at Berkeley, Harvard, and Texas A&M, were analytically limited and far from ideal, most empirical results were assessed as providing evidence in favor of quantum mechanics. (Aspect 1999, p. 189) However, what were to become known as 'loopholes,' disputing the practical integrity of these approaches, were to further mar their unambiguous interpretation and uncritical acceptance. A range of pragmatic impediments to certainty that became increasingly evident included the inefficiency of photodetectors, the limitation imposed by single-channel polarizers together with polarization analyzers that imperfectly passed or blocked polarized light, together with the finite size of optical devices and the divergence of light beams. (Robinson 1982, p. 435)

The "Loopholes" problem

Following some encouraging preliminary efforts, (Aspect, Grangier & Roger 1981, 1982) the use of an improved laser and optical technology, and a more stringent approach specifically configured to investigate the Einstein-Podolsky-Rosen-Bohm deliberations and exploit the utility of the CHSH inequalities, found its first practical expression in work conducted by Alain Aspect and his associates of the Institute of Theoretical and Applied Optics at the University of Paris, Orsay, in 1982. (Aspect, Dalibard & Roger 1982) Utilizing time-varying angled analyzers to emphasize practical non-conformity to EPR locality and causality expectations, Aspect claimed that the "results are in good agreement with quantum mechanical predictions" but most importantly "violate Bell's inequalities by 5 standard deviations."[26] (ibid., p. 1804) In

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[24] The CHSH "inequality stated that a certain sum S must lie between -1 and 0. The maximum value of S allowed by quantum mechanics occurs when the polarization analyzers have orientations such that the angle between them is 22.5 or 67.5 degrees. The theoretical quantum mechanical value for S is then 0.112. The value observed by Aspect and his colleagues is 0.101 + 0.020. This is five standard deviations away from the limit imposed by realistic, local theories." (Robinson 1983, pp. 40-41)

[25] "Many different versions and cases, with family resemblances, were inspired by the 1964 paper and are subsumed under the [terminology], 'Bell's Theorem' being the collective name for the entire family." (Shimony 2005 intro.) However, differing versions of Bell's Theorem, employing separate sets of test premises and individually sponsoring a derived Bell-type inequality, each incorporate some 'locality' condition. Those versions which consider deterministic local realist theories "employ as a locality condition the requirement warranted by relativity theory, that superluminal signals not be possible." (Jarrett 1984, p. 569)

[26] In terms of statistical significance, "[a]s a rule of thumb, a [result] that is 5σ [standard deviations] (or more) from zero is difficult or impossible to ignore …" (Roberts & Schleif 2007)

particular, whereas previous attempts had been unable to eliminate the possibility of unrecognized signals passing between parts of the experimental setup as being responsible for the nonclassical results, the Aspect protocol was hailed as having closed this significant loophole "for all practical purposes." (Robinson 1983, pp. 40-41)

Jubilation was however to be short-lived. When publishing his deflationary critique shortly after, James Franson was to claim:

> The most recent experiment by Aspect, Dalibard, and Roger does not appear to rule out a class of theories in which the outcome of an event is not determined until some time after its occurrence. This class of theories includes not only the quantum theory but various local, realistic theories as well. (Franson 1985 abs.)

Central to the Franson 'loophole' was a practical demonstration of the quantum mechanical 'measurement problem.' According to the Franson critique, when measuring time delays between photon pair detections, Aspect and his associates had estimated such times on a more classical formulation based on photon speed and their supposed localized distance between polarizers and detectors. Franson noted that quantum theory forbids making such assumptions as to the specific location of quantum particles between detections. The subsequent history of EPR related experimentation and its quest for unimpeachable proof of a Bell's inequality violation was consequently dictated by significant research efforts to discover, theoretically evaluate, quantify, experimentally minimize, and ultimately to close such loopholes, of which the inefficiency or "fair sampling"[27] problem loomed large.

Closing the "Detection" loophole

In 1987, a quantum efficiency for detectors in excess of 83% was claimed necessary to validate experimental data (Garg & Mermin 1987, p. 3831), and thus appeared to prorogue most existing results which could claim only 5-30% efficiency. Further experimental efforts continued to confirm the non-classical claims of quantum entanglement with improving precision, such that a 28 standard deviation from CHSH classical expectations was again reported in 1995, although of necessity such reports now routinely invoked a cautionary disclaimer imposed by the "fair sampling"

[27] "The fair sampling assumption … is essential in all the optical Bell tests performed so far for linking the results of polarization or direction analysis, which are not directly observable, with detection rates, which are observable. The absence of an experimental confirmation of the fair sampling assumption, together with the difficulty of testing Bell's Inequality without this assumption or another one equally remote from confirmation is known as the 'detection loophole' in the refutation of Local Realistic Theories, and is the source of scepticism about the definitiveness of the experiments." (Shimony 2005)

assumption. (Chiao, Kwait & Steinberg, p. 5) Beyond the obvious need to improve detector technology, this class of "detection loopholes" was subsequently broached by such inventive strategies as incorporating detector inefficiency into the Bell's inequality algebraic formalism, (Larsson 1998) and improving detectability by use of an "ultra-bright' source "orders of magnitude brighter" than previously employed.[28] (Kwiat et al. 1999 abs.)

Commenting on the detection loophole problem, Brown (1999b) stressed its disproportionately large philosophical import as compared with its minuscule experimental probability. Brown's assessment noted that:

> … all EPR tests to date have relied on relatively low detection efficiencies, leaving open the theoretical possibility that the data is systematically biased in just such a way as to mimic the predictions of quantum mechanics. …But [analysis] shows that in order to match a particular set of quantum-mechanical result[s] we not only need to invoke a systematic bias in the measurement efficiencies, we need an *asymmetric* systematic bias, i.e., a bias that works on only one of the two particles in each coupled pair [exhibiting] a very specific asymmetry, i.e., absolute perfection on one arm of the experiment and just the right amount of imperfection on the other arm to match the predictions of quantum mechanics. (Brown 1999b)

Such an infinitesimally small probability was succinctly dismissed by John Gribbin in his popular book "*In Search of Schrödinger's Cat*," with the observation that "… it does seem like the height of desperation to make that argument." (cited in Brown 1999b) Nonetheless, its importance loomed disproportionately large when its potential role as an arbitrator in debates questioning such theoretical fundamentals as causality and locality were to be taken into account.

Although optimistic researchers such as Aspect predicted that any remaining ambivalence due to limited detector efficiency, "can be closed by a technological advance that seems plausible in the foreseeable future" (Aspect 1999, p. 190), in the event, a non-optical experimental approach measuring correlation in the classical properties of massive entangled $^{9}Be^{+}$ ions (Rowe et al. 2001), claimed priority in actually closing the 'detection' loophole through higher efficiency. Registering a 2.25 ± 0.03 CHSH inequality breach of the classical expectation of ≤ 2 (ibid., p. 791), Rowe *et al*. claimed in their concluding summary that the "detection efficiency was high enough

[28] Kwiat and associates claimed a 242 standard deviation violation of Bell's inequalities obtained in less than three minutes. (Kwiat et al. 1999 abs.)

[at about 88%], for a Bell's inequality to be violated without requiring the assumption of fair sampling, thereby closing the detection loophole in this experiment." Nonetheless, although the "ions were separated by a distance large enough that no known interaction could affect the results … the lightcone loophole remains open here." (ibid., p. 793)

Closing the "Locality" or "Lightcone" loophole

On the other hand, attempted resolution of the class of "communication (locality or 'lightcone') loopholes" was to be vigorously prosecuted via third generation experiments of ever increasing sophistication following on from Aspect's 1982 claims and their subsequent critical re-evaluation. The Aspect approach had relied in part on a rapid switching between analyzer paths to create events with effective space-like separation but was shown vulnerable to a variety of criticisms including non-randomness of the switching times.[29] In response, subsequent experiments were to exploit the potential for increasing the real spatial separation of source and detectors. Utilizing an optical fiber Franson-compliant 4.3 km separation with correlated photon pairs at Malvern in 1994, observed fibre interferometer interference visibilities of 86% readily surpassed the classical expectation maximum of 70.7% (= 1/√2), and were hailed as positively confirming quantum-mechanical expectations. (Tapster, Rarity & Owens) An ensuing experimental adaptation in Geneva and surrounds and published in 1998 (Tittel et al.) further extended the spatial separation. This Franson-compliant approach utilized energy and time entangled 1310 nm wavelength photons sent into a telecom fiber network with all-fiber interferometer detectors located 10.9 km apart. The observed two-photon fringe visibility of up to 81.6% again easily surpassed the 70.7% required to infer a Bell's inequality infraction. However, Tittel *et al*. noted that "neither this nor any of the previous [optical] experiments can close the detection loophole," acknowledging that fair sampling had still not been successfully proven in such purely optical configurations.

However, publishing shortly after, Weihs et al. (1998) claimed to have fully enforced strict Bell-Bohm-Franson requirements claiming that "for the first time any mutual influence between the two observations is excluded within the realm of [EPR] locality." In an optical fibre experiment laid out across the Innsbruck University science campus, their equipment examined polarization entangled photon pairs and incorporated a spatial separation of 400m between independent observer stations. Incorporated were

[29] "Aspect et al. … used periodic sinusoidal switching, which is predictable into the future. Thus communication … even at the speed of light could in principle explain the results .." (Weihs et al. 1998, p. 1)

ultra-fast but random analyzer settings, and completely independent data registration, in a concerted effort to minimize recognized loophole issues. Again the results confirmed quantum theoretical predictions recording a 30 standard deviation infringement of the CHSH inequality for 14,700 coincidence events. Nonetheless, their report included the caution that "however unlikely, local realistic or semi-classical interpretations are still possible," in that the assumption was still required that the sample of pairs registered by their optical configuration was a faithful representation of the complete emitted ensemble.

More recently however, the validity of Bell's proposal has again been questioned. A further 'loophole' claimed that Bell assumed that speculated hidden parameters "do not depend on time and are governed by a single probability measure independent of the analyzer settings." Existence of such an inadequacy claimed to "not permit Bell-type proofs to go forward." The claim further suggested that an expanded set of hidden variables with a larger variety of their probability densities would perfectly fulfill the EPR locality conditions. (Hess & Phillip 2001 abs.) In response however, Gill *et al*. have claimed to demonstrate that such a model is not only nonlocal when measured against EPR requirements, but also fails to support the crucial element of an experimenter's free choice of experimental settings as intrinsic to Bell's theorem. (Gill et al. 2002)

Infringement of Bell and Bell-based inequalities have now been recorded for a diverse range of systems involving photons, protons, *K* mesons, ions, neutrons, *B* mesons, heterogeneous atom-photon systems and atomic ensembles, and is now routinely incorporated into practical quantum-based technologies. However, no experiment to date has simultaneously closed both loopholes. Nonetheless, many proposals exist for doing so, with a promising protocol taking advantage of properties of both photons and trapped ions within the one experiment. (Matsukevich et al. 2008)

Measuring the 'speed of quantum correlations'

Given that both the locality and detection loopholes have independently been proclaimed "closed," and given that such breaches of Bell-conformal inequalities exclude a common-cause explanation of nonlocal effects and deny classically-based communication, attention has moved to the possibility of quantifying a 'speed' for what became variously termed, "quantum information", or "superluminal influences", or the perhaps more popular "spooky-action-at-a-distance." In their analysis of 10.6 km separated correlations of entangled photons performed between two telecom stations at

Bellevue and Bernex, near Geneva, Zbinden *et al*. (2000a; 2000b), claimed priority for a new kind of experimental investigation of the tension between quantum nonlocality and relativity. Focusing on the wave-packet collapse engendered by the interferometer detection events, and unique to their investigative configuration, the two detection stations were set in relative motion such that each detector, in its own inertial reference frame, could claim to be the first to perform the measurement. (Zbinden et al. 2000b) Achieving an exceptional precision of symmetry for the photon paths,[30] Zbinden *et al*. estimated a lower bound on a hypothetical "'speed of quantum information' to 0.66 x 10^7 and 1.5 x 10^4 times the speed of light in the Geneva and the background radiation reference frames, respectively." (ibid. abs.) In extending the model, the subsequent Scarani review (Scarani et al. 2000) hypothesized a preferred reference frame model[31] and recommended the ~2.7°K cosmic microwave background radiation (CMB) as a "good candidate." (ibid., p. 2) In agreement with the Zbinder team's more general frame estimate, the Scarani model also determined a lower bound for a quantum information interchange as 1.5 x 10^4 *c*. Such conjectured quantifications do not however guarantee either the existence of the entity in question or a finite propagation rate, since restricted experimental accuracy precludes investigating perfect simultaneity. In justifying their approach, Scarani notes,

> … if two events are space-like separated, there is always a frame [of reference] in which the two events are simultaneous … and a family of frames in which the ordering of the arrivals is inversed with respect to the laboratory frame. Therefore the supposed "superluminal influence" is a real physical process only in a preferred-frame theory. (Scarani et al. 2000, p. 2)

In a more recent effort to determine whether entangled correlations are mediated by a superluminal information exchange, Salart et al. (2008a) also conceded that in order to render the exercise experimentally feasible, "this hypothetical influence … would need to be defined in some universal privileged reference frame." (ibid., p. 861) Again adopting the "speed of quantum information" terminology to distinguish it from classical signaling, their report further warned that "this is only the speed of a hypothetical influence and that our result casts very serious doubts on its existence." (ibid., p. 861) Acting as a giant Franson interferometer, experimentally suitable 'energy-

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[30] "… the two photons arrive at the detectors within a time interval of less than 5 ps [5.0 x 10^{-12} seconds]" which corresponds to a difference in path length of less than 1 mm in more than 10 km. (Zbinden et al. 2000a)

[31] Historically , the conjectured 'luminiferous aether' fulfilled the role of "preferred frame" in the 19th century until its abandonment both in response to lack of empirical support and under terms imposed by special relativity theory. Scarani *et al*. admit that their speculation of a preferred frame "is still an intellectual tool (or trick)," (Scarani et al. 2000, p. 1) although the CMB is now regularly proposed elsewhere for this purpose. (e.g. Braxmaier et al. 2002)

time' entangled photon pairs, created at a Geneva laboratory using a standard parametric down-conversion process, were split, and sent separately through the Swisscom fibre optic network on a nearly east-west terrestrial orientation. With one sent to the village of Jussy, 10.7 km to the east of Geneva using 17.5 km of optical fibre, the other went to the village of Satigny, 8.2 km to the west of Geneva, and 18.0 km from Jussy, using a 13.4 km fibre supplemented with an additional 4.1 km coil to ensure exact symmetry. (Salart et al. 2008b) An interference visibility of 87.6% ± 1.1%, indicating a substantial CHSH inequality breach, was maintained and observed so as to cover a complete 24 hour period. In quantifying a lower bound propagation speed required to achieve the observed correlations, a 'spooky action' of greater than 10,000 times the speed of light was calculated relative to a postulated universal reference frame.

In a follow-up elaboration of the same experiment, an additional potential loophole was also closed, insofar as different interpretations of quantum mechanics pose different answers to the question as to when a quantum measurement is actually finished. Whereas the more common view suggests the detector absorption events as conclusive, others have required closure 'when the result is secured in a classical system,' or 'when the information is in the environment' or the view independently proposed by Penrose (1996) and Diósi, (1987) in which an assumed connection between quantum measurement and gravity would require the movement of some tangible mass. In acceding to the more stringent Penrose-Diósi criterion, Salart *et al*. observed a 96.7% ± 1.4% (net) interferometer visibility,[32] but also claimed that their timing[33] ensured space like separation from the moment a photon entered its interferometer until its detection triggered a piezoelectric actuator displacing a 2mg gold-surfaced mirror. (Salart et al. 2008c, p. 220404)

Superluminality and quantum tunneling

The modus operandi of entangled quantum states is not, however, alone in suggesting superluminal quantum phenomena. "Tunneling" is one of the equally nonclassical consequences of quantum mechanics arising from the wave nature of electrons. The phenomenon occurs when electrons are transported through spaces otherwise forbidden by classical physics due to a potential barrier. Quantum tunneling gained its place in the history of science as one of the earliest triumphs of quantum

[32] Equivalent to a 2.74 ± 0.04 breaching of Bell inequalities by 18 standard deviations.
[33] "the total time [conservatively required to move the 'mass'] is … 7.1 μs, almost 1 order of magnitude shorter than the 60 μs the light needs to cover the 18 km between the receiving stations." (Salart et al. 2008c pp. 220404-2)

mechanics, when by 1928 physicist George Gamow and mathematician Nikolai Kochin had successfully explained the phenomenon of alpha decay[34] – the spontaneous, and classically inexplicable, emergence of an energetic helium-4 nucleus – from certain heavy radioactive nuclei such as radium. The Josephson effect in solid state physics,[35] fusion in nuclear physics, and instantons[36] in high energy physics are all deemed variants of the same phenomenon. Whereas Schrödinger had praised quantum entanglement, quantum tunneling was to find high favor with physicists such as Günter Nimtz of the University of Cologne, who is reported as saying: "In my opinion, tunneling is the most important physical process, because we have it in radioactivity and we have it in nuclear fusion [as in the sun]." (Gomm 2007) Nonetheless, due to momentum in the barrier region being deemed imaginary, the issue of the barrier traversal time became controversial[37] and led to much conflicting theoretical work, (Chiao, Kwait & Steinberg 1995, p. 11; Steinberg 1995, p. 2405) with yet no definitive solution or universal consensus. (Winful 2007b) Addressing the question in 1955, Eugene Wigner and his student Leonard Eisenbud, concluded that, under certain circumstances, the transit time reaches a constant value that is independent of the width of the barrier, a phenomenon later termed the "Hartman effect,"[38] with the classically disturbing implication that, for suitably wide barriers, the transit velocity could appear faster than the vacuum velocity of light. (Cramer 1995) Although such apparent superluminality has found theoretical rejection by such as Winful (2007b), experimental confirmation of the superluminal prediction was claimed by various researchers in the early 1990s including Enders and Nimtz who contended that:

> … with increasing length *a* of the evanescent region, the group velocity extrapolated for this region exceeds the velocity of light. The traveling through an evanescent region appears to be done in zero time … evanescent electromagnetic waves propagate superluminally in opaque regions since the traversal time is independent of the region's length. (Enders & Nimtz 1993, p. 633)

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[34] Ronald W. Gurney and Edward U. Condon independently solved the same problem and also published in 1928.

[35] "The tunneling of a pair of electrons between superconductors separated by an insulating barrier was first discovered by Brian Josephson in 1962 [when he] discovered that if two superconducting metals were separated by a thin insulating barrier such as an oxide layer 10 to 20 angstroms thick, it is possible for electron pairs to pass through the barrier without resistance." (Dull & Kerchner 1994)

[36] Instantons are topologically nontrivial solutions of Yang-Mills equations that turned out to be localized in space-time, prompting their alternate designation *pseudoparticles* (Vandoren & van Nieuwenhuizen 2008)

[37] "Characteristic of the discussion of the [Faster-Than-Light] tunneling experiments is that the experimental results are relatively uncontroversial – it is their interpretation that the debate is about." (Pössel 2001)

[38] "…the Hartman effect [named for work published in 1962 by T. E. Hartman] is that the tunneling time of the photons becomes independent of barrier length in the limit of opaque barriers." (Winful 2007b)

In their pioneering attempt at a traversal velocity quantification, Steinberg and associates measured the time delay for a photon wave packet to tunnel through a multilayered dielectric mirror, composed of quarter-wave layers of alternating high and low-index of refraction materials. Alternating titanium oxide (index 2.22) with fused silica (index 1.41) to a thickness of 1.1 μm, would imply a classical traversal time of 3.6 fs (3.6×10^{-15} seconds). However in practice: "The peak of the [undistorted] single-photon wave packet appears on the far side of the barrier 1.47 ± 0.21 fs *earlier* than it would if it were to travel at the vacuum speed of light *c*." Such a negative delay equates to an equivalent tunneling velocity of 1.7 ± 0.2*c*. (Steinberg, Kwiat & Chiao 1993, p. 708)

However, such a claimed infraction of the relativistically dictated universal speed limit was to appear almost trivial when compared with later cases such as were mounted by researchers in 2002 at both the University of Moncton, Canada, (Haché & Poirier) and the Middle Tennessee State University (Munday & Robertson). Commenting that previous superluminal tunneling effects had typically been either relatively modest in magnitude or limited in range, Haché and Poirier reported highly superluminal pulse propagation over much larger distances than previously observed. Utilizing the tailored properties of a simple, wire-like structure – a coaxial line with designed periodic impedance,[39] or 'coaxial photonic crystal' – the Canadian researchers claimed the first demonstration of superluminal group velocities of up to three times the speed of light and over distances exceeding 100m (Haché & Poirier 2002, p. 520). With scant regard for the implications, in proclaiming the achievement, an impulsive press reported: "*Electrical pulses break light speed record*." (Pennicott 2002). For their part, Munday and Robertson described "an easily configurable experimental system" in which the peak of a tunneled electrical pulse was observed to exit a 119.5m coaxial photonic crystal before the peak of the input pulse had reached the beginning of the sample. The result not only accorded with a now extensive empirical base that claimed tunneling velocities could exceed the vacuum speed of light, but consolidated the counterintuitive proposal of so-called negative group velocities wherein mathematically depicted values were now claimed to "exceed infinity." (Munday & Robertson 2002, p. 2127) News of such an experimental success was again devoured by an enthusiastic but incautious media with the proclamation: "*Speed of light broken with basic lab kit*," and a claim that: "Electric signals can be transmitted at least four times faster than the speed of light using only basic equipment … costing just $500." (Choi 2002)

[39] "The periodicity causes anomalous dispersion and the appearance of a stop band in transmission near 10 MHz. Group velocities of up to three times the speed of light are observed in that spectral region, in accordance with calculations based on an effective index theory." (Haché & Poirier 2002, p. 518)

Superluminal velocities, signals and special relativity

With increasing experimental capabilities and the theoretical insights afforded by intensive research, investigations of the potential for superluminal phenomena were not to be restricted to the purely ‘quantum’ modes exhibited in entanglement and tunneling, nor was the metaphysical discussion to be limited to that of locality. Prominently also at issue was the foundational notion of causality as that notion found its parameters dictated by the constraints of special relativity theory wherein an effect must belong to the future light cone of its cause. Experimentally the issue presented as to whether signals, and by extension, information, was being observed to propagate superluminally in what would traditionally be a contravention of adjunct edicts of the Minkowski space-time construct which preclude (real) mass, energy, or information from traveling faster than the speed of light in vacuum.

Recalling that “the analogy between electrons following the Schrödinger equation and electromagnetic wave propagation” had suggested lines of approach in developing quantum mechanics, Martin and Landauer noted that:

> Furthermore, the analogy between the tunneling of electrons and the evanescent waves[40] found in a low-dielectric-constant region separating two regions of higher dielectric constant, has [also recently] been appreciated.” (1992, p. 2611)

As researchers Nimtz and Stahlhofen explain, “evanescent modes have a purely imaginary wave number and thus represent the mathematical analogy of the [particle] tunneling solutions of the Schrödinger equation.” (2007) Evanescent wave modes are prominent in optics in such locations as ‘frustrated total internal reflection’ (FTIR), but elsewhere occur in such as the forbidden frequency bands of a photonic lattice, in microwaves in a wave guide with a frequency below cutoff or where the guide is undersized, and in periodic dielectric heterostructures such as coaxial photonic crystals. The potential utility[41] of evanescent modes for superluminal investigations was not generally exploited until the 1990s, when the mathematical analogy between the Schrödinger and Helmholtz equations, discussed as early as 1949 by Arnold

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[40] “evanescent” – meaning ‘tending to disappear’ - characterizes wave phenomena where the associated electric field attenuates exponentially with distance from an interface or site of formation, rather than varying sinusoidally with distance in all directions as is characteristic of a ‘normal’ wave. In practical application, evanescent waves formed at an interface under conditions of total internal reflection form the basis of Total Internal Reflection Microscopy. (Prieve 2009)

[41] “In contrast to the electronic case, where measuring the arrival time … is a quantum mechanically disturbing procedure, electromagnetic wave packets can be occupied by many identical photons … Thus measuring the separation in arrival time of a pulse’s center of gravity … is a feasible measurement in the photon case.” (Martin & Landauer 1992, p. 2611)

Sommerfeld, began to inspire microwave and optical experiments involving evanescent regions. Although initially engaged to obtain more precise data on the tunneling time, the empirical efficacy of the analogy was early claimed by Enders and Nimtz when measuring the tunneling time of frequency limited microwave packets in the evanescent regime of waveguides. (Enders & Nimtz 1993) Their subsequent report claimed that: "The data … reveal superluminal wave-packet velocities for opaque evanescent regions," and that "the group velocity extrapolated for [the evanescent] region exceeds the velocity of light." (ibid., pp. 632-633). In their summaries of the many experimental efforts which were to utilize evanescent modes of electromagnetic waves in the 1990s, Heitmann and Nimtz concluded that: "The microwave experiments have revealed superluminal velocities for the center of gravity and for the envelope of the amplitude modulated signals," (1994, p. 154) and Weiss summarized that:

> Experiments dating back to the early 1990s by Nimtz, Steinberg, Chiao, and others have shown superluminal tunneling of optical photons through mirrors and of microwaves through co-called forbidden zones of waveguides. (Weiss 2000, p. 375)

By the turn of the century, and in a more controversial context, Daniela Mugnai, Anedio Ranfagni and Rocco Ruggeri of the Italian National Research Council in Florence, claimed to have demonstrated "the possibility of observing superluminal behavior in the propagation of localized microwaves over distances of tens of wavelengths." (Mugnai, Ranfagni & Ruggeri 2000, p. 4831). They added that "… these types of waves, better than the evanescent modes of tunneling, can contribute to answering the question on the luminal limit of the signal velocity." Their report indicated "a slight superluminal behavior" wherein the mean velocity was calculated as greater than light velocity by 5.3% - 7.2%. Their work encountered a mixed response. Although acknowledging the report as a significant claim that superluminal wave propagation in vacuum was indeed possible, some believed it necessary "to question whether there is either any experimental or theoretical basis for such a conclusion." (Bigelow & Hagen 2001, p. 059401). Others were less lenient, pronouncing: "In summary, we feel that the authors have made an inadequate error analysis of their data which does not support true superluminal propagation …" (Ringermacher & Mead 2001, p. 059402) More recently, the European group, "Oscillation Project with Emulsion-Tracking Apparatus," or OPERA, has claimed to have sent neutrinos generated at the European Centre for Nuclear Research (CERN) near Geneva, 720 km. to Gran Sasso in Italy, at "a speed greater than light by about 0.0025 per cent." Amidst calls for independent experimental confirmation, CERN theorist, Dr. Alvaro DeRejula, is reported to have claimed: "If it is true, then we truly haven't understood anything about anything," adding that, "It looks too big to be true. The correct attitude is to ask oneself what went wrong." (Overbye 2011)

Despite ambivalence in such claims for superluminal propagation, a substantial breach of the classical light-speed constraint had also been heralded in 2001 by Lijun Wang and his associates at the private NEC Research Institute in Princeton, N.J. Wang's team exploited the novel properties of gain-assisted linear anomalous dispersion to demonstrate superluminal light propagation in atomic caesium gas. (Wang, Kuzmich & Dogariu 2000). Startling in respect of the magnitude of the claimed observation, Wang's team alleged measuring a pulse timing advancement of 62 ± 1 ns equating to a group-velocity of –310 ± 5 times the equivalent in-vacuum transit time of their 6 cm test cell. According to their report:

> … in practice, this means that a light pulse propagating through the atomic vapour cell appears at the exit side so much earlier than if it had propagated the same distance in a vacuum that the peak of the pulse appears to leave the cell before entering it. (Wang, Kuzmich & Dogariu 2000, p. 277)

Reporting the result, Wanare noted that:

> The experiment has created a great deal of excitement and rekindled discussions on a variety of fundamental issues like, the validity of [the] Special Theory of Relativity, the Principle of Causality, the possibility of a signal travelling faster than the speed of light in vacuum … (Wanare 2000)

The potential for sensation was avidly captured by the scientific reporting media where a flurry of impetuous, and often misleading, headlines variously proclaimed: "*Laser smashes light-speed record*. [the light] speed limit has been smashed in a recent experiment in which a laser pulse travels at more than 300 times the speed of light." (PhysicsWeb 2002); "*Eureka! Scientists break speed of light* – Scientists claim they have broken the ultimate speed barrier: the speed of light." (Leake 2000); "*Light can break its own speed limit, researchers say* – Researchers say it is the most convincing demonstration yet that the speed of light – supposedly an ironclad rule of nature – can be pushed beyond known boundaries." (CNN 2000); and "*Faster than a speeding photon* - The textbooks say nothing can travel faster than light, not even light itself. New experiments show that this is no longer true …" (Marangos 2000). Other reports, however, included allusions to the more politically sensitive implications underlying the claim such as: "*Light pulses flout sacrosanct speed limit* - A few scientists argue that those experiments hint that Einstein was wrong." (Weiss 2000). Even the NEC research spokesman, Lijun Wang, was quoted as asserting that "… our experiment does show that the generally held misconception that 'nothing can travel faster than the speed of light' is wrong." (CNN 2000). However, it was not simply the confrontational implications of the empirical claim that fuelled subsequent contention. Central to the ensuing inquest was disquiet as to whether such observed pulses were, in fact, *signals* as carriers of information within the ambit of information theory and the prevailing interpretations of special relativity. Such contentions evoked the Italian researchers' concession that "the question as to whether a wave packet can be considered a signal is a much debated and complicated one." (Mugnai, Ranfagni & Ruggeri 2000, p. 4830). Consequently the claim by "Dr. Günter Nimtz of the University of Cologne … that a number of experiments, including those of the Italian group, have in fact sent information superluminally," (Glanz 2000) became equally contentious.

Dispersion – normal and anomalous

Of note in the portentous debates engendered by mounting faster-than-light claims, much of the difference in interpretation of superluminal phenomena, particularly as observed in zones of evanescence and anomalous dispersion, has found its genesis in meanings attached to the manner in which such regions are believed to physically disturb, reconstruct, reshape or re-phase a traversing pulse. Additionally, even the basic physics surrounding anomalous dispersion itself has proven to be fertile ground for differing interpretations. In elementary terms it can be said that electromagnetic phenomena propagate in transparent media at a rate determined by the local refractive index which itself depends not only on intrinsic dielectric values characteristic of the medium but also on the frequency of the radiation. Standard for most 'normal' or naturally occurring transparent media, the dispersion rate of wave-trains of differing frequencies exhibits an increase in refractive index concurring with an increase in frequency. As a familiar example, the range of electromagnetic frequencies constituting white light, experiences a corresponding range of refractive indices within media such as glass or water, a fact made visible as the spectrum of colours as seen emerging from a prism or as a rainbow. In normal dispersive material, the comparatively higher frequency blue light travels slower being more profoundly refracted, and displays closer to a prism base, than that of red light. However, typical of frequency bands in the medium's absorption spectrum near linear and nonlinear gain lines – such as where traversing radiation finds resonance with natural atomic frequencies of the medium - the refractive index is conversely found to *decrease* with increasing radiation frequency, giving rise to an 'anomalous' dispersion. In such a region, blue light travels faster than red when experiencing the lesser impact of refraction. However, such regions are also notably more opaque when coupled with a strongly absorbent behaviour.

Whereas an *ideal* monochromatic electromagnetic wave is definable as a disturbance with a single frequency and extending both from and to infinity, a pulse – or wave packet – results from a collection of monochromatic waves of differing frequency which mutually interfere. Constructive interference gives rise to a collective envelope limited to a calculable region of space. Where the frequency range of components is decreased, the wave packet becomes relatively longer and conversely the more frequencies added to the range the shorter the wave packet. (Chiao, Kwait & Steinberg 1993, p. 43). In normal dispersion, where each component frequency is independently velocity constrained, the wave packet also exhibits its own independent velocity which will always be less than the relativistic causal limit, *c*. However, in regions of dispersive anomaly, the wave packet velocity can exceed *c*, or even appear negative - conditions that were known, both theoretically and empirically, early in the 20th century and prior to the advent of special relativity. (Gauthier, Stenner & Neifeld 2010; Jackson, Lande & Lautrup 2001).

Various, and sometimes conflicting, explanations are offered as underwriting this phenomenon: "… superluminal propagation results from a pulse reshaping effect by which a dispersive medium preferentially amplifies the front or absorbs the back of the pulse." (Sprangle, Peñano & Hafizi 2001, p. 2); "This counterintuitive effect is a consequence of the wave nature of light and can be well explained invoking the rephrasing process in an anomalous dispersive medium." (Dogariu, Kuzmich & Wang 2001, p. 1); "…the observed superluminal propagation is not at odds with causality, and is instead a consequence of classical interference between its constituent frequency components in a region of anomalous dispersion." (Weisstein & Rodrigues 2006); "The measured group delay is the lifetime of the stored energy leaking out at both ends [of the gap]." (Winful 2007c, p. 3) In Ragni's opinion,

> … time delays of different meaning were proposed to describe the time spent during the interaction of a massive particle or an electromagnetic wave with a barrier in the tunneling process … the *phase time* introduced by Wigner; the *dwell time*, introduced by Smith and Büttiker; the Larmor-Baz'-Rybachenko times; the Büttiker-Landauer *traversal time*; the Sokolovski and Baskin *complex time*; and the *universal tunneling time*, suggested by Haibel and Nimtz, Nimtz and Stahlhofen, and revisited by Esposito … The different definitions … have contributed to generate additional confusion in this already complicated matter. (Ragni 2009, p. 1)

For others, the phenomenon is only apparent rather than real:

> Although the wave front of a signal passing through a classically forbidden region can never move faster than light, an attenuated replica of the signal is reproduced 'instantaneously' on the other side of the barrier. The reconstructed signal, causally connected to the forerunner rather than to the bulk of the input signal, appears to move through the barrier faster than light. (Jackson, Lande & Lautrup 2001, p. 1)

> The measured group delay is not a transit time and hence cannot be related to 'group velocity' … since the pulse is orders of magnitude longer than the barrier and cannot be localized within it. (Winful 2007c, p. 15)

In particular, reviews of the NEC team's claims have added further contentions:

> Even if the speed of transmission was infinite, that could only shave 0.2 ns off the time. The actual advance was much larger, representing time travel into the past, not faster than light travel. But it was really only *anomalous dispersion*. (Wright 2000)

So controversial has the exchange become, that in reporting on such a "seeming" superluminal propagation of light in dispersive media, observers noted that a follow-up on the NEC team's work had been hastily discarded. Justification for the decision to abandon further claims cited "controversies over the interpretations," and that such phenomenaa was "poorly understood." (Macke & Ségard 2001, p. 2)

Velocity definitions for wave phenomena

Given that relativistic causality is often colloquially described by the claim that 'nothing travels faster than the speed of light in vacuum,' concerns were raised early in the 20th century that such anomalous superluminality could appear to conflict with newly embraced relativistic doctrine.[42] In response, Arnold Sommerfeld in collaboration with Léon Brillouin, conducted a meticulous analysis of pulse propagation, specifically in unbounded dispersive media, publishing findings as early as 1914. Included were

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[42] As early as 1881, Lord Rayleigh had noted that a pulse of light travels at a 'group velocity' rather than the 'phase' velocity in a medium, and went on to investigate anomalous dispersion, publishing comment in 1899 that later "raised difficulties with the theory of relativity." Brillouin recalled that "Group velocity, as originally defined, became larger than *c* or even negative within an absorption band. Such a contradiction had to be resolved and was extensively discussed in many meetings about 1910." (Dogariu, Kuzmich & Wang 2001) An important outcome of such deliberations was a reformulation that stated that it was 'information,' rather than 'objects,' whose propagation is limited to *c* by causality requirements. (Stenner, Gauthier & Neifeld 2003)

definitions of wave phenomena such as phase, phase-time,[43] group, signal, energy, and particle velocities. Defining the "speed of light in vacuum," *c*, they proposed an unpretentious but also hopefully unambiguous and causally compliant judgment:

> At the point $x = 0$ is a source, which is switched on at the time $t = 0$. Some distance d away from x no effect can be detected coming from x before the time d/c. (cited in Heitmann & Nimtz 1994, p. 155)

Extending their definitions, *phase* velocity, v_{Ph} , described the propagation velocity of the 'crests' of a monochromatic wave extended infinitely in space and time, with the additional understanding that at very high frequencies v_{Ph} would become greater than *c*,[44] or even imaginary, in regions of dispersive anomaly. Of particular import, however, was to be their discussion of the wave packet velocity – termed the *group* velocity, v_{Gr} , - and analogously described as the 'centre of gravity' of a wave packet. Insofar as "the group velocity was then believed to be identical to the energy propagation," careful definition became important. (Jackson, Lande & Lautrup 2001, p. 1). However, although licensed to exceed *c*, or even become negative at the resonance absorption of a dispersive medium, $v_{Gr} > c$ was pronounced to be physically meaningless. Yet, difficulties were to also attend the assignment of v_{Gr} as imaginary, such as in cases involving tunneling and evanescence:

> As v_{Gr} also corresponds to the velocity of a particle by describing the particle as a wave packet, the interpretation of an imaginary velocity becomes physically unclear causing much confusion. (Heitmann & Nimtz 1994, p. 155)

Brillouin further defined the term *signal* velocity, v_S, as designating the velocity of the "beginning of the essential part" or main body of the wave packet envelope, distinguishing it from 'forerunners' or the first stirrings of disturbance which are discernable immediately preceding the onset of the wave packet proper.[45] In practice, the 'signal' can be recognized as distinct from its forerunners in the wave packet ensemble not only by its much higher amplitude but also as containing substantially lower frequencies. (Heitmann & Nimtz 1994, p. 155) Experimentally, detection of some suitable percentage, such as 50% of the maximum signal amplitude, is chosen as evidence of its presence.

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[43] The 'phase-time' velocity is defined as the real part of a complex group velocity.

[44] Current optical texts confirm that "at very high frequencies electromagnetic waves propagate through dielectric media with phase velocities which exceed the velocity of light in a vacuum." (Fitzpatrick 2009)

[45] Two stages of relatively small amplitude forerunner activity is discernable; the *Sommerfeld precursor*, propagating at *c* and often of X-ray frequency, and then the *Brillouin precursor*, both completely independent of the frequency of the incident wave. (Fitzpatrick 2009)

Nevertheless, of primary importance for subsequent researchers and the potential fate of relativistic causality, was to be a determination of meaning for the propagation of classical information. For their analysis, Sommerfeld and Brillouin examined the propagation of 'square' pulses, for which, "in almost any case the beginning of the signal is a discontinuity in the signal envelope or in a higher derivative." It was to be in this step-like front velocity, v_F, - which may not exceed c in order to preserve causality - that the fiducial role of c as the limit of the front velocity of a wave packet "is deduced from the Lorentz invariance of the Maxwell equations." (Heitmann & Nimtz 1994, p. 155) The work of Sommerfeld and Brillouin was considered definitive, being cited as a textbook authority and a conclusive demonstration that information may not propagate faster than c, even where the group velocity was shown superluminal. (Gauthier, Stenner & Neifeld 2010). Endorsing this view, in 1932 the Hungarian mathematician John von Neumann produced his claim to proof that "the nonlocality of quantum mechanics cannot ever be parlayed into a mechanism to transmit messages instantaneously." For many decades, such authoritative works underpinned a dominant world-view providing an assurance that quantum nonlocality and special relativity could peacefully coexist. (Albert & Galchen 2009, p. 31)

It was to be 1970 before the velocity of information and its potential causality implications regained the interest of researchers. Geoffrey Garrett and Dean McCumber, then both at Bell Laboratories in the US., demonstrated theoretically that Gaussian pulses – pulses with 'smooth' leading and trailing ends – should propagate with minimal distortion at group velocities greater than c, provided that the pulse has a narrow bandwidth and the region through which it travels is short. (Boyd & Gauthier 2002, p. 508; Garrett & McCumber 1970) Vigorous debate, which followed the dramatic experimental verification of their proposal in 1982,[46] (Chu & Wong) and 1985, (Ségard & Macke) was to be fuelled by an ensuing but sometimes conflicted experimental record. Further, the received doctrine of Sommerfeld and Brillouin was found, at least in part, potentially inadequate to describe more recent discoveries, as Jackson *et al*. note:

> While the efforts of Sommerfeld and Brillouin would seem to have settled the issue of true superluminal propagation definitively, the situation is somewhat more subtle. Their work conclusively demonstrated that Maxwell's equations preclude superluminal propagation for media with a causal form of $v(\omega)$. It did not, however, extend to a proof that the

[46] "Chu and Wong verified that this unusual behavior actually occurred for weak picosecond laser pulses propagating near the center of a bound *A*-excitation line of a GaP:N sample." (Chiao 1993, p. R34)

> singularities of $v(\omega)$ must lie in the lower half plane. In simple electron resonance models of dielectrics, such behavior follows from the absorptive nature of the material. (Jackson, Lande & Lautrup 2001, p. 1).

A revised definition of the energy velocity, v_E , was offered by Brillouin in 1953 with Raymond Chiao of Berkeley prompted to comment: "One goal [was] to help correct the common misconception that the group velocity, when it is 'superluminal,' is somehow unphysical or unuseful." (Chiao 1993, p. R34) By 1994, Chiao felt empowered to claim:

> Analytic, limited bandwidth signals, e.g. Gaussian wavepackets, whose frequencies lie in a transparent spectral window far below the resonance of an amplifying atomic medium, can propagate with phase, group, energy and 'signal' velocities (as defined by Sommerfeld and Brillouin) all exceeding the vacuum speed of light *c*. (Chiao 1994, p. 359)

Notably missing from the litany of successes was specific reference to the velocity of information. For the most part, prominent researchers such as Steinberg, Chiao, Wang, and Gauthier, have deferred to the textbook definition of the front velocity and have routinely added a disclaimer to the effect that their superluminal results make no claim that either the classical information velocity constraint was breached or that causality was threatened.[47] However, the controversy has left even the more conservative offering a conciliatory note:

> Although relativity emerges unscathed from these experiments, our understanding of exactly which velocities are limited (or not) by *c* continues to evolve … For the time being, physicists will be kept busy trying to clarify their intuition about relativity and learning how to accurately describe the information carried in real optical or electronic pulses. (Steinberg 2000, p. 22)

On the other hand, and to some extent implicitly challenging the status of the textbook's front velocity definition, other notable researchers have expressed reservations or frank disagreement. Commenting on the issue, Ranfragni of the Italian group is reported as maintaining that: "This problem is still open," (Glanz 2000) When evaluating their results, NEC team spokesperson Lijun Wang, also appeared to question prevailing authority:

[47] Typical disclaimers assert such as: "… the output peak arises from the forward tails of the input pulse in a strictly causal manner, and no abrupt disturbance in the input pulse would travel faster than *c*." (Steinberg, Kwiat & Chiao 1993, p. 711)

> It has been suggested that the true speed at which information is carried by a light pulse be defined as the "frontal" velocity of a step-function-shaped signal which has been shown not to exceed *c*. … the signal velocity of a light pulse, defined as the velocity at which the half point of the pulse front travels, also exceeds the speed of light in vacuum, *c*, in the present experiment. (Wang, Kuzmich & Dogariu 2000, pp. 277-279)

Less cautiously, however, physicists such as Günter Nimtz, of the University of Cologne, and Alfons Stahlhofen, of the University of Koblenz, have made repeated claims to have demonstrated violation of relativistic causality and thence, by definition, violation of special relativity. (Nimtz & Stahlhofen 2007, 2008; Stahlhofen & Nimtz 2006) Reporting in 2006, they claimed that:

> Recent experimental studies confirmed [studies of evanescent mode] predictions about non-locality, non-observability, violation of the Einstein relation and the existence of a commutator of field operators between two space-like separated points. Relativistic causality thus is violated in the near-field phenomenon evanescent modes … (Stahlhofen & Nimtz 2006, p. 189)

It was, however, their report in 2007 entitled: "*Macroscopic violation of special relativity*" that created the greater controversy with its explicit claim that "evanescent modes lie outside the bounds of the special theory of relativity." (Nimtz & Stahlhofen 2007) Their press release in *New Scientist,* (Anderson 2007) declared that "two physicists are now claiming they have propelled photons faster than the speed of light." In mounting an opposing view, Winful has asserted that "all these claims are erroneous and are based on a misinterpretation of a purely classical measurement accurately described by Maxwell's equations." (Winful 2007a, p. 1, 2007c, p. 3) However, the more recent claims by Nimtz and his coauthors keep faith with much of their previous practical work involving microwaves, prisms and mirrors, and published since the early 1990s. Of note is their recent claim to "demonstrate the quantum mechanical behavior of evanescent modes with digital microwave signals at a macroscopic scale of the order of a meter …," (Nimtz & Stahlhofen 2007) which bears heavily on the controversial issue of information transfer and their prior claims that information has been demonstrated to propagate with superluminal velocity. Such claims include having utilized a superluminal group and signal velocity to transmit parts of Mozart's symphony no. 40, encoded in a microwave beam, "through a tunnel of 114 mm length at a speed of 4.7*c*." (Heitmann & Nimtz 1994)[48]

[48] Although no English translation appears available, academically reliable reports indicate that Günter Nimtz and Horst Aichmann performed the tunneling experiment at laboratories of Hewlett-Packard in 1994, and have subsequently published results as a conference abstract by Aichmann, H., Nimtz, G., and

Any realistic wave packet, or envelope employed as a signal, is of necessity frequency limited and in consequence cannot exhibit a sharp and accurately step-like front. To fully accord with theory, such a front would require the superposition of an infinite ensemble of frequencies. Nonetheless, it is only the velocity of such a front that is relevant to the Sommerfeld and Brillouin definition guarding relativistic causality. In the practical domain of electrical engineering, information is deduced from the amplitude and frequency modulations of a carrier and, in the now ubiquitous world of electronic data handling, information often derives from the half-height detection of the practical approximation of a square wave pulse. Heitmann and Nimtz point out that application of such practicalities to evanescent and tunneling phenomena would validate claims of information superluminality in their cases. They further note that the prevailing definition and the theoretical base from which it is drawn are irrelevant for their work since "there is no experimental condition known by which such a well defined front could be generated." (Heitmann & Nimtz 1994, p. 157) Other researchers concur complaining that "it does not seem possible to obtain a sharp edge by means of which the beginning of [an information carrying] pulse can be … identified." (Ragni 2009) In pressing the issue Heitmann and Nimtz also question the front velocity definition's general applicability in that,

> … all the velocity terms discussed in [the superluminal phenomena] literature are based on an interaction mechanism of electromagnetic waves and bounded charges … described by a Lorentz-Lorenz dispersion, which differs essentially from the dispersion of an evanescent region being only determined by the boundary conditions. The first one holds for waves which have always a real wavenumber component, whereas the last one has a purely imaginary one, which does not correspond to a wave. (Heitmann & Nimtz 1994, p. 158)

Having challenged both the applicability and practicality of the textbook version, in their conclusion, Heitmann and Nimtz summarize that: "From the experimental point of view, [if conceding the current definition] it is not possible to violate Einstein causality: A [sharp] front cannot be generated due to the frequency band limitations in a real experiment." (ibid., p. 158) The debate on information velocity has been maintained without loss of intensity into the twenty-first century with little evidence to suggest that consensus might prevail in the near future. Unfortunately, the issue is now conflicted with some researchers feeling indignant that special relativity appears granted singular

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Spieker, H. in *Verhandlungen der Deutschen Physikalischen Gesellschaft*, vol. 7, 1995, p. S.1258. (Pössel 2001) immunity from prosecution on terms, which in their view, amount to no more than the disputable fine-print of a technicality.

Chapter 2
Science in the 21st century - paradox and dilemma

It is the undoubted objective of any evidence based inquiry such as science, that at some juncture the weight of accumulated evidence will be deemed sufficient to justify a verdict - the decision to endorse one of the hypothetical explanations on offer and hold it, at least within the realm of a healthy scientific skepticism, as proven and as having prevailed against its competitors. Although consensus has not been reached in respect of claims to breaches of classical causality or the recording of superluminal information transport, the evidence surrounding locality and separability presents differently. Given that the wisdom of the dictum 'extreme claims demand extreme proofs' has been rightly and assiduously observed, and irrespective of any interpretation to be placed on experimental results, unambiguous and repeatable empirical distinction is now generally accepted as demonstrated between the predictions made by quantum mechanics and relativity – a distinction in turn designating different and essentially incompatible descriptions of the world at a fundamental level. It is now to be frankly acknowledged that the notion of incompatibility between two of the foundational theories of contemporary science has moved beyond the realm of polemic and conjecture to take its place as empirically substantiated fact. In response Maudlin has claimed that "the experimental verifications of Bell's inequality constitutes the most significant event of the last half-century." (Maudlin 2002, p. 4) For a sober philosophy of science, the inevitable consequences are dire:

> We cannot simply accept the pronouncements of our best theories, no matter how strange, if those pronouncements contradict each other. The two foundation stones of modern physics, Relativity and quantum theory, appear to be telling us quite different things about the world. (Maudlin 2002, p. 24)

> Insofar as inconsistent theories cannot be true, we know that the standard collapse formulation of quantum mechanics and special relativity taken together are false … a logical contradiction insofar as it is logically false is as far from the truth as possible. Our current best physical theories do not describe any possible world whatsoever, so in this sense, they are not even in the ballpark of accurately describing the actual physical world. (Barrett 2003, p. 1215)

However, such a dilemma is further compounded by paradox. Despite two cornerstones supporting the same modern scientific edifice being proven to be inconsistent, nevertheless "[t]ogether they provide the most accurate empirical predictions we have ever had.” (Barrett 2003, p. 1206) In terms taken as indicative of the correctness of scientific theories – their ability to accurately predict experimental results within their domains of applicability – both quantum mechanics and relativity have amassed enviable records. Nonetheless, insofar as both quantum theory and relativity theory purport to differently explain fundamentals of the same physical reality, they must fall victim to the Frigg and Hartman injunction that “if several theories of the same system are predictively successful and if these theories are mutually inconsistent, they cannot all be true, not even approximately.” (Frigg & Hartmann 2006 sec. 5.1)

The modern dilemma and the evolving status of special relativity

The characteristic conservatism of the scientific endeavor has conventionally endorsed such notions as ‘separability’ and ‘locality’ as rational, fundamental, and sacrosanct components of its hard-won 19th century legacy. It is then understandable that physicists have perhaps taken their lead from the EPR advocacy of the relative merits of received formalisms in that it has traditionally been quantum theory that has been held liable for justifying its non-classical and particularly non-local assertions. As others have noted: “Einstein, Bohr and everyone else had always taken it for granted that any genuine incompatibility between quantum mechanics and the principle of locality would be bad news for quantum mechanics.” (Albert & Galchen 2009, p. 30) In consequence, prodigious effort has attended re-interpreting quantum mechanics so as to minimize tensions with a relativistic world prescription. However, as Maudlin has pointed out;

> Bell’s theorem can be proven without so much as a mention of quantum theory, and although one uses quantum theory to predict the violation of Bell’s inequalities, the violation itself is confirmed by straightforward laboratory technique. The observed facts, not merely some interpretation of [quantum] theory, stand against locality. (Maudlin 2002, p. 6)

Also reflecting on the import of the empirical record, quantum physicist Nicholas Gisin has echoed similar sentiments – nature is demonstrably nonlocal; it is, however, Relativity, as historically conceived, that fails to advance an explanation:

> In modern quantum physics, entanglement is fundamental; furthermore space is irrelevant – at least in quantum information science, space plays no central role and time is a mere discrete clock parameter. In relativity, space-time is fundamental and there is no place for

> nonlocal correlations. To put the tension in other words: No story in space-time can tell us how nonlocal correlations happen … (Gisin 2009, p. 1358)

Thus, to an extent perhaps not yet fully recognized, the EPR 'tables have been turned.' To the degree that a prima facie non-localism has been substantiated, and to the extent that "locality is contravened only on pain of a serious conflict with relativity theory," (Jarrett 1984) it now ironically appears to be relativity theory that is insufficient when explaining empirical results and is thus found 'hoisted on its own petard,' and vulnerable to the EPR argument of theoretical incompleteness. The repercussions have not been lost on analysts such as David Albert and Rivka Galchen who believe that;

> The greatest worry about nonlocality … has been that it intimates a profound threat to special relativity as we know it. In the past few years this old worry – finally allowed inside the house of serious thinking about physics – has become the centrepiece of debates that may finally dismantle, distort, reimagine, solidify or seed decay into the very foundation of physics …
>
> The status of special relativity, just more than a century after it was presented to the world, is suddenly a radically open and rapidly developing question. (Albert & Galchen 2009, pp. 28, 33)

What to do?

According to philosophers of science such as Albert and Barrett, peaceful co-existence between quantum theory and Relativity, as historically tolerated, is no longer an option. In agreement, Maudlin frankly admits: "Something has to give: either Relativity or some foundational element of our world-picture must be modified." (2002, p. 242) Although presumably most physicists would yet be unwilling to concede the need for paradigm change, nevertheless, faced with a proven theoretical inconsistency indicting an elemental of relativistic doctrine, a growing number of physicists are entertaining the possibility that special relativity is not quite correct, (Cho 2005, p. 866) and at least some philosophers of physics are beginning to seriously entertain the possibility "that relativity might be given up as being descriptive of the fundamental spacial-temporal structure of the physical world." (Barrett 2003, p. 1211) Hesitancy, however, is born of multiple concerns. In his distinguished analysis of the structure of scientific revolutions, Thomas Kuhn noted that, historically, the scientific community had stolidly endured the invidious dilemma of an evident contradiction until a viable alternative was clearly available. Then, and only then, would allegiance to a manifestly unworkable theoretical scheme be abandoned. (Kuhn 1962) It is in this respect that

Barrett's succinct appraisal is relevant that: "we know that the standard formulation of quantum mechanics together with special relativity is descriptively wrong, but we do not have any idea what the descriptive mistakes are." (Barrett 2003, p. 1217)

Relative to this contemporary crisis, for many, string and quantum gravity theories, incorporating attempts to unify quantum mechanics and gravity, have long held out potential promise as a provider of needed salvation. In Brown's view;

> … it's entirely possible that the theory of relativity is simply wrong on some fundamental level where quantum mechanics "takes over". In fact, this is probably the majority view among physicists today, who hope that eventually a theory of "quantum gravity" will be found which will explain precisely how and in what circumstances the theory of relativity fails to accurately represent the operations of nature, while at the same time explaining why it *seems* to work as well as it does. (Brown 1999h)

Attempts to unify quantum theory with gravity, or, pertinent to the same raft of problems, to heal the rift between quantum theory and contrary implications of local Lorentz invariance, has been productive in provoking a number of recent novel but hypothetical approaches each offering plausible alternatives to received formalisms. Each has so far been deemed problematic and has failed to find a general acceptance. Prominent among unification proposals has been a 'Loop Quantum Gravity' approach which in overview suggests quantization of both space and time at their irreducible minimums at the Planck scale. The proposal is actively researched by many eminent physicists, including Rodolfo Gambini and Jorge Pullin, (1996) and Lee Smolin. (2006, pp. 224-225; 2010) More recently "a candidate quantum field theory of gravity" has been advanced by Petr Hořava, (2009) in which the amalgamated components of Minkowski space-time are again dissociated and which has attracted much favorable attention. (Ananthaswamy 2010) On the other hand, reformulations and extensions to the received precepts of relativity, almost unthinkable as allowable into mainstream contention until recently, have been proposed in various forms. Of note, 'Doubly Special Relativity,'[49] conceived by Giovanni Amelino-Camelia of the University of Rome, (2002) has been researched and modified by João Maguejio and Lee Smolin. (Magueijo & Smolin 2002; Smolin 2006, p. 226) The concept supposes that quantum gravity effects might constrain individual particle energies, and proposes a new absolute observer-independent length (momentum) scale "in addition to the familiar observer-independent velocity scale *c*," and existing at the Planck energy range. (Amelino-

[49] Also known as 'extra-special relativity' and 'deformed special relativity, [DSR]' however 'Special Relativity with two invariant scales' has also been suggested. (Amelino-Carmelia 2002, p. 2)

Carmelia 2002) Elsewhere, Petr Beckmann proposed “Einstein Plus Two” in which the ‘observer’ status of the classical Einstein-Minkowski approach to Relativity, finds review. (Beckmann 1987)

Nevertheless, for string theory advocates such as David Gross, the string-based program still retains the dominant potential for addressing the quantum gravity problem, suggesting that the most useful response to the current impasse is to ‘hold the line’ despite the mounting portends of failure. Insofar as, by analogical repute, the ‘darkest hour is just before the dawn,’ Gross has encouraged maintenance of a string-based program reminding fellow physicists of the theoretically uncertain days attending the first Solvay Conference[50] just before the fortuitous advent of quantum mechanics granted conceptual liberation. (New Scientist 2005)

Special relativity and the experimental record

On the other hand, and consolidating the appearance of paradox, the claim to an almost impeccable empirical record is believed by many to censure any call for impeachment. As befits theory held to be foundational, relativity, both special and general, has historically been subjected to intense experimental scrutiny. According to conventional recount, relativity has positively met “all of these requirements and expectations” presented in the form of “literally hundreds of experiments …[entailing] an enormous range and diversity, and the agreement between theory and experiment is excellent.” Thus, within its domain of applicability, most hold it among the most well-verified of all our physical explanations. (Roberts & Schleif 2007) On the other hand, such authors openly admit that “SR is not perfect (in agreement with every experiment), and there are some experiments that are in disagreement with its predictions.” They also admit that “few if any standard references or textbooks even mention the possibility that some experiments might be inconsistent with SR, and there are also aspects of publication bias in the literature.” Nonetheless, by their recent assessment: “It is clear that most, if not all, of these experiments have difficulties that are unrelated to SR [and that] there are no reproducible and generally accepted experiments that are inconsistent with SR, within its domain of applicability.”

[50] Named for their founder, Belgian industrialist Ernest Solvay, the Solvay Conferences in Physics and Chemistry began in the autumn of 1911 chaired by physicist Hendrik A. Lorentz, and met to consider the enigmatic problem of *Radiation and the Quanta*. A then recently formulated quantum theory was discussed at the fifth Solvay Conference on *Electrons and Photons*, held in October 1927, and also chaired by Lorentz.

So ingenious, abundant, and eclectic have the research strategies become[51] that theorists felt motivated to create "test theories" wherein a relatively few necessary and sufficient experimental procedures could address the essentials of the subject formalism granting a vicarious, yet general and tractable method of verification. To this end, test theories "applicable to any theory formulated in terms of geometric objects defined on a 4-dimensional spacetime manifold" and that subsume theories that embody the equivalence principle of General Relativity, are well documented, and have found wide acceptance. (Thorne, Lee & Lightman 1973, p. 3563)

Development of 'test theories' for special relativity

For the particular case of special relativity, a kinematical test theory necessarily entails the mathematical generalization of the Lorentz Transforms as representing its intrinsic and pivotal relativistic conception of (local) Lorentz invariance. Generalization of the transforms generates additional test equation parameters and the fit of experimental results[52] to such parameters is taken as indicating confirmation or disconfirmation of a test formalism.[53]

As early as 1949, H. P. Robertson proposed the first of what was to eventually become three main approaches to creating such a kinematical test theory, with each differing only in the possible variations the inter-frame transformations might conceivably take. Initial motivation for Robertson was determination of the extent to which theoretical postulate could be replaced by empirical observation when deriving the kinematical basis of the theory. Robertson concluded that three prominent second-order optical experiments "do in fact enable us to replace the greater part of Einstein's postulates with findings drawn from the observations," (Robertson 1949, p. 378)

- the 1887 'aether-drift' interferometer experiment by Michelson and Morley that tests the isotropy of space;
- the subsequent 1932 interferometer experiment of Kennedy and Thorndike, that tests the independence of the speed of light from the velocity of the laboratory;

[51] A comprehensive overview as of 1962, was prepared by Gerald Holton at the request of the Commission on College Physics. (Holton 1962)

[52] For a RMS test theory such parameters are often designated α, β and δ, or A, B and O. (Schiller & Görlitz 2009) For special relativity *theory*, such parameters equate to zero whereas alternative theories predict, and specific experiments produce, distinctive values for the test theory parameters permitting adjudication on their claims and significance.

[53] It is to be noted that special relativity is the local limit of General Relativity and any experiment in which gravitational effects appear significant falls outside its domain of applicability. In most optical and elementary-particle configurations performed on the Earth's surface - situations that usually closely approximate inertial conditions - disparity of results due to a non-inertial environment is calculated to be "vastly smaller than the experimental resolution" and can be safely ignored. (Roberts & Schleif 2007)

• the 1938 and 1941Doppler spectroscopy experiments of Ives and Stilwell, that quantitatively assesses the relativistic Doppler shift as a test of time dilation.

The 'Robertson-Mansouri-Sexl' test theory protocol

In 1963, W. F. Edwards investigated the propagation of light in the context of a supposed anisotropic space and the consequences for special relativity theory where the definition of simultaneity employed differed from that originally assumed in the 1905 version of electrodynamics. (Edwards 1963) Then, in 1977, Reza Mansouri and Roman Sexl published a series of studies which offered a mathematical variation on the Robertson approach. (Mansouri & Sexl 1977a, 1977b, 1977c) Subsequently, MacArthur demonstrated that these major test theory approaches were equivalent, (MacArthur 1986) and Zhang proposed a more tractable interpretation of test theory parameterization[54] in which the three previous approaches are unified and which claims demonstration that any further development of test theories would be reducible to those already advanced. (Zhang & Chang 1997) The combination of kinematical test theories into a single framework has become routinely referred to as the "RMS" (Robertson-Mansouri-Sexl) test theory of special relativity. The RMS approach now facilitates detection of potential Lorentz invariance breaches exhibiting as possible modifications of the time dilation factor, or a dependency of the speed of light on the velocity of the source or observer, or an anisotropy of the speed of light in vacuum. In practice, RMS test theory implications are explored by modern high-precision interpretations of Robertson's choice of historical second-order optical experiments. (Eisele 2009) Following directly from technological "developments in frequency metrology and ultrastable oscillators, especially in the optical domain," precision laboratory testing of Lorentz invariance by RMS techniques has become increasingly accessible. (Braxmaier et al. 2002, p. 1; Seel et al. 1997) In response, Zhang confidently forecast that such a range of test procedures, given modern experimental precision, could determine the

[54] in terms of inertial reference frames, as analyzed by Zhang, each test theory approach is identical in defining a Euclidean three-dimensional vacuum space with location interpreted by a Cartesian coordinate system. Differences occur in defining the frame as inertial, for which the time coordinate common to each frame as a system, permits measurement of linear motion. Synchronization of the local times of events within a frame requires a definition of simultaneity – Edwards definition proves to be a generalization of that proposed in the 1905 Einstein electrodynamics whereas the Mansouri-Sexl simultaneity is a generalization of the Robertson definition. In terms of a test theory, "the velocity for a given moving body would take different values according to different definitions of simultaneity. A similar conclusion is also valid for any quantity related to the definition of simultaneity" (Zhang & Chang 1997, pp. 15-18)
Such kinematical "frameworks postulate generalized transformations between a preferred frame candidate $\Sigma(T,\boldsymbol{X})$ and a moving frame $S(t,\boldsymbol{x})$ where it is assumed that in both frames coordinates are realized by identical standards … Coordinates are defined by the clocks and length standards, and only the transformations between those coordinate systems are modified." (Wolf et al. 2006, p. 453)
"Since light is a consequence of Maxwell's equations, each test of SR is also a test of the validity of Maxwell's equations." (Schiller & Görlitz 2009)

Lorentz transforms to within a few parts per million thereby placing "rather strong *experimental* constraints on any alternative theory." (Roberts & Schleif 2007) Vindication of Zang's optimism was rapid and has surpassed early expectations with improvements in precision, often by orders of magnitude, now common.

In more recent practice, specialist centres have been created to concentrate efforts and expertise in the pursuit of RMS test theory ideology.[55] Exploiting the functionality of modern Michelson-Morley adaptations, specialist groups have been founded at Stanford University, Berlin's Humboldt University, and the Observatoire de Paris, to pursue the best possible limits on light-speed anisotropies. Comparing the resonant frequencies of two orthogonal crystalline sapphire optical cavities over very long time periods, the Humboldt group has reported a limit for light-speed variation of $\Delta c/c \approx 2 \times 10^{-15}$, and "were able to place independent constraints on nearly all Lorentz-violating terms that cause anisotropy in the speed of light." (Pospelov & Romalis 2004, p. 43) Elsewhere, at the Institute for Experimental Physics, Heinrich-Heine-Universität Düsseldorf, Christian Eisele and his associates have performed modern RMS compliant versions of both the classical Michelson-Morley[56] and Kennedy-Thorndike experiments. (Eisele 2009; Schiller & Görlitz 2009) Extremely high precision compliance to isotropic space-time Lorentz invariance is reported in the Institute's results claiming fidelity to several parts in 10^{16}, (Eisele et al. 2008) followed soon after with a claim to compliance to the level of 0.6 parts in 10^{17} (Eisele, Nevsky & Schiller 2009, p. 1) Elsewhere, tests of light-speed dependence on the laboratory reference frame performed using Kennedy-Thorndike styled experiments, which earlier presented a lower level of RMS test compliance, have improved markedly from a reported $1.9 \pm 2.1 \times 10^{-5}$ (Braxmaier et al. 2002) to $\sim 1.2 \times 10^{-9}$ and $\sim 1.6 \times 10^{-7}$ for two RMS test parameters, in a period of about three years. (Wolf et al. 2005, p. 1) Test results of relativistic time dilation have also enjoyed a recent increased precision. Utilizing laser spectroscopy on fast moving ($0.064c$) Lithium ions, both forward and backward Doppler shifted frequencies have been measured to a precision of 1 part in 10^9. Such results concur with relativistic prediction and impose an upper bound of 2.2×10^{-7} on deviations from the time dilation

[55] For their part, the National Institute of Standards and Technology, based in Maryland, have devised a method for testing the speed of light in vacuum using the limiting velocity for massive particles as enshrined in the formula, $E = mc^2$. Such a procedure is claimed to avoid assumptions relating laboratory motion to a preferred frame and also avoids the "null" result interpretation ambiguity noted for some RMS tests. (Greene et al. 1991)

[56] One novel experimental apparatus employs a monolithic ultra-low-expansion-coefficient glass block housing two orthogonal, crossing Fabry-Perot cavities and contained in a temperature stabilized vacuum chamber. Cavity frequencies are monitored during extensive stationary and rotational periods. (Eisele 2009) Elsewhere, the Müller group has used simultaneous rotating Michelson-Morley experiments over more than one year - one in Berlin using optical Fabry-Perot fused silica resonators – the other, in Perth Australia, using microwave whispering gallery sapphire resonators. SME Lorentz test compliance of ~ 10^{-16} was claimed. (Müller et al. 2007)

factor. (Saathoff et al. 2003) More recently, increased precision of the Mansouri-Sexl time dilation test parameter has been reported with a claim of ~8.4×10^{-8}. (Reinhardt et al. 2007)

The 'Standard-Model Extension' test theory protocol

Nonetheless, despite the appearance of an extreme compliance, in response to an increasing provocation at least partly deriving from the empirical confirmation of foundational theory inconsistency, there has been a renaissance of interest in improving rigorous experimental testing of special relativity. (Roberts & Schleif 2007) Even if testing basic theoretical assumptions as rigorously as possible were not intrinsically a key scientific principle, the increasingly strategic role played by relativity as "underpinning all of present day physics" has revitalized the quest for new experiments, or improvement on existing ones. Importantly, the discovery of infractions of special relativity, however minute, "would most certainly lead the way to a new conception of physics and of the universe surrounding us." (Schiller & Görlitz 2009; Wolf et al. 2006, p. 452) However, the most potent call for either higher precision, or an extended range of testing, has found its particular incentive in implications suggested by recent quantum gravity conjectures. These hold that special relativity may prove to be violated at extremely small distances and very high energies.[57] Such projections derive from a speculation that relativity and the Standard Model might be low-energy approximations of a more fundamental unifying theory that exhibits small violations of Lorentz invariance at energy levels far beyond those normally encountered. (Eisele 2009) Unfortunately, candidates for a quantum gravity theory "can be tested directly only with particle collisions a million, billion times more energetic[58] than any produced with a particle accelerator." (Cho 2005) Pioneering a practical approach to such seemingly intractable problems, Indiana University's V. Alan Kostelecký and his colleagues extended the Standard Model - which currently offers consistent explanation for all the known elementary particles - by proposing an additional profusion of 'background' fields lacing empty space, and now known as the 'Standard Model Extension' (SME) (Kostelecký 2004, 2006; Wolf et al. 2006) Insofar as conjectured orientations of SME

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[57] The quantum effects of gravity are anticipated to become significant at the 'Planck scale.' In energy terms, the 'Planck energy' is approx. 1.22×10^{28} eV ($\approx 10^{19}$ GeV) and relates by mass-energy equivalence to the 'Planck mass' at ~ 2.18×10^{-8} kg and the extremely small 'Planck time' as ~ 5.4×10^{-44} sec. Quantification of a 'Planck length,' although potentially problematical since it is related to the Planck energy by the Uncertainty Principle (and at a Planck scale, quantum indeterminacy becomes virtually absolute) yet achieves definition from the speed of light in vacuum, '*c*', the Gravitational Constant, 'G,' and Planck's Constant, '$\hbar$', and is held by some as the smallest possible length, at ~1.6×10^{-35} m.

[58] At present, the trans-GZK cosmic rays of 10^{11} GeV are the most energetic particles known. (Mattingly 2005)

background fields would be antagonistic in principle to a space-time isotropy, testing special relativity via its fundamental tenet of Lorentz invariance and symmetry "provides a far more practical, albeit indirect, way of probing quantum gravity," in that it allows a precise, extensive, and systematic test for Lorentz violations at practicable energies. (Cho 2005)

Consequently, in accordance with a test theory domain whose raft of additional parameters derive from a generalized SME, possible new effects and potential breaches of Lorentz invariance and symmetry are now sought in a variety of forms. New test parameters seek a difference in behavior between matter and antimatter; a difference in the vacuum velocity of light as dependent on a property such as polarization; a possible birefringence, or a dispersive character of the vacuum of space; [59] or the existence of underlying but weakly interacting fields conferring preferred orientations or anisotropies on space. (Eisele 2009; Schewe & Stein 2004) Such a diversity of potential variants embraces a wide range of scientific fields including atomic, nuclear, high-energy physics, and astrophysics. (Mattingly 2005) In early tests and employing a geometry that ensures optimal sensitivity to a variety of generalized SME test coefficients,[60] assay of space-time anisotropy conducted by a Stanford group found no evidence of anisotropy to one part in 10^{13} for velocity-independent terms and one part in 10^{9} for velocity-dependent terms, placing stringent bounds on four linear combinations of parameters in the general standard model extension. (Lipa et al. 2003; Schewe, Riordon & Stein 2003) Results published shortly after further claimed "the first clean test for the fermion sector of the symmetry of spacetime" within the standard-model extension framework, to a level of 10^{-27} GeV. (Canè et al. 2004, p. 1) By 2006, and by measuring linear polarization in gamma rays from gamma-ray bursts, vacuum birefringence had been constrained to better than one part in 10^{37}. (Kostelecký & Mewes 2006)

However, as with the RMS testing regimen, specialist centers have been established for testing Lorentz invariance in various sectors of the SME domain, such as the Walsworth Group patronized by the Harvard-Smithsonian Center for Astrophysics and the Harvard University Department of Physics. (Walsworth 2010) The Group has explored Lorentz invariance and symmetry exploiting the experimental advantages of a

[59] Ralston and Nodland have claimed a controversial but unconfirmed cosmological anisotropic polarization effect derived from radio waves emitted by 160 distant galaxies. (Cowen 1997; Nodland & Ralston 1997; Ralston & Nodland 1997; University of Rochester 1997)
[60] Microwaves are maintained in two resonant cavities – one oriented east-west with the other held vertical – and are monitored for both diurnal terrestrial motions and complete orbital passage of the earth around the sun. "Any orientation – or speed-dependent changes - in the speed of light would alter the resonant conditions of the cavities in a measurable way." (Schewe, Riordon & Stein 2003)

‘Dual Noble Gas (^{129}Xe / ^{3}He) Maser,’ permitting probes of pertinent physics down to the Planck scale. (Glenday, Phillips & Walsworth 2010; Pospelov & Romalis 2004)[61] Other Group publications include reports of the absence of isotropic Lorentz invariance infractions for high-energy physics up to 104.5 GeV for electrons tested at the CERN Large Electron Positron collider and 300 GeV for photons tested at Fermilab’s Tevatron. (Hohensee et al. 2009) Elsewhere, a torsion pendulum experiment specially developed by the Eöt –Wash group at the University of Washington provides high precision testing of Lorentz violation for electrons. Consisting of two different kinds of permanent magnets, the hanging torsion pendulum is highly sensitive to any extraneous Lorentz-violating spin coupling as the Earth’s motion continually re-orientates the apparatus relative to a galactic reference frame. Experimental results have constrained relevant parameters to less than 2×10^{-29} GeV. (Pospelov & Romalis 2004, p. 43)

Further, insofar as SME testing of the isotropy of space-time finds common ground with counterparts in the RMS investigative jurisdiction, modern descendants of the classical Michelson-Morley test are also conducted for assessment of both RMS and SME parameters sensitive to direction-dependent anisotropies in the speed of light. (Antonini et al. 2005) More recent results have claimed a 7.4×10^{-9} constraint on one SME test parameter “and the first analysis of a single experiment to simultaneously set limits on all nine non-birefringent terms in the photon sector of the SME.” (Hohensee et al. 2010, p. 1) Also utilizing Michelson-Morley styled testing procedures within the SME test theory domain, the Düsseldorf based Institute for Experimental Physics claim to have imposed upper bounds on many of the SME coefficients to within a few parts in 10^{17}. (Eisele 2009)

Interpreting a strong empirical compliance

Although it is generally explained that such results continue to improve the level of precision at which the SME’s photon sector is bounded and to tighten constraints on conjectured alternative formulations of relativity theory, (Hohensee et al. 2009) in all, breaches of Lorentz invariance have remained illusive. On the other hand, the philosophical justification supporting the test theory paradigm has more recently attracted a renewed level of criticism such as from Daniel Gezari who claims that “the experimental basis for the special theory of relativity in the photon sector is not robust.” (Gezari 2009, p. 1) Gezari is not the first to raise doubts or discuss limitations intrinsic to the test theory model. As early as 1963, Edwards had broached the constraint “that

[61] After a 90-day observation period, no modulation of the ^{3}He precession frequency was observed at Earth’s rotation rate, imposing a relevant parameter precision of 5×10^{-32} GeV. (Pospelov & Romalis 2004, pp. 42-43)

one cannot unambiguously measure the 'one-way' velocity of light," which is the quantity actually specified by the second postulate of the Einstein electrodynamics and is, at least in terms of contextual consistency, the definition intended for '*c*' in that 1905 exposition of Lorentz invariance. For Edwards, it was just this ambiguity that "allows certain liberties in synchronizing clocks and [that] leads to interesting differences in the formalism of special relativity," and, as Edwards noted, also gave sustenance to a school of thought that held,

> … that relativity theory contains an important assumption that has not and possibly cannot be tested and that, because of this, the foundations of relativity are uncertain. (Edwards 1963, p. 482)

Although Edward's research into alternative definitions of simultaneity resulted in a valuable adjunct to test theory development, he nonetheless prefaced his contribution by noting that there "are an infinite number of correct Lorentz transformations corresponding with the infinity of possible clock synchronizations," and further warned that "using electromagnetic means alone, the measurement of the one-way speed of light or the establishment of an absolute simultaneity cannot be accomplished." (Edwards 1963, p. 483) Zhang reiterated the moral, noting that "it is the two-way speed but not the one-way speed of light [that] has been measured," (Zhang & Chang 1997, p. 3) and he therefore cautions that existing light-speed isotropy experiments have limited domains of applicability.

Featuring in another complaint, a deficiency deriving from non-specificity finds mention in much of the test theory related literature. The claim that a test theory functionally adjudicates between competing formalisms is seriously challenged by many, including its practicing disciples. In particular, Zhang has noted that an RMS model must fail to distinguish between non-aether theories and theories in which a supposed aether frame was claimed as physically unobservable. (Roberts & Schleif 2007) Roberts and Schleif expand on the theme to suggest that,

> … some of the parameters in these test theories are not determined at all by SR (or by experiments) – this means that many different theories, characterized by different values of such parameters, are equivalent to SR in that they are experimentally indistinguishable from SR … (Roberts & Schleif 2007)

For Gezari, such revelations, although acknowledged as of grave importance, fall short of a more complete indictment, which might additionally advise that the implicit equivalence of relative source and observer motions has not been experimentally established. As Gezari explains, all relevant experimentation to date has involved establishing the independence of light-speed from motion of the source, whereas it is “the invariance of *c* to motion of the observer [that is] the tacit assumption underlying all of the predictions of special relativity in the matter and photon sectors.” (Gezari 2009, p. 1) Among other charges Gezari notes that none of the new optical effects predicted by special relativity, including the foundational claim of invariance of *c* to motion of the observer, “have ever been directly observed.”[62] (ibid., p. 1) He further notes that the enclosure of resonant cavities within metallic chambers, as found in many modern optical resonator and laser adaptations, admits of the functional possibility that the apparatus might shield the experiment from the conjectured weak field effects intended for detection, and so cannot be dismissed as the inadvertent cause of a ‘null’ result. (Gezari 2009, p. 5) Although the potency of Gezari-styled critiques has yet to be fully appraised, Edwards felt comfortable in dismissing the import of the ‘one-way’ light-speed contention as far as it only impinged on philosophical rigor, arguing that it would have no pertinent effect in purely practical terms. However, although the implications of potential experimental corruptions through ‘shielding’ have yet to ‘have their day in court,’ most physicists have adopted a similar pragmatism.

Thus, to that extent that intensive theoretical and experimental programs might claim to be counselors to metaphysics, hopefully to inform us literally and unambiguously how the world actually is, the current record is discouraging. Given the extremes of precision now claimed by both test theory and independent investigations detailing an intense compliance to local Lorentz invariance, any proposed alternative to current relativity theory must produce experimental results either identical with the prevailing formulation of Lorentz invariance, or sponsor results exhibiting within the extremely small, and rapidly diminishing, experimental uncertainties - that is, to ‘live within the error bars’. (Roberts & Schleif 2007) So unrewarding has the program become, that some philosophers of science have suggested a ‘rethink.’ Even when technological investigations of quantum nonlocality were still in their infancy, John Bell had already considered that,

> … we have an apparent incompatibility, at the deepest level, between two fundamental pillars of contemporary theory ... It may be that a real synthesis of quantum and relativity theories requires not just technical developments but radical conceptual renewal. (Bell 1984, p. 172)

[62] Gezari notes that “the majority of tests of special relativity in the photon sector investigated secondary or implied effects … The invariance of *c* can now only be inferred from indirect experimental evidence.” (Gezari 2009, p. 1)

The possibility that misconception may exist at a fundamental level – that the advance of theoretical physics may have inadvertently taken a wrong turn early in the generative epoch of our currently received formalisms – is also implicit in commentary by such as Tim Maudlin who suggests that,

> To understand, and perhaps resolve, the conflict we must consider carefully just what Relativity tells us about space and time. (Maudlin 2002, p. 24)

Lee Smolin concurs claiming that: "To continue the progress of science, we have to again confront the deep questions about space and time, quantum theory, and cosmology," (2006, p. xxiii) and advocates a break with current practice - albeit inspired by such as Richard Feynman and Freeman Dyson and temporarily quite fruitful - wherein reflection on foundational problems had no place in research. On this view, different modes of scientific activity are required to address different sorts of problems. As Smolin sees it, the increasingly apparent dilemma requires a necessary recapture of the way "early-twentieth-century revolutionaries did science," wherein work progressed within a broader philosophical tradition encouraging "deep thought on the most basic questions surrounding space, time, and matter." Insofar as Smolin considers that the 'answer' is often to be found "in a direction other than the one pursued by the mainstream," (2006, p. xxi) he expresses deep concern about that popular trend in which he believes the string theory approach has gained an untoward monopoly of people and resources, effectively starving other potentially fruitful approaches:

> It is a trend with tragic consequences if, as I … argue, the truth lies in a direction that requires a radical rethinking of our basic ideas about space, time, and the quantum world. (Smolin 2006, p. xxiii)

Chapter 3
Considering a ‘conceptual renewal’ response.

Suggestive of more than is contained in the now damaged hope of discovery of some as yet unknown but essential principle still obscured in the enveloping complexities of contemporary theoretical speculations, the Bell-Maudlin-Smolin opinion allows for the notion of recovery or re-discovery of concepts perhaps previously misinterpreted or over-looked yet potentially reclaimable through a redirection of research effort. Given that inconsistency is demonstrable between received formalisms, it would seem reasonable to ask where, when, and under what circumstances, the germ of a now visible discrepancy was introduced. Pursuing a similar sentiment, but in the more general sense, prominent Einstein scholar Gerald Holton has warned that “sound historical investigations have lately perhaps been overlooked as important bases of sound philosophical discussions.” Holton has fielded his belief that it is “through the dispassionate examination of historically valid cases that we can best become aware of the preconceptions which underlie all philosophical study.” (Holton 1960, pp. 627, 636) Shortly after, Kuhn was to add his insistence on the value that he believed the history of science had for the benefit of the philosophy of science. (Bird 2009; Kuhn 1962) In sympathy with such views, clues to potentially fertile historically based research questions are not hard to discover. In their analysis of the rapidly changing fortunes of special relativity, Albert and Galchen suggest it useful to recall that it is

> from wave functions … the concept Erwin Schrödinger introduced eight decades ago to define quantum states … that physicists infer the possibility (indeed the necessity) of entanglement, of particles having indefinite positions [and] it is the wave function that lies at the heart of puzzles about the nonlocal effects of quantum mechanics. But what is it, exactly? Investigators of the foundation of physics are now vigorously debating that question. Is the wave function a concrete physical object, or is it something like a law of motion … or is it merely our current information about the particles? Or what? (Albert & Galchen 2009, p. 33)

However, bearing in mind that it now would appear that it is special relativity in particular that has matured carrying the taint of theoretical paucity - that nonlocality is an authentic feature of the world that special relativity theory fails to include or to explain - it would also appear reasonable to redirect an enquiry more towards its particular historical and philosophical origins and theory development. “How and why,” we are entitled to ask, “did such a strategic theory develop devoid of an aspect of nature that was later to prove so intrinsic?" Was this truth so successfully veiled by an inscrutable and reticent natural order that it proved impenetrable to even the best of minds, or did clues go unrecognized, or diminished, or misinterpreted, or were possible pathways to understanding left unexplored? Alternatively, and of equal importance, we might ask: "How and where did constructs such as 'locality' and 'separability,' that now appear disconfirmed as theoretical predictions, enter into theory construction?" Sensitive to the need to ask such questions, Keswani has offered his assessment that irrespective of the future of special relativity, "sufficient research has not been made into its origins." (1965, p. 286)

Engaging the history and philosophy of science as research tools

Following then from the implications of such as Bell, Maudlin, Smolin, and others, a line of enquiry seems at least plausible – the possibility that an appropriate investigation specifically centered on origins, developmental influences, choices available and choices made, might uncover early defects in theoretical construction – and further, if taken to a logical sequel - explore the possibility of redevelopment of a relativistic construct consistent with a non-local world. The most appropriate subject matter for a review intended to uncover potential developmental anomalies would present in the broad view as that of optics. In particular a review of origins and theory construction might benefit from a philosophical and historical analysis surrounding Einstein's electrodynamics, that posed an alternative approach to Lorentz invariance when admitting electromagnetic phenomenon into an equal status with mechanics. Additional analysis might profitably include Minkowski's geometrical reformulation of space and time that posed a four-dimensional spacetime continuum that eventually engaged an Einsteinian interpretation of Lorentz invariance as governing all inertial reference frames. Yet it must also be admitted that such contributions to theory construction did not occur in isolation. Each occurred within an intricate historical and philosophical context, with each the legatee of the yet broader historical preface.

Displayed in its most traditional mould, the historical and philosophical pageant of optics and electromagnetism is presented with heavy emphasis on a rehearsal, often in narrative form, of those contributions of individual thinkers and their experiments that are judged crucial to the view of an orderly accretion of a scientific knowledge base and the theorizing deriving from it. (Herzberger 1966; O'Connor & Robertson 2002b; Römer 2005; Spencer 1999; Vohnsen 2004; WSRNet 2004) However, when introducing their highly regarded text on the *Principles of Optics*, Born and Wolf claimed that "it is impossible to follow the historical development [of optical theory], with its numerous false starts and detours," and found it pragmatically necessary to replace a detailed analysis with a more manageable but necessarily edited recount of those events they judged as "the main landmarks in the evolution of ideas concerning the nature of light." (Born & Wolf 1999, p. xxv) However, and without casting aspersions, such conventional rehearsals of historical factors are necessarily the carefully crafted work of the historical editor. Each datum has been chosen from a much larger data set that most often exhibits as an array of disparate, conflicting and counter-factual elements. Editorial selection may well tend to impose a theme – a story responsive to such perceived needs as cohesion, continuity and a sense of linear progress. Typically, historical editors might feel obliged to make sense of, and to ultimately justify, the contemporary undertakings and prevailing world-view of a science, to maintain its desired self-image, and pay homage to its chosen heroes. As historian Alistair Combie noted when accepting the 1968 Galileo Prize:

> … it seems to me that [Galileo's] reputation illustrates the universal habit of creating myths to justify attitudes taken to the present and future, myths intimately tied in Western culture to our conception of time and history … But our intellectual inheritance is also an essentially critical one, predisposing each generation to take to pieces the history written by its predecessors in their image, before re-writing it in its own. (Combie, 1970, cited in Matthews 2006, p. 211)

Although it would be churlish to lightly dismiss their legacy, or doubt the best of intentions of laudable commentators and analysts, basic wisdom would suggest that possible inadvertencies, misconceptions, or counterproductive choices are more likely to be discovered in a review of raw uninterpreted historical data. In this, however, further difficulties become apparent. An admission of irremediable historical complexity presents a more complex view of those inter-revolutionary periods of activity, denoted by Kuhn in his not uncontroversial account of scientific development, as 'normal' science. In the Kuhn characterization, such episodes are portrayed as both relatively stable and yet progressive in terms of problem solving under the aegis of the prevailing scientific mindset and norms. (Bird 2009) In the Born and Wolf assessment, however, such periods appear to suggest a more complex dynamic wherein the approach guiding research at any given time may well prove obstructive, irrelevant, or even plain wrong as judged by later review. Others have similarly identified impediments to the notion of a stable and incremental scientific progress:

> Concepts that have proven useful in ordering things can easily attain an authority over us such that we forget their worldly origin and take them as immutably given. They are then

rather rubber-stamped as a "sine-qua-non of thinking" and an "a priori given", etc. Such errors make the road of scientific progress often impassable for long times. Therefore, it is not at all idle play when we are trained to analyze the entrenched concepts, and point out the circumstances that promoted their justification and usefulness and how they evolved from the experience at hand. This breaks their all too powerful authority. They are removed when they cannot properly legitimize themselves; they are corrected when their association with given things are too sloppy; they are replaced by others when a new system can be established that, for various reasons, we prefer. (Einstein 1996)

Quantum theorist, David Bohm has also gone on record to not only identify a very human cause of obstruction, but to call for a reassessment of foundational concepts and assumptions facilitated by a profound change to our way of thinking:

> When science won its battle with the Church for the freedom to entertain its own hypotheses, it in turn became the principal repository of the idea that particular forms of knowledge could either be absolute truths or at least could approach absolute truths. Such a belief in the ultimate power of scientific knowledge evoked strong feelings of comforting security in many people, almost comparable with the feelings experienced by those who have an absolute faith in the truths of religion. Naturally there was an extreme reluctance to question the very foundations upon which the whole basis of this sort of truth rested . . . Clearly, the whole problem of ending the mind's defense of its tacitly held ideas and assumptions against evidence of their inadequacy cannot be solved within the present climate of scientific research. For within this context, every step that is taken will, from the very outset, be deeply conditioned by the automatic defense of the whole infrastructure. (Bohm & Peat 1987, pp. 24-25)

Research contributions from the philosophy of science

Although a role for the philosophy of science has been either depreciated,[63] perverted,[64] or held irrelevant as a valid participant in matters of research by many influential scientists, prominent philosophers of science, such as Alfred North Whitehead have legitimized its role as being the 'critic of abstractions," and the

[63] 1979 Nobel Laureate, Steven Wienberg is reported to have remarked: "The knowledge of philosophy ... helps us (only) to avoid the errors of other philosophers." (Hartmann 1995, p. 51)
[64] Giere notes a perversion when "reflecting on the role of philosophy in the Christian era prior to the scientific revolution. Philosophy then was closely allied with theology -- its goal being to systematize and justify religion. The current widespread conception of the goal of the philosophy of science as being to systematize and justify science may be seen as a modern, secular version of the earlier role. The battle [now] is with the scientific skeptic rather than the religious skeptic, but the nature of the combat is the same. For some philosophers it may ease the transition away from this view of the philosophy of science if one thinks of it as 'the theology of science,' or 'scientific apologetics.'" (Giere 1986a, p. 323)

“constant critic of partial formulations.” (1938b, pp. 75,106,167) For Whitehead, such philosophy is “the survey of sciences, with the special objects of their harmony, and their completion,” (1938b, p. 106) and has identified its inclusion and research contribution as essential:

> If science is not to degenerate into a medley of *ad hoc* hypotheses, it must become philosophical and must enter upon a thorough criticism of its own foundations. (Whitehead 1938b, p. 29)

To assist with such a philosophical criticism, Whitehead identified what he believed were ubiquitous but possibly unconscious presuppositions limiting the modes of scientific thinking. He also suggested that such distortions bore destructively on the validity of scientific conclusions and thus potentially perverted the course of competent theory construction. Among these he identified the “fallacy of simple location,” and the “fallacy of misplaced concreteness,” and warned of the result of “these patterns of thought without recognizing their serious deficiencies.” (Anderson 2000)

Whitehead’s ‘Fallacy of simple location’

Although some ambiguity is claimed to surround his use of the term *simple location* (Alston 1951, pp. 713-715), Whitehead identified the idea as an assumption which, in his view, “underlies the whole philosophy of nature during the modern period,” and is “embodied in the conception which is supposed to express the most concrete aspect of nature.” According to Whitehead’s usage, (1938b, p. 64) the term exhibits a “major characteristic which refers equally both to space and time:”

> To say that a bit of matter has simple location means that, in expressing its spatio-temporal relations, it is adequate to state that it is where it is, in a definite finite region of space, and throughout a definite finite duration of time, apart from any essential reference of the relations of that bit of matter to other regions of space and to other durations of time. (Whitehead 1938b, p. 74)

Attempting further amplification, Alston suggests that Whitehead’s designation of a “fallacy” of simple location stems in principal either from an exclusion of reference to any relations a physical object might have to other spatiotemporal regions, or that it occupies an identified spatiotemporal location “to the exclusion of *occupying* any other.” (Alston 1951, p. 716, orig. emph.). For Whitehead and philosophers sharing elements of a similar philosophical appraisal, simple ascription of discrete and isolated individual independence to bits of phenomena as sufficiently descriptive of physical reality, denies or ignores other essential aspects equally defining of real objects. On such a view, enduring patterns, processes, and "change of every sort – physical, organic, psychological" - are appraised as " both pervasive in nature and fundamental for its understanding" and thus also for comprehensive definition. (Rescher 2002, intro) For Herstein, although "related to specific vector quantities" derived from sampling at selected spatiotemporal locations, "physical facts such as electromagnetic phenomena are single, relational wholes ... spread out across the cosmos" demonstrative of a "holism and the relational connectedness of reality" (Herstein 2007, 2e)

In particular, potential limitations inherent in a 'simple location' credo, when applied to some specifics of science as practiced in physics, suggests an equivalent potential for limitation particularly impinging on its sanctioned ontological repertoire. A physics biased towards 'substantial things' exhibiting the spatiotemporal property of simple or 'single'[65] location, potentially devalues relevance, or even ontological legitimacy, to enduring patterns or structures deriving from processes that are "physically realized without being literally embodied." (Rescher 2002, sec.6). In pursuit of the distinctions involved, Whitehead has added:

> … taking a metaphor from the ocean tides, the system will sway from one high tide to another high tide. This system, forming the primordial element, is nothing at any instant. It requires its whole period in which to manifest itself. In an analogous way, a note of music is nothing at an instant, but it also requires its whole period in which to manifest itself. (Whitehead 1938b, pp. 50-51)

In this respect, entities manifesting as fields, waves, and expanding wavefronts, essentially involving temporal processes and limited durations, appear to have at various times been the casualties of a bias towards the simply locatable and discretely countable. Thus models attempting their representation, particularly in the realm of optics, may have equally been disadvantaged. By Rescher's assessment, the supremacy of the "thing-quality" paradigm has in consequence relegated "the concept of process to some remote and obscure corner of the ontological warehouse." (1962, p. 412) This bias, Rescher believes is far-reaching, representing "the virtually standard position among current writers on ontological subjects, (1962, p. 410) It is then not without significance that critical research, if including the implications of a bias toward simple location, might be thought to profitably include within its historical and philosophical

[65] Alston suggests in this latter regard that the "fallacy of simple location ... could have been more unequivocally termed "the fallacy of *single* location ..." (Alston 1951, p. 716)

scope, the need for careful review of such concepts as *separability* and *location* as those ideas, common to an earlier mechanistic world-view, interacted with early twentieth century theory construction.

Whitehead's 'Fallacy of misplaced concreteness'

Taking the last few centuries as exemplifying the rise of modern science, Whitehead identified what he interpreted as another general failing which he termed "the fallacy of misplaced concreteness" to account for an unwarranted emphasis on materialism together with an uneven development between science and its philosophy that he believed characterized this period of scientific evolution. (Whitehead 1938b, p. 72) Others have elaborated on this view:

> While misplaced concreteness could include treating entities with a simple location as more real than those of a field of relations, it also went beyond this. Misplaced concreteness included treating "points" of space or time as more real than the extensional relations that are the genuine deliverances of experience. Thus this fallacy resulted in treating abstractions as though they were concretely real. In Whitehead's view, all of contemporary physics was infected by this fallacy, and the resultant philosophy of nature had reversed the roles of the concrete and the abstract. (Herstein 2007, 2e)

Expanding on this view and in quite practical terms, misinterpretation and misunderstanding of abstraction can be claimed to unwittingly empower the capacity to reify[66] a philosophical conception, a mathematical formula, or an idealized model, assigning to them the status of the physically real and the concrete. As an unsuspected and unconscious tendency, it stands accused by some of tainting the development of the sciences, and generating a source of great confusion in philosophy, even from early times. In interesting comment, Stahl suggests that 'objectification' or 'reification' is an idiosyncratic characteristic of that language group which above all, fostered the rehabilitation of European science into the modern era:

> One of the most significant characteristics of Indo-European linguistics is objectification ... Objectification is not limited to classical physics; moreover, the concept 'objective' is not limited to 'concrete'. Percy Bridgman discussed what he termed the 'reification' of ideas in English, i.e. construing ideas as things through nouns. In Indo-European usage, a noun-as-thing generally serves in place of a verb-form-as-state-of-being ... (Stahl 1987, p. 57)

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[66] "... the mistake of confusing a system, which is a construct, with the physical entity described in its terms." (Krippendorff 2002)

Whereas simple location might tend to restrict physical significance to little more account than localized and separable embodiment, such reification might grant an unwarranted substantiality to the abstract, and exhibit in a range of guises. To the extent that mathematics became the 'language of science,' a mathematized physics was engaged as a dominant component of what we now call 'classical science.' (Brown 1986, p. 53) Literalist overtones of an arcane doctrine of the early Pythagorean brotherhood - that number underlies the basis of the real world - was then further processed via the philosophy of Plato to take root as a concretion in contravention of the purely abstract role Whitehead demanded for mathematics. Whitehead disavowed this as error. As Gare explains:

> Whitehead treated mathematics as abstraction.[67] Whitehead did not denigrate abstraction. He praised it and what has been achieved by it, particularly the abstractions of mathematics; but he decried the tendency to take abstractions as concrete reality, ignoring the level of abstraction involved and ignoring the background concrete unity of experience. This is the 'fallacy of misplaced concreteness', a fallacy which characterises the world-view of scientific materialists who take colourless atoms moving in space to be real and dismiss as unreal the sensible world around us. (Gare 2006, p. 9)

It might then be felt significant that Göttingen University, contemporaneously with early relativistic theory construction, is acknowledged as the historical seat of a specific development of the mathematization of physics and coincident with that era in which Göttingen mathematician, Hermann Minkowski, advanced his radical geometrical reformulation of space and time. Significance also extends to Albert Einstein's elaborations on a similar theme. In appreciating the extent to which such science became mathematized, Morris Kline claims:

> Einstein seized the idea [of non-Euclidean geometry] in order to fit our physical world into a four-dimensional, mathematical one. Thereby gravity, time, and matter became, along with space, merely part of the structure of geometry. Thus a belief of the classical Greeks that reality can be best understood in terms of geometrical properties [has] received sweeping affirmation. (Kline 1985, p. 219)

[67] "… mathematics is the science of the most complete abstractions to which the human mind can attain." (Whitehead 1938b, pp. 48-49)

Epistemologically, for many mathematicians such as Kline, a reification of mathematical abstractions as an integral component of a now dominant scientific mind-set, continues to find wide-spread support:

> The theory of relativity warns us against taking appearances, which hold only for a particular reference system, as the truth in any absolute sense. Here as in other physical areas mathematical laws tell us what is truth and really objective. (Kline 1985, p. 219)

Consistent with such an interpretation, modern ontological assertions include such views as those of cosmologist Max Tegmark who asserts that "there is only mathematics; that is all that exists," (Frank 2008) and the contemporary cosmological construct of 'spin nets' which views the universe as "conjured up from pure mathematics." (Johnson 1999) Posing such an alternative but minority view, Peirce has claimed the role of mathematics at most as ancillary and analogical:

> [Mathematical] deduction consists in constructing an icon or diagram the relations of whose parts shall present a complete analogy with those of the parts of the object of reasoning, or experimenting upon this image in the imagination, and of observing the result so as to discover unnoticed and hidden relations among the parts. (Peirce cited in Gare 2006, p. 7)

Further, when writing recently for a more general audience, Arianrhod has also gently chided the contemporary neo-Pythagorean premise advising that:

> … some of us moderns like to think we see true reality in our mathematics … Although we can prove mathematical propositions to be completely true, in terms of the rules of pure mathematical language, when we *apply* our equations to describe our sensory impressions of the physical world, we cannot be so sure of their 'truth' … they do not fit *perfectly* with the actual physical reality … after all, any description, including a mathematical one, can only be an approximation of the thing itself. (Arianrhod 2003, p. 147)

The historical theory-constructive role played by mathematics, then, suggests need for careful review, particularly if its functionalism as an abstract representational agent is acknowledged.

Theory construction and scientific method

Beyond the distorting role potentially played by biases such as suggested by Whitehead, the philosophy of science might further be thought useful in promoting insights into an investigation of the construction of a theory whose predictions are eventually held to fail empirical compliance. Although it is generally recognized that a philosophy of science offers more than simply separating science from non-science, it is also well recognized as a critic of the logic underwriting methodologies employed in the cause of scientific enquiry and entailing the construction of scientific theory.

Justifying the vast, and generally realist, enterprise of scientific enquiry are certain philosophical assumptions: that in some form there exists a reality that is both objective and consistent and that through an appropriate system of enquiry, rational explanations can be discovered both for the way it exists and behaves. A majority view held by its practitioners, and tracing its beginnings to the seventeenth century, contends that the most appropriate system of enquiry is expressed as "the scientific method." Such a method has long been expressed as a sequence of steps leading from initial questions about the world to their successful resolution in the form of an explanatory theory. In contrast, a minority view is typical of that expressed by Paul Feyerabend. In his "*Against Method,"* Feyerabend

> explicitly drew the "epistemological anarchist" conclusion that there are no useful and exceptionless methodological rules governing the progress of science or the growth of knowledge. The history of science is so complex that if we insist on a general methodology which will not inhibit progress the only "rule" it will contain will be the useless suggestion: "anything goes". (Preston 2009)

A review of introductory notes on methodology prepared by both university and secondary level science courses explaining the expected conduct of science by their students, is notable for the singular lack of a consistent catalogue of required steps. Here, scientific method presents as a body of related techniques. For some four, (Wolfs 2005) five, (Wudka 1998a) seven, even eight, steps are variously listed giving the appearance of a diversity which might provide some small comfort for the Feyerabend conclusion. On the other hand, a small, but key group of elements remains consistent, if not in word, then at least in intent. By dictionary reference, the scientific method has been summarized as:

> principles and procedures for the systematic pursuit of knowledge involving the recognition and formulation of a problem, the collection of data through observation and experiment, and the formulation and testing of hypotheses. (Merriam Webster's Collegiate Dictionary 2006)

Most conventional rehearsals view the method as conforming to a hypothetico-deductive approach to reasoning (Hesse 1953, p. 189) and further expand "*the formulation and testing of hypotheses*" to reveal a recursive scheme. Here, hypothesis proposal is followed by empirical testing of its predictions resulting in its possible modification, followed again by renewed predictions and retesting in a process towards an empirically successful theory. Such a reiterative scheme is held to be unprejudiced and ultimately self-correcting, its results to be independently repeatable, and to hold all forms of theoretical construction to remain falsifiable in principle. (Wudka 1998a) However, caution needs to be exercised in the correct use of terms as in any scientific context. In particular, the difference between the scientific meaning of words like *theory* and *hypothesis* and the conventional use of those terms can prove crucial:

> Scientists frequently use the terms 'hypothesis' and 'theory' and although the precise use of these terms is critical to understanding the scientific method there is little understanding of the scientific meaning of these words among the general public. (Fisher 2007)

Pursuing the issue as it reflects on the validity of some current debates that intend to elevate the status of conjectural pseudo-scientific claims by diminishing the status of scientific theory to that of the provisional, Fisher notes that:

> Having discussions about controversial issues is frequently muddied by misconstruing or deliberately misrepresenting what scientists mean by key concepts like 'hypothesis' and 'theory.' In order to have rational discussions ... we need to understand what *scientists* mean by these terms. [In short] we need to know the difference. (Fisher 2007)

For the purposes of scientific discussion, Fisher defines hypothesis as: "a provisional conjecture or preliminary explanation as to the cause of an event or relationship among things, frequently taking the form of an "if then" statement." (Fisher 2007). Elsewhere *hypothesis* is defined as "a working assumption," (Wudka 1998b) and for many scientists, *hypothesis* necessarily implies insufficient evidence to provide more than a tentative explanation. In contrast, the American Association for the Advancement of Science defines a scientific theory as: "a well-substantiated explanation of some aspect of the natural world, based on a body of facts that have been repeatedly confirmed through observation and experiment." (American Association for the Advancement of Science 2012) Although, in principle, even mature theories that have remained unchanged in the face of decades of intense scrutiny, retain the possibility of falsification, in a realist context, extensively corroborated theories are generally taken as reliable explanatory accounts of the real world.

On the other hand, in doubting that concepts such as 'truth' have useful definition, or that, even if conceded, are ultimately attainable, antirealists[68] such as Hawking assert, "that a physical theory is just a mathematical model and that it is meaningless to ask whether it corresponds to reality." (Hawking & Penrose 1996)

Models and scientific method

Despite the generally acclaimed success of the scientific method, attempts to empirically verify some theoretical predictions can nonetheless be thwarted:

> For practical or even ethical reasons, however, it is seldom possible to experiment on large organized systems as a whole in their natural environments. This results in an apparent dilemma: to gain understanding of complex systems experimentation seems to be necessary, but it cannot usually be carried out. This difficulty is solved by the use of *models*, representations of the system under study. Provided the model is good, experiments (called "simulations") can be conducted on it. (Encyclopædia Britannica 2006)

However, since a rise in interest in the roles modeling might play in science, a select literature has developed that additionally promotes the use of models as an integral element of a general scientific method. In such an expanded scheme of methodology, hypotheses are construed as developed from purposely chosen or constructed predisposing models. Hypotheses may then be construed as the model reformulated in a testable form. On this view, "modeling is a pervasive and necessary methodological feature, assisting scientists to arrive at useful theoretical explanations of real systems. (Suárez 1999a, p. 168) Giere adds that in the realization of this role "scientists use designated similarities between models and aspects of the world to form both hypotheses and generalizations," (Giere 2004, p. 742) and Hartmann, in describing the dynamic process of physics as "the continuous endeavour of theory construction," defends his thesis: "As a major tool for theory construction, scientists use models." (Hartmann 1995, p. 49) In such an expanded scheme, empirical evaluation of hypothetical prediction is believed to reflect back on the adequacy of the sponsoring model, which might need to be suitably modified before reformulation of a potentially more successful hypothesis. Before attempting to harness the potential of such a scheme as an adjunct to a philosophical enquiry into the construction of a theory now believed to fail empirical adequacy, some attempt to understand the contemporary intent of

[68] "The realist-antirealist issue is usually put in terms of whether all scientific statements are conjectures about the nature of the reality under investigation or whether they, or members of some class of them, are mere instruments for prediction and manipulation, devoid of any descriptive content." (Grobler 1995, p. 37)

modeling terminology and the claimed roles and functioning of models in science seems appropriate.

Although a significant corpus of literature exists testifying to the significant role now assigned to representational modeling as a valued methodological adjunct in all branches of the scientific enterprise, (Morrison & Morgan 1999, p. 10) a scientific interest specifically in the analysis and utilization of scientific models and their relationship to theories and theory construction, seems to be relatively recent. According to Giere (1986b, p. 277), pioneering works on scientific modeling as a subject of interest in the philosophy of the sciences first appeared as early as the 1930's, 1940's and 1950's with the works of McKinsey, Beth, and von Neumann. However, the topic had to wait until the 1960's to rise to any prominence, with the work of Patrick Suppes (1960a; 1960b) and Mary Hesse (1963; 1967) being of particular note. Nonetheless further interest in the topic did not fully manifest for some further years. Summarizing, Koperski notes that:

> For most of the 20th century, the use of models in science was a neglected topic in philosophy. Far more attention was given to the nature of scientific theories and laws. Except for a few philosophers … most did not think the topic was particularly important. The philosophically interesting parts of science were thought to lie elsewhere. (Koperski 2006 ch.1)

Once considered at most merely heuristic, the role of models in the contemporary context has broadened such that the "situation is now quite different. As philosophers of science have come to pay greater attention to actual scientific practice, the use of models has become an important area of philosophical analysis." (Koperski 2006 ch.1). Further, when recognizing the practical role played by models in the teaching of physics, Hestenes asserts that: "Scientific practice involves the construction, validation and application of scientific models, so science instruction should be designed to engage students in making and using models" which he labels "coherent units of structured knowledge." (1996, abs)

The unhelpful diversity of modeling concepts and terminology

The term "model" has a range of meanings in general usage but the potential for ambiguity appears not mitigated by restricting its application to the exact sciences alone where there seems to be little agreement on the adoption of a consistent interpretation and terminology.[69] In common scientific practice, a model is generally deemed to be some sort of representational vehicle relating to a 'real-world' system under investigation, whereas, in its more formal setting of set-theory logic, a model is considered to represent that underlying structure "that makes all the sentences of a theory true, where a theory is taken to be a (usually deductively closed) set of sentences in a formal language' (Frigg & Hartmann 2006, sec. 1.3). Addressing then what might seem, on face value, an apparently simple dichotomy of meaning, Hesse observed:

> There is a sense of 'model' in science that is both nearest to its sense in ordinary language and furthest from the logician's sense. Replicas, scale models, and analogues are familiar in various contexts and may be said to provide, after logical models, the second main source of ideas associated with the term 'model' in the sciences. (Hesse 1967)

Expanding elsewhere on the same theme, McMullin has also acknowledged a basic dichotomy of meaning wherein:

> The logician's 'model' is an entity of known properties which satisfies a particular formal system and which can thus serve, for example, as a warrant for the consistency of the system. The physicist's model, on the other hand, is a tentative representation intended to explain some aspect of a real-world situation. For the logician, the model is the 'anchor'; for the physicist, it is the observed world. Thus, though there are affinities between the two sorts of models, since in each case there is a structure which 'satisfies' a formal system, the model plays opposite roles in the two contexts. (McMullin 1985, p. 257)

Nevertheless, it was to be the opinion of Stanford University's director of the Institute for Mathematical Studies in the Social Sciences, Patrick Suppes, in strongly favoring a formal structuralism early in the era of renewed interest in models, that placed the greatest strain on the development of a generally accepted interpretation. Having offered his postulate that "a model of a theory may be defined as a possible realization in which all valid sentences of the theory are satisfied," and that a realization of the theory is "an entity of the appropriate set-theoretical structure," (1960b, p. 252) Suppes announced a dissenting and challenging conviction, impugning the developing dichotomy of meaning, and that was to generate a period of both vigorous debate and the potential for much on-going contention:

[69] Hartmann records that Apostel in 1961 "claimed that a definition proper of the term "model" was, in fact, impossible since "model" is used with so many different meanings in science, logic, and philosophy." (Hartmann 1995, p. 51)

> It is my opinion that this [logician's] notion of model is the fundamental one for the empirical sciences as well as mathematics … Perhaps the most persuasive argument which might be singled out for mention here is that the notion of model used in any serious statistical treatment of a theory and its relation to experiment does not differ in any essential way from the logical notion of model … I would assert that the meaning of the concept of model is the same in mathematics and the empirical sciences. The difference to be found in these disciplines is to be found [only] in their use of the concept. (Suppes 1960a, p. 289, 1960b, p. 252)

As Psillos notes, Suppes' conception was offered as a potentially unifying approach "appropriate to characterize all uses of models in scientific practice, i.e. theoretical models as well as scale models and analogies." (Psillos 1995, p. 108) As illustration:

> [given that] *M* is a model for *T* if and only if *M* and *T* are structurally isomorphic ... the statements that constitute the model *M* of a theory *T* describe a system that is assumed to be distinct from the system of which it is a model. If a theory *T*, say the kinetic theory of gases, describes a system *X*, say the state of motion of a vast number of molecules, a model *M* of *T*, say the billiard balls model, describes billiard balls in a box and *not* molecules in a gas ... That is to say, the only constraint for choosing ... the billiard balls model is that its underlying calculus is just that of the kinetic theory of gases. (Psillos 1995, p. 108)

However, a rapid polarization of opinion is evident from the subsequent literature on the subject. Unfortunately, in the absence of a generally recognized theory of scientific representation, the ontological characterization of a model as an object within the scientific panoply became a subject of contention. A model's power to represent, its relationships to theories, laws, and even other models, were to be presented in a rapidly expanding corpus of literature by an unhelpful diversity of opinions, antagonistic claims and conflicting and inconsistent definitions. Commenting on this discord and its potential for developmental disruption, Wartofsky frankly deplored the 'model muddle' which he took to be "a semantic muddle – ultimately, an ontological muddle," regarding on one hand, the status of models as entities, and on the other, the status of what it is they model:

> In much of model-talk, models inhabit a limbo between worlds. On the one hand, they are not citizens of the blood-and-guts world of real objects and processes; or at best have only a derived citizenship by way of their reference to such a world. On the other hand, they are denied full equality in the cognitive world of purported truths, assigned only the function of

> instruments of such cognition: crutches, aids to the imagination, inference-machines, heuristic devices, data-ordering frameworks and whatnot.” (Wartofsky 1966, p. 3)

Although such a variety of interpretation might have been expected to contract with ongoing debate, it may be thought that the situation has actually deteriorated. (Hartmann 1995, p. 51)

A modern science of theories – the ‘model-theoretic’ view

The notion of a model in the logical sense, as an entity integral to elucidating the structure of scientific theories, presents as a relatively recent investigative exercise in the philosophy of science. Commenting on the historical development of such ideas, Koperski notes:

> For most of the 20th century, philosophers considered theories to be special sets of sentences. Theories on this so-called ‘syntactic view’ are linguistic entities [wherein] the meaning of the theory is contained in the sentences that constitute it … (Koperski sec. 6)

With some additional detail Giere informs us that:

> Until around 1970, the ‘received view’ … was that, for philosophical purposes, scientific theories were to be thought of as interpreted, formal, axiomatic systems. The axioms of a theory were statements which, in principle, were either true or false …Scientific theories had the structure of an axiomatic, deductive system. This account is now often referred to as the ‘statement view’ (or ‘law-statement view’)[70] of theories … (1986b, p. 276)

However, in the more recent history of philosophical investigations into the constitution of theories, the “semantic” view as promoted by Suppes, also referred to as the “nonstatement” , “predicate”, or “model-theoretic” view (Giere 1986b, p. 277), has emerged to provide a prominent alternative interpretation. Whilst notably employing the language of mathematical logic and set theory, the model-theoretic view has promoted models ontologically as non-linguistic entities rather than descriptions (Frigg & Hartmann, sec. 1.1; Suppes 1960a, p. 290). As Frederick Suppe recalls: “In the 1960s, the Positivistic Received View on theories was attacked …” with the more important charge based on the assertion that “theories are not linguistic entities and thus theories

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[70] For the purpose of nominative characterization, Chakravartty suggests that a “more perspicuous label might be *sentential* view.” (2001, p. 325)

[as syntactically analyzed] are individuated incorrectly.” (2000, p. S103) Suppe further recalls that historically:

> In 1969 a variety of alternative analyses were on the table[71] such as construing theories as answers to scientific problems, or as paradigms, or conceptual frameworks. Gradually analyses that construed theories as extra-linguistic set-theoretic structures came to dominate post-positivistic understanding. (Suppe 2000, p. S103)

Such an approach has been claimed as advantageous in that it tends to avoid the “time-honored problem of how language relates to reality” to which much attention had been directed in the philosophy of language. By the semantic account, fundamental theories are abstract ‘uninterpreted’ formalisms. Such a definition stands in contrast to a syntactical definition wherein theory is itself primarily and overtly explanatory. According to the semantic view, the model, as an underlying structure or postulate set having an identical calculus[72] to the axiomatized theory, can be said to formally ‘represent’ the theory. In further clarifying a semantic view of theory:

> “... fundamental theory represents nothing, there is simply nothing for it to represent since it doesn’t describe any real world situations. Instead real things and the ways in which they behave are represented by models.” (Morrison 1998, p. 69)

> I want to urge that fundamental theory represents nothing and that there is nothing for it to represent. There are only real things and the real ways they behave. And these are represented by models, models constructed with the aid of all the knowledge and technique and tricks and devices we have. (Cartwright, Shomar & Suárez 1995, p. 140)

As Suppe further explains: ”*The Semantic Conception* identifies theories with certain kinds of *abstract theory-structures*, such as configurated state spaces, standing in *mapping relations* to phenomena. Theory structures and phenomena are referents *of linguistic theory-formulations.* The basic idea is that theory structures are identified with suitably connected families of models” (Suppe 2000, p. S105, orig emph). Further, according to model-theoretic apologists da Costa and French:

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[71] Resulting primarily from the pivotal *Symposium on the Structure of Scientific Theories* held in Uubana, Illinois, March 26 to 29, 1969 (Suppe 1977, p. vii, 2000, p. S102)

[72] or class of formulae - the logical skeleton of the theory, considered as devoid of any empirical meaning (Nowak 1972, p. 541; Spector 1965, p. 122). Using Carnap’s terminology as referenced by Achinstein, the calculus, as uninterpreted strings of symbols, can be distinguished from a model, exhibiting as the same axioms, but considered as interpreted propositions. (Achinstein 1965, p. 108)

The essence of this approach is captured in the idea that a scientific theory can be considered in terms of a description of its set of models, regarded as the structures it makes available for modeling its domain … By ‘models’ is meant … relational structures for which all the sentences of the theory express true properties about the structure when the latter acts as an interpretation of the theory. This then allows us to apply the mathematical and logical techniques of formal model theory … to the analysis of scientific theories – one of the primary motivations for this approach. (da Costa & French 1990, pp. 249-250)

Explorations of the model-theory relationship

Nonetheless, alternative and more relaxed interpretations of the semantic model-theoretic relationship are advanced by some who feel the need for a more inclusive formulation based on a less restrictive structuralism – one that might incorporate aspects of the physicist’s model – and that might serve to unify terminology and meaning. In contrast to the robust formal identity relationship based on isomorphism[73] (Suppes 1960a, p. 295) or homomorphism, (Bartels 2006, p. 8; Suárez 2003, p. 239) others attempt to accommodate the less restraining interpretation of modified ‘partial structures’ or ‘embedding.’[74] (da Costa & French 2000, p. S123; Suárez 2003, p. 240). Promoting this weaker view, French optimistically suggests that “through appropriate modifications, a form of isomorphism can serve to underpin representation in both the arts and science.” (2003, p. 1473) However, others such as Giere (2004, p. 748) offer the weaker notion of “specified similarities.” This less confining approach claims an advantage over isomorphism by accommodating a wider range of modeling types and representational styles, even to including limited degrees of simplification and idealization (Frigg 2006b, p. 60; Frigg & Hartmann, sec. 1.1; Morgan & Morrison 1999, pp. 4-5) Commenting on such a diversity of approaches, Chakravartty suggests a justification in that, at least to some extent, such a difference of opinion among supporters of the semantic view “concerns the issue of how best to achieve an appropriate level of independence on behalf of theories from language”, a desirable outcome since such independence “saves us from the worry that one and the same theory given different linguistic formulations – that is, expressed in different languages – must be viewed as different theories.” (2001, p. 326)

[73] from the Greek *isos* "equal", and *morphe* “form” or "shape". A mathematical isomorphism is generally defined as a one-to-one correspondence or mapping between objects such as set-theoretic structures wherein a map f and its inverse f^{-1} are each ‘homomorphisms’, or ‘structure-preserving’ mappings.
[74] “Embedding is a relation between models, and relies on the isomorphism of one model with part of another ... [for] example the sequence [2,2,4 ...] can be embedded in the set of all sequences of integers.” (Downes 1992, p. 145)

On the other hand, other commentators discount Suppes' monopolistic claims made for formal structuralism, even when presented in modified forms. In this respect, an exclusive structuralism has attracted vigorous criticism on various grounds, most notably from those who believe that the practice of science and theory construction employs a wider range of modeling types and representational strategies than can be accommodated within such formal frameworks, however interpreted. (Frigg & Hartmann, sec. 2.3; Morrison & Morgan 1999, p. 10).

For Frigg, one fundamental objection claims that models "involve, but are not reducible to structures." (2002, sec. 1; 2006b, p. 53) Other objections are based on issues of efficacy noting that "isomorphism is symmetric and reflexive while representation is not" and that "isomorphism is too inclusive a concept to account for [all forms of] representation." (2006b, p. 54) In further critical comment, Frigg argues for retaining some syntactical understanding:

> … descriptions are an integral part of any workable conception of scientific representation and we cannot omit them from our analysis of representation… [this] casts doubt on a central dogma of the semantic view of theories, namely that models are non-linguistic entities. Models involve both non-linguistic and linguistic elements. (2006a, sec. 9)

Supporting this view, Wartofsky submits that "it remains clear that language is crucial in theoretical activity, and that both natural and constructed languages are central to theory-formation in the sciences" (1979, p. xviii). In this, Byerly concurs to the extent that "… 'complete' isomorphism for model-structures makes sense … only for mathematical objects, not for model-objects having descriptive content." (1969, p. 138)

In his own detailed criticism of claims advanced in support of formal structuralism, Suárez maintains that neither isomorphism nor structured similarities, on their own, "can account for the *means* of scientific representation." (2003, p. 229) Further, he also claims that they "cannot on their own *constitute* representation." (p. 231) In pursuing a deflationary argument from 'variety,' that additionally claims that such structures cannot be applied to the full variety of representational devices that crop up in the practice of science, he reiterates arguments by Frigg, contending that they lack some of the logical properties required of representation. (p. 232). Beyond this, Suárez also deems structural approaches deficient in that they fail to "allow for misrepresentation or inaccurate representation" (p. 233) whereas a successful theory of scientific representation should account "for the ubiquitous phenomena of mistargeting and/or inaccuracy." He further finds them inadequate on grounds of a 'non-necessity' argument

(p. 235) wherein "the relation of representation may obtain even if [set-theoretical relations] fail," and a 'non-sufficiency' argument (p. 236) since "the relation of representation may fail to obtain even if [set-theoretical relations] hold." Expanding on this latter contention, Suárez further suggests that his previous arguments point to a feature of representation that is not entailed in formal structures: the essential "directionality of representation." Claiming the argument from misrepresentation as most explicit, he proposes that the object that constitutes the source of a formal model "has no directionality *per se*," however in what is believed an authentic representational role, "the source leads to the target:"

> Neither similarity nor isomorphism can capture this capacity of the representational relation to take an informed and competent inquirer from consideration of the source to the target. But it is this feature that lies at the heart of the phenomenological non-symmetry of representation. (Suárez 2003, p. 236)

For Swanson also, it is this asymmetry of model and theory which proves essential to the model functioning as a cardinal source of insight. (1966, p. 300)

The model in general scientific practice

In the literature appraising models as scientific components distinct from the logician's account, the concept of a scientific model is notably more liberal, yet suffers from an uncertain ontology and finds itself beset by a lack of universally accepted definitions. In this, Hartmann regrets that: "it is already hard to distinguish between the meaning of 'model' and 'theory'" in the 'loose way' some physicists use these terms. (Hartmann 1995, pp. 49, 52) For others:

> Models typically represent either some aspect of the world, or some aspect of our theories about the world, or both at once. Hence the model's representative power allows it to function not just instrumentally, but to teach us something about the thing it represents .... So far, there seems to be no systematic account of *how* they operate in both of these domains. (Morrison & Morgan 1999, pp. 10, 11)

When early attempting some degree of categorization of the "physicist's view," or "classical view," (Hartmann 1995, p. 52) of models, Hesse suggested:

> It is sometimes possible to label models unequivocally as 'mechanical', 'electrical', 'mathematical', and so on, but more usually a model will be a mixture of several types. In

this use the word 'model' becomes co-extensive with the word 'hypothesis', but is more suggestive, since it calls attention to the heuristic properties of the early mechanical models, and asserts that these properties must be found in any hypothesis which is scientifically useful. (Hesse 1953, pp. 200-201).

Elsewhere, Achinstein holds that abstractions such as "the Bohr model of the atom, the billiard ball model of gases, the corpuscular model of light, the shell model of the atomic nucleus, and the free-electron model of metals" are most suitably termed "theoretical models" and that such abstractions be distinguished from supplementary (objective) artifacts such as diagrams, or replicas, "which are not to be identified as the model itself." (1965, pp. 102-103) Advancing a similar view, Giere suggests that "models in advanced sciences such as physics and biology should be abstract objects constructed in conformity with appropriate general principles and specific conditions." However, such abstractions are specifically human constructions with the ability to create them deriving from symbolic artifacts such as language and mathematics, but nonetheless are not to be identified with linguistic entities such as words or equations. (2004, p. 747) For others, doubts as to the use of fictional entities such as the *Gedankenexperiment* have fostered strong philosophical reservations. (Essen 1996) Given that such "fictional entities are notoriously beset with ontological riddles," some philosophers have even questioned their validity enough to suggest that "apparent ontological commitments to them" should be renounced. (Frigg & Hartmann, sec. 2.2).

Nevertheless, for Bailer-Jones, some relaxation of a judicious separation of representations *as* abstractions and representations *of* such abstractions can to be accommodated:

… a [scientific] model is an interpretative description of a phenomenon (object or process) that facilitates perceptual as well as intellectual access to that phenomenon [and is] intended as a term wide enough to admit various different forms of external representations, propositional or non-propositional. (Bailer-Jones 2003, p. 61)

Weisberg, although agreeing in principle that some distinction be drawn between models and their representations - which he in turn calls *model descriptions*, such as equations, graphs and the like - nonetheless also pleads for a wider gamut of ontological inclusion such that: "Models are abstract structures or physical structures that can potentially represent real-world phenomena." (Weisberg 2007b, pp. 216-217). It is in this more inclusive sense that the more liberal interpretation of scientific models, implied in the general scientific literature, regularly construes models as including those more objective entities such as are found in Bailer-Jones' "external representations," and Weisberg's "model descriptions." In the regrettable absence of an accepted theory of models or of representation, for better or worse, common usage must provide provisional definition. In respect of this, abstract constructs, physical replicas, mechanisms, sketches, verbal or visual entities, maps, diagrams, plots of empirical data, computer programs and algorithms and mathematical formulae are all to be found equally subsumed under an undifferentiated umbrella term as scientific models in themselves.

Representational strategies

Whereas it would seem unnecessary to argue that an object can be successfully represented by a diversity of methods, such as evident when considering the representational variety existing between a scale replica of an airplane wing, the draughtsman's pen and ink drawings of the same wing and the set of mathematical formulae that describe it as an aerofoil, the absence of an agreed theory of representation has required various attempts to categorize the functionalism whereby 'representation' is deemed to have taken hold. Of the representational methods appearing in the literature, 'idealization' and 'analogy'[75] appear the more important, not merely based on their frequency of occurrence but in particular on the necessity for correctly understanding the traits of the representational strategies involved, including their practical and philosophical limitations. Here, the user of a model needs to correctly understand the model's intended purpose and that domain within which its strategy and use remains valid.

Analogy

According to Achinstein, "… the terms 'model' and 'analogy' have frequently been used interchangeably by scientists and historians of science, especially in those cases in which an analogue of a given object or system is described in quantitative detail." (1964, p. 330; 1965, p. 116) Analogy appears, however, as one of the earliest recorded, and is therefore amongst the oldest known, representational strategies. In suggesting an example, Koperski (sec. 2) cites "ping-pong balls blowing around in a box (like those

[75] "Analogy, in its broadest sense, comprehends any mode of reasoning that depends on the suggestion or recognition of a relationship of similarity between two objects or sets of objects. It includes not only four-term propositional relationships of the type A:B::C:D (for which the Greek term is *ἀναλογία*), but also both explicit and implicit comparisons, for example the use of models (*παραδείγματα*) and of images (*εἰκόνεσ*). In early Greek thought analogies played a fundamental role ..." (Lloyd 2003, p. 60) "Metaphor", meaning analogy by literal denotation, and "simile", meaning a comparison on the basis if a specific resemblance, are figures of speech of Greek etymology also suggesting representation.

used in some state lotteries) constitute an analogue model for an ideal gas." Others note the case where an oscillating electric circuit can perform as an analogue of a mechanical system such as a pendulum. Familiar examples based on an analogical principle include the Bohr model of the atom which postulates electron orbits on an analogy with a planetary system, the 'liquid-drop' model of the atomic nucleus wherein fission is exemplified by analogy with the separation of a liquid drop into smaller droplets, whereas the 'shell' model of the same nucleus invokes analogy between extra-nuclear electron shells and the nucleus itself. (Achinstein 1965, pp. 105-106) In each case the analogy helpfully suggests some perhaps familiar system governed by known laws or principles, or recognizably similar ones, which are to be thought as also governing, or at least characterizing, the modeled system. Achinstein further observes, that in each case,

> … the model itself can be distinguished from any analogy upon which it might be developed. The billiard ball *model* consists of those propositions asserting that gases are composed of tiny elastic spheres, etc. The *analogy* is drawn between gases so described and perfectly elastic billiard balls. (1965, p. 106, orig. emph.)

In his review of pertinent aspects of Whitehead's philosophy, Desmet notes that for Whitehead: "The procedure of rationalism is the discussion of analogy," and adds that:

> Physics is all about modeling [although] Whitehead does not speak about "models" (as philosophers of science tend to do since the rise of model theory), but about "analogies," and sometimes about "metaphors." Also … he uses the more general expression "pattern" instead of "relational structure," hence including not only structural, but also qualitative aspects.
>
> So for Whitehead, physics is all about analogies, about the recognition of similarity amid diversity. Each analogy reveals a pattern that the two subjects of the analogy – the clarifying and the clarified, the source and the target – have in common. [thus] for Whitehead, the discovery of fertile analogies or metaphors is the motor of the progress and success of rational thought in general, and physics in particular. (Desmet 2010, p. 9)

Although identifying a substantive or material analogy to obtain between model and subject, Hesse extends beyond the range of physical models to include the formal isomorphic or set-theoretic model-theory relationship interpreted as a class of 'analogue models.' (Hesse 1953, p. 202; Koperski sec. 2). Such a representational relationship is mediated by the proposition that the same laws or principles govern both model and subject, although they may exhibit as physically quite different:

> In the case of a logical model of a formal system, there is analogy of structure or isomorphism between model and system, deriving from the fact that the same formal axiomatic and deductive relations connect individuals and predicates of both the system and its model … A swinging pendulum and an oscillating electric circuit, for example, are analogous by virtue of the formal relations described in a wave equation satisfied by both. (Hesse 1967)

A further example, of historical import, discusses the formal analogy discovered by Maxwell to exist between gravitational processes and those of heat conduction, wherein:

> The laws of the conduction of heat in uniform media appear at first sight among the most different in their physical relations from those relating to attractions. The quantities which enter into them are *temperature, flow of heat, conductivity*. The word *force* is foreign to the subject. Yet we find that the mathematical laws of the uniform motion of heat in homogeneous media are identical in form with those of attractions varying inversely as the square of the distance. We have only to substitute *source of heat* for *centre of attraction*, *flow of heat* for *accelerating effect of attraction* at any point, and *temperature* for *potential*, and the solution of a problem in attractions is transformed into that of a problem in heat. (Kroes 1989, p. 147 orig. emph)

Additionally, the history of science also records that a parallel discovery by Sir William Thomson, (Lord Kelvin),[76] showed that:

> … the analogy between the theory of heat and that of electrostatics … can be described by the same equations if one reads 'temperature' for 'potential', 'source of heat' for 'positive electric charge', and so on. In other words, the mathematical structure of the two theories is the same, and consequently one theory may be used as a model for the other … (Hesse 1953, p. 201)

Thus, when described by an abstract mathematical formalism, no distinction is to be seen between the phenomena of heat flow, that of gravitation or that of electrostatics, and the three analogous systems, if interpreted by edicts of a model-theoretic approach, are seen related by isomorphism. However, as analyzed by Kroes (pp. 148-149), isomorphism of a governing law is, of itself, neither sufficient nor necessary for the

[76] At the age of seventeen, in "September of 1841, just after matriculating at Cambridge, William Thomson submitted to the *Cambridge Mathematical Journal* ... his seminal paper "On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its Connexion with the Mathematical Theory of Electricity." (Wise 1981, p. 33)

establishment of structural analogy which rather requires that the *mathematical equations* describing the system's behaviors have the *same form*. Thus, from a model-theoretic view, these physical systems all qualify as members of that family of models which interpret an inverse square formula as the uninterpreted principle. However, the *analogous* representational style that relates them derives independently from the similarity of structure of their mathematical equations.

Simplification and idealization

In principle, it may be asserted that idealization employs the deliberate approximation[77] or simplification of a representation with the objective of making it more tractable. (Frigg & Hartmann, sec. 1.1; Hesse 1953, p. 202; McMullin 1985, p. 248; Redhead 1980, p. 147) However, McMullin further insists that the practice is both ubiquitous and endemic in that: "Every theoretical model idealizes, simplifies to some extent, the actual structure of the explanandum object(s)." (p. 258) Although McMullin, Frigg and Hartmann (sec. 1.1) and Chakravartty (2001, p. 327) deem idealizations to be suitably classified by two broad categories, Weisberg suggests three general, but non mutually exclusive, classifications which suitably analyze these activities and the justifications offered for their employment: *minimalist idealization, Galilean idealization*, and *multiple-models idealization* (Weisberg 2006, sec. 1; 2007a, p. 640). However, the ramifications of such strategies in the quest of scientific truth-seeking have fuelled extensive philosophical debate. Some focus on both the utility and validity of the method, noting that strategic misapplications may possibly stand accused of having historically distorted the very developments in theoretical construction their introduction was intended to facilitate.

Minimalist idealization, also variously labeled 'abstraction' (Chakravartty, p. 327), 'negligibility assumptions', 'method of isolation', 'material idealization', (McMullin, pp. 258-259, 262) or 'Aristotelian idealization'[78] (Frigg & Hartmann, sec 1.1),

[77] Such representational modifications are to be distinguished, however, from that of 'mathematical approximations' with which they are closely related but which may be characterized as strictly formal modifications wherein, unlike idealizations, the issue of physical interpretation need not arise (Frigg & Hartmann sec. 1.1; Redhead 1980, p. 152). The expansion of a mathematical function into a power series, of which only the first few, most influential, terms are retained, or the recording of π as $^{22}/_{7}$, serve as examples.

[78] According to Frigg (2007) the term "Aristotelian idealisation" has become common but is not necessarily historically accurate. Nonetheless, it is retained in the sense that "Aristotelian science does focus on specific properties and makes statements about these, whereby others are left aside" and further that, "the properties under investigation are not distorted in any way." Elaborating on Aristotle's treatment of mathematical objects, Mendell, expanding on one of five concepts he finds Aristotle uses in mathematical discussions, reviews objects treated as from 'abstraction' or 'taking away' or 'subtraction' (*aphairesis*) or 'removal' (*ta ex aphaireseôs*) "In the *Analytics*, where the notion of matter is absent, Aristotle begins with a particular geometrical perceptible figure. What is removed is its particularity ...

simplifies a system by an intentional omission or 'abstracting away' of properties and relations of the system. Where deemed suitable to a particular enquiry, such omissions permit focusing on a limited set of remaining properties in isolation. and their omission is believed not to subvert a minimalist model's representational power. (McMullin, pp. 258-259)

On the other hand, Galilean idealizations - also denoted 'formal', or 'construct' idealizations (McMullin, pp. 254-255) - introduce a usually intentional, and often openly acknowledged element of 'falsehood' into their pursuit of tractability by simplification. Apparent properties of the target system may be simplified, distorted, or even omitted, and accordingly the style is also referred to as 'distorted models,' (Frigg & Hartmann, sec. 1.1) If acting together with 'abstractions', the resulting representations have also been termed 'caricatures' (Chakravartty, p. 327). Although simplifications of a similar kind are found even in remote antiquity, this mode of representation has been named for Galileo in acknowledgment of his particular use of the strategy. Some evaluate his individual contributions as axial in the history of science:

> Galileo systematically applied the method of idealization. And that was the real meaning of the revolution in the natural sciences which was named after him. (Nowakowa & Nowak 2000, p. 21, cited in Nola 2006, p. 237)

For Matthews, original Galilean idealization proved not only crucial for the emergence of modern science but remains of primary pedagogical importance:

> Galileo's discovery of the properties of pendulum motion depended on his adoption of the novel methodology of idealization. Galileo's laws of pendulum motion[79] could not be accepted until the empiricist methodological constraints placed on science by Aristotle, and by common sense, were overturned … Proof of the laws required not just a new science, but a new way of doing science, a new way of handling evidence, a new methodology of science.

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including its being perceptible. What is left then is a universal of some sort ... Elsewhere, Aristotle usually seems to mean that the attributes not a part of the science are removed. What is left may be particular, a quasi-fictional entity ... [however] is the status of this entity ... a representation in the soul or is it the perceptible object treated in a special way? ... Ancient and medieval readers tended to take the former approach ... that the object left is a stripped down representation with only the required properties ... Most modern readers ... take the second approach. The objects studied by mathematical sciences are perceptible objects treated in a special way, as a perceived representation, whether as a diagram in the sand or an image in the imagination." (2004, sec. 7.1)
To this Weinberg adds: "That abstraction need not involve any falsification is insisted upon by the medievals, and the first statement of this is to be found in Aristotle." (Weinberg 2003, p. 1)

[79] Although Galileo confidently asserted the isochronism of circular motion as underwriting pendulum phenomena, it too was to be revealed as an idealization. Some decades later Christiaan Huygens proved that it was the motion in a cycloid rather than a circle, that was isochronous. (Matthews 2006, p. 216)

> This was Galileo's method of idealization. It was the foundation of the Galilean-Newtonian Paradigm which characterized the Scientific Revolution of the 17th century, and the subsequent centuries of modern science … appreciating the role of idealization … is an instructive way to learn about the nature of science. (Matthews 2006, p. 209)

For Galileo the practicing physicist, investigations otherwise embroiled with the complexities inherent in real-world situations, were rendered less intimidating by invoking a simpler, and potentially more familiar, epitome of the problem. Solutions deriving from the more modest portrayal then held out the promise of insight into the more complex. However, Galilean idealization moves beyond a minimalization criterion to include overtly 'false' propositions. Galileo provides both example and justification in his unpublished work on motion: '*De Motu Antiquiora'* [80]

> …I am not unaware that someone here may object that I have presupposed as true for these demonstrations what is false: namely, that the weights suspended from the balance maintain right angles with the balance; even though the weights, since they tend to the center [of the world], would be convergent. To these people I would answer that I cover myself with the protecting wings of the superhuman Archimedes (whom I never mention without admiration). For he has presupposed the same thing in his Quadrature of the Parabola …; (Galilei 2000, p. 300).

In his 1638 "*Dialogue Concerning Two New Sciences*", when discussing projectile motion, Galileo invokes the abstraction of a frictionless ball on a perfectly flat horizontal plane without air resistance. To the charge advanced by Galileo's 'Aristotelian'[81] advocate Simplicio, that " various difficulties render it highly improbable that a result derived from such unreliable hypotheses should hold true in practice", Galileo's ideological actor candidly replies:

> All these difficulties and objections which you urge are so well founded that it is impossible to remove them; and, as for me, I am ready to admit them all … I grant that these conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither will the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the path of the projectile a parabola … (Galilei 1914, pp. 248-254).

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[80] Galileo's *De Motu Antiquiora* ('Older Writings on Motion') comprise a set of unpublished documents dating most probably from Galileo's Pisan period (1589-1592). (ECHO 2007)

[81] Although Galileo, as also in his "*Two Chief World Systems*" is understood to be attempting establishment of the Copernican world-view in contradiction to the then classical Aristotelian earth-centred cosmology, Simplicio is to be specifically understood as enunciating those objections Galileo judged most likely to represent the Aristotelians of his own day, a version possibly adulterated by a neo-Platonism, wherein a discernable distrust of mathematics in the context of physical explanation had became evident. (McMullin 1985, p. 250)

Nonetheless, in proposing a justification for embracing such 'fallacy' in the cause of truth-seeking, Galileo has his ideological spokesperson enunciate a philosophical precept integral to the then dawning scientific revolution:

> Of these properties of weight, of velocity, and of form, infinite in number, it is not possible to give any exact description; hence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; and having discovered and demonstrated the theorems … to use them and apply them with such limitations as experience will teach. (ibid)

Thus, for Galileo, idealization was a justifiable option on the path to truth-seeking for the emerging mechanical science. Idealized constructs presented as both descriptive and diagrammatic representational models, some of which pragmatically utilized simplifications and idealizations involving 'fallacies.' As Redhead succinctly summarizes: "Science depends on the possibility of ignoring accidents, of isolating certain key features in a situation. These are captured by models, although in the very act of idealization and approximation we convince ourselves that the model is indeed false" (Redhead 1980, p. 162).

Multiple-models idealizations

Completing Weisberg's idealization trilogy, '*multiple-models idealization'* (2006, p. 7) appeals as a succinct designation of modeling communities. Elsewhere recognized in the literature in terms such as "several incompatible models of one and the same target system," (Frigg & Hartmann 2006, sec 5.1) or "various different models of one and the same phenomenon," (Bailer-Jones 2003, p. 66) such collectives are not elsewhere assigned so laconic a collective label. The notion examines both the practice of, and justification for, generating and employing multiple but often incompatible models, each of which contributes distinctive content towards a more competent understanding of complex phenomena. For Cartwright: "Different incompatible models are used for different purposes; [in terms of theory construction] this adds, rather than detracts, from the power of the theory." (Cartwright 1983, p. 158) For biologist, Richard Levins, one justification for multiple-modeling stems from conflicts resulting from irreconcilable representational goals whereby:

> The multiplicity of models is imposed by the contradictory demands of a complex, heterogeneous nature and a mind that can only cope with few variables at a time; by the

> contradictory desiderata of generality, realism, and precision; by the need to understand and also to control …These conflicts are irreconcilable. (cited by Weisberg 2006, p. 8)

In agreement, Weisberg also argues that fundamental factors of some complex classes of phenomena may remain obscured if represented only by a single simple or minimalist idealization. Multiple-modeling is then justified in such cases by the paucity of a single model's representational power. (Morrison 1999, p. 45) In 1958, Werner Heisenberg, in discussing the statistical character of quantum theory, so justified a multiple-model approach promoted by Niels Bohr as explanatory of his *concept of complementarity* formulation:

> By this [Bohr] means that the different intuitive pictures which we use to describe atomic systems, although fully adequate for given experiments, are nevertheless mutually exclusive. Thus, for instance, the Bohr atom can be described as a small-scale planetary system … For other experiments, however, it might be more convenient to imagine that the atomic nucleus is surrounded by a system of stationary waves whose frequency is characteristic of the radiation emanating from the atom. Finally, we can consider the atom chemically. We can calculate its heat of reaction when it becomes fused with other atoms, but in that case we cannot simultaneously describe the motion of the electrons. Each picture is legitimate when used in the right place, but the different pictures are contradictory and therefore we call them mutually complementary. (Heisenberg 1958, pp. 40-41)

For optics, the fundamental principles, delivered from a protracted and difficult gestation and advanced as those underlying a contemporary understanding, are often expressed by a quorum of as few as two contrasting representations – the wave and the particle. Nonetheless, the greater corpus of optical literature entails a multiple-models ensemble of diverse, often patently contradictory models. Concepts such as an aetherial optical medium contrast with electromagnetic fields; dynamic modes include emission, ballistics and continua; and constituents are variously described as rays, beams, waves, quanta, photons, particles and wavefronts. Although it may be argued that each model has evolved from what were historically justified circumstances supported by cogent philosophical reasoning, any such model would nonetheless be found inadequate if taken independently as the sole interpreter of optical phenomena. Recognizing the limitation, the Arizona State University, Modeling Instruction Program notes that: "Presenting a single unified model without background in the particle nature and the wave nature of light would challenge any concept flow." (ASU 2003). Such an eclectic collection of models presenting overtly contradictory positions would then appear indicative of a multiple-models idealization (MMI) as portrayed by Weisberg.

Since, in an MMI, discordant models are not intended to act in competition, no expectation exists for a single 'correct' model to emerge. Nonetheless, describing modern optics as served by an MMI does more than sue for peace in a theoretical war-zone. Of singular importance is that an MMI can be viewed as the modeling equivalent of the proverbial Arthurian 'Round Table'. An MMI concept offers a common respect and reciprocal worth to otherwise mutually destructive antagonisms by recognizing a common citizenship, guaranteeing to each a valid role in facilitating particular insights into complex subject matter, yet without dogmatism or the expectation of individual monopoly. Once recognized as an MMI, past problems might be recognized as misplaced attempts to promote an individual model exclusively at the expense of other useful schemes. Other misadventures may involve the misconstruing of contradictory models as conflicted theoretical inconsistency demanding repudiation of one or another otherwise valid representational aid to theory construction. It is in this respect that a "wave-particle duality," when recognized as a theoretical development employing equally valid but otherwise incompatible models, has diminished the potential for tension between formerly exclusive and competing claims to explanation. It is even arguable that a reappraisal of modern optical theory, interpreted as a subject whose explanatory power has been facilitated by an MMI, may offer hope of both identification and resolution of entrenched historical misunderstandings and misinterpretations and in so doing, return to science in general an enriched understanding of its own nature. (Wong & Boo 2005)[82]

The model and the modeler

However, in the spectrum of views that constitutes dissent from an exclusively isomorphic interpretation of modeling, surprisingly little attention seems to focus on the human element of representing with its subjective intent and cognition. Raising this indispensable aspect of the scientific model's power to represent, Bailer-Jones argues that "the representational relationship is constituted by model users "agreeing" on the *function* of the model, [and] on the *aspects* of a phenomenon that are modeled. Model users weigh the propositions entailed by a model and from this decide which of these propositions are crucial to the acceptance and continued use of the model." (2003, abs) In pursuing the question of the human activity of *representing* and what it is for something to be deemed a representation, Wartofsky asserts: "*Anything* (in the strongest

[82] It must, however, be acknowledged that Mach correctly identified these separate roles including a description of an MMI: "The ... six [fundamental propositions of physical optics] are evidently *descriptions*; they are not, however, descriptions of a single case, but *comprehensive* descriptions of analogous cases." (1926, p. 6, #12)

and most unqualified sense of 'anything') can be a representation of anything else" – a proposition which both denies the existence of independent intrinsic relational properties, and which posits that it is ultimately subjective human cognition and intent that underscores representation – "It is *we* who constitute something as a representation of something else. It is essential to something's being a representation … that it be *taken* to be one … and that nothing *is* a representation except insofar as we construct or construe it as one …" (1979, pp. xx-xxi, orig. emph.). As Giere summarizes: "Anything is similar to anything else in countless respects, but not anything [intrinsically] represents anything else. It is not the model that is doing the representing; it is the scientist using the model who is doing the representing." (Giere 2004, p. 747)

Modeling function in the cause of theory construction

In a number of discussions on the employment and function of models in science, the model, in many senses of the term, appears to exist, and to find its vocation, in a domain located between the world and the domain of theories. Here, traffic apparently proceeds in two directions, and models are held to perform two fundamentally different representational functions.[83] In the semantic account, as the interpretative representation of scientific theories,[84] mediating models are intended as accurate to the phenomenon, and are intended to interpret theories as analogues "of the messy and complicated phenomenological laws that are true of [the] system." (Psillos 1995, p. 105) In doing so, such models are also held to "provide theories with genuine empirical content." (Suárez 1999b, p. 168) Alternatively, on the 'traditional' syntactic reading, where a theory incorporates a set of claims, representative models are intended to be models of the theory, such that all the claims of the theory are satisfied and no further assumptions are included except ones "legitimately grounded in a description of the phenomena to be represented." (Suárez & Cartwright 2008, p. 64)

On the other hand, as models representing aspects of the world, such as physical models of phenomena, it is scientific theorizing that can be considered the intended beneficiary of modeling intercession. Here, mediation between the enigmas of the world and their hopeful elucidation by theory, is considered the function of what Hartmann classifies as "developmental" models, and where such models enact a role that is claimed as "an indispensable tool for theory construction." (Hartmann 1995, p. 58)

[83] "These two notions are not mutually exclusive as scientific models can be representations in both senses at the same time." (Frigg & Hartmann, sec. 1)

[84] "... models that we construct with the aid of a theory to represent real arrangements and affairs that take place in the world – or could do so under the right circumstances." Cartwright calls these *representative models* [although previously called] *phenomenological* to stress the distance between fundamental theory and theory-motivated models that are accurate to the phenomena (Cartwright 1999, p. 242)

Different models for different theoretical constructions

In her 1983 book, provocatively entitled "*How the Laws of Physics Lie,*" Nancy Cartwright claimed that:

> Really powerful explanatory laws of the sort found in theoretical physics do not state the truth ... If we attend closely to the manner in which theoretical laws figure in the practice of science, we see that despite their great explanatory power these laws do not describe reality. (Cartwright 1983, p. 1)

As Kyburg understands it, Cartwright's claim was that, although "…'theoretical' entities of physics and their causal interactions are as real as anyone could want them to be; the *laws* that are alleged to characterize their behaviour are largely fictions." (Kyburg, p. 174). For Cartwright: "The lesson for the truth of fundamental laws is clear: fundamental laws do not govern objects in reality; they govern only objects in models." (Cartwright 1983, p. 18) However, by 'models' Cartwright here intends idealized models of the world. Here, idealized predisposing models are selected as appropriate devices to help formulate those constructs that describe generalizations. Such models are elsewhere designated "approximation models," in that they aim, through idealized simplification, to capture "the main characteristics of the problem being considered" and as omitting minor details. (Morgan & Morrison 1999, p. 7) If applied in detail to individual cases, predictions by such theoretical constructs may be found inaccurate or 'false'.[85] On this reading, theoretical laws are, as Whitehead categorized them, 'abstract generalizations' (Whitehead 1938b, p. 13) For Whitehead, an analogy can at best only reveal a partial similarity of pattern leaving as a residuum, a tension between similarity and diversity. (Desmet 2010, p. 9) Where the discovery and description of a generalization, or covering law, is to be admitted as the aim of theoretical construction, expectations of 'truth' and realism, if by truth is meant something like 'ultimate truth', are thus both misplaced and unwarranted. In agreement, Giere suggests that the "better solution" is "to keep the simple law statements, but understand them as part of the characterization of an abstract model and thus being true of the model." (2004, p. 749).

Expanding on the theme, "caricature models take one (or perhaps more) of those main characteristics [of approximation models] and distorts that feature into an extreme case." In justification it is claimed that although such a distorted model is clearly false

[85] However Cartwright qualifies by adding: "I do not mean that there could never be situations to which the fundamental laws apply. That is only precluded if the theory employs properties or arrangements which are pure fictions. (Cartwright 1983, p. 160)

as a realist description, its exploration in some testable form “may illuminate certain relevant aspects of the world.” (Morgan & Morrison 1999, p. 7)

On the other hand, apart from attempts to capture gross features of the world as regularities, much of the effort of theory construction is reserved for attempts to provide realistic explanation for individual and specific aspects of phenomena. Here, a different kind of model is believed required for a different intended result. For some, such models are denoted as ”phenomenological” or “developmental” models, (Hartmann 1995, p. 58) or by others, “physical models.” (Morgan & Morrison 1999, p. 5)

> ... physical models can be constructed in a variety of ways; some may be visualisable, either in terms of their mathematical structure or by virtue of their descriptive detail. In all cases they are thought to ... suggest hypotheses, aid in the construction of theories and are [thus] a source of both explanatory and predictive power. (Morgan & Morrison 1999, p. 6)

In this model inclusive portrayal of scientific method, it is logically the construction of a preliminary developmental model that presents as a first step. However, there appears to be no generally agreed rules for such model construction as have been offered for say, experimental design. Nowak has summarized this view of the scientific method such that a useful initial model might be seen as the formula connecting the “full list of idealizing conditions.” (Nowak 1995, p. 228) Elsewhere, Cartwright suggests that the initial stage might be called ‘preparing a description.’ (Cartwright 1983, p. 15) For Hartmann, such a description should involve collecting all the data that the theory is eventually intended to explain. (Hartmann 1995, p. 59) However, others suggest that preliminary model selection and building is both an art and a craft involving creativity and imagination, elements better learnt than taught. (Morgan 2004, p. 754; Morrison & Morgan 1999, p. 12) Here, a certain skill is brought to bear when choosing and integrating a set, or *mixture*, of items, including elements from outside the original domain of investigation, which are considered relevant to the task at hand. (Morrison & Morgan 1999, pp. 13, 14) For another, in similar vein: “Model building is like baking a cake without a recipe. The ingredients are theoretical ideas, policy reviews, mathematisations ... [and] metaphors.” (Boumans 1999, p. 67) Additionally, this combination might profitably include useful analogies, pre-existing models of the domain in question, empirical evidence and facts, and importantly “existing background theories and theoretical frameworks,” (Psillos 1995, p. 107) all blended into a sketch that Cartwright frankly admits begins as a “work of fiction.” (Cartwright 1983, p. 153) It is in this respect that:

> Some properties ascribed to objects in the model will be genuine properties of the objects modelled, but others will be merely properties of convenience ... Not all properties of convenience will be real ones. There are the obvious idealizations of physics—infinite potentials, zero time correlations, perfectly rigid rods, and frictionless planes. (Cartwright 1983, p. 153)

In another account, pragmatically such preliminary models "do not explicitly specify more than they have to for the immediate purposes at hand," (McMullin, p. 262) [and] usually begin by deploying the simplest structure that still "retains the 'essence' of the original problem situation." (McMullin, p. 259) Espousing a compatible view of the modeling role in theory construction and that also suggests some practical advice on model construction, Box positively asserts: "All models are wrong but some are useful" (Box 1979, p. 202). For Box, an effective initial approach is found by incorporating 'robustness' into modeling. Robustness, defined by Box as that property of a procedure which "renders the answers it gives insensitive to departures, of a kind which occur in practice, from ideal assumptions," requires examination of "the process of scientific modelling itself." (ibid., p. 201) Here 'parsimony'[86] pragmatically presents itself as a serviceable attribute.

> The scientist, studying some physical or biological system and confronted with numerous data, typically seeks for a model in terms of which the underlying characteristics …may be expressed simply… One important measure of simplicity of such a model is the number of parameters that it contains. When this number is small we say the model is <u>parsimonious</u>. … it would be very remarkable if any system existing in the real world could be <u>exactly</u> represented by any simple model. However, cunningly chosen parsimonious models often do provide remarkably useful approximations[87] … For such a model there is no need to ask the question "Is the model true?". If "truth" is to be the "whole truth" the answer must be "No". The only question of interest is "Is the model illuminating and useful? (Box, pp. 202-203 orig. emphasis)

In Weisberg's parallel account, which argues that modeling, in a realist understanding, "is the indirect theoretical investigation of a real world phenomenon using a model," (Weisberg 2007b, p. 209) three stages of model preparation and deployment are distinguished. Having first constructed a preliminary model, it is then refined and its

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[86] Perhaps more correctly termed '*the law of parsimony*', or '*the rule of simplicity*'. "This principle goes back at least as far as Aristotle who wrote '**Nature operates in the shortest way possible.'**" The principle of simplicity, as a formulation stronger than Occam's Razor, "works as a heuristic rule-of-thumb" but its value is perverted if employed as an axiom of physics. (Gibbs 1996)

[87] Box cites the 'Ideal Gas Law' (PV=RT) which often produces acceptable predictions of real gas empirical values, as a case in point. (Box, p. 203)

properties and possible dynamics further articulated. For Hartmann, this stage is responsive to the inventiveness of the physicist, and where data analysis may reveal patterns and similarities potentially leading to new insights and concepts analogous to already known ones. In wider overview, this need to refine is also deemed to arise from the probable inclusion of idealized, abstract and fictional elements in the preliminary model mix that would eventually prevent the model's properties from applicability to the real world objects and conditions intended to be addressed by theory. (Morrison & Morgan 1999, p. 14) For McMullin, Box, (pp. 203-204) and Nowak, this process includes models being 'concretized' and/or 'de-idealized.' (Nowak 1972, p. 537) For McMullin, at such a stage, a developmental model, when formulated in the mode of a testable hypothesis, is deemed suitable to "initialize the process of explanation as a continuing research program." (McMullin, p. 259) Elsewhere, Nowak has expanded on the notion of "concretization" to explain:

> [A preliminary model] is concretized by gradually admitting the previously neglected properties ... and modifying the initial formula. The [idealized] law becomes more and more complicated yielding idealizational statements ever closer to the empirical reality. (Nowak 1995, p. 228)

Elsewhere others concur and explain that:

> From an initially idealized model we can then build in the appropriate corrections so that the model becomes an increasingly realistic representation of the real [world phenomenon] (Morrison & Morgan 1999, p. 15)

Despite a wide variation of terminology, common to all these accounts, the ever more realistically customized[88] model submits hypothetical predictions for adjudication by suitable experiments. The recursive research program is further enhanced by 'adding back' model properties originally omitted either deliberately in the cause of preliminary simplicity or even through ignorance. Then, each in turn is the subject of renewed empirical testing. By McMullins' assessment:

> This technique will work only if the original model idealizes the real structure of the object. To the extent that it does, one would expect the technique to work. If simplifications have been made in the course of formulating the original model, once the operations of this

[88] 'concretization' is also termed as "customising" and "deidealising." (Cartwright 1999, p. 250)

> model have been explored and tested against experimental data, the model can be improved[89] by gradually adding back the complexities. (McMullin, p. 261)

Hartmann provides a useful example drawn from the "ideal gas law":

> [Consider] the equation of state of an ideal gas:
>
> $$pV = RT$$
>
> Here, p, V and T represent the pressure, volume and temperature of the gas respectively, R is the gas constant. [Here,] idealized assumptions facilitate the description of [an *ideal*] system. By means of *concretization* ... the model can now be 'improved' [i.e.] to make the model ... more realistic [by recognizing that] molecules are *not* point-like particles. They have a finite volume V_0 and this volume affects the system's equation of state, too. A modification of the model object along these lines leads to the van der Waals equation:
>
> $$(p + a/V^2) (V - b) = RT$$
>
> with adjustable parameters a and b depending on the special system under investigation. This improved equation ... allows now ... a qualitative understanding of the liquid-gas phase transition. (Hartmann 1995, p. 53)

On the other hand, however, if the course of model de-idealization and concretization leads to *ad hoc* corrections wherein techniques are required which lack theoretical justification, or the hypothesis makes invalid predictions, this is taken to count against the adequacy of the predisposing model. Remedial action may ultimately require replacement of the model with one more coherent. (McMullin, pp. 261, 264) In Nowak's view, a model is to be rejected at the point where "no concretization proves to be able to cover the discrepancies between the theoretical, predicted outcomes" and empirically derived data. (Nowak 1995, p. 229)

The Model-Theory Distinction

Apart from other benefits claimed for a model-theoretic account, such an approach appears to provide an unambiguous distinction between the roles of model and theory: theory is an uninterpreted mathematical formalism; a theory's model is any example whose calculus faithfully exhibits, or interprets, that formalism. However, the case is less clear for the more historically abundant syntactic account, and here most particularly where developmental models are employed in the process terminating in successful theory construction.

[89] For Hartmann, and appealing to the minimalist, instrumentalist interpretation: "To 'improve' [in this sense] means 'getting empirically more accurate." (Hartmann 1995, p. 53)

When analyzing this relationship, for some, beyond a presupposition of the idea of models as mediators between theories and the world, is the idea of such models acting as autonomous agents, demonstrating their ability to function independently of theory. (Suárez 1999b, p. 171) In fulfilling this role, the model is claimed to act as an autonomous entity, wherein its "partial independence" from both theory and phenomena is essential to its use both as an instrument of exploration in both worlds and its ability to "mediate between theory and the world." (Morrison & Morgan 1999, pp. 10-11) In this account "models can remain autonomous despite some significant connections with high level theory." (Morrison 1999, p. 39) Here, autonomy is held to derive from a model's ability to "*function* in a way that is partially independent of theory," and that a model can be "*constructed* with minimal reliance on high level theory." For Morrison, it is this "partial independence in construction that ultimately gives rise to the functional independence." (Morrison 1999, p. 43) An abiding distinction between a theory and its developmental model also appears implicit in McMullin's account:

> Every physical theory involves a model of the physical object(s) whose behaviour the theory is expected to explain. The theory is not identical with the model, it is the 'text' in terms of which the model is specified. (McMullin, p. 257)

On the other hand, as Suárez notes, in accounts such as by McMullin, Nowak and Hartmann, where models are cast as an integral but adaptive and progressive component in a process towards theory construction, their method effectively dispenses with the need for models as autonomous mediators since the process invariably construes models as improving approximations to, or even components of, theory. (Suárez 1999b, p. 173) In sympathy it must be admitted that in such accounts, as the predisposing model is progressively customized by concretization, the model is expected to become ever more realistic and the distance between an initially idealized model and the theory it is employed to facilitate, is correspondingly expected to diminish. However, clarifying the end-point of this process appears under-represented in the literature, and might be thought to contribute to a complaint that the distinction between 'model' and 'theory' can sometimes appear ill-defined[90] or 'hazy.'

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[90] Hartmann also notes that terms are sometimes used inconsistently in the language of the physicist: "the Standard **Model**" of the strong electroweak interactions is certainly considered a fundamental theory." (Hartmann 1995, p. 52)
It might be further noted that the model-theory distinction as claimed specifically for theory by McMullin, bears a stiking resemblance to a "*theoretical model*" as described by Hartmann, as based on earlier work by Bunge. Here a theoretical model "consists of two components:

1 A general theory
2 A special description of an object or system (*model object*)"

For Weisberg, the final stage of theory construction includes assessment of the relationship between the model and the world, wherein: "If the model is sufficiently similar to the world, then the analysis of the model is also, indirectly, an analysis of the properties of the real-world phenomenon." (Weisberg 2007b, p. 209) In Cartwright's parallel assessment, when this desirable stage is reached, the model is "realistic if it presents an accurate picture of the situation modelled: it describes the real constituents of the system—the substances and fields that make it up—and ascribes to them characteristics and relations that actually obtain." (Cartwright 1983, p. 149) It might then be asked what advantage could be thought to exist in attempting to maintain an ontological distinction between a customized *model* advancing a predictively successful concretized depiction of reality, and a *theory* when offering an equally successful depiction of the same reality? In rare comment on the situation, Redhead suggests that:

> The possibility of the model being an exact theory is not excluded, but once this possibility is seriously entertained the model changes its status and acquires the honorific title of theory. (Redhead 1980, p. 147)

A similar sentiment is expressed in comment by Psillos:

> ... if the warranted degree of confidence in the adequacy of the representation is high, one can take a realist stance towards a model. Eventually, I think there is , in principle a point – *being a function of the available evidence* – where a model of *X* can give rise to a theory of *X*. (Psillos 1995, p. 130)

It might then prove clarifying to take the observations of Redhead and Psillos as a general condition intended to describe the end-point of a successful model-sponsored theory construction program. The criterion for judgement would become a matter of perceived maturity, or as Psillos suggests, "a difference of degree of belief," where, for a theory, the investment of confidence is reliant on the wide extent and high degree of empirical corroboration. (Psillos 1995, p. 115) On this interpretation, developmental models, when fully concretized and proven to be predictively successful, would be considered to be theories, and within the ambit of an appropriate scientific skepticism, could be taken as reliable characterizations of reality. On the other hand, in the context

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The often cited model of an ideal gas serves to illustrate: here the general theory is Newton's mechanics, whereas the model object characterizes an aspect of the nature of a gas wherein its molecules are pictured as particles moving chaotically in a container. (Hartmann 1995, pp. 52-53) Such scenarios may, at least in part, be thought to additionally contribute to a perception of ambiguity in articulating a model-theory distinction.

of a model-sponsored theory construction, the term *model* would refer to all stages of building and deployment of a developmental model in terms of 'preliminary physics.' (Hartmann 1995, p. 52) The term would then specifically denote that further model 'improvement' was yet believed possible.

However, it is within the context of the model-theory distinction, that a potential for misadventure is reported. According to Weisberg: "Many standard philosophical accounts of scientific practice fail to distinguish between modeling and other types of theory construction." For Weisberg, this proves to be an unfortunate oversight since "there are important contrasts among the goals, procedures, and representations employed by modelers and other kinds of theorists." (Weisberg 2007b, p. 207) In providing a specific example, it is Cartwright's opinion that the developmental physicist may mistakenly consider a yet partly de-idealized model "as an exact replica of reality, and ... attribute to the objects modelled not only the genuine properties of the model, but also the properties of convenience." (Cartwright 1983, p. 156) In further expanding the warning she notes that it would be:

> a mistake to think entirely in terms of idealizations—of properties which we conceive as limiting cases, to which we can approach closer and closer in reality. For some properties are not even approached in reality. They are pure fictions. (Cartwright 1983, p. 153)

For Cartwright, fictitious "properties of convenience" include geometrical abstractions such as the idealization of physical extension by a point. The result of such a miss-attribution is held to bear negatively on an attempt to develop a realist version of theory. Cartwright is not alone in voicing a concern:

> It ... remains a fact that problems of idealization have been disregarded in the works of empirically-minded methodologists, i.e. people who are concerned not so much with formulating methodological programs as with codifying those procedures which are actually being used in science ...
> The problem of idealization in empirical sciences is very rarely taken up in works concerned with the methodology of those sciences. It seems to be common knowledge that in advanced natural sciences references are made to concepts such as "perfectly rigid body", "material point" ... etc.,[91] but it remains a fact that the most important methodological concepts, concepts which have determined the present-day form of the philosophy of

[91] Nowak cites *The Structure of Science* by Ernest Nagel, p. 131, as an example: "It is common if not normal for a theory to be formulated in terms of ideal concepts such as the geometrical ones of straight line and circle, or more specifically physical ones of instantaneous velocity, perfect vacuum, infinitely slow expansion, perfect elasticity, and the like." (Nowak 1972, pp. 533-534)

> science, have been advanced without regard to the peculiarities of the [idealization] procedure ... (Nowak 1972, p. 533)

Thus, in a research effort directed towards origins and the developmental landscape pertinent to an apparently well-corroborated theory whose predictions now appear to be conflicted, it would seem potentially more profitable to explore the construction of relevant theories in the context of the building and deployment of their predisposing models, particularly as those roles are advanced as integral to a version of the scientific method.

It should, however, also be kept in mind, that a model-theoretic approach to modeling and theory definition, is a recent adventure in the philosophy of science. The literature relevant to the history and development of relativity and quantum theories, as also for the history before them, reflects an extensive era in which the practitioners of science were devoted exclusively to a syntactic understanding of theory and theory construction, together with a classical understanding of modeling and its various roles in scientific endeavor.

Part 2

Chapter 4
A model inclusive analysis of the complex history of optics

Deriving as its name does, from the Greek word for ‘eye’, the modern science of Optics betrays its more humble beginnings as a study of sight, which, under the scrutiny of early Greek scientific thought, developed as a study of perspective wherein the mathematical functioning of so-called ‘rays of vision’ were explored by deductive geometry (Calvert 2000a, fn. 25; Mach 1926, p. 9). However, in its twenty-first century incarnation, optics as an exact science, is not restricted in scope to analysis of human vision, nor limited to those frequencies of radiation to which the retina of the eye is sensitive. Following Maxwell and recognition of light as a particular exhibition of electromagnetic phenomena, optics has been rightfully subsumed as a topic within the greater domain of electromagnetism.

From its inception, the evolution of optics has exhibited itself as highly contentious, giving rise at almost all times in its historical development to impassioned rivalries of thought, often philosophically classifiable into antagonistic dichotomies that in turn all too frequently devolved into acrimonious partisan conflicts. Various contentions arose between advocates of extramission and intromission, particles and continua, objectivism and abstraction, secular or materialistic interpretations and teleological or theological speculations, selectionism and undulation, and atomism or corpuscularity and pulses, waves and waves fronts. Additionally, further dissentions were caused by international political rivalries, the accretion of myths and folklore around strong personalities, historical detours, such as the search for a conjectured luminiferous aether, or the conceptual distortions historically incited by ‘natural theology.’ The most prominent of contemporary optical models, those supporting of wave-based formulations and light-quanta as photons, although now expressed as a ‘wave-particle duality’ in a strategic attempt at synthesis within a quantum electrodynamics theory, nonetheless continue to engender individual antagonisms and misunderstandings. Beyond these, a contemporary rise in neo-aether speculations and a resurgence in relativistic challenges, are both this turbulent history’s natural sequel and legacy.

Entailing more than simply a succession of conflicting views, the dynamics of optical history appears to display a sequence of doctrinal conquests and dominances together with alternating hegemonies and paradigms that Whitehead views in terms of “[g]eneral climates of opinion [that] persist for periods of about two to three generations, ...[or] for about sixty to a hundred years ... [and] also shorter waves of thought, which play on the surface of the tidal movement." (Whitehead 1938b, p. 29) Hesse portrays the same history as exhibiting that dialectical process wherein opposing ideologies posit contradictory doctrines whose inevitable confrontation equips a driving force for conflict resolution often sought through synthesis. (1995, p. 375)

Early models of light: Greek and Greco-Roman optical adventures

Early optical conjectures, which initially under Greek supervision,[92] first entered that primal arena of organized thought and practice recognizable as a science, toyed with an extensive and eclectic assortment of speculations:

> … the emission theory of sight of Euclid and Ptolomy, which postulated visual rays emanating from the observer's eye; the older Epicurean intromission theory, which reversed the rays and made them corporeal; the combined emission-intromission theories of Plato and Galen; and some enigmatic statements of Aristotle about light as qualitative change in a medium. These Greek theories [subsequently] generated a wide assortment of optical theories in Islam. (Lindberg 1967, p. 321)

Such diversity, nonetheless, admits of some compliance to broadly based classification. Although the classical commentators failed to appreciate such phenomena as light, sound, and water waves as related by the conjoining concept of energy radiation, their efforts to employ analogy are suggestive of an unvoiced presumption that some consistency of principles may underwrite such natural phenomena. As Whitehead noted: "... there can be no living science unless there is a widespread instinctive conviction in the existence of an *Order of Things*, and, in particular, of an *Order of Nature*." (1938b, p. 14). Analogy is a natural offspring of such a conviction and appears as an accepted modeling tool employed by the ancients including Aristotle.[93] As Boyer (1946, p. 93) remarks: "Aristotle on several occasions pointed out analogies between the

[92] "... Greece was the mother of Europe; and it is to Greece that we must look in order to find the origin of our modern ideas." However, it must also be acknowledged that Rome played a parental role, "and this Roman strain explains its gain in an energy of thought kept closely in contact with the world of facts." (Whitehead 1938b, pp. 17, 27) However it must also be recognized that Greek science was in character a metaphysical endeavour pursuing explanation of the physical world predominately through philosophy. (Boorse & Motz 1966a, pp. 3-4)

[93] "In early Greek thought analogies played a fundamental role in the expression of cosmological doctrines, in the development of natural sciences, and in ethical and political arguments." (Lloyd 2003, p. 60) Further, "analogy provided an important, indeed in some cases the only, means of bringing empirical evidence to bear on obscure or intractable problems, especially in such fields as astronomy and meteorology, embryology and pathology, where direct experimentation was generally out of the question. Various writers, beginning with Anaxagoras at the end of the fifth century, refer to this use of analogy under the general heading of making 'phenomena the vision of things that are obscure ...'" (p. 63)

phenomenon of sight and sound," and cites supporting passages as from Aristotle's *Problemata* :[94] "in an echo ... the whole continues to exist and there are two parts of it of similar form; for [optical] refraction[95] takes place at the same angle."

Early particle modeling of light – 'Ray' atomism

In the natal field of scientific speculation, the notion that phenomena was not infinitely divisible may have developed from more pressing problems associated with the appearance of ongoing change seen the world. (Van Helden 1995) A substratum beyond which subdivision was impossible limited an otherwise infinite regression and may have suggested a constancy for the underlying reality of the world – a notion that seems to have gained popularity in both Greek and Greco-Roman science. However, as Berryman notes, the Greek adjective *atomos* means, literally, 'uncuttable,' and the domain of ancient atomism included more than a notion about the nature of matter, but extended to the idea that there are "indivisible parts in any kind of magnitude – geometrical extensions, time, etc." [96] Additionally, early atomists "formulated views on ethics, theology, political philosophy and epistemology." (Berryman 2005, intro.) Although indivisible, inert, imperishable, and conceptually extremely small, the atomic constituency of phenomena was accorded individual shape and size, and by some, weight and movement. Such atoms were, at least in principle, individually located and countable. One of the most persistent models of light of this type, both in classical times and since, has been that of the 'ray' and its acclaimed property of rectilinear propagation. For many, a ray was more than the locus of passage for bits of light stuff, but rather an indivisible object, discrete and countable in its own right. For such, up to and including the Selectionists of the nineteenth century, "a ray was simply the reification of the path traversed by light ..." (Shapiro 1973, p. 137) Much support for such a speculation derived from the supposed property of rectilinear propagation. Robert Hooke in 1680 recalled in his "*Posthumous Works"*;

> ... both Ancient and Modern Authors ... all conclude [light] to radiate every way in straight Lines from the Luminous Point, whether they supposed it a Flame, as divers of the Ancients; or whether they supposed it a Flux of Atomes, as the *Epicureans* ...they all supposed it to pass in straight Lines. (Hooke cited in Shapiro 1973, p. 135)

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[94] Boyer cites *Problemata* XI. 23. 901. However, as Boyer points out: "The *Problemata* is probably not by Aristotle himself, but is thoroughly Peripatetic and may be regarded as derived largely from the master's teachings." (1946 fn. 21)

[95] As Boyer correctly notes, 'refraction' in Aristotle's usage means 'reflection'. (1946, fn. 21)

[96] "For Democritus and the Atomists, knowledge as well as sense perception arises from effluvia of atoms which are continually thrown off from the surfaces of physical objects ..." (Weinberg 2003, p. 1)

Although probably deriving from direct unaided observations of the apparently sharp and geometrically straight edges of shadows cast from sources of strong light (Mach 1926, p. 3, #4; Römer 2005), the origins of a formulation of rectilinear light rays is lost in antiquity. However, the notion received a robust reinforcement from the "ensuing geometrization of optics" (Shapiro 1973, p. 135), at the hands of Euclid of Alexandria (c. 300 BCE), the pre-eminent mathematician of Greco-Roman antiquity. Although perhaps more renowned for his treatise on geometry and related mathematics, the *Elements,* Einstein praised his contributions as that "admirable triumph of reasoning [that] gave the human intellect the necessary confidence in itself for its subsequent achievements." (Einstein 1934a) Although, as with many written works of great antiquity, a degree of uncertainty surrounds their authenticity, (Lindberg 1971 fn. 13), within the greater corpus of extant Greek literature, two works on optics are generally accredited as Euclidean: *Optica* and *Catoptrica.*[97] As the first truly mathematical treatment of the subject of vision, Euclid's *Optica* is notable both for its methodical technique, developing from initial postulates or axioms in the same formal style as the *Elements*, and for its almost rigorous exclusion of any material, such as physiology, deemed peripheral to a purely geometrical exposition. (Lindberg 1971, pp. 471-472). Although some variation of meaning is possible from translations of these primal optical texts, discrete light rays demonstrating a rectilinear propagation, but portrayed as visual rays emanating from the eye and extending to touch the observed object, appear indispensable to their comprehension.[98] As Ernst Mach understood the Euclidean discrete visual ray construct:

> The rays of sight, propagated rectilinearly, are represented to be such that they densely fill space, but have empty interstices between them. Now, in that it is assumed that only that object is seen on which a ray of sight falls, it is obvious that each optical image contains only a finite number of units. Each object disappears when it is so far away or so small that

[97] Lindberg castes doubt on the provenance of *Catoptrica*, referring to it as 'pseudo-Euclidean.' (Lindberg 1967, p. 331) but also elsewhere admits it as possibly a later recension of a genuine work. (Lindberg 1976, p. 220, fn. 75)

[98] Lindberg, quoting from Cohen & Drabkin, translates Euclid's first postulate as: "That the rectilinear rays proceeding from the eye diverge indefinitely." (Lindberg 1971, p. 472) Herzberger, quoting Burton, translates: "…that lines drawn directly from the eye pass through a space of great extent [not infinite]" (Herzberger 1966, p. 1384) Mendell's more literal translation (Mendell 2007a) has the rays travelling "a distance of large magnitudes," but suggests that "the so-called Recension of Theon (late 4th cent. CE) which has: 'Let it be hypothesized that sight-lines from the eye travel in straight lines, making some distance from one another'," may be the more accurate as to meaning. (Mendell 2007b) Mach also supports a weaker and less objective interpretation suggesting: "In the case of Euclid ... references to rays emanating from the eyes can scarcely be looked upon literally, but rather in the geometrical sense in which we use the expression to-day with regard to perspective ..." (Mach 1926, p. 8) In terms of modern Geometrical Optics, light paths are treated as reversible since "sometimes it is handy to trace the path of the light away from the observer, even though the light is actually moving toward the observer. Reversibility makes this process valid." (Young 2002)

it falls in the space between neighbouring rays. This is a reasonable hypothesis which reminds one of the Faraday lines of force. (Mach 1926, p. 9)

Whereas Euclid treated a rectilinear property of discrete visual rays as an assumed postulate preparatory to geometrical analysis, Aristotle (384-322 BCE), employing both direct observation and reasoned argument, had previously surmised rectilinear propagation from "the rays of the sun ...and the fact that we can only see them from directly opposite" (Shapiro 1973 fn. 3). The later Greek geometer, mathematician and inventor, Heron[99] of Alexandria (c. 62 CE), attempted a more metaphysical demonstration, while Damianus, the son of Heliodor of Larissa, although echoing much of the Euclidean paradigm, included justification for his model from a teleological premise. (Mach 1926, pp. 9-10). On the other hand, pursuing a more empiricist approach, astronomer and mathematician Claudius Ptolemaeus, or simply Ptolemy (c. 100 –170 CE), concluded that visual rays were not discrete lines, but formed a continuous cone, yet also established optical principles of both reflection and refraction, based on a premise of rectilinear propagation.

However, an early basis for contention is evident wherein the direction of the speculated rectilinear rays, modeled then principally as endemic agents of vision, was variously ascribed either as originating in or at the eye or else from a luminous source or both. Writing in the fifth century BCE, the Greek thinker, Empedocles, credited the divine Aphrodite with creating the human eye from the four elements of fire, air, earth and water. Elemental fire, lit in the eye, then projected visual rays outwards to meet[100] and interact with luminous rays emanating from a source such as the sun. (Crowell 2006, p. 12; O'Connor & Robertson 2002b) Plato and Galen also advanced combined extramission-intromission models of vision. (Lindberg 1967, p. 321).[101] A notable variant model of visual intromission was proposed by the atomist Athenean philosopher Epicurus (341-270 BCE) wherein sight resulted from an emission of *eidola* [102]– discrete images or 'films' as coherent graphical elements retaining both source shape and colour

[99] Also known as Hero of Alexandria

[100] From Greek συναύγεια, (*synaugeia*) – lit. 'the meeting of the rays' (Mach 1926, p. 8). A contemporary 'Cooperative Emission' model of vision, which reproduces essentials of the earlier Greek concept, has been reported to be predominant amongst modern 10 –11 year old children's ideas on light and vision, (Wong & Boo 2005, p. 4) and even retained by a significant percentage of tertiary graduates. (Winer et al. 2002)

[101] Elsewhere, Lindberg suggests "[t]hat identity of luminous and visual radiation seems to have been taken for granted throughout antiquity. It was specifically defended by Hero, Damianus, and apparently by Ptolomy." (Lindberg 1971, p. 474, fn.24)

[102] Often referred to by the Latin 'simulacra'. (Kubbinga 1995, p. 723)

– detaching and emanating from a perceived object before entering the eye.[103] (Lindberg 1967, pp. 334-335; Masson 1884, pp. 16-17). Mach,[104] on the other hand, thought this view "could only have been due to an insufficient distinction between physical and physiological circumstances ... and too strong an accentuation of the analogy between sight and feeling." (Mach 1926, p. 8). However, in contrast, and exhibiting a robust individuality of thought, the atomist thinker Lucretius, although an admirer of Epicurus, proposed a relatively modern-sounding intromission model in 55 BC in his almost epic poem *De rerum natura* ("On the nature of things"):

> The light and heat of the sun; these are composed of minute atoms which, when they are shoved off, lose no time in shooting right across the interspace of air in the direction imparted by the shove. (cited in O'Connor & Robertson 2002b)[105]

Aristotle was possibly the first to query the validity of extramission on the basis that he noted vision to be impaired by darkness, a condition not readily explained if visual rays were generated from the eye. (Herzberger 1966, p. 1383)

Early continuum modeling and the concepts of process and change

Nonetheless Rescher (2002) argues, "another variant line of thought was also current from the earliest times onward" and maintains that "the concentration on perduring physical things as existents in nature slights the equally good claims of another ontological category, namely processes, events, occurrences – items better indicated by verbs than nouns." Notably, the primary problem that concerned Heraclitus (c. 480 BCE) was that of change. In contrast to thinkers such as Zeno of Elea and Parmenides for whom the concept of change was problematic to the point of impossibility, Heraclitus espoused the thesis "that *to be* was to change." Thus, he reasoned that a universal constituent matter should share the same property, and identified this in fire which exhibited an elemental continuity although changing in composition from one instant to the next. (Boorse & Motz 1966a, p. 4) Although

[103] Lindberg cites Epicurus, in his *Letter to Herodotus* as describing *eidola* as "particles ... continually streaming off from the surface of bodies ... And those given off, for a long time retain the position and arrangement which their atoms had when they formed part of the solid bodies." (1967, p. 334)

[104] In an unaccountable lapse, Mach casts the Epicurean view as an extramission: "Epicurus held ... that sight was caused by an emanation of pictures from the eye ..." (Mach 1926, p. 8)

[105] The more classical translation by William Ellery Leonard gives: "And first, One oft may see that objects which are light And made of tiny bodies are the swift; In which class is the sun's light and his heat, Since made from small primordial elements Which, as it were, are forward knocked along And through the interspaces of the air To pass delay not, urged by blows behind; For light by light is instantly supplied And gleam by following gleam is spurred and driven. Thus likewise must the images have power Through unimaginable space to speed Within a point of time, ..." (Titus Lucretius Carus, 4.183 - 4.193)

perhaps sharing less investigative attention but nonetheless sharing the ubiquitous modeling appeal to analogy, the Stoics noted:

> We hear when the air between the sonant body and the hearer is struck in spherical waves which impinge on the ears, just as waves in a pool expand in circles when a stone is thrown into it. (Shapiro 1973, p. 136)

Such an analogy incorporating a patently non-linear motif, served well to explore and possibly explain the behavior of sound as contrasted to the rectilinear behavior accredited to light. However, the analogy offered even more. The propagation of water waves portrayed a sense of motion from source to recipient without a physical transportation of substance, and by the agency of analogy offered comfort to those inclining to continuum[106] models of light radiation as opposed to competing atomist conceptions. An emission versus continuum contention, ostensibly a contention between modeling by discrete atomist mechanics as against modeling by processes and change, can be discerned as characterizing some of the differences which sponsored early optical dissentions[107] and is defined by Shapiro such that:

> in an emission theory of light there is a transport of matter from the luminous source to the eye; whereas in a continuum theory there is only a propagation of a state, such as a pressure or motion, through an intervening medium. (Shapiro 1973 fn. 5).

Of significance, however, optical rays, as abstractions descriptive of the path by which an impartation of 'powers', pressures, or processual effects could be accomplished, were retained as a concept necessary to the formulation of various continuum or processual models of light propagation. However, to correctly place such rays into an appropriate optical ontology requires an understanding of the relationship between mathematics and physical nature as posited by classical continuum exponents.

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[106] Shapiro suggests that 'emission' and 'continuum' as expressions are less misleading and avoid possible contradictions otherwise inherent in terms such as 'corpuscular' and 'wave' when descriptive of the early modeling of light up to and including the seventeenth century. (Shapiro 1973, p. 136, fn 5)

[107] Van Helden summarizes an aspect of early Greek metaphysics as: "Some believed that all was change, others that change was illusory." (1995) Berryman notes that: "According to Aristotle's presentation (*On Generation and Corruption*), the motivation for the first postulation of indivisible bodies is to answer a metaphysical puzzle about the possibility of change and multiplicity. Parmenides had argued that any differentiation or change in Being implies that 'what is not' either is or comes to be ... and was understood to have raised a problem about how change can be possible without something coming from nothing. Several Presocratics formulated ... philosophical systems in which change is not considered to require something coming into being from complete nonexistence, but rather the arrangement of preexisting elements into new combinations ..." (Berryman 2005, sec.2) (Parmenides, #8)

Aristotle, who may be classified as supporting a continuum-styled modeling[108] of light propagation based on such rays, asserted that: "The minute accuracy of mathematics is not to be demanded in all cases, but only in the case of things which have no matter. Therefore its method is not that of natural science; for presumably all nature has matter." (Chen-Morris 2001, p. 453) Of concern for Aristotle was distinction both from various versions of Platonism, wherein the objects of mathematics, as perfect and eternal forms, existed as separate entities in a parallel universe, and from Pythagorean claims, that essentially identified number with physical artifact. (Gare 2006, p. 5; Mendell 2004, intro.)[109] In contrast, Aristotle claimed that mathematical features were "inseparable from material things but could be thought of separately," and that the "objects of mathematics are treated as separate but cannot exist separately." (Weinberg 2003, p. 1) Chen-Morris points to an Aristotelian formula that summarizes this relationship for the mathematical sciences that constitutes a denial of reification and succinctly exposes Aristotle's recognition of representation and his stance on realism:

> While geometry investigates natural lines but not qua[110] natural, optics investigates mathematical lines, but qua natural, not qua mathematical. (Aristotle Bk.2, ii) [111]

Thus, as Chen-Morris explains, Aristotle's dictum places optics as subordinate to, and in turn dependent on,

> entities whose origin lies in another discipline. The observer of nature creates through abstraction geometrical figures that are applied artificially when the need arises according to accepted rules of the intermediate disciplines such as astronomy, optics and music, Thus, in optics, the scientist constructs artificial lines and angles, although according to Aristotle's analysis in *De Anima*, in reality there are no rays, nor reflected rays or angles of incidence. The scientist, by assuming these imaginary constructs to be real, can explain away optical puzzles in order to save optical phenomena. (Chen-Morris 2001, p. 456)

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[108] Aristotle's continuum formulation did not, however, necessitate a process of temporal progression and Aristotle even censured Empedocles for suggesting that light 'travelled'. As Sabra observes: "Rather than being a [temporal] process or movement, light for Aristotle was a state or quality which the medium acquired all at once from the luminous object ..." (1967, p. 46)

[109] As Gare notes, for the Pythagoreans, "the basic principle and root of all things was taken to be number ... There was no distinction between physical bodies and ideal mathematical constructions, or between rigid geometrical form and the vital processes of living things ..." (2006, p. 5)

[110] "The word Aristotle uses is commonly translated with the English word *'qua'* which itself translates the Latin relative pronoun *'qua'*, but with one important grammatical difference. The English adverb is normally followed by a noun phrase. As a relative adverbial pronoun in the dative case, the Greek word captures all possible meanings of the dative, including, 'where', 'in the manner that', 'by-means-of-the-fact-that', or 'in-the-respect-that' ... Hence *X qua Y* should be understood as elliptic for: '*X* in the respect that *X* is *Y*' or '*X* by means of the fact that *X* is *Y*'... (Mendell 2004, sec. 7.4)

[111] Chen-Morris cites Aristotle, '*Physics*', Bk. 2, 194a 10-11, in *The Complete Works of Aristotle*, vol. 1, p. 331.

However, it is pertinent to the development of optical modeling that elsewhere in the *Problemata,*[112] Aristotle ventured a comparison apparently intended to delineate between emission and continuum models whilst retaining correspondences such as rectilinear propagation:[113]

> As ... in a mirror the image appears at the end of the line along which the sight travels, so the opposite occurs in moving objects, for they are repelled at an angle of the same magnitude as the angle at the apex, ... and in these circumstances it is clear that moving objects must rebound at similar angles. (Aristotle cited in Boyer 1946, p. 94)[114]

Analogy, relating the more ephemeral with characteristics of the tangible to illuminate principle, is a fundamental of modeling methodology and legitimate when retained within its intended domain of applicability that then also defines specific limitations of use. However, just as representation is not endemic in that which is represented, modeling intent is not endemic in the model. Denotation by the model builder in partnership with a tacit 'agreement' by the model user is essential for a valid representation to take hold. (Bailer-Jones 2003, abs) Ambiguity of intent, whether a failing of the modeler, or even as inadvertently perceived by the model user, is inimical to deployment of an authentic representation. Unfortunately, Aristotle's analogy was to so suffer and engender enduring ramifications. As Shapiro notes:

> At the turn of the seventeenth century the Scholastic optical tradition, which adhered to a continuum theory of the propagation of light, still dominated optical thought. This Aristotelian tradition was based on rays ... and the analogy between the reflection of a body and that of light and sound. In truth, one cannot deny the appeal of the analogy or that these phenomena (the rebounding of a ball from a wall, echoes, and the reflection of light from a mirror) have reflection in common, yet the analogy [if intended to compare a mechanical analogue with light] is specious. Aristotle himself went to some effort to establish that light and sound (and even odor) are propagated in a continuum and not by emission. The distinction which he intended to establish between the two fundamentally different modes

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[112] Boyer cites *Problemata* XVI. 13. 915 (fn. 25)

[113] Sabra suggests the passage is "concerned to bring out a *contrast*, rather than an analogy," (1967, p. 69) whereas Boyer ventures the suggestion that "[h]ere the author probably has in mind the Platonic doctrine of visual rays emanating from the eye. The visual ray proceeds to the mirror, at which point it meets the ray of light from the object. Both rays therefore travel toward the mirror, whereas in the case of a rebounding object one motion is toward the wall while the other is away from it. In this sense, then, the behaviour in the mechanical situation is opposite to that in the optical reflection; but the definite implication is that the cases are alike in other respects, namely, in the rectilinearity of the motions and in the equality of the angles." (1946, p. 94)

[114] Boyer notes "that the equality of angles in optical reflection is ... explained in terms of the rebound of objects in rectilinear motion" also by Heron. (p. 95)

> of propagation was vitiated, and the crucial problem of explaining the reflection and refraction of motion was avoided. (1973, p. 139)

Significantly, Walter Charleton, (1619-1707) a leading exponent of Epicurean atomism in mid seventeenth century England, and adherent to an atomist modeling of light rays, "cited Aristotle's analogy approvingly." (Shapiro 1973, p. 139, fn 16) Even amongst adherents to continuum modeling, this misapprehension of original modeling intent was to stultify scientific thinking and impede progress. Shapiro further notes:

> In the middle ages [Aristotle's ball] analogy was pursued, and the concept of the multiplication of species[115] was adopted to explain the continuum propagation of light and sound. [Adoption of the analogy] can be seen in the optical work of Alhazen and in his Latin followers, Roger Bacon, and Witelo. Consequently, their continuum theories of light suffered from the same difficulties as Aristotle's. (1973, pp. 139-140)

Shapiro perceives an irony in the historical record: "Thus in the same tradition alongside the non-mechanical multiplication of species there were mechanical analogies to the reflection and refraction of a body." (1973, pp. 139-140). On the other hand, physicist Vasco Ronchi, (1897-1988) charges the thirteenth century promulgators of optical 'species', principally Robert Grosseteste, Roger Bacon, John Pecham and the Polish born Witelo,[116] with virtual Epicurean recidivism. For Ronchi, 'species', as individual likenesses, are tantamount to the earlier atomist's '*eidola*' and amount to a repudiation of those valuable advances in optics attributable to later Arab science. According to Ronchi:

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[115] According to Shapiro, "Species are a power emitted from a body and correspond to the quality of the body" and fall securely within the meaning of a continuum or processual model in that Roger Bacon wrote: " ... a species is not a body, nor is it moved as a whole from one place to another; but that which is produced [by an object] in the first part of the air is not separated from that part ...rather, it produces a likeness to itself in the second part of the air, and so on. Therefore there is no change of place, but a generation multiplied through the different parts of the medium ..." (Shapiro 1973, p. 139, fn 17) (Lindberg 1976, p. 113)

[116] Witelo of Silesia (1230-1275) Latin: Vitellio (Fermat), Vitello (Risner), Vitellion (Descartes) "In the thirteenth century, Witelo wrote an exhaustive ten-volume work on optics entitled *Perspectiva* that served as the standard text on the subject until the seventeenth century ... Witelo's work on optics was so extensive that the first major addendum to it was not undertaken until several centuries later, when Johannes Kepler published *Ad Vitellionem Paralipomena, Quibus Astronomiae Pars Optica Traditur* (*Supplement to Witelo, in Which Is Expounded the Optical Part of Astronomy*) in 1604." (Davidson 2008) Alternatively, Eastwood asserts that the medieval Latin treatise on optics by John Pecham, "was probably the most influential medieval source in optics until the time of Kepler... Pecham's work was seen or used by far more students than the advanced treatises of Ptolomy and Alhazen or the more extensive ... work of Pecham's contemporary, Witelo the Pole." (Eastwood 1972, p. 322)

What ensued was an indescribable atrophy of thought. Ideas tended more or less to cluster about the doctrine of species, a new edition of the ancient *eidola*.[117] These *species*, however, were produced by the *lumen*, when it impinged on a body, and they moved along the observer's visual rays as though along rails guiding them toward the eye. During this motion they contracted in order to be able to enter the pupil. The contraction no longer constituted a serious obstacle [as it had in ancient times] because [the Muslim scientist] Ibn al-Haitham's mechanism had provided a sort of justification for it. In other words, an effort was made to combine the classical with the new. The merger was a monstrosity, with which the philosophers and mathematicians of the later Middle Ages tried to reason when confronted by optical problems." (Ronchi cited in Lindberg 1967, p. 332)

Finally, Shapiro (1973, p. 140), Boyer (1946, p. 95), Sabra (1967, pp. 78-79) and Herzberger (1966, pp. 1389-1390) note that the analogy, as mechanically interpreted, was integral to Descartes' fallible, and later criticized,[118] justification of the laws of reflection and refraction,[119] although he contemporaneously held to a continuum modeling of light. According to Shapiro, inappropriate model choice began to impact insidiously on theory construction:

This approach culminated in the seventeenth century in the work of Descartes ...the two characteristic elements of this tradition are very much in evidence: the explanation of reflection and refraction based on the analogy to the motion of a ball, and the use of rays but not waves. In *La Dioptrique* (1637) Descartes explained reflection by an analogy to the reflection of a ball from a plane, and refraction by an analogy to the refraction of a ball entering a fluid. (Shapiro 1973, p. 140)

Nonetheless, a perversion of interpretation that appears so persistent and found co-existent in the same explanatory tradition with its philosophical rival, begs further

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[117] In support of his contention that the "multiplication of species is more like the propagation of waves than like the motion of projectiles," Lindberg additionally cites Pecham and Grosseteste as providing similar descriptions of 'species' to that of Bacon, thus rejecting attempts to recognize 'species' as a form of atomist *eidola,* [rather than as a continuum model], as claimed by Ronchi. (Lindberg 1967, pp. 336-337)

[118] Fermat entered into controversy with Descartes in 1637, pursuing the principle of least time. Although this verified the sine ratio as advanced by Descartes, in contradiction it also required that light propagate faster in rarer media, a result later supported by Foucault's experiments and Huygen's principle. (Sabra 1967, p. 137)

[119] Although Descartes is to be considered one of the discoverers of the correct 'sine law' of refraction, having published it in his *La Dioptrique* in 1637, much of his reasoning was criticized by Fermat, who showed that water would need to be less dense than air to align with Descartes 'ball analogy' explanation. (Herzberger 1966, pp. 1389-1390), (Sabra 1967, pp. 85-92) Further, in 1662, Isaac Vossius publicly accused Descartes of plagiarizing the law from the Dutch mathematician Willebrord Snellius. However, "neither the question of Descartes' originality nor ... priority can be settled on the evidence available ..." (Sabra 1967, pp. 101-103)

explanation. Although the evidence is at best circumstantial, we might well be reminded of Rescher's assertion that a bias in favor of the substantial and simply locatable is endemic in Western metaphysics often to the detriment, subordination and even omission of the processual. (1962, p. 410) It is arguable that such a bias would favor an interpretation of Aristotelian intent that supported a particle styled outcome, however the evidence is insufficient to secure a conviction.

Optics, modeling and Islamic science

Although Alexandria remained a vigorous centre of scientific endeavour after the demise of the Greek hegemony and the concomitant intermission in European scientific progress lasting for more than a century and a half, the rise of Islamic empire ensured establishment of the Arab scientific and mathematical proclivities into northern Africa, Sicily and Spain. Here the "Muslim heirs of Greek thought" attempted to assess, revise and incorporate its remarkably diverse philosophical and scientific legacy, not the least of which expounded on the topic of optics and its potential for mathematical treatment. (Lindberg 1967, p. 321).

The first great scientist-philosopher of this new Islamic dominion, Abu Yusuf Ya'qub ibn Ishaq al-Sabbah al-Kindi (ca. 800-870 CE),[120] usually Westernized simply as 'Alkindi',[121] characterized his motives as twofold: an unexpurgated communication of the full Greek legacy to the Islamic world[122] and to "complete the theoretical sciences" by a critical review and correction of that inheritance. (Lindberg 1971, p. 470) For Alkindi, optics was of supreme scientific importance, deriving its significance from his cosmological stance that viewed "the universal activity of nature, exercised through the radiation of power or force" as that activity that "binds the world into a vast network in which everything acts upon everything else to produce natural effects." By Alkindi's assessment: "Everything that has actual existence in the world ... emits rays in every direction, which fill the whole world." Thus, in this early Muslim science, the laws of radiation are the laws of nature, and "optics is consequently a prerequisite to other studies." (Lindberg 1971, pp. 470-471) The application of such a cosmology was to foster criticism of his Euclidean inheritance. Perceptions of a discrete-ray reification with its representational realism, were to be rejected as unwarranted practices in the prior Greek approach.

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[120] "To his people he became known as ... the philosopher of the Arabs. He was the only notable philosopher of pure Arabian blood and the first one in Islam" cited from A A al'Daffa, *The Muslim contribution to mathematics* (London, 1978) by O'Connor & Robertson (1999b)

[121] Also transliterated as 'al-Kindi' or 'al Kindi' and sometimes Latinized to Alkindus. He is credited with more than 260 works extending to all branches of scholarship. (Lindberg 1971, p. 470)

[122] "His own writings might be thought of as a sustained public relations campaign intended to display and advertise the value of Greek thought for a contemporary ninth century Muslim audience." (Adamson 2006, sec.1.1)

Although it is well noted that Euclid's *Optica* is a stringently geometrical treatment of the subject of vision and perspective, from which all superfluous physiological and psychological factors have been assiduously stripped, Lindberg qualifies this view as being "true only to a first approximation" (1971, p. 473) noting that "even as Euclid's ancient commentators recognized, the postulates and several propositions ... have inescapable implications vis-à-vis the ontology of visual rays and thus spill over into the physical realm." Indeed the interstices between adjacent visual rays at some distance from an observer are invoked as having specific physical ramifications for the clarity, and even the possibility, of vision. As Lindberg concedes: "It is clear that rays having such properties cannot be mere constructions intended to [solely] represent the geometry of sight; they must be the physical agents of sight." (1971, p. 473) Although it must be admitted that Alkindi shared Euclid's appeal to both visual and luminous rays in defence of an extramission modeling of vision and also similarly demonstrated a heavy reliance on mathematics in treating his subject matter, (Adamson 2006, sec.5.1) the spectre of a reified realism in the Euclidean treatise did not pass unnoticed. For Alkindi, such an implicit corporeality had dire implications. In his work on optics *De aspectibus*, he was to assert:

> Certain of the ancients have judged that many rays issue from the observer along straight lines, between which are intervals. From which opinion follows an absurdity,[123] namely that the definition of a line ... is a magnitude having one dimension, namely length without width, whereas a ray is the impression of luminous bodies in dark bodies, denoted by the name "light" because of the alteration of accidents produced in the bodies receiving the impression. Therefore a ray is both the impression and that in which the impression is. However, the impressing body has three dimensions – length width, and depth ... for everything that is perceived [either] has width or is conveyed by that which has width. But a line does not have width. Therefore a line is not seen, nor what is conveyed by it ... (cited in Lindberg 1971, pp. 478-479)

Continuing in what amounts to one of the earliest recognitions of both a "fallacy of misplaced concreteness" and a "fallacy of simple location", Alkindi mounted a forceful and comprehensive attack on discrete ray reification. Although it must be admitted that Alkindi's censure of such objectification derived ultimately from views previously expressed by his intellectual mentor, Aristotle, (Adamson 2006, sec. 2.1) the breadth of argument and the ensuing insights gathered by him attest to a robust originality. After

[123] Lindberg translates from *De Aspectibus*, Prop. 11, pp. 12-13. Later in the same Proposition Alkindi refers to the reification of visual and luminous rays as ".. magis horrendum inconveniens." (a very horrendous absurdity) (Lindberg 1971, p. 479, fn. 41)

having amply demonstrated that discrete Euclidean geometrical ray constructions must be solely that of representational abstractions, Alkindi further dismissed the possibility that the system could be composed of beams or pencils of light. By Alkindi's assessment, interaction with the target system, although pragmatically portrayable by abstract constructional rays,[124] must "constitute a single continuous radiant cone." (Lindberg 1971, p. 479) [125] Although it is significant that his radiant cone is a continuum, more akin to antecedent Galenic, Ptolemaic and Stoic concepts than to atomism, a principle of the process was as yet absent – that of temporal sequence or process. Although denying the extramission of *physical* entities from the eye, Alkindi nonetheless maintained that "a transformation of the ambient air produced by the visual power of the eye" propagated instantaneously. (Lindberg 1971, p. 486).

Modeling of vision and of radiation in general, that correctly designated the luminous source and inert recipient, had to wait for the analyses offered by Abu Ali al-Hasan ibn al-Haytham.[126] (965 – 1040 CE)[127] Usually known as Alhazen, his extensively reconstructed optical modeling, acclaimed as the "first comprehensive and systematic alternative" to Greek optical thinking, enshrined principles which are recognized as initiatory to our modern understanding of optics, and for which Alhazen is honoured as "the father of modern optics." (Lindberg 1967, p. 322; Zahoor 2004) [128] Central to his optical thesis, which rested on a philosophy of science wherein "nothing ought to be believed except through reason or by sight," was a denial of corporeal visual rays and the affirmation that "*every visible object* is seen by the emission of its own

[124] "... the principle ... that [abstract constructional] rays issue in all directions from every point on the surface of a luminous body" was "first stated by Alkindi" and was later in the thirteenth century "reproduced by Roger Bacon and Witelo down to the very lettering on the figure ..." (Lindberg 1971, p. 488)

[125] As Lindberg points out, even in this, Alkindi was in debt to Ptolemy, whose similar but prior opinion was readily available to Islamic readers. (Lindberg 1971, p. 478, fn. 37; 1976, p. 224, fn. 29)

[126] Roshdi Rashed (1990, pp. 464-466) suggests that a historical injustice is generally perpetuated by historians of the optical tradition, wherein insufficient credit as assigned to Arab science of this period. In general he requires acknowledgement that "(1) Ibn al-Haytham was not the first to have effectively used Ptolomy's *Optics* ... (2) al-Kindi was not the only significant figure in the history of Arabic optics before Ibn al-Haytham (Alhazen) – rather there existed a tradition of research in this field that included names just as prestigious; and (3) it is vital to take into account research on burning mirrors ..." Of these were Hunain ibn Ishaq (d. 877), Avicenna (Ibn Sina, d. 1037) and Abu Sa'd al-'Ala' Ibn Sahl (d. 896) who pioneered work on burning mirrors and lenses [anaclastics] which was known to, and pursued by Alhazen. (Middleton 1961, p. 533) Having completed an extensive analysis of burning mirrors in both parabolic and ellipsoidal forms, Ibn Sahl examined both plano-convex and hyperbolic biconvex lenses. Additionally he pre-empted Snell's law and studied the mechanical drawing of the conic sections. (Rashed 1990, p. 490)

[127] "Ibn al-Haytham is sometimes called al-Basri, meaning from the city of Basra in Iraq, [where he was probably born c. 965] and sometimes called al-Misri, meaning that he came from Egypt [where he visited and worked]. He is often known as Alhazen [or Alhacen or Alhaycen] which is the Latinised version of his first name "al-Hasan." (O'Connor & Robertson 1999a)

[128] "Modern optical thought issues, by direct descent, from the work of Alhazen and his immediate followers." (Lindberg 1967, p. 322)

light, though illumination by a self-luminous body is a normal prerequisite," (Lindberg 1967, p. 323, orig. emph.)[129] Further, reiterating a prior position expressed by Alkindi, he also censured the reification of optical rays:

> ... willing only to allow mathematicians, who are concerned with a mathematical account of the phenomena rather than with the real nature of things, to use visual rays to represent the geometrical properties of sight ... [since] according to Alhazen, all mathematicians who postulate visual rays 'use only imaginary lines in their demonstrations, and they call them 'radial lines.'' Moreover, the belief 'of those who consider radial lines to be imaginary is true, and the belief of those who suppose that something [actually] issues from the eye is false.' Thus visual rays ... are mere geometrical constructions, useful in demonstrating the properties of sight. They can serve as a mathematical hypothesis, but they have no physical existence. (Lindberg 1967, pp. 326-327)

Alhazen's texts make an unambiguous case for luminous radiation to be understood as "a quality of bodies that can be propagated in straight lines through a transparent medium to produce visual sensation," as opposed to corporeal atomist rays however conceived, and in so doing recalls a qualitative Aristotelian sentiment. Nonetheless, it is noteworthy that both reflection and refraction are here modeled by analogy on the motion of projectiles such as "a sphere ... fixed to the tip of an arrow" in a scenario disturbingly reminiscent of Aristotle's prior, but misapprehended, ball analogy. Nevertheless, in appealing to further explanation offered by Alhazen as mitigation, Lindberg interprets a genuine representational functionalism:

> If we are to understand Alhazen's position, we must recognize that these comparisons are not literal statements of identity, but similes, meant to elucidate the [abstract] geometry ... of reflection and refraction but not the [real physical] nature of the reflected or refracted entity. (Lindberg 1976, p. 80)

Indeed, the fact that Alhazen continued his discussion by including contrasts between the subsequent actions of material projectiles and that of light is suggestive that he perhaps correctly understood the antecedent Aristotelian analogical intent. Although only one of various authorities cited by a revived scientific interest in thirteenth century Europe, Alhazen's influence is generally acknowledged as the most dominant. His work was deemed fiducial by Bacon, Pecham and Witelo and, although an interpretation

[129] "For the first time an intromission theory of vision has become a viable alternative, adequate to compete on geometrical as well as physical and psychological terms with the theory of visual rays." (Lindberg 1976, p. 78)

contested by Ronchi,[130] was widely incorporated in their optical works and exerted a considerable influence on subsequent generations. (Lindberg 1967, p. 331)

Optical modeling and the rise of modern science

That period in European history which might lay claim to be the era from which modern science emerges identifiably from a stultifying medieval background, depends on the criterion employed. For some, an appeal to empiricism places the time as synchronous with Galileo's experimental validation of hypotheses[131] or to his "complete decentering of the human abode in 1610, when his telescope revealed the existence of moons around Jupiter" (Holton 2000, p. 329). Others might extol the systematization of experimentation stimulated by the writings of Bacon.[132] Yet others may appeal to the heliocentric reorientation of cosmological understanding ushered in by Copernicus with its attendant reliance on direct observation,[133] whilst yet others may argue that a plausible date for the onset of an "offensive modernity" to be February 1605, when Johannes Kepler "laid out his breathtaking ambition … to show that the celestial machine is to be likened not to divine organism, but rather to a clock-work" (Holton 2000, p. 329). Yet again, others might appeal to Newton's "widespread reputation" as "instrumental in jump-starting the Scientific Revolution," (Spring et al. 2004) although others may make a compelling case for that moment when Giordarno Bruno chose martyrdom rather than relinquish his right to "free imaginative speculation." (Whitehead 1938b, p. 11)

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[130] As already noted, Ronchi rejects a valid assimilation of Alhazen's optical tenets by the West as portrayed by Lindberg, and often characterizes medieval Western optical modeling as a "corruption of Muslim views and a return to the less satisfactory theories of Greek antiquity." (cited in Lindberg 1967, p. 332) "In Ronchi's view ... Alhazen's ideas [were only poorly apprehended] by Bacon, Pecham, and Witelo in the thirteenth century, but afterwards forgotten, rejected, or misunderstood until the time of Maurolycus and Kepler at the end of the sixteenth century." (Lindberg 1967, p. 333) Lindberg however admits that the extramission model of visual rays, although vanquished by Alhazen, "continued to be debated as late as the seventeenth century." (Lindberg 1976, p. 227, fn. 66)

[131] By Einstein's assessment: "Pure logical thinking [as exemplified by the Greek approach] cannot yield us any knowledge of the empirical world; all knowledge of reality starts from experience and ends in it. Propositions arrived at by purely logical means are completely empty as regards reality. Because Galileo saw this ... he is the father of modern physics – indeed, of modern science altogether." (Einstein 1934a)

[132] According to Ashley (1952, pp. 153-154), "[u]nquestionably it was the teaching of [Sir Francis] Bacon that stimulated scientific investigation as distinct from haphazard experimenting ... and H.B.Wheatley, a later historian of the [Royal] Society, considered that Bacon inspired the early history of the Society as much as Newton commanded the second phase." Brown notes that "Bacon put forward a 'new method' (*Novum Organum*) based on the idea that scientific knowledge is cumulative ... [and] visualised teams of people carrying out a multitude of experiments planned to embrace all possible enquiries which are relevant to the welfare of society." (1986, p. 6)

[133] Copernicus wrote his first version of the heliocentric planetary model *De Revolutionibus orbium caelestium* ("On the revolutions of celestial bodies") in May 1514, but withheld publication until 1543, which, as a date, "is a good beginning point for the Scientific Revolution." (Bunch & Hellemans 2004)

According to Holton, description by the word "*modernity* and its cognates entered the English language starting as early as the 1620s." (Holton 2000, pp. 329-330) Characterizing this rise of modern science as that "balance of mind" which exhibits a "union of passionate interest in detailed facts with equal devotion to abstract generalisation," Whitehead notes that "although in the year 1500 Europe knew less than Archimedes who died in the year 212 B.C., yet in the year 1700, Newton's *Principia* had been written and the world was well started on the modern epoch. (1938b, pp. 13, 16) Nonetheless, he warns that:

> It is a great mistake to conceive this historical revolt [of the Reformation and the scientific movement] as an appeal to reason. On the contrary, it was through and through an anti-intellectualist movement. It was the return to the contemplation of brute fact; and it was based on a recoil from the inflexible rationality of medieval thought ... although one outcome of this reaction was the birth of modern science, yet we must remember that science thereby inherited the bias of thought to which it owes its origin. (1938b, pp. 19, 21)

Modern science and the Aristotelian legacy

According to Whitehead, "science started its modern career by taking over ideas derived from the weakest side of the philosophies of Aristotle's successors." (1938b, p. 29) In an observation that perhaps finds some middle ground between Lindberg and Ronchi, Chen-Morris observes: " ... medieval perspectivists asserted that the scientist abstracts his lines and visual angles directly from observed optical phenomena. In other words, they somewhat turned the Aristotelian formula on its head ..." (2001, p. 456) On the other hand, Roger Bacon reformulated the medium that separates the luminous source and its reception at the eye such that the medium was itself "the body to whose potential it is due that a species is 'a corporeal thing actually existing in body'." Again, although he drew substantially from Aristotle's *Problemata*, (Shapiro 1973, p. 135, fn.1) he asserted that "species multiply according to straight lines, and that these lines have '*profunditatem et latitudinem*', i.e. these lines are not [merely] geometrical lines but have physical reality and presence." (Chen-Morris 2001, p. 464) Such sentiments had far-reaching implications and potentially lend weight to Ronchi's reproof. For Aristotle, mathematics had posited idealized imaginative cases whose faithful exhibition in a physical context was problematic. Further, both Aristotle and his Islamic successors and interpreters had emphatically rejected optical theorizing enabled by atomism and radiant reification. However, both Bacon and the influential Witelo apparently compromised their Aristotelian and Muslim optical legacy and again courted some form of radiant reification. (Chen-Morris 2001, p. 456) Beyond this, the Aristotelian legacy was to be subjected to an even more robust insult, wherein Chen-Morris detects a "major intellectual shift between Copernicus and the mid-17th century" which involved the patent "rejection of Aristotelian assertions concerning the relationship of mathematics to physical nature." As evidence he notes that in contrast to Aristotelian assertions, "astronomical and mechanical texts from Kepler through to Newton insist that the laws of mathematics [actively] supply the underlying scheme that governs the natural world and its perishable phenomena." (2001, p. 453) Such a paradigm drew its inspiration principally from a "Neopythagorean"[134] resurgence, a subsequent literalist interpretation of arcane Pythagorean philosophy that was to dominate during both the Middle Ages and Renaissance. (Huffman 2006, §1(4))

Optical modeling and Neopythagoreanism

Whereas the Polish astronomer Copernicus (1473-1543) had acknowledged his debt to Pythagoras and later Pythagoreans for providing heliocentric inspiration,[135] it is Johannes Kepler (1571-1630) who is lauded as the "last great Pythagorean" (Huffman 2006, §5.2). However, Kepler's astronomical modeling appears equally indebted to Platonic and esoteric Christian teleology.

> Kepler began by developing the Copernican system in light of the five regular solids (tetrahedron, cube, octahedron, dodecahedron and icosahedron), to which Plato appealed in his construction of matter in the *Timaeus* ... [asserting] that the *Timaeus* was a commentary on the first chapter of [the Biblical] *Genesis* ... [adding] that his purpose is to show that God used the five regular bodies, 'which have been most celebrated from the time of Pythagoras and Plato' as his model in constructing the universe ... (Huffman 2006, §5.2)

When assessing Kepler's new Pythagorean mathematical paradigm rather as "a new understanding of the old mathematical tools themselves," Chen-Morris suggests that:

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[134] Although Neopythagoreanism cannot be characterized as a homogeneous philosophical tradition, nor as contained within definite historical boundaries, it is typified by views, lacking both responsible scholarship and historical accuracy, that venerates Pythagoras as both the originator and central figure of the entire Greek philosophical panoply. The original figure is blurred by mythological and historical embellishments such as being accredited with providing the source material upon which both Plato and Aristotle later drew or as the progenitor of the mathematical sciences, the *quadrivium* of arithmetic, geometry, astronomy and music, together with portrayals variously as a master magician, religious expert or primal esoteric sage. It is this Pythagoras that Copernicus eulogizes and that extensively influenced medieval education, literature, art, and philosophy as well as the sciences. (Huffman 2006, §1 (4), §5) (Thom 2007, p. 67)

[135] According to Huffman, a "number of the followers of Copernicus saw him as primarily reviving the ancient Pythagorean system rather than presenting anything new." (2006, §5.2)

> The Aristotelian and Neoplatonic traditions assumed that either mathematics is abstracted from concrete natural objects or is imposed as an ideal construct on chaotic nature. In contrast, Kepler suggests that geometrical figures are embedded in the motions of physical objects and that they serve as exact representations of these motion[s] and not as mere secondary abstractions. This move demanded a new definition ... of the [Aristotelian] relationship of the imagination to reality. (Chen-Morris 2001, p. 454)

Kepler, in his thrust to supplant the Aristotelian psycho-mathematical model with its Neopythagorean antagonist, recognized the essentials of an Aristotelian psychology in the optical tradition of Witelo and Alhazen as his principal object for censure. Denouncing principles of Aristotle's *De Anima* in his major optical work *Ad Vitellionem Paralipomena, Quibus Astronomiae Pars Optica Traditur* (*Supplement to Witelo, in Which Is Expounded the Optical Part of Astronomy*) of 1604, Kepler portrays "Witelo's already compromised application of abstraction to the science of optics, as creating a yet unnecessary separation between the mathematical aspect of reality and its material necessities and effects." In an apparent repudiation of prior Muslim reformation, Kepler emphasized that the geometrical lines utilized by the opticians were not abstractions of material phenomena but the real "motion of a two-dimensional (i.e., mathematical) surface." (Chen-Morris 2001, p. 457) In this is recognizable a principle element of misplaced concreteness wherein mathematics, as a precise but idealized modeling tool, became confounded with a physical explanation intended for expression by theory. Kepler's endeavours, however, present as one of the great ironies of optical history. The success of Kepler's ensuing optical scheme may appeal as somewhat serendipitous given its superstitious and teleological justifications together with its substantial ontological and philosophical misapprehensions. Nonetheless, in appraising a seemingly naive literalism and fallacy of misplaced concreteness, Shapiro detects a fortuitous departure from a stagnant medieval scholasticism and awards credit where he believes cedit is due:

> Kepler took the first significant step toward developing the concept of ray and recognizing the relation between a luminous surface ... and a ray[136] ... Kepler conceived of light as a power (*virtu*) emanating spherically from its source. The spherical surface, 'the highest perfection' and 'the most excellent figure of all,' with its center, radius, and surface, reflects the Trinity. The luminous surface, expanding with an infinite velocity, lacks corporeal

[136] "Since light radiates spherically from any point, and a sphere has infinitely many radii, an infinite number of rays flow out from any point. By the principal of economy, namely that a straight line is the shortest of all lines, the rays are rectilinear. Kepler stresses that 'a ray of light is nothing at all flowing out from light itself,' but 'only the motion itself of light.' According to Kepler, a light ray is a construct, the locus of a point on the expanding luminous surface, while light itself is the physically existant (sic) luminous surface." (Shapiro 1973, pp. 141-142)

> matter (*materia corporea*) but does possess a 'kind of matter' (*quadam materia*) which assumes geometrical dimensions and can be considered as a geometric body; this allows Kepler to treat light mathematically. (Shapiro 1973, p. 141)

Although a separate modeling function went unrecognized and the modeled entities and modeling justification required significant development, not again until the model of abstract ray as normal to a finitely expanding spherical wavefront found full expression from Christiaan Huygens near the end of the seventeenth century, would a representation of optical radiation embrace so clearly that fundamental scheme that can be recognized as truly modern.[137]

Atomism and the mechanistic worldview

The Epicurean atomist philosophy subordinated the pursuit of physics to the necessities of a system of ethics and retained influence for many centuries until a Roman Epicurean adherent, Titus Lucretius Carus (c 99-c 55 BCE), enshrined its principles in poetry. (Van Helden 1995) In 1473, Lucretius's epic poem *De rerum natura* ("On the nature of things"), printed from the single surviving copy found in 1417, made the atomistic philosophy common to Democritus and Epicurus known to Western scholars. (Bunch & Hellemans 2004) However, although the natal Greek view was entirely mechanistic and exhibited an unabashed causal determinism, (Cohen 2002) Van Helden explains that:

> In the Christian world, nature was seen as the product of a transcendent creator and was therefore fundamentally rational. Aristotelian notions of purpose and order fit the Christian mindset better. Moreover, in atomism there was an unbridgeable gap between the level of the atoms and the observable phenomena, whereas Aristotelian natural philosophy addressed observable phenomena directly. (Van Helden 1995)

Atomism in the mould of Epicurus had been criticized by Aristotle both for what he gauged as logical inconsistencies and for its failure to adequately account for 'secondary' qualities such as colour and taste. Aristotelian matter was itself qualitative with qualities built into the fabric of tangible substance. Nonetheless, Aristotle had also postulated '*minima naturalia*' as a limit to divisibility of such matter. As interpreters of the Aristotelian legacy, European Scholasticism pondered the implications of this

[137] Kubbinga however warns that: "One of the shortcomings of Kepler's *Dioptrice* was the absence of an adequate refraction law. Recent historical research has established that the sine law as such was discovered by Thomas Harriot about 1601 ... [yet] even the English physicists attribute this discovery to Willibrord Snel ... in the early 1620s." (1995, p. 726)

'minima.' Eventually, although the "doctrine of *minima* never questioned Aristotelian notion of form, and was usually seen as distinct from atomism ... in the second half of the sixteenth century, Scaliger[138] interpreted *minima* as particles ..." (Clericuzio 2000, p. 9)[139] Synergistically, this interpretation merged with a revival of interest in classical atomism resulting from publication of the Lucretian poetry, providing "the material foundation of the mechanistic philosophy of the seventeenth century." (Van Helden 1995)[140] However, in attempting to correctly assess the historical and philosophical import of seventeenth century atomism, some caution needs to be exercised. In an enthusiastic eulogy in celebration of the achievements imputed to ancient Greek atomists such as Leucippus of Miletus, his student Democritus of Abdera, and Epicurus, as expounded by Lucretius, Masson has claimed that the

> propositions in which Lucretius had stated his atomic theory anticipate some recent discoveries in ... physics in a most marvellous way. Science has now proved that his propositions as to the constitution of matter, in each case, are either certainly true, or else foreshadow the truth … (1884, pp. 1-3)

However tempting it may appear to join with Masson in according praise, his view, even as presented in the nineteenth century, required a more discerning analysis. Atomism, as propounded by Lucretius, was a worldview purporting to explain fundamentals of nature, inclusive of more ephemeral aspects such as mind and an extensive spiritual world, by *a priori* philosophical argument alone. Although seventeenth century versions[141] of atomism, as unfolded by mechanical philosophers

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138 Joseph Justus Scaliger (1540-1609). Although primarily a notable French religious leader, Latin scholar and forthright critic, he included, amongst others, Babylonian, Persian, and ancient Egyptian texts along with those of Greece and ancient Rome, producing an expanded corpus of classical history. He was also noted for creating and systemtizing rules for textual criticism and correction.

139 "Scaliger's version of *minima* contains a relevant innovation, which is the importance he gave to the motion, size and arrangement of *minima*. He believed that *minima* of different substances, including the four elements, differ in size. The *minima* of earth are the biggest, followed by those of water, air, and fire." (Clericuzio 2000, p. 12)

140 In Leonard's translation, Luctretius defined his atoms as: "This ultimate stock we have devised to name Procreant atoms [*genitalia corpora*], matter, seeds of things [*semina rerum*], Or primal bodies [*corpora prima*], as primal to the world." (Titus Lucretius Carus, 1.58 - 1.61) In Latham's translation, they are given "such names as the 'raw material,' or 'generative bodies' or 'seeds' of things. Or I may call them 'primary particles,' because they come first and everything else is composed of them." (Lucretius, p. 7)

Clericuzio suggests that the rise of seventeenth century atomism ensues from "two distinct doctrines which contributed to the emergence of corpuscular theories of matter ... namely *minima naturalia* and *semina rerum* ...[however the] Aristotelian notion of *semina rerum* had a complex [philosophical] history [and] gradually received different interpretations ... in medicine, notably in the works of Fracastoro which dealt with contagion, *semina* received a clear corpuscular interpretation – which in fact originated from Lucretius." (Clericuzio 2000, pp. 9-10)

141 According to Chalmers, there were "plenty of seventeenth-century versions of atomism that were not mechanical," wherein properties were attributed to 'natural minima' such as deriving from Aristotelian

such as Pierre Gassendi (1592-1655) and Robert Boyle (1627-1691), shared a root philosophical basis for justification, the domain of applicability was deemed specifically that of physical phenomena, and is thus defined by Chalmers (2005, §1) more succinctly as "mechanical atomism." Beyond this, such a philosophically derived atomism needs to be differentiated from the experimentally based "scientific atomism" which subsequently emerged as modeling for such as Dalton's chemistry and the kinetic explanation of gaseous behaviour in the nineteenth century, where justification was sought primarily through empiricism.[142]

Where the notion of atomism benefits from examination, so does the notion of the 'mechanical philosophy' it underwrote. It is customary to accord both René Descartes and Isaac Newton some joint responsibility for framing the seventeenth century Mechanical Philosophy. Both Descartes and Newton were in principal agreement that the material universe could be reduced to fundamental parts whose motion and general interactions, exhibiting strict determinism, were governed by a few universal laws. Both were also careful to frame their philosophies within an orthodox theological boundary as necessary appeasement to the prevailing ecclesiastical establishment. However, opinions differ as to the respective roles played,[143] and on analysis, the seeming congruity of paradigmatic content is rendered less substantial. Descartes is cast as a philosophical opponent both of the 'void,' valued as integral to an orthodox atomism (Chalmers 2005, §2.1) and of a stringent materialism. In contrast to stark and uncompromising proclamations of a universally applicable materialism such as in Thomas Hobbes' *Leviathan* (1651),[144] the Cartesian dualist stance resolutely denied that phenomena such as the human mind could find satisfactory explanation in a reductionist treatment of corporeal matter. Further, whereas in the Cartesian mechanical model, particle interactions were only mediated by physical contact,[145] the Newtonian approach was suggestive of an exotic yet mechanically abhorrent 'action-at-a-distance' and added

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assertions, "with those minima possessing the capability of combining with the minima of other chemicals to form compounds." (2005, intro)

[142] However convincing the empirically based atomic models of nineteenth century physics and chemistry may have appeared, scientific atomism was still rejected by some leading scientists and philosophers, including Ernst Mach, up to the early twentieth century. (Chalmers 2005, intro)

[143] According to Brown (1986, p. 13) "it was really Descartes, not Newton, who laid the foundations of what came to be known as the 'Mechanical Philosophy.'"

[144] In initial remarks in *The Leviathan*, Hobbes (1588-1679) asserted: "The universe is corporeal; all that is real is material, and what is not material is not real." (Uzgalis 1996) Uzgalis further characterizes Hobbes' philosophy as "perhaps the most complete materialist philosophy of the 17th. century." Hobbes' materialism rejected Cartesian duality and notions of free will in favour of determinism. He further rejected Aristotelian and scholastic philosophy and enthusiastically embraced the mechanical philosophy of Galileo and Gassendi, after having met them during travel to Europe between 1634 and 1637. (O'Connor & Robertson 2002d)

[145] The Cartesian version of the mechanical philosophy was dubbed the "true and sane Philosophy" by Huygens who expected to explain motion by motion alone, without resort to forces of attraction. (Sabra 1967, p. 166)

the new concept of 'force'[146] to those of the then conventional Cartesian space, matter and motion. (Brown 1986, p. 52)

Newton, optical emission modeling and the mechanical philosophy

According to Herzberger, the development of science in the seventeenth century was greater than in any century either before or since. (1966, p. 1388) In addition to Galileo Galilei, (1564-1642), Johannes Kepler, (1571-1630), René Descartes, (1569-1650), Pierre Gassendi (1592-1655), Robert Boyle (1627-1691), Thomas Hobbes, (1588-1679), Sir Francis Bacon, (1561-1626), and Sir Isaac Newton, (1642-1727), luminaries including such as Willebrord Snel, (1591-1626), Robert Hooke, (1635-1703), Marin Mersenne, (1588-1648), Ignace Pardies, S.J., (1636-1673), Francesco Grimaldi, S.J. (1613-1663), Ole Rømer, (1644-1710), Blaise Pascal, (1623-1662), Pierre de Fermat, (1601-1665), Gottfried Leibniz, (1646-1716) and Christiaan Huygens, (1629-1695), also made enduring contributions in many scientific fields including optics. Of this august assembly, Whitehead selected four - Galileo, Descartes, Newton and Huygens - whose combined work might rank as "the greatest single intellectual success which mankind has achieved." (1938b, pp. 45, 61) An examination of optical modeling in the seventeenth century, however, importantly reveals a similar separability into rival "emission" and "continuum" approaches substantially as seen in all prior epochs. In this, just two names suffice to adequately portray the complexities of this philosophical dissention within its seventeenth century setting: Newton and Huygens. In contrast to earlier times, however, the modeling stratagems of the seventeenth century developed under the aegis of a pervasive mechanistic climate of opinion. It is in this context that Whitehead asserted:

> So long as any theory of space, or of time, can give meaning … to the idea of a definite region of space, and of a definite duration of time, the idea of simple location has a perfectly definite meaning. This idea is the very foundation of the seventeenth-century scheme of nature. Apart from it, the scheme is incapable of expression. (Whitehead 1938b, p. 74)

Historically, Gassendi[147] in the 1660's revived a model of optical transmission wherein light was again composed of a stream of tiny 'particles,' portrayed principally

[146] Newton expressed abhorrence of 'action-at-a-distance' writing: "That gravity should be innate, inherent, and essential to matter so that one body may act upon another at a distance through a vacuum ... is to me so great an absurdity that I believe that no man who has in philosophical matters a competent faculty of thinking can ever fall into it." (Boorse & Motz 1966b, p. 319) Further, "The introduction of forces as irreducible entities flew in the face of the major aim of the mechanical philosophers for clarity and intelligibility on ontological matters." (Chalmers 2005, §3.1)

[147] Gassendi's significance in the development of early modern scientific thought has been 'rediscovered.' Amongst other insights, Gassendi extended the Galilean principle of inertia to that of a generalized

in the Greek atomist mould. His intromission model of optics followed that of Epicurus and Lucretius and suggested that vision follows from apprehension of rays of image-bearing atoms. (Fisher 2005, §10) It is also relevant to recognize that Gassendi's atomist picture derived its warrant from a theological tenet which posited the existence of only one kind of divinely endorsed particulate matter that exhibited "an inherent tendency to motion." (Heisenberg 1958, p. 126) However, the most influential and enduring emission-styled model to emerge from the seventeenth century melting-pot was the corpuscular version held by Isaac Newton – an approach that embodied both a theologically endorsed atomism[148] and deterministic mechanism. The intellectual stature of Newton, often portrayed as genius, is established on the basis of his many and varied exploits in the sciences. Conspicuous are his outstanding contributions to mathematics, as in his authorship of the *Principia* and his invention of differential and integral calculus contemporaneously with Leibnitz. Equal is his contributions to physics, as in his formulation of the Laws of Motion,[149] and the then novel concept of universal Gravitation. Nonetheless some believe he would have achieved a worthy place in the history of science by virtue of his contributions to optics alone.

Concluding his draft of an optical *'Discourse of Observations'* in 1672, Newton included an "hypothesis" deriving from his detailed optical experiments. In his revised *'Discourse of Observations'* of 1675, this note is included as a separate paper expanded and consolidated as: *"An Hypothesis explaining the Properties of Light discoursed of in my several Papers."* (Newton 1757) It is this hypothesis, almost universally mislabelled as his 'corpuscular *theory* of light,' that is his *speculation*, or "conjecture at the causes of these Properties." In turn, this hypothesis is supported by an atomist and emission-styled *model* of optical phenomena in accord with fundamentals of the prevailing mechanistic mind-set: "Light is corporeall & consists of multitudes of very tiny bodies emitted every way from shining substances." Such corpuscles he further supposed to

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principle and in proposing that "any body set in motion in any direction continues, unless impeded, in rectilinear path," anticipated Newtonian principle. (Fisher 2005, §10)

[148] In the Queries concluding his *Opticks* (Query 31 of Book III), Newton surmises "that God in the beginning form'd Matter in solid, massy, hard, impenetrable, movable Particles ... as most conduced to the End for which he form'd them; and that these primitive Particles being Solids, are incomparably harder than any porous Bodies compounded of them; even so very hard, as never to wear or break in pieces" (Newton 1730, p. 102)

[149] Historically, although Galileo's work established foundational inertial principles, elements of Newtons' Laws were 'in the air' in the seventeenth century prior to their Newtonian collective expression, wherein both Gassendi and Descartes enunciated similar propositions. In contradiction to the Aristotelian doctrine on a motion towards its 'natural place,' Descartes' first "Law of Nature" asserted that "every part of matter in particular continues always to be in the same state as long as it is not forced to change it by coming into contact with other parts." His second and third laws asserted exchange of momentum during collision and the tendency, if unimpeded, to rectilinear motion. (Sabra 1967, pp. 51-53)

exhibit both differing velocities and sizes[150] (Westfall 1963, pp. 89-90) However, elsewhere in the same document, in further elaborating on his hypothesis with its predisposing atomist modeling, Newton also speculates that "whatever light be, I suppose, it consists of rays differing from one another in contingent circumstances, as bigness, form, or vigour;" (Newton 1757, p. 255) In addition, the notion of a light-bearing medium, or aether, permeating the traditional atomist void was presumed an essential adjunct to optical mechanism:

> In passing from one medium to another [light-particles] are deflected, that is to say refracted. By different media Newton meant regions in which the ether differs in density; variations in density are due to the presence of bodies, in the pores of which the ether is more rare. (Newton 1757, p. 252; Westfall 1963, p. 90)

Thus, the experimentally observed refracted direction of solid bodies away from the perpendicular when entering a denser medium, as opposed to the contrary direction in the case of light, found a conjectured explanation in Newton's version of mechanical and particle modeling,[151] Measurement of the relative velocity of light in media of different density, which would eventually void his surmise, was not yet technically possible. Additionally, Sabra notes a pertinent sequel to the now notorious Aristotelian "ball analogy" in that:

> Simultaneously, the [ball] model which Descartes had incongruously applied to his continuous picture now coincided with what it was taken to represent in Newton's theory; the [Cartesian] sphere shrank into the [Newtonian] light corpuscle. (Sabra 1967, p. 13)

On the other hand, an early elaboration of his corpuscular model included a (possibly *ad hoc*) periodic property of ethereal particles[152] that seemed a necessary

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150 The particle size forming violet rays, being more refrangible, were thought to be smaller than those of red rays. (Sakkopoulos 1987, p. 126) (Sabra 1967, p. 294) It is of note, however, that in his earliest trials recorded in his note-book prior to the end of 1665, the speed of light rays was one attempted criterion for refraction: "1. Note y^{t} slowly moved rays are refracted more than swift ones. 2^{ndly} ... from y^{e} slow ones blew, sky colour, & purple from y^{e} swift ones red yellow ..." and he "guessed that the colour of the refracted ray was dependent on its velocity." (Hall 1948, pp. 247, 249) However, further notes attempted the notion of equal velocity but with particles of unequal size. (Westfall 1962, p. 349)

151 It is noteworthy however, that Newton "grounded his dioptrics on the Cartesian law, with sines *inversely* proportional to the velocities," disregarding work by such as Fermat, [and Maignan and his Lucasian predecessor Isaac Barrow], who proposed their laws of refraction based on direct proportionality. (Lohne 1968, p. 176) Sabra elaborates that Newton "incorporated Descartes' ... assumptions together with the conclusion, which was to be generally accepted until the middle of the nineteenth century, that the velocity of light was greater in denser media." (Sabra 1967, p. 13)

152 In a potentially clarifying discussion, Sabra quotes from Newton's paper (Newton 1757, p. 251) delivered to the Royal Society, December 9, 1675: "... it is to be supposed, that the aether is a vibrating medium like air ...so, I suppose, the aethereal vibrations differ in bigness, but not in swiftness." Sabra

complication to account for such phenomena as "Newton's rings." Discovered previously and independently by both Boyle and Hooke, some recurrent mechanism appeared essential to account for the phenomenon of colours exhibited by thin films. Later, in his *Opticks* of 1704, Newton advanced an alternative notion of "fits of easy transmission and of easy reflection" (based on an analogy to regular returning bouts seen in some diseases), to account for the phenomenon.[153] (Shapiro 1989, pp. 239-240) Nonetheless, it is his extensive experimentation with the spectral display of colours mediated by prisms, that has encouraged the enduring popular legacy of Newton's investigation into properties of light. Theoretical conclusions based on Newton's atomist-emissionist modelings of light and colour, asserted by Newton as indisputable fact from a narrowly interpreted induction,[154] were to become the 'received view' until the nineteenth century. However, considerable academic disputation has more recently arisen when evaluating both his inductive experimentally-based philosophy and its achievement of a long-term supremacy over competing continuum or wave-like optical models. (Laymon 1978, p. 51) Latitude now exists in interpretation of both Newton's experimental intent and final scientific achievement, engendered at least partly by inconsistencies in both Newton's claimed experimental setup and the conclusions to be drawn.

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concludes that Newton "was loosely using the word 'vibrations' to mean waves, and by 'bigness' ... he meant wave-lengths." (1963, p. 268)

[153] As Westfall observes, in Newton's earlier accounts, such as sent to the Royal Society in 1675, "we do not find any mention of fits ... At that time, Newton believed in an aether capable of vibrating periodically ... When he came to publish the *Opticks* in 1704, however, Newton had ceased to believe in ... an aether [as shown by *Opticks Query 28* and thus] found himself with the impossible task of reconciling the periodicity he had demonstrated to exist ... with his corpuscular conception of light." Westfall then explains that "by fits of easy transmission he did not mean periodic changes taking place in the [light] corpuscles ... he was merely stating the demonstrated fact that at certain ... thicknesses of thin films light is transmitted and at the intermediate thicknesses it is reflected ... [i.e.] the ... periodicity belonged, not to light, but only to the phenomena associated with thin films," (Westfall 1967, pp. 155-156) a view with which Shapiro concurs. (Shapiro 1989, p. 236) In disconcerting contrast, Mach contends that Newton "attributed to the light periodic 'fits' of reflection and transmission," and further that "Newton assumed that the periodic 'fits' were contained in the light as soon as it left the luminous body producing it ..." concluding that Newton gave "*a clear demonstration ... of the periodic nature of light* ..." (Mach 1926, pp. 140-141, orig. emph.) However, confirming that the periodic property was that of the medium and not of light itself, Shapiro claims that Newton "was careful not to attribute [periodicity] to light, but rather to treat it descriptively as an observed property of thin film. (Shapiro 1989, p. 236) In agreement Sakkopoulos says: "The basic assumption in the theory of fits is that waves accompany the rays of light ... these waves are excited by the impact of the light particles on the surfaces of ... transparent bodies," (p. 124) and concludes that "the modern concept of particle-wave dualism is essentially different from a simple coexistence and cooperation between these two entities." (p. 126)

[154] "what I shall tell … is not an Hypothesis but most rigid consequence … evinced by y[e] mediation of experiments concluding directly & w[th]out any suspicion of doubt." (Newton cited in Shapiro 1984, p. 34)

Newton's particle modeling and his "*experimentum crucis*"

Central to early pronouncements on the production of the spectrum of colours emerging from prismatic refraction, is what Newton claimed was his "*experimentum crucis*."[155] As the second of three selected experiments detailed in his early 1672 paper on optics, it followed an initial experiment (Newton 1672a, p. 3076) wherein the elongation of the spectrum[156] of colours claimed successful challenge to the 'received laws of refraction.' As published by Descartes in 1637, light had been characterized as conforming to a single refractive index quantitatively identifiable for each species of medium.[157] However, the second experiment, developed both from previous experimentation by others,[158] and experimental notes from Newton's earlier Lucasian "*Lectiones Opticae*," utilized two prisms such that two boards each pierced by small holes supposedly allowed a selected 'single colour' emerging from the first prism to be refracted by the second. (Newton 1672a, p. 3078) Newton summarized his observations by claiming: "*Light consists of Rays differently refrangible*." (Laymon 1978, pp. 52-53) However, academic opinion is now divided as to the merit of this and additional claims. Newton appears to have posited differing intent and explanation in his alternative accounts and in various defences against criticism.[159] For Sabra, a primary Newtonian experimental intent is interpreted as establishing the immutability of colour – an attempt which he evaluates as having failed:

> Newton argued that if colours were modifications of light [as was then widely held] due to the action of the reflecting or refracting bodies, then those bodies should produce the same effects whether the light falling on them has already been disturbed or not. But this consequence was proved false by the *experimentum crucis*: the rays emerging from the first prism preserved their respective colours and degrees of refrangibility upon their refraction

[155] Although the designation has become somewhat synonymous with Newton's usage, it was coined by Robert Hooke drawing on Francis Bacon's references to *instantia crucis*, in his *Novum Organum* and was first employed in his optical work *Micrographia* from whence Newton borrowed it. (Westfall 1962, p. 354) In Newton's usage it functions both to refute false formulations and to positively validate correct ones. (Sabra 1967, p. 249)

[156] The technical term 'spectrum' was coined here by Newton. (Hunt 2000, p. 186; Lohne 1968, p. 181)

[157] The phenomenon of colours were well known and 'celebrated' before Newton's time. Westfall suggests that in historical and philosophical context, "Newton's experiment differed, deliberately, in crucial factors from any that the tradition ... tracing its descent back some two thousand years, had delivered to him. The planning of the experiment, not its observed results, constituted the revolution in the theory of colours." (Westfall 1962, p. 339)

[158] Newton apparently can not claim historical priority for this discovery or even for the prism arrangement of his *experimentum crucis*. He was anticipated by, among others, Marcus Marci (1595-1667), a university professor of Prague, and mathematician Thomas Harriot (1560-1621) of England. (Ben-Chaim 2001, p. 407; Hunt 2000, p. 189; Lohne 1968, pp. 174-176)

[159] In fairness it must be conceded that, despite his seeming intractability, Newton's thinking also evolved both by his own continuing experimentation and also in some, albeit small, positive response to criticisms during the period extending from 1672 to 1704. (Shapiro 1980, p. 211, 1989, p. 224)

through the second prism. Therefore, Newton concluded, colours must be unalterable properties of the rays and, consequently, white light must be an aggregate of rays that already possess those properties. This conclusion, however, does not follow from the experiment. (Sabra 1967, p. 295)[160]

On the other hand, Shapiro sees the *experimentum crucis* as intended by Newton to constitute a rebuttal of continuum-based criticisms, wherein it would distinguish between competing models of a "ray" – the one a discrete and separable physical ray as consistent with his particle-emission model and the other inseparable but diffused as Newton imagined wave-like models to require. Newton's chosen model of light openly rejected the Arab legacy of the light-ray depicted as a useful abstraction. To accord with his predisposing corpuscular model, Newton embraced a ray as physical artefact. Much of Newton's description recalls properties familiar from Euclid's earliest geometry. A Whiteheadian assessment might again detect the display of a bias for objectivity over abstraction, and simple location over process, in the reified path of light atoms acting like finite projectiles with each exhibiting properties of separability and locality:

...by the rays of light I understand its least or indefinitely small parts which are independent of one another; such as are all those rays which luminous bodies emit either simultaneously or successively in straight lines ... [which] parts may be intercepted without the others and be separately reflected of refracted into any areas (Newton cited in Shapiro 1975, p. 200)[161]

For Buchwald, (1989, p. 7) this definition "amounts to defining a ray as an atom of light" and notes that by the 1790s French physicists "rarely distinguish between a 'ray' and a 'molecule of light'" since "Newton's definition of a ray was treated as essentially synonymous with an emission theory of light." Further, in both an attempt at clarification of his "simultaneous" ray proposition, and, as Shapiro believes, an attempt to neutralize aspects of wave-model criticism by accommodation, Newton elsewhere expanded his notion of rays:

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[160] As Sabra expands it: "The effects of this 'demonstration' is ... almost hypnotic. Nevertheless, it is certainly inconclusive. Why should the second prism have the same effect on a *coloured* beam as the first had on a *white* beam? ... Some of Newton's contemporaries saw the inconclusiveness of his argument; it is therefore curious that most historians of science have fallen under its spell." (Sabra 1967, pp. 249-250, orig. emph.)

[161] Shapiro nonetheless requires that "Newton did not intend this definition to be taken as an ontological definition, which asserts the existence of light corpuscles, but rather as a phenomenological definition, which asserts the [claimed] property of light that every 'part' of a beam is ... refracted independently of any other 'part'." On this view it is the "physical independence of the parts, not their size" that here concerns Newton. (1975, pp. 200-201)

> If, however, someone has difficulty imagining many successive rays in the same [geometric] line, let him imagine ... a small space in a physical sense equivalent to the line [*i.e.* a beam] in which very many parallel but indefinitely close rays flow, so that they are taken as sensibly coincident ... these rays must not be conceived as similar, but as a mixture of red-, yellow-, green-, blue-, and purple exhibiting rays with all their intermediate degrees. (Shapiro 1975, p. 207)[162]

Sabra further suggests that by employing this image of 'rays', Newton was attempting to secure a general acceptance and to avoid disagreements. On this view, Newton strove to render his atomism less conspicuous and less overtly aligned to either a corpuscular or continuum explanation (Sabra 1967, p. 274) However in Sabra's view, Newton failed in that he was not fully aware of the strength of his personal commitment to optical atomism as the underlying model. (1967, pp. 15, 284) In respect of this, Newton's rays were consistently those reflecting a corpuscular model and were specifically denied any meaning they would need to have in a wave or continuum model. Hence Sabra believes that it "would turn us into victims of the illusion that by formulating his theory in terms of *rays* instead of *corpuscles*, Newton successfully made it neutral to both corpuscular and wave interpretations." (Sabra 1967, pp. 187-188, orig. emph.) Beyond this, as Shapiro further interprets the case, Newton hoped that the concept of a pencil or beam of separable rays would prove decisive in any contest between his emission model and competing continuum models, particularly since Newton believed the latter necessitated some principle of chromatic diffusion in explanation of the same experimental results:

> ... if the first Prism had spread & dissipated every ray into an indefinite number of diverging parts, the second should in like manner have spread & dissipated every one of those parts into a further indefinite number, whereby the Image would have been still more dilated; contrary to the event. And this ought to have happened because those linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly & completely rays as ye whole was before its incidence; as may appear by intercepting them severally. (Newton 1672b, p. 178)[163]

Academic appraisal of the value of Newton's analysis varies. As interpreted by Sabra, a refutation of continuum modeling failed due to Newton's inadequate grasp of what such models necessarily entailed. Alternatively, Shapiro evaluates Newton's argument as successful insofar as it refuted a specific notion of chromatic diffusion. (Laymon 1978,

[162] Shapiro cites MS Add. 4002, Lect. 7, Sec. 78, p. 57
[163] The argument forms part of Newton's complaint to Oldenburg following Hooke's initial critique.

p. 51) However, for Lohne, when attempting to discover the Newtonian intentions attending the *experimentum crucis*, "there is no short and final answer to [the] question." Even the question of *how* Newton performed the experiment is open to discussion in that "the majority of past and present second-hand accounts betray a bewildering confusion about Newton's arrangement of prisms and diaphragms ..." This difficulty is then further exacerbated by variations, modifications and reinterpretations existing in Newton's own accounts. (1968, p. 170) In summary he concludes that "Newton's statements about his intentions with this experiment are inconsistent." (1968, p. 182)[164] Nevertheless, where his experimental intentions may appear clouded, at least one result seems to have firmed in Newton's own appraisal of the evidence:

> ... his realization of the inherent difficulties which the wave theory had to face and which at that time had not been solved ...[and] which he believed to be insurmountable, [and through which] the wave theory could not explain the rectilinear propagation of light; that is to say, it seemed to him to be falsified by experiment. (Sabra 1967, p. 277)

Specific to these perceived impediments was a claimed failure to experimentally observe optical diffraction despite Grimaldi's contrary evidence. As Sabra explains:

> In [Newton's] view, both *'Experiment & Demonstration'* (Newton 1672c, p. 175) proved that pressures, waves or vibrations in any fluid bend round obstacles into the geometrical shadow. Consequently, [wave models were] proved false by the rectilinear propagation of light, that is, by the formation of shadows in the ordinary way. (Sabra 1967, p. 283)

In his own analysis of Newton's rejection of a potential wave-continuum solution to enigmas inherent in the phenomena of light, Rosenfeld identifies three specific issues which Newton raised as obstructions, additional to his belief that waves necessitated chromatic diffusion.[165] Firstly, experiments by others demonstrating polarization were taken as fatal to an undulatory explanation. Indeed, within the confines of a *longitudinal*

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[164] Westfall notes that the *experimentum crucis* enjoyed centre stage in Newton's 1672 optical treatment, but has neither that name nor such a position in the *Opticks*. (1963, p. 94) In his summary, Laymon suggests that "Newton intended the *experimentum crucis* show by itself: (1) that [sun]light consists of rays differently refrangible. (2) that each light ray has an immutable degree of refrangibility." However, only in conjunction with other experiments was it to "support the claim (3) that each light ray has an immutable color associated with it." A further claim that the experiment shows, "(4) that there exists a one-to-one correspondence between color and refrangibility" is found in the 1672 paper and in correspondence with Pardies but then denied in correspondence with Lucas. (Laymon 1978, p. 59) Lohne confirms this: "During the first years of controversy Newton emphasized that he had invented the experiment to show that different colours have different laws of refraction. Then he changed his mind and to [the Belgian] Lucas denied that the *Experimentum Crucis* had anything to do with colours." (Lohne 1968, p. 185)

[165] Newton further developed this as a primary objection in both his *Principia* and later *Opticks*. (Rosenfeld 1928, p. 10)

wave modeling alone, his objection appears sound, and not seriously challenged until the concept of transverse wave motion was eventually, and successfully, postulated by Fresnel. (Rosenfeld 1928, p. 10) Secondly but almost inexplicably, Newton not only discounted the experimental results published by Grimaldi, which unequivocally described the discovery of the diffraction of light into its geometrical shadow, but following attempts at replication (Newton 1846, p. 246) on the contrary claimed that:

> In truth, rays which pass very close to the edges of an arbitrary body are bent a little by the action of the body ... but this deviation is not made towards the shadow, it is made only in the opposite direction. (Newton, cited in Rosenfeld 1928, p. 10)

In seeking to exonerate Newton and excuse his bewildering faux-pas by what seems an equally unsatisfactory absolution, Rosenfeld submits that "the hesitations and the mistakes of the great physicist" are to be easily understood in the case of diffraction since "it is a domain he left unexplored, [and] after a fragmented survey he gives the feeling that he is not the master of it." (Rosenfeld 1928, p. 10) Thirdly, in a discussion raised by Newton in the more openly speculative arena of his rhetorical "Queries" which conclude the *Opticks*, a further supposed impediment to a wave solution of the "rings" phenomenon is given by what Rosenfeld judges as "one which might well astonish the modern reader." In Query 28 Newton speculates that:

> [the phenomenon of rings] is no less difficult to explain, by means of [wave] hypotheses, how the rays can be alternately easily accessible to reflection and transmission, if it is possibly only that one wishes to imagine that space is filled with two ethers, each with its own particular vibrations, one supporting light and the other, more with rapid vibrations, interacting with the first to allow this accessibility. But would their reciprocal movements not become muddled? (Newton, Query 28, cited in Rosenfeld 1928, p. 10)

In pursuing his own analysis, Laymon characterizes Newton's discussion of his crucial experiment as entailing "an idealized sketch" or model wherein Newton's claim of a truly monochromatic beam of identical rays is a potent example. Amongst numerous criticisms both of Newton's published results and of his inflexible explanations, the continental scientist, Lucas, asserted that his own repetition of the experiment exhibited both a discernable divergence and colour change following the second prism, an observation clearly at variance with Newtonian claims. In response, Newton conceded Lucas' description as the more accurate, but additionally predicted that if a 'smaller' aperture size was chosen, then the apparatus would demonstrate color separation. (1980, p. 339) For Laymon, Newton's response opens a wider philosophical discussion that would question the validity "of explanations which lack true or even usefully approximate predictions," and that would also focus on a more scientifically responsible account of what a claim of 'approximately true' might usefully intend when conscripted to the cause of theory confirmation. Of import to a discussion of the role of models in theory development, is Laymon's assessment of Newton's experimental significance. In summary, Laymon asserts that "the *experimentum crucis* ... does not refute the alternative wave theories. The intended refutation is successful only if Newton's idealized descriptions[166] are used. If ... the more accurate descriptions that were historically available are used, then the experiment does not refute." (Laymon 1978, p. 51) On such an assessment, the success or failure of Newton's developed optical theory rested with the usefulness and appropriateness of the underlying model.

[166] In Laymon's assessment there "were essentially two idealizations used. The light refracted by the second prism was described as being of a single color. It was also strongly implied that this light was not diffused or dilated." (Laymon 1978, p. 53) According to Lohne, Grimaldi already in 1663, had "demonstrated that it was impossible to obtain a very slender sunbeam by sending it through two successive pinholes, for the beam dispersed after the second hole and colours appeared in the confines between light and shadow." (Lohne 1968, p. 196)

Chapter 5
Optical continuum modeling and the mechanical philosophy

The seventeenth century rise of the continuum modeling of light to meet its destiny and fully embrace the concepts of both waves and wavefronts is inspirational as an insight into science in action. As Shapiro observes: "The concept of ray as a construct – the normal to a really existing wave-front – was a difficult one to grasp and was not yet understood in the beginning of the seventeenth century," and was only "gradually formulated through the course of the … century." (Shapiro 1973, p. 137) Nevertheless, by the beginning of the eighteenth century, Antoine Parent in 1713, felt justified in proclaiming that:

> ...in general, the straight, reflected, or refracted rays are nothing other than all the possible perpendiculars in all the points of the waves. These rays are nothing other than the lines along which it is imagined that the light acts ... and consequently exist only in thought; a wave[front], on the contrary, is a luminous surface which really exists ... And this is what is of the most importance in the propagation of light. (cited in Shapiro 1973, p. 138)

Nonetheless, early in the seventeenth century, proponents of the quest to further develop continuum models of light encountered serious difficulties in formulating physical models consistent with the prevailing mechanical paradigm. They could not simply appeal to the analogy of water waves, as they had done for sound, "for one could see the violation of rectilinear propagation in water waves and hear it in sound waves. How could one understand rectilinear propagation?" (Shapiro 1973, p. 137)Their endeavours struggled to explain a process of motion in what was then conceived as supported by a pervasive and mechanically compliant aethereal medium.

In the first attempt at a fully mechanical proposition in the modern scientific era,[167] Descartes canvassed a continuum model of light propagation that envisaged a plenum of hard and incompressible yet aethereal particles through which an optical "pressing" or "endeavour" was transmitted instantaneously without requiring motion. However, his explanations for reflection and refraction effectively drew on the misapplication of the Aristotelian "ball" analogy,[168] and effectively disqualified himself, along with those

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[167] As Sabra points out, "mechanical analogies had been used to explain optical phenomena long before Descartes, but the Cartesian continuum theory was the first clearly to assert that light itself was nothing but a mechanical property of the luminous object and of the transmitting medium... [thus] we may regard Descartes' theory of light as the legitimate starting point of modern physical optics." (Sabra 1967, p. 48)
[168] "...he availed himself of a model consisting of a moving sphere which could be imagined to change direction and speed on meeting a refracting surface. The model may be called a 'substitute model' since it did not represent what, in Descartes' view, actually took place in reality, but was employed rather as a

who advocated a fully Cartesian system, from contributing to the evolution of a successful wave and wavefront solution.[169] On the contrary, Thomas Hobbes, for whom optics played a pivotal role, fully appreciated the import of the behaviour of light which bends towards the perpendicular when entering water from air, whereas a solid body does the opposite. His explanation required that "the motions of a body and of a pulse are two fundamentally different physical processes and that, in turn, they require fundamentally different physical explanations." (Shapiro 1973, p. 156) This distinction became a basis for a protracted correspondence between Hobbes and Descartes, mediated entirely by their mutual friend Marin Mersenne, in which Descartes either could not, or would not, concede the difficulties endemic in his own scheme. (Shapiro 1973, pp. 156-160)

The value of Hobbes' model of light propagation can be attributed primarily to his definition of a ray. The devastating attack on reified Euclidean rays mounted by Muslim science had borne some fruit with a general medieval acceptance that modeling of optical rays must at least concede some finite width. Hobbes built on this view to define his ray as a solid space in the optical medium. Hobbes ray was not a solid itself, but a geometrically defined pathway. The motion of a pulse, originating from the minute expansion and contraction[170] of a luminous body, was then communicated along the ray by direct contact between adjacent aethereal particles. Hobbes model was in complete accord with the mechanical philosophy wherein all action was local motion and fully amenable to mathematical treatment particularly by his beloved geometry.[171] It is, however, his geometrical treatment of the pulse itself that uniquely identifies his optical contribution. The finite width of the pulse front was conceived as always travelling as an orthogonal to the ray path. Ultimately, its interaction at an optical interface was geometrically interpreted to derive a correct explanation for both diffraction and the sine law. In this - Hobbes pulse front, or as he described it, the "propagated line of light" - can be seen in embryo a finite section of an expanding luminous wavefront. (Shapiro 1973, p. 151) Hobbes also realized the significance of his pulse-front treatment and its distinction from medieval modeling to the extent that he "abandoned the term 'ray' (*radius*) and introduced the new term 'radiation' (*radiatio*)." (Shapiro 1973, p. 151)

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substitute to which a handy mathematical device – the parallelogram of velocities – could be directly applied." (Sabra 1967, pp. 12-13)

[169] Nonetheless, Descartes incorporated the refraction law "into a physico-mathematical [model] which, despite, or rather because of, its many defects, constituted the starting point for ... Fermat, Hooke, Huygens and even Newton. Thus Descartes gave optical research a new impetus, but also a new set of problems, and hence a new direction." (Sabra 1967, p. 12)

[170] Hobbes apparently uses an analogy with the heart beat by his choice of "*systolem et diastolem*" (Shapiro 1973, p. 147)

[171] According to John Aubrey, Hobbes was "in love with geometry" to the extent that he "would often complain that algebra ... was too much admired ... that it made men not contemplate ... so much the nature and power of lines ..." (Aubrey 2003)

Hobbes' sole optical publication, *Tractatus opticus*, was published by Marin Mersenne in 1644 but was often attributed to others.[172] It is also ironical that the principle presentation of Hobbes' work by Emanuel Maignan and Isaac Barrow was subsequently distorted to accord with their own adoption of emission models. (Shapiro 1973, p. 172)

One further difficulty remained unaddressed in Hobbes' model, but one common also to most of his contemporaries whether of an emission or continuum philosophical disposition. No mechanical interpretation of physical phenomena can adequately accommodate the concept of infinity when assigned to its 'real' properties. Nevertheless, most optical modeling up to and including Hobbes' scheme, however construed, tacitly invoked an infinite velocity for light propagation.[173] As Shapiro notes:

> Although the velocity of light was infinite for [Descartes' and Hobbes' modeling of light] they were, in effect, forced to consider the velocity of light finite for their mechanical derivations. This of course led them to write nonsense. While specifically avoiding the terms speed and velocity, they considered one infinite velocity to be faster or slower, or stronger or weaker, than another. (Shapiro 1973, p. 174)

It was to be Emanuel Maignan who acknowledged this inherent absurdity, and in advocating reform "places great emphasis on the requirement that the velocity of light be finite." (Shapiro 1973, p. 174) Empirical support for his strenuously advocated position, nonetheless, had to wait for the results of Ole Römer's later work. Maignan abandoned Hobbes' continuum framework, such that a light-pulse was, unsuccessfully, replaced by a ray-front modeled as a rigid body. Nevertheless, the notion gave rise to a familiar analogy:

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[172] Hobbes' model was well known and read in the seventeenth century, being known to Descartes, Hooke, Huygens, Newton, Grimaldi and Pardies amongst others. Yet contemporary historians of science seem generally unaware of it or its place in scientific modeling development or else attribute it to others. (Shapiro 1973, p. 143) The subsequent obscurity of Hobbes' authorship has various overtones which amongst others include the ominous influences and potential interference of political, social and religious convictions on the objective pursuit of science. His publication of *De Cive* (Concerning Citizenship) in 1642, advanced a very authoritarian version of the social contract. (Uzgalis 1996) His *Leviathan* in 1651, associated his name with atheism and materialism and his writings engendered ridicule and reprisal from the public, religious, and academic establishments, his books being eventually banned by university decree in 1683 and publicly burned. He was ostracized by the Royal Society, (Skinner 1969, p. 219) and hysteria gave rise to his writings being cited in Parliament as causal of the 1665 plague and Great Fire of 1666. (Shapiro 1973, p. 181) Discounting this appraisal of Hobbes, John Aubrey records in "*From a letter to John Aubrey from James Wheldon, 16 January 1679*" that "For his being branded with atheism, his writings and virtuous life testify against it." (Aubrey 2003)

[173] Notable exceptions were, of course, Alhazen and his medieval interpreter, Roger Bacon. (Sabra 1967, p. 47)

> Just as we see one and the same axle with two equal wheels move in a straight line when both wheels have one velocity; and on the contrary, we see that it is carried transversely and deflected from a straight path because one wheel turns more quickly than the other. (Shapiro 1973, p. 179)

As Shapiro observes: "It is perhaps one of the ironies of history that this analogy, still used today in elementary science courses to explain the refraction of a plane wave, was first introduced in an emission [model] of light." (1973, p. 179)

The rise of wave and wavefront modeling

Hobbes pulse-front model, albeit in modified versions as published in Barrow's *Lectiones opticae* (1669) and Maignan's *Perspectiva horaria*, (1648) was known to the Dutch scientist, Christiaan Huygens. However, in his own *Traité de la Lumière* which ultimately brought the wavefront model to fruition, it is two other predecessors of "those who hitherto have begun to consider the waves of light" who enjoyed Huygens' academic acknowledgement: "Mr. Hooke" and "Father Pardies." (Huygens 1690, p. 20) Although both ultimately failed to prosecute a fully successful wavefront model, each contributed elements that were to become incorporated into such a model.

Robert Hooke retained adherence to Hobbes continuum approach, emphasizing optical analogy to water waves. His enquiry, however, primarily pursued an explanation for colours, wherein his model of refraction incorporated an oblique pulse front. When abandoning the normality of ray to ray front portrayed by Hobbes, that had proved the necessary and sufficient condition for the sine law of refraction to hold, he exposed his concept to severe, even fatal criticism. As Shapiro records it:

> Just seven years after the publication of [Hooke's] *Micrographia*, [in 1665] Newton's "New theory about light and colors" appeared. In the ensuing controversy between Hooke and Newton, Newton totally devastated Hooke's theory of color. In the process Newton destroyed Hooke's theory of light too, since the two were inextricably related. Henceforth a continuum theory of light would have to be compatible with Newton's theory of color. (Shapiro 1973, p. 189)

Bitterness and rancour ensued in exchanges between Newton and Hooke that was to "burst into flame fitfully but persistently for thirty years," initially incited by Hooke's critique of Newtons's original 1672 paper on colours as sent to the Royal Society. (Hooke 1672) In this, Westfall unhappily sees more than a basic divergence of scientific principles. "Hooke's commentary ... was not so much critical as offensive." Hooke, as then curator of the Royal Society experiments, probably did not intend to be insulting, but he wrote with the pompous and patronizing "air of a master ... addressing a beginner … the paper adopted an infuriating tone of condescension." (Westfall 1963, pp. 82, 87) Newton's initial drafts of reply betray no irritation, however his tone deteriorated with time. (Bechler 1975, pp. 110-113) As Westfall also notes:

> Newton prepared to communicate to the Royal Society a paper of greater weight ... than the first ... And then he drew back. He eliminated from the paper all but the portions immediately relevant to Hooke; he infused the reply with bitterness and insult that left the unsuspecting Hooke dumbfounded and humiliated ... (Westfall 1963, p. 91)

In comment, Westfall notes that although Newton had secured the Lucasian chair in 1669, and had met with considerable academic recognition including Fellowship of the Royal Society, his character was insecure and complicated, rendering him "ill-equipped to cope with criticism ... it struck deeply into his vitals."[174] Westfall further notes that "something of this tension had come out already in the heat of his reply to Pardies." (1963, p. 91)[175]

Ignace Gaston Pardies, a Jesuit who taught both physics and mathematics at the *Collège de Clermont* at Paris, advanced a continuum model of light propagation which advanced beyond that of Hooke, with the introduction of abstract constructional rays and luminous wavefronts, and to this extent anticipated Huygens. Whereas Hooke left as a principal legacy the concept of a pulse front as a portion of a spherical luminous surface of constant phase, Pardies' optics was both kinematic and dynamic, advocating the principle that a ray acts always as a normal to the wavefront. Additionally he attempted to understand optical wave dynamics through application of the principles of harmonic motion. (Shapiro 1973, p. 209) Unfortunately, much of his work was yet unpublished at his premature death at age 37, and was subsequently passed to his lesser-

[174] Lord Keynes, in summarising "Newton, the man", for his lecture celebrating the Newton Tercentenary noted that "Newton was profoundly neurotic ... but ... a most extreme example. His deepest instincts were occult, esoteric ... with profound shrinking from the world, a paralyzing fear of exposing his thoughts ..." (Keynes 1947, p. 94) According to Boorse and Motz, "Newton ...had a pathological aversion to engagement in any kind of controversy; as time passed his involvement with Hooke filled him with such a loathing that he would neither publish his book on *Opticks* nor accept the presidency of the Royal Society while Hooke was alive." (1966e, p. 52) Newton's *Opticks* was eventually published in 1704 after Hooke's death in 1703.

[175] Pardies, in always "temperate and courteous" correspondence, taking arguments from the Jesuit Francesco Grimaldi who had originally discovered diffraction, contested aspects of Newton's "extraordinary hypothesis." (Westfall 1963, p. 86) However, "Newton did not take it well," taking umbrage at the term "hypothesis," but nevertheless was forced "to clarify his thinking." Eventually, Pardies recognized defects in his own position, wherein he had confused chromatic dispersion with diffraction, and made "a rather generous admission of error" to Newton. (MacDonnell S.J. 1997)

known confrere Pierre Ango, who, perhaps inadequately, assigned himself the role of Pardies' editor, retaining what he considered its "best parts" but adding his own material.[176] (Shapiro 1973, p. 209)

Both Hooke and Pardies remained untroubled by Newton's rejection of a continuum model for light based on his arguments from rectilinearity. For them, the prior discovery of diffraction by Grimaldi established that light indeed violates the law of rectilinearity by encroaching into the region of its geometrical shadow.[177] However, in summarizing their contributions as Huygens' predecessors, Shapiro observes: "Hooke based his optics on the wave-front as a surface of constant phase, which Pardies ignores, while Pardies' optics is based on the normality of the rays, which Hooke rejected." (1973, p. 217)

Huygens' "Principle" and the wavefront model

In 1690, the eminent Dutch scientist Christiaan Huygens, second son of the celebrated poet, intellectual, athlete, and diplomat, Constantin Huygens, finally published his *Traité de la Lumière*, (*Treatise on Light*) as a fitting sequel to an already prodigious and stellar scientific career.[178] With this publication, came the "culmination of the seventeenth-century kinematic tradition in the continuum [modeling] of light." (Shapiro 1973, p. 207) In setting forth his reasoning and the conclusions he felt most secure in deriving from it, Huygens retained optics in that domain of physical activity he believed most amenable to analysis and explanation by "the true Philosophy, in which one conceives the causes of all natural effects in terms of mechanical motions." (Huygens 1690, p. 3) Nonetheless, although deferring to the prevailing mechanical paradigm, he placed his analysis firmly within the ambit of continuum and particularly processual modeling. Drawing directly on the ancient analogies to both sound and water waves, he advanced cogent reasoning for rejecting an emission-styled model of the kind his renowned English contemporary, Newton, had espoused.

> ... when one considers the extreme speed with which light spreads on every side ... [and that] the rays traverse one another without hindrance, one may well understand that when

[176] Ango incorporated, edited and selected portions of Pardies' work in his *L'Optique, divisée en trois livres etc*., Paris, 1682 (Lohne 1968, p. 30, fn. 3)
[177] Hooke in fact verified diffraction by personal experimentation, observing a beam of light pass the edge of a razor wherein "the Light of the sun Did Deflect very deep into the [geometrical] shadow ... and if Mr. Newton try ye Experiment I am ... confident he wi[ll] ascribe that deflection ... neither to Reflection nor Refraction ..." (cited in Shapiro 1973, p. 211, fn. 237)
[178] "His reputation was so great that in 1665 Louis XIV offered him a pension if he would live in Paris, which accordingly then became his place of residence." Ball, however, assesses Huygens' *Horologium Oscillatorium* (Paris, 1673) as his most important work. (Ball 1908)

> we see a luminous object, it cannot be any transport of matter coming to us from this object, in the way in which a shot or an arrow traverses the air; for assuredly that would too greatly impugn these two properties of light, especially the second of them. It is then in some other way that light spreads; and that which can lead us to comprehend it is the knowledge which we have of the spreading of Sound in the air... If, in addition, light takes time for its passage ... it will follow that this movement, impressed on the intervening matter, is successive; and consequently it spreads, as Sound does, by spherical surfaces and waves: for I call them waves from their resemblance to those which are seen to be formed in water when a stone is thrown into it, and which present a successive spreading as circles though these arise from another cause, and are only in a flat surface. (Huygens 1690, pp. 3-4)

Unlike many of his predecessors, who had also utilized these ancient analogies only to rely on unsound analogical relationships, Huygens demonstrated adroit circumspection of the pitfalls, recognizing the limitations of individual representations:

> Now the successive movement of Light being confirmed in this way, it follows ... that it spreads by spherical waves, like the movement of Sound. But if the one resembles the other in this respect, they differ in many other things; to wit, in the first production of the movement which causes them; in the matter in which the movement spreads; and in the manner in which it is propagated. (Huygens 1690, p. 10)[179]

On the other hand, Huygens' passing reference to the "extreme speed" of light propagation, might seem to trivialize one of the most significant milestones in the historical development of optical science. In its turn, a finite propagation rate proved to be crucial to the secure development[180] of Huygens' modeling. In November of 1676, Ole Römer announced to the Royal Academy of Sciences that a quantitative evaluation of the velocity of the propagation of light had been achieved using the motion of Jupiter's moons as a kind of celestial timepiece. (Roemer 1677) In appraising the impact of this empirical quantification of a luminal propagation velocity on Huygens' optical development, Mach asserts that the "principal fruits of Huygens' labours lay in his demonstration of the possibility of deriving all the essential features of rectilinear propagation, reflection, and simple and double refraction from the *rate of propagation*

[179] Whereas Hobbes had envisaged unified motion of the entire optical source as for sound, Huygens recognized the necessity of individual point vibration of every point on the source. Secondly, he noted the fundamental differences of medium as between air and the speculated aether which supposedly penetrated all matter. Thirdly, a mechanically consistent aether was thought to consist of particles in contact whereas the particles of air were in motion and interacted through collision.

[180] As Huygens expressed it: "But that which I employed only as a hypothesis, has recently received great seemingness [*vraisemblance*] as an established truth by the ingenious proof of Mr Römer ...the successive movement of Light being confirmed in this way, it follows ... that it spreads by spherical waves ..." (Huygens 1690, p. 7)

of light." (Mach 1926, p. 255, orig. emph.) Shapiro concurs suggesting that Huygens "theory of light is founded on virtually one principle – the finite velocity of ... light. Only the fundamental kinematic concepts of space, time, and velocity enter in ..." (Shapiro 1973, p. 208) Huygens virtually admitted of such a view himself in that he "made no difficulty ... in supposing that the emanation of light is accomplished with time, seeing that in this way all its phenomena can be explained, and that in following the contrary opinion everything is incomprehensible." (Huygens 1690, p. 7)[181] Although René Descartes had been a frequent guest in the home of Huygens' early years and was greatly admired, (Boorse & Motz 1966g, p. 63) included in what Huygens termed "incomprehensible" was much of the Cartesian optics that both advanced an infinite speed of optical transmission yet a particle-styled optical refraction:

> For it has always seemed to me that even Mr. Des Cartes, whose aim has been to treat all the subjects of Physics intelligently ... has said nothing that is not full of difficulties, or even inconceivable, in dealing with Light and its properties. (Huygens 1690, p. 7)[182]

Christiaan Huygens is now appropriately honoured for advancing his 'principle' of wave-like radiation.[183] Relying on a finite propagation rate, Huygens envisaged a physically realized optical surface, spreading away from an optical source, by a process that continually reconstructed an expanding spherical luminous wavefront.

In evaluating Huygens' contribution to optics, it would seem prudent, in the face of many disparate claims and labels, to first determine what it is not. Although most literature on the subject applauds Huygens as the originator of "the wave theory of light," (Boorse & Motz 1966g) even a cursory examination of Huygens' claims and deliberations, demonstrates that what emerges as his 'principle' is a partial and preliminary exploration of a mode of propagation with light as the primary object of interest in its application.[184] Given that Huygens "left vast areas of optics untouched" (Shapiro 1973, p. 208) such as colours, polarization, periodicity and the dynamics of

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181 Huygens did not hear of Romer's 1676 success until 1677. His completed explication of the double refraction enigma was produced on August 6, 1677. He presented his findings to the Royal Academy in 1678/9 but refrained from publication of the *Traité* until 1690 having intended its prior translation into Latin.

182 In general it can be summarized that Huygens rejected Cartesian apriorism but embraced the Cartesian hypothetico-deductive programme "conceived in accordance with Cartesian mechanism". (Sabra 1967, p. 14)

183 Not all historians of science grant equal merit. For O'Connor and Robertson, "[Huygens] quite wrongly, criticised Newton's theory of light ..." and fail to mention "Huygens' principle" by name, although it is admitted that "... Leibniz owes much to Huygens from whom he learnt much of his mathematics." (O'Connor & Robertson 1997)

184 Huygens is candid in his admission that his *Traité* represented only "beginnings" and that he had left "certain difficulties without having resolved them, and still more from matters which I have not touched at all, such as Luminous Bodies of several sorts, and all that concerns Colours ..." (Huygens 1690, p. vii)

wave-train propagation, any characterization of his contribution as being explanatory of waves, at least as conventionally understood, also seems overly ambitious.[185]

For the author of the *Traité*, no claims of a Newtonian incontestable certainty exist, and his "speculations" and "demonstrations [are] of those kinds which do not produce as great a certitude as those of Geometry," but nonetheless may "attain to a [high] degree of probability." In Huygens' view, being reflections consistent with a hypothetico-deductive temperance, they contrasted with the theoretical certitude claimed by Newton. Huygens' approach readily recalls the Morrison–Morgan-Cartwright discussions of preliminary model construction, (Cartwright 1983, pp. 15, 153; Morrison & Morgan 1999, pp. 13, 14) where the building of a preparatory model is portrayed as an integral but early step when pursuing the scientific method in the cause of eventual theory construction. In recognizable agreement, Huygens' model emerges, formulated from initial problem specifications and a diverse and imaginative admixture of relevant analogies, bits of relevant theories, various idealizations and the empirically confirmed fact of a finite speed of light. A Newtonian-like particle model is considered but discarded as inadequate. Also integral to the success of Huygens' model construction was his insightful crafting of properties of the then supposed optical medium, the universal luminiferous aether - properties not elsewhere incorporated by either his early philosophical mentor Descartes or his principal antagonist, Newton. Although it can be claimed that, unlike Newton, Huygens adopted an "outdated" concept of Cartesian styled fluidics, such that Shapiro considers his aethereal particle impact model "rather crude and simplistic," he also concedes that this property of the preliminary model is "admittedly adequate for Huygens' purposes." (Shapiro 1973, pp. 220-221) However, it is crucial to note that, whereas Descartes subscribed to a plenum of extremely hard ball-like aethereal atoms in intimate contact, thence underwriting an infinite rate of propagation without actual motion, Huygens entertained a 'degree' of hardness, adding to his model a property of elasticity, or in Huygens' terminology, "springiness".[186] It is then from this fiducial property of elasticity that Huygens developed attendant physical properties of pulse propagation which he suggested must necessarily follow. A finite propagation rate was also necessitated by the transmissions being successive. A uniform velocity of propagation, independent of the impulse strength, also followed from a consideration of such an elasticity. As Huygens notes:

[185] "There seems to be fairly general agreement that Huygens' theory ought more properly to be called a pulse theory... [since] periodicity was nowhere introduced" (Bell 1947, p. 191)

[186] It should be understood, however, that Huygens did not invent the notion of aethereal elasticity but employed it judicially. The speculation itself, and its possible cause, were the subject of much debate at the time. (Shapiro 1973, p. 220) Descartes had also previously speculated that light was to be considered "essentially a pressure transmitted through a perfectly elastic [aetherial] medium ... which fills all space ..." (Born & Wolf 1999, p. xxv)

> ... if one wishes to seek for any other way in which the movement of Light is successively communicated, one will find none which agrees better, with uniform progression ... than the property of springiness; because if this movement should grow slower in proportion as it is shared over a greater quantity of matter, in moving away from the source of the light, it could not conserve this great velocity over great distances. But by supposing springiness in the ethereal matter, its particles will have the property of equally rapid restitution whether they are pushed strongly or feebly; and thus the propagation of Light will always go on with an equal velocity. (Huygens 1690, p. 15)

Pursuing an elastic property even further, Huygens hypothesized the principle of superposition.[187] Following a demonstration that tested his hypothetical predictions, he concluded "it being certain that one and the same particle of [aethereal] matter can serve for many waves coming from different sides or even from contrary directions," and notes that such an elastic sphere, when simultaneously struck "will yield and act as a spring at both sides, and so will serve ... to transmit these two movements." (Huygens 1690, pp. 17-18) However, before construction of a satisfactory developmental model of wave spreading could be concluded, an additional property of such elastic spheres constituting the optical medium was yet needed – a specification of their spatial arrangement. In Huygens' model, each was to be in contact with several others such that "the particles of the ether are not ranged in straight lines ... but confusedly, so that one of them touches several others." (Huygens 1690, p. 15) With this, the fundamental requirements for initiating "Huygens' Principle" were in place such that:

> each particle of matter in which a wave spreads ... also imparts some of [its motion] necessarily to all others which touch it ...So it arises that around each particle there is made a wave of which that particle is the centre." (Huygens 1690, p. 19)

Pursuing the fate of this ever increasing myriad of 'secondary' wavelets with a detailed geometrical analysis, Huygens hypothesized that they would become perceptible where they concur, forming a single wavefront as a spherical surface of constant phase at their common tangent.[188] Essentially, such a spherical wavefront emerges as "the locus of the points reached by the disturbance in a given time." (Shapiro 1973, p. 224)

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[187] Whereas Huygens' Principle is now recognized in modern physics as a universal principle applicable to virtually all propagation phenomena which can be described through explicit differential and difference equations, his 'principle' as such is seen as comprehending the "principle of action-by-proximity" and the superposition of secondary wavelets is separately describable as "Huygens' construction." (Enders 1996, abs)

[188] technically, Huygens described such a wavefront as the 'involute of the caustic.'

Were Huygens' exposition to be judged as a '*theory* of light,' assessments of some aspects as "crude and simplistic" may well be construed as inimical to the legitimate expectations required of a realistic scientific truth-bearer. However, when applied to a developmental model, such aspersions are neither necessarily derogatory nor incapacitating. Although no agreed theory of representation has yet been adopted, 'simplistic' in modeling terms is readily accommodated as a strategic minimalism, that is, as an *Aristotelian idealization* wherein the selection of a suitable subset of phenomena is examined in isolation. In sympathy with the view that "[o]ne key to Huygens' success was the limited range of phenomena that he attempted to explain," both de Lang (1990, p. 21) and Shapiro (1989, p. 232)[189] note that Huygens either ignored, or 'rejected' periodicity as dispensable, and in vindication Shapiro argues that:

> One should not make too much of Huygens' rejection of periodicity, for I suspect that Huygens recognized that his [model] applied equally to pulses and periodic waves. From Huygens point of view, since he did not accept Newton's theory of color,[190] the admission of periodicity would be an unjustified restriction of his [model]. (1973, p. 222)

On the other hand, Huygens adopted aspects of the Cartesian fluid model which Newton had previously abandoned as inadequate, having ascertained that the propagation velocity in a medium is dependent on both its elasticity and density. Thus, although creatively adopting an elastic property of the supposed aether particles, whether by design or default, Huygens excluded its complementary potential for density. Nevertheless, such portrayals, otherwise critical to a more realistic depiction, can be argued as denotative of a Galilean-styled idealization.

However, representational efficacy necessarily bears on the question of a model's legitimate domain of application and the inherent restrictions thus imposed on its use. In this respect, Shapiro suggests that Huygens placed his model at a disadvantage as not remaining valid in that wider domain of application which included refraction. Here, his aether medium would need to have been endowed with a property of density in order to fully account for the required change in velocity when encountering a change in medium. Further in this vein, it must also be acknowledged that, in his anticipation of a functional modeling of rectilinear propagation, Huygens is also to be found overly optimistic.(Shapiro 1973, p. 227) In the event, modeling minimalism to the extent employed by Huygens, proved counter-productive to coincident accommodation of both

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[189] Sabra concurs suggesting that Huygens' "only concern is to give a clear mechanical explanation of a few properties which ... other philosophers had failed to explain before him." (Sabra 1967, p. 229)
[190] In his *Traité*, Huygens admits "I leave certain difficulties without having resolved them ...[such as] all that concerns Colours; in which no one until now can boast of having succeeded." (Huygens 1690, p. vii)

rectilinear propagation and diffraction as propositions a resulting hypothesis could predict. Clarification of this issue along with accommodating other phenomena such as polarization, was not destined to be retained under Huygens' stewardship and had to wait until the nineteenth century when 'concretization' of his developmental minimalist model conferred admittance to an expanded domain for his principle's application. Nevertheless, in summary, Huygens' twentieth-century translator, Thompson, offers the eulogy:

> If Huygens had no conception of transverse vibrations, ... or of the existence of the ordered sequence of waves in trains, he nevertheless attained to a remarkably clear understanding of the principles of wave-propagation; and his exposition of the subject marks an epoch in the treatment of Optical problems. (Huygens 1690, pp. ix-x)

Thus it may be argued that "Huygens' principle," even as Huygens advanced it, is a valid continuum-styled developmental *model*, drawing on the representational power of appropriate analogies to sound and water waves and incorporating both minimalist and Galilean idealizations. As advanced, it was intended to usefully represent and offer insights into a specific, albeit restricted, subset of unobservable phenomena relating to radiatory propagation. Further, the physical product of this model, as an expanding spherical surface of constant phase, was "of a kind of matter" (Huygens 1690, p. 3) – highly reminiscent of Kepler's *quadam materia* – a physically realized entity as a dynamic structure generated by, and persisting throughout, the radiation process, yet lacking permanent physical embodiment.

The developing wavefront model, as an hypothesis, was in turn shown to be successfully predictive of crystalline birefringence. In a manner recalling Newton, Huygens' wavefront dynamics was claimed "inextricably related" to what Huygens assessed as his own "*experimentum crucis.*"[191] This experimental investigation gave rise to his "crowning achievement ... a complete account of the newly discovered double refraction in Iceland spar (transparent calcite)." (Shapiro 1973, p. 207) As Shapiro explains, double refraction of such minerals appeared to contradict a fundamental requirement of a successful continuum modeling of refraction, wherein abstract geometrical rays of necessity should retain normality to the expanding luminous surface. Double refraction demanded that, in at least one of its refractive expressions,

[191] In a letter to Leibniz in 1694, Huygens declared, "...I do not see ... how in their [Newton and his sympathetic associates] hypothesis they can explain the laws of refraction and, still less, that of Iceland crystal, which serves for me as an *Experimentum Crucis* ..." (cited in Shapiro 1973, p. 247)

this normality was apparently renounced.[192] Huygens, however, "was able to resolve this crisis with 'Huygens' principle', a definition of wavefront broad enough to encompass both ordinary and extraordinary refractions." (Shapiro 1973, p. 208) It is nonetheless pertinent to recall that such a model's ability to sponsor useful theorizing is circumscribed by the limitations of its intended domain of application and does so with 'agreement' of the model user. Failure to recognize Huygens' initial contribution as a preliminary developmental model of propagation, capable, as in McMullin's scheme, of supporting an ongoing research program, has sadly contributed to a destructive depreciation of Huygens' initial contribution extending even into modern times:

> It is unjust to Newton to call his a pure corpuscular theory and to contrast it with Huygens's so-called wave theory. Huygens's theory was nothing like a modern wave theory. In his famous 'construction' the only part of the wavelet that was effective was the pole, the point where it touched the envelope. The particular waves, or wavelets, were incapable of causing light. He thus gave his wavelets point or particle properties. Again, the pulses followed one another quite irregularly. He had no conception of a transverse wave. Newton's theory had many points of resemblance to the most recent [photon] theory … (Andrade 1956, p. 262)

Nevertheless, in response, it is pertinent to recall that an idealized model is potentially amenable to de-idealization and concretization. Concepts, such as periodicity or transverse waves, can be appreciated as having been originally forfeited in the interests of simplicity or pragmatism or indeed, simply ignorance. However they can be re-incorporated by 'adding back,'with the expectation that a valid model's competence is thereby reinforced and its applicable domain and efficacy extended. (McMullin 1985, p. 261) Such was to eventuate when the much later works of Young, Fresnel, Maxwell and Kirchhoff were brought to bear on Huygens' preliminary seventeenth-century model.

The Newton–Huygens "Conflict" – A model inclusive review

If science can claim that some purchase on its anticipated inheritance of a true knowledge of the world is sponsored by open debate between its able practitioners, then the latter stages of the seventeenth century potentially provided one of its more excellent opportunities. Whereas debates in the optical arena in the form of acrimonious rivalries of varying value had been the scientific *modus operandi* for many centuries,

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[192] In his study of such birefringence, Huygens' initial discovery was the crystal's axis of symmetry, or optic axis. Next he hypothesized that in all directions normal to this axis, light could propagate at two different speeds – a conventional speed for the 'ordinary' ray with light spreading as a sphere, and a larger speed for what Bartholinus termed an 'extraordinary' ray (Wolf 1990, p. 8) with light spreading as an ellipsoid of rotation, and both resulting from the process now termed 'Huygens' principle.' (Ziggelaar 1980, p. 184)

the contemporaneous emergence of two champions, one maintaining an emission-styled approach as against the other ably supporting a continuum stance, suggested the possibility for resolving foundational optical contentions. Leibniz recognized the moment and in a letter to Newton of Mar. 7, 1692 (Mar. 17, 1693 N.S.) he urged:

> I do not doubt that you have weighed what Christaan Huygens, that other supreme mathematician, has remarked ... about the cause of light ... I would like your opinion in reply: for it is by the friendly collaboration of you eminent specialists in this field that the truth can be unearthed. (Albury 1971, p. 446, fn. 5)

Sadly, in view of the potential the occasion offered, intransigence on the one part eventually necessitated withdrawal by the other. An epitaph to the encounter was later formulated by Huygens:

> Touching the Solutions, given by M. *Newton* to the scruples by me propos'd about his Theory of Colors, there were matter to answer them, and to form new difficulties; but seeing that he maintains his opinion with so much concern, I list not to dispute. (Huygens 1673)

Early indications of what was to become a notable scientific polemic were discernable by 1672. Although already Lucasian Professor of Mathematics, Newton's election to Fellow of the Royal Society[193] had been mediated by his successful construction of a serviceable reflector, or 'catadioptrical' telescope, with attendant but inaccurate claims by the Society, that he was also its inventor.[194] (Bechler 1975, p. 101) However, as historical coincidence records it, in Europe, Christiaan Huygens had also risen to scientific pre-eminence, particularly in the field of optics, and also through a parallel ability to fashion lenses and telescopes. On behalf of the Royal Society, its then secretary, Henry Oldenburg, informed Newton of efforts "to use some meanes to secure this Invention from ye Usurpation of forreiners" by drawing up a description of it and sending "a Solemne letter to Paris to M. Hugens, thereby to prevent the arrogation of such strangers." (Oldenburg 1672) Yet other parallels, both as to areas of scientific interest and personal achievements, fitted Newton and Huygens for equitable exchange.[195] In this respect, both developed highly articulate but disparate versions of

[193] In full the *Royal Society Of London For The Promotion Of Natural Knowledge*

[194] As Bechler explains: "Not only was the idea a hundred years old ... but also Newton himself took the idea straight from the most recent ... suggestions, namely that included in James Gregory's *Optica promota* of 1663." (Bechler 1975, p. 101) Nonetheless, The Royal Society drew up a document describing Newton's specifications and proclaiming it: "The new invention of Isaac Newton, Professor of Mathematics in the University of Cambridge ..." (Turnbull 1959, p. 75) This was sent to Huygens in Paris with a letter from Oldenburg dated 15 January 1672. (Turnbull 1959, pp. 81-82)

[195] The inevitable contest between the "two titans" was much in evidence as a mutual challenge when they first met in London at a meeting of the Royal Society on 12 June 1689. In what is now seen as one of

modeling for gravity[196] and both were prominent in their respective scientific institutions –Huygens as virtual 'founder' and then guide to the natal Physical Assembly of the *Académie Royale des Sciences* in Paris in 1668 and Newton rising to presidency of the Royal Society in 1703 (Sabra 1967, p. 171) It was, however, in exercising these respective roles, that both Newton and Huygens took the opportunity to advance, and potentially inculcate, their individual views on the 'proper' way to conduct scientific research.

Although both claimed to draw heavily on the Baconian paradigm which stressed the necessity of initial experiment and observation, a manifest contrast in procedure then emerged as differing interpretations of the value and ideological status to be conceded to 'hypotheses.' For Huygens, a hypothetico-deductive methodology was advocated wherein:

> the difficulties involved in physical research cannot be overcome except by starting from experiments ... and then contriving hypotheses against which the experiments are weighed, in which task the method of [Francis Bacon, Baron of] Verulam[197] seems to me excellent and deserving of further cultivation. (Huygens to Tschirnhauss, 1687, cited in Sabra 1967, p. 173)

Conversely, Newton drew on a reading of the method of induction[198] advanced in Bacon's *Novum Organum,* and as a paradigm of scientific method then preferred, even required, by the Royal Society. (Lohne 1968, p. 174; Sabra 1967, pp. 248-249) As Newton viewed it, the certainty of theory was to be free from all facilitating hypotheses:

> A naturalist would scearce expect to see ye science of those [colours] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of

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the great ironies of history, 'Mr Hugens of Zulichen' gave account of his intended publication of 'a Treatise concerning the Cause of Gravity' and 'Mr Newton considering a piece of the Island Chrystall did observe' and describe the two sorts of refraction." (Shapiro 1989, p. 223)

196 "[Huygens] derived the law of centrifugal force for uniform circular motion. As a result of this Huygens formulated the inverse-square law of gravitational attraction." (O'Connor & Robertson 1997)

197 In 1613 Bacon was appointed Attorney-General. In March 1617, he was appointed Lord Keeper of the Great Seal, before being made Lord Chancellor and First Baron of Verulam in the following year. Subsequent to completion of his celebrated work, *Novum Organum,* in 1620, he was further created Viscount St. Albans in 1621.

198 Sabra summarizes this as: "Bacon's advice was not to venture anything in the affirmative (conjectures, hypotheses) but to work for eliciting the affirmative from the experiments [which] consisted in rejections which finally gave way to affirmatives necessarily implied by the experiments. Thus he maintained that to man 'it is granted only to proceed at first by negatives, and at last to end in affirmatives after exclusion has been exhausted,." (Sabra 1967, p. 249, fn. 44) Lohne concurs, suggesting it as "an induction that examines scrupulously the experiment in question, views it in all possible lights, rejects and excludes whatever does not necessarily belong to the subject; then and not till then, concluding from the affirmatives left." (Lohne 1968, p. 174)

Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phaenomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt. (Newton 1672c, pp. 96-97)

Elsewhere, Newton frequently repudiated the hypothetico-deductive methodology of his contemporaries. Perhaps with Huygens in mind, in his 1672 paper he noted: "I have observed the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted an hypothesis to explain them by ... ," (Newton 1757, p. 249) Such a retort recalls his well-known declaration "*Hypotheses Non Fingo*" – "I do not feign [or frame] hypotheses." In his "*General Scholium*" which was first appended to the 2nd (1713) edition of the *Principia*, Newton later remarked:

> But hitherto I have not been able to discover the cause of those properties of gravity from phaenomena, and I frame no hypotheses. For whatever is not deduc'd from the phaenomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. (Newton 1726)

Elsewhere, an edict against the use of hypotheses in inductive practice equally existed in the *Principia,* Book III as Rule IV of his *'Rules of Reasoning in Philosophy'*:

> In experimental philosophy we are to look upon propositions collected by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions. This rule we must follow, that the argument of induction may not be evaded by hypotheses. (Newton 1846, p. 385)

Much of the more traditional assessment of Newtonian science and of the polemic between Newton and Huygens, quite understandably tends to emphasize these salient differences in approach, principally in terms of the relative merits of an Inductive versus Hypothetico-Deductive strategy.[199] Much attention has been given, and quite correctly, to the foundational issues raised between such contrasting approaches, with insights

[199] Lohne notes that: "Besides being of supreme interest for the history of optics, [Newton's *experimentum crucis*] has often been held as a model of true scientific method," adding that "the treatise is popularized science at its best ..." (Lohne 1968, p. 181) Shapiro adds that: "Historians have long studied every twist and turn by Newton and his adversaries in the controversies over his theory in order to illuminate his new science and method." (Shapiro 1996, p. 61)

gained deemed valuable to the history and philosophy of scientific theory construction.[200] (Hamilton 1966, p. 1035) In this respect, the degree of Newton's enthusiasm for unquestioning theoretical certainty "admitting of no objections against the conclusions," and claimed as deriving directly from empiricism via induction, (Hamilton 1966, p. 1035) benefits from further investigation. It is particularly in respect of Newton's aversion to hypotheses that some reassessment of his role as a prime exemplar of modern scientific method might be thought to benefit. In commenting on "Common mistakes in applying the scientific method," Wolfs claims that:

> The most fundamental error is to mistake the hypothesis for an explanation of a phenomenon, without performing [subsequent confirmatory] tests. Sometimes "common sense" and "logic" tempt us into believing that no [further] test is needed. There are numerous examples of this, dating from the Greek philosophers to the present day. (Wolfs 2005, Sec. III)

It may well be argued, that in casting his initial conclusions in a role more suitable to that of a well-corroborated and mature theory, Newton depreciated an essential recognition of scientific enquiry as a truly human pursuit and as such, fallible. It might be also argued that it is the innate admission of fallibility that provides the essential incentive for holding hypotheses as provisional and submitting hypotheses to strict and appropriate experimental testing - a process held to distinguish science from all other forms of explanation. (Wolfs 2005, Sec. VI) Unfortunately, in holding to his certainties, Newton might be thought to have denied his theory of light one of the primary attributes demanded by science of all its theoretical constructs – falsifiability.

On the other hand, little analysis appears to have been brought to bear on these issues from an appreciation of the role modeling is now portrayed as playing in this same pursuit. Such appraisals might additionally place emphasis on the potentially productive role of developmental modeling on the construction of theory and could feature different philosophical concerns dictated by different criteria. Of significance, recognition of a theological well-spring nourishing a mechanical and atomist mind-set is now seen by many as indispensable to such an inquiry. It is in this light, that Newton's various religious dispositions and convictions are portrayed as potentially inciting, or at least exacerbating, his uncompromising zeal, but as also imposing an extra-scientific

[200] However Ben-Chaim claims that a "historically adequate account of Newton's achievement has long been hindered ... by interpretations that were based on a fundamentally ahistorical view of science as a quest for empirically adequate theories. The study of Newton's theory thus became, especially during the 1960s and 1970s, a battlefield of competing philosophical explications of the 'scientific method' that deliberately set out to evaluate it by ahistorical standards." (Ben-Chaim 2001, p. 395)

interpretative ideology that might be thought to impinge both on preliminary model selection and the properties such a model might be required to demonstrate.. According to Ashley (1952, p. 158) "Newton and Boyle both emphatically believed that their scientific work served the cause of religion ..." and in contrast to the more common reviews that uncritically depict Newton as the model of a modern scientist, Ben-Chaim portrays a Newton who:

> ... based his research on the assumption that causal explanations must function as rules that govern perceptual judgment, rather than as interpretations of given phenomena. His endeavor to explore and identify such rules followed his practical reflections on human beings as agents who belonged to God's dominion and were created to serve its divine ends. These reflections suggested ... that the aim of natural philosophy was the discovery of divine rules that instrumentally constrained and facilitated human conduct. Within this vocational framework, experiment did not merely function as an aid to observation. Rather, it formed the discipline of a practical understanding of the natural order ... His experimental reports thus proposed explanations, by elucidating the rules that could practically govern the perceptual judgment of 'matters of fact.' (Ben-Chaim 2001, p. 397)

Newton's intransigence might then be appreciated to that extent that he shared Boyle's belief that "'Right Reason'[201] [as natural reason seasoned with divine revelation] could allow the enlightened philosopher to judge the truth between philosophies, and only *he* could emerge as the exponent of truth, even in the realm of natural knowledge." (Mulligan 1994, p. 243) However, beyond its potential to explain a cavalier certitude sustained by claims deriving from personal religious privilege, such an insight into Newton's theology goes far to illuminate his underlying atomist model of light. For Newton, the compositeness of light must be expressible in enduring and fundamentally concrete terms itemized from a primal theologically endorsed cosmic inventory, and then, in accordance with his emissionist inclinations, projected in terms of physical rays.[202] Additionally, arguments advanced by Newton also suggests that his subscription to such an atomist rendition of the mechanical philosophy, equally endowed a robust theologically incited epistemology that then flowed through to influence theory

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[201] Styling himself as 'the Christian Virtuoso,' Boyle distinguished between 'reason' as a "superior faculty of mind, furnished with its own original notions," and "Right Reason" as natural reason augmented by divine revelation. "The non-Christian virtuoso was seen as irremediably hampered. His godly counterpart, on the other hand, had an instrument not available to other 'mere natural men.'" (Mulligan 1994, pp. 236-237, 243)

[202] Later in the *Principia*, Newton framed this view more cautiously as "the analogy [which] there is between the propagation of the rays of light and the motion of bodies ...not at all considering the nature of the rays of light, or inquiring whether they are bodies or not: but only determining the [trajectories] of bodies which are extremely like the [trajectories] of the rays." (Albury 1971, pp. 448-449; Newton 1846, p. 246)

construction. For Newton, optical theory was advanced on the certainty of optical experiment, and experimental interpretation was advanced on the surety of theological revelation as an underlying reality. Adoption of a reified and simply locatable model of optical rays as necessarily definitive, possibly then also impeded his ability to acknowledge, or even to comprehend[203] the abstract ray models advanced by his opponents.

> ... if any man think it possible [to maintain that a pressure or motion in a medium may be propagated in straight lines], he must at least allow that those motions ... caused in ye Æther by the severall parts of any lucid body wch differ in size figure and agitation, must necessarily be unequall. Which is enough to denominate light an aggregate of difform rays according to any of those [wave-based] Hypotheses. And if those originall inequalities may suffice to difference the rays in colour & refrangibility, I see no reason why they that adhere to any of those Hypotheses, should seek for other causes of these effects ... (Newton 1672c, p. 176)

Notwithstanding the high regard that Newton's general scientific work justly commands, his inflexible claims to certainty in optics, supported by a realism assigned by theological precepts to atomist modeling, came at a great cost. A less complimentary literature now exists[204] which exposes some less palatable aspects of Newton's sometimes imperious scientific practices and their sometimes less than pure scientific motivations. Included in modern surveys is historical evidence that:

- Newton selectively ignored some significant but perhaps inconvenient optical discoveries made by his contemporaries, such as made by Grimaldi on diffraction, Bartholin and Huygens on double refraction, and Fermat's correctly designated refraction law. (Lohne 1968, p. 176)

[203] Hooke's continuum-styled composition of white light with abstract rays was for Newton "not onely *insufficient*, but in some respects *unintelligible*." Later he again confesses that "... though I can easily imagin how unlike motions may crosse one another, yet I cannot well conceive how they should coalesce into one *iniforme* motion, & then part again & recover their former unlikenesse..." (Newton 1672c, pp. 176-177, orig. emph.)

[204] As Ben-Chaim views it "[the] image of Newton as the archetype of today's scientist (specifically the physicist) has been widely undermined recently ... by detailed historical enquiries into his lifelong interests in theology, alchemy, ancient lore and prophecy. Leading these enquiries ... Dobbs has argued that the works that were later regarded as 'stunningly successful' scientific achievements were in fact 'by-products' of his essentially religious goals." (Ben-Chaim 2001, p. 396) On the other hand, Lohne claims that: "Few historians have examined in detail Newton's experiments and statements. Brewster and other biographers fashioned and painted Newton as a superman and a knight *sans erreurs et sans reproche*. Modern man, who is skeptical and disbelieves in legends, soon tires when he must look at a statue that in all illumination casts no shadows. Newton can captivate the minds of our age only if his biographers admit that also in him there is an interplay of light and shadows, and only so will his true greatness emerge, against a background of considerable weakness and mistakes." (Lohne 1975, p. 392)

• Newton was not above creating a fictitious experiment where this appeared to him desirable:

> ... during a short period of 1672 [Newton] declared that he possessed a solution to the problem of constructing ... [compounded] lenses, but later it became a custom with him to ignore the whole issue in a rather pointed manner. Eventually he came to work out a so-called 'theorem' which 'proved' the impossibility of constructing an achromatic lens telescope. He published this theorem in his *Opticks* of 1704, together with an alleged experiment meant to serve as its proof. Only after 1755 did it become general knowledge that this experiment was fictitious. Newton himself, however, knew this already in 1672. (Bechler 1975, p. 103)

• Newton was capable of an unacceptable inconsistency when dealing with the scientific work of an opponent. As example, Shapiro advises us that:

> Newton's response to Huygens' investigation of Iceland spar was extraordinary. Without giving any justification, Newton rejected Huygens' law for the irregular refraction and replaced it by his own, which was wrong ... After verifying Huygens' law in 1808, Etienne-Louis Malus [although] an adherent of Newton's emission theory of light, attempted to explain Newton's rather unusual behavior: "He [Newton] knew the work of Huygens; however, he substituted for the law of this geometer a law more simple in appearance, but absolutely contrary to the phenomena ..." (Shapiro 1973, p. 249, and fn. 361)

• Newton was not always fastidious about the accuracy or even validity of experimental data[205] particularly where these might be perceived detrimental to a desired theoretical confirmation or in conflict with theological predeterminants:

> When Newton, often after lengthy irresolution, had taken a stand and assumed a mechanical hypothesis, he exploited his model to its utmost possibilities, in some cases even adjusting experimental results to his hypotheses rather than the hypotheses to the experiments. (Lohne 1965, p. 130)

Concurrently, however, and in defence of an otherwise great scientist, some evidence has been offered in mitigation if not exoneration. Keynes suggests gross

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[205] Although Lohne admits that Newton was generally careful and reasonably accurate, his lapses are considered noteworthy and the conclusions drawn from them "reckless." Where Newton attempts to define his new laws of refrangibility by appealing to a demonstrably skewed spectrum in his Opticks, Book1, Prop. VI, Lohne assesses Newton's effort such that: "Seldom has a physical law been 'demonstrated' by experiments so inaccurate and by deductions so faulty." (Lohne 1975, p. 391)

personality defects of Newton, the man, wherein he is depicted as the victim of extreme neuroses which rendered him psychologically incapable of coping with opposition. (Keynes 1947, p. 94) Such a view might also go far to explaining Newton's involvement in many interminable, highly acrimonious and unbecoming disputes, of which those with Hooke and Leibniz are still historically significant.[206]

On this view then, it may be argued that Newton failed to recognize certain facets as predispositions of the mind-set of the times with their theological endorsement, and as merely representational models with their innate limitations and idealizations. Newton imagined that his empirical interpretations were certainties that carried all that he could need to know as an aid to theory construction. On such a view, his failure to recognize the existence and functionalism of a facilitating model enabled Newton to unwittingly sweep up with his explications purely fictional entities deriving from the predisposing model. Thus his theoretical explanations included assignment of an unwarranted realism and factualism to abstractions, representational artefacts, and idealizations. Providing some comfort for this view is Sabra's claim that:

> Perceiving an atomist tendency in Newton's ideas, [Huygens, along with other objectors such as Hooke and Pardies] regarded his theory as a hypothesis which indeed agreed with the experiments, but which was no more than one among several possible interpretations of them. Newton rejected this characterization of his doctrine, claiming that there was nothing in the propounded properties of light that was not positively and directly concluded from the experiments. (Sabra 1967, p. 231)
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> ... Newton took it for granted that his ... doctrine of the heterogenicity of white light ... was one of [the] experimentally established facts. On this understanding, he ascribed to his *experimentum crucis* a role which was not accepted by his opponents. They rightly perceived that what he presented as a neutral doctrine was ... identifiable with a corpuscular view and, therefore, no more than a plausible interpretation of the experiments. (Sabra 1967, p. 276)

Thus, in overview of the conflicts attending Newton's assertive proclamation that "*white light consists of rays differently refrangible*" it would appear not to have been the "differently refrangible" claim that primarily incited contention, for such was amenable to independent experimental investigation. Rather the impediments arose

[206] "One exchange, with English Jesuits in Liège, was carried on from 1674 until 1678, when Newton finally broke it off in an enraged outburst and withdrew from the scientific community until 1687." (Shapiro 1996, p. 65) In Huxley's estimation: "As a man he was a failure; as a monster he was superb." (Aldous Huxley quoted in Keynes 1947, p. 277)

from the baggage perceived to accompany his 'rays.' In this a dilemma is uncovered embedded in the foundations of the great optical debate. Following from Newton's apparent failure to distinguish a claimed factual exposition from facilitating modeling,[207] a philosophical trap became apparent – a vote for "rays differently refrangible" was also perceivable as a vote for a version of ray reification and realist atomism, a ballot that those inclined to a continuum and undulatory-based resolution to optical problems were equally disinclined to cast.

For Huygens, subsequent requests to Newton for a fully mechanical explanation[208] of his doctrine, now asserted by Newton to be an indisputable "matter of fact,"[209] may possibly have been an artifice employed to expose the thinly disguised underlying atomist schema. For his part, however, Newton was to interpret Huygens' requests as a further ploy to embroil him in speculative and needless hypothetical machinations.[210] Huygens quickly tired of Newton's perceived intransigence and withdrew from further exchange. Debate, acrimonious and sometimes unseemly, nonetheless continued between Newton and various of his antagonists. Sabra summarizes its eventual conclusion:

> The most important among Newton's critics, viz. Hooke, Pardies and Huygens, eventually conceded the only thing that Newton could have justifiably claimed to have proved experimentally, namely the fact that to every colour there is attached a constant of refrangibility which is not the same for any other colour. But they correctly refused to grant

[207] "The real sticking point ... for Newton's contemporaries ... was his interpretation of the phenomena, that is, his theory and not matters of fact." (Shapiro 1996, p. 61) Additionally, Sabra believes Newton to have been unaware of his atomist commitment suggesting that "owing to his conception of what constituted a ray of light, Newton was in fact committed to a corpuscular view. This Newton was not aware of ..." (Sabra 1967, p. 284)

[208] In a letter of reply dated 17 Sep 1672 to one of Newton's recently published claims to certainty on colours, Huygens ventured his opinion that: "Nevertheless, the matter could well be otherwise; and it seems to me that he should be content to propose [his doctrine] as a very plausible hypothesis. Besides, if it were true that the rays of light were, from their origin, some red, some blue etc., there would still remain the great difficulty of explaining by the mechanical physics wherein this diversity of colours consists." (Huygens 1672, pp. 235-236, as trans. in Sabra 1967, p. 270) The same accent on rays and challenge to mechanical explanation is repeated in a letter to Oldenburg dated 14 Jan. 1673, "... he hath not taught us, what it is wherein consists the [mechanical] nature and difference of Colours, but only this accident (which certainly is very considerable,) of their different refrangibility." (Huygens quoted in Oldenburg 1673, pp. 255-256, trans. in Sabra, 1967, p. 271)

[209] In June of 1673, Newton hardened his principal claim to "(1) The Sun's light consists of rays differing by indefinite degrees of refrangibility. (2) Rays which differ in refrangibility, when parted from one another do proportionally differ in the colours wch they exhibit. These two Propositions are matter of fact." (Newton 1673, p. 293)

[210] In discussion of the more conventional analyses available in the literature, Sabra notes that: "Various aspects of this controversy have been studied more than once, sometimes even with a view to clarifying Newton's methodological position. The judgement generally [but one unfortunately also following Newton's own view] has been that Newton's critics failed to understand his theory; their fault [perceived as] that while they were faced with an experimental discovery, they preferred to wrangle over hypotheses." (Sabra 1967, p. 232)

an equal status to Newton's doctrine of white light as a heterogeneous aggregate. (Sabra 1967, p. 233)

The fate of Huygens' wavefront process model

The *Traité de la lumière* appeared well received after its reading before the Royal Academy of Sciences in 1679 and later publication in 1690. Philippe De La Hire was to extol: "your treatise seems to me to be a *chef d'oeuvre* both in mathematics and physics, and I cannot cease to admire the turn you have taken to explain such extraordinary phenomena ..." Denis Papin concurred with: "I ...admired, Monsieur, how your hypothesis for the double refraction of Iceland crystal perfectly satisfies so many phenomena, which you have observed with so much care and penetration ..." (Shapiro 1973, pp. 245-246) However, the highest compliment was to come from Leibniz, who wrote:

> Your use of waves to explain the effects of light surprised me. Nothing is more fortunate than the facility with which this [tangent] which touches all the particular waves and composes the general wave satisfies the laws of reflection and refraction established by experiment. But when I saw that you use the supposition of spheroidal waves with the same facility to solve the phenomena of the double refraction of Iceland crystal, I passed from esteem to admiration. (Leibniz to Huygens, Oct. 1690, cited in Shapiro 1973, p. 246)

It comes then as a surprise to read the historical sequel. Shapiro states that: "By 1700, within a decade after its publication, the *Traité de la lumière* was almost totally ignored" and that after 1691 it did not gain a single adherent. In Shapiro's assessment, this "is scarcely the reception which one expects to be accorded to what is now rightly judged a scientific masterpiece." Finally, an almost total obscuration seems completed with Huygens' work failing to rate a mention in subsequent eighteenth century optical investigations, including those by rare exponents of continuum modeling such as Leonard Euler.[211] Nonetheless, an interregnum of more that a century, during which a conception of light that was to be eventually abandoned, prevailed virtually unchallenged, yet a formulation that was eventually to be applauded as a universal principle governing "virtually *all* propagation phenomena," (Enders 1996, p. 226) was rejected and sequestered, is owed more than a historical footnote. For more than a century, the conventionally held view of the power of scientific method and its inevitably resulting progress, proved fallible. Analysts such as Kubbinga (1995, p. 737),

[211] Shapiro notes that Euler's *Opera omnia* (1746) in which abstract ray as normal to an expanding wavefront is treated as established, frequently cites Newton, the arch enemy of Euler's continuum concepts, and also Descartes, but almost inexplicably Huygens is not cited at all. (Shapiro 1973, p. 256)

Rosenfeld (1928, p. 120) and Born (1999, p. xxvii) appear content to cite a simple explanation of Newton's preeminence as historically sufficient. In some agreement, Huygens' eventual English translator, Thompson, suggests the possibility of academic embargo incited by the ensuing Newtonian hegemony:

> Considering the great influence which [Huygens' Treatise eventually] exercised in the development of the Science of Optics, it seems strange that two centuries should have passed before an English edition of the work appeared. Perhaps the circumstance is due to the mistaken zeal with which formerly everything that conflicted with the cherished ideas of Newton was denounced by his followers. (Huygens 1690, Note by the Translator)

However, Shapiro argues that:

> While the almost universal acceptance of the Newtonian theory of light undoubtedly contributed to the rejection of Huygens' explanation ... this cannot be considered the sole cause. (Shapiro 1973, p. 257, fn. 405)

In sympathy with this sentiment and with the potential for learning important historical lessons, a select literature now presents a range of ancillary rationalizations, some of which unfortunately prove on examination to be either self-contradictory or historically unsafe.

For his part, Rouse Ball suggests that Huygens' demonstrations were "rigidly geometrical" and lacking appropriation of the newly available differential calculus "were expressed in an archaic language, and perhaps received less attention than their intrinsic merits deserved." However, in an apparent inconsistency, he also notes that "almost all" of Newton's demonstrations were equally geometrical. (Ball 1908) As to linguistic expression, according to Wolf, (1990, p. 7) "Newton ... considered Huygens 'the most elegant writer of his time'." Pursuing the geometrical theme, Greene asserts that: "[Huygens'] method was highly geometric, and at the time had no mathematical basis." (Greene 2004) In what appears as a directly opposed opinion, Sabra suggests that: "It was Huygens' merit to have been the first to mathematize this picture [of light propagation] successfully. This he achieved by imagining a truly representative model of contiguous and elastic spheres to which he applied a new mathematical technique ..." (Sabra 1967, p. 13) In a further twist to the questions of archaism and mathematical credibility, Shapiro claims that "Newton's optical work ... by his own standards ... had not fully succeeded, and the *Opticks* remained a patchwork of mostly thirty year old writings with internal contradictions and no underlying mathematical structure."

(Shapiro 1979, p. 128). He further advises that despite his early claims to an intrinsically "mathematicall" theory of light and colours, "Newton did abandon his mathematical theory and suppress the mathematical part of the *Optical Lectures*." (Shapiro 1984, p. 40)

An equally contentious discussion ensues when the subject turns to Newton's possible theoretical and experimental superiority. Opinion is sharply divided as to whether Newton 'disproved' his opposition, both as to continuum modeling in general and wave-like solutions in particular. (Laymon 1978, p. 51) Further, although we are regularly reminded of the 'fatal' short-comings of seventeenth century undulatory proposals to convincingly embrace phenomena such as rectilinear propagation and polarization, it is equally true that Newtonian modeling was conspicuously inadequate when faced with double refraction and interference. Potentially less contentious is the insight found in argument that assigns a philosophical – and perhaps psychological - advantage to the forcefulness and authoritative certitude of Newton's inductive imperatives as against the more cautious and conciliatory hypothetical approach adopted by Huygens. (Laudan 1992, p. 212) Even in this, however, an opposing view is canvassed:

> Galileo had given an excellent example but Huygens carried the inter-relation of mathematics and experiment a long way further. All his work illustrates this quality, and certain continental writers have even argued that Huygens's conception of scientific method was in some respects superior to that of Newton. Certainly in regard to the position of hypotheses in scientific work a case may be made out for Huygens's superiority. (Bell 1947, p. 207)

It might well be that most of these various submissions made contributions to the ultimate occlusion of Huygens' work. However, viewing this episode from a wider philosophical perspective, Whitehead claims that Newton's role exemplifies:

> ... one of the dangers of unimaginative empiricism. The seventeenth century exhibits [an] example of this ... danger; and, of all people in the world, Newton fell into it. Huygens had produced the wave theory of light. But this theory failed to account for the most obvious facts about light as in our ordinary experience, namely, that shadows cast by obstructing objects are defined by linear rays. Accordingly, Newton rejected this theory and adopted the corpuscular theory which completely explained shadows... [This] illustrate[s] the danger of refusing to entertain an idea because of its failure to explain one of the most obvious facts in the subject-matter in question. (1938b, pp. 62-63)

Paraphrasing Whitehead's observations in the recent context of scientific modeling, it might be claimed that Huygens' offering becomes classifiable as an idealized model of a limited subset of optical phenomena yet simplified such that its domain of applicability did not yet extend to adequately model linear propagation. Misunderstood by Newton as presenting a realist and competing *theory*, its variance from literal expectations provided opportunistic but adequate grounds for its wholesale dismissal. Significantly, for Einstein, who would eventually argue for a revived form of a Newtonian emission-styled atomism, exoneration in these matters is readily forthcoming with the almost startling, historically and factually questionable suggestion that:

> It is not surprising that Newton would not listen to a wave theory of light; for such a theory was most unsuited to his theoretical foundation ... The strongest empirical arguments for the wave nature of light, fixed speeds of propagation, interference, diffraction, polarization, were either unknown or else not known in any well-ordered synthesis. He was justified in sticking to his corpuscular theory of light. (Einstein 1940)

A further elaboration on the theme is fielded by de Lang (1990, p. 19), wherein the "ultimate stumbling block" to acceptance of Huygens' principle is found in the resilience of the entrenched physical or reified ray model of light. In this respect, a perhaps fortuitous observation by Shapiro, which notes the "constant conjunction of 'Huygens' principle' and his explanation of double refraction," may further develop this otherwise under-represented observation:

> ... when considered historically, double refraction is ... the *experimentum crucis* which Huygens had claimed it was. All those who accepted "Huygens' principle" ... did so because of double refraction. Even Newton implicitly recognized its importance and tried to draw attention away from it. Newton succeeded in this, and Huygens' explanation of double refraction remained discredited until the beginning of the nineteenth century. (Shapiro 1973, p. 252)

Much academic time and effort has been directed towards proposing more satisfactory historical and philosophical analyses of what is now conceded an important historico-scientific developmental drama, and consideration of Shapiro's observation appears relevant. Huygens' principle was by far the most complex and advanced product of a 'process' approach to physics yet portrayed. As the most viable model demonstrable of double refraction, not only was Huygens' concept inclusive of a *'quadam materia'* and the abstraction of geometrical rays common to other continuum models, but it additionally required that a complex process of interaction by secondary waves acted such as to nullify even *the physical perception* of such phenomena except at the continually advancing locus of their mutual reinforcement. If, as Shapiro suggests, that the "concepts of [abstract] ray and wave-front were not easily conceived" (1973, p. 137) earlier in the seventeenth century, no evidence is forthcoming that these concepts, now augmented by complex ephemeral and intangible processes, were in any way more comprehensible by the century's end. Exposition does not guarantee comprehension. Yet beyond the intellectual effort required, acknowledgement of Huygens' model was to give credence to incorporeal prospects for the material universe heretical to conservative dogmas of the prevailing mechanical paradigm. Newton, following his theologically endorsed atomist rendition, found such concepts "unintelligible". Even dedicated wavefront advocates such as Hooke,[212] Halley[213] and Parent[214] failed to comprehend this excursion into process. (Shapiro 1973, p. 250) It is then arguable that the Newton-Huygens contention was in particular a contest between 'particle' and 'process' rather than simply between emission and continuum modelings of light, and a contest in which processes were found to be at that distinct disadvantage alleged by Rescher and Whitehead.

For many historians of science, the ensuing eighteenth century story of optics proves as barren as the seventeenth century was fertile, with the "usual explanation [citing] the apparently oppressive authority accorded Newton's epoch-making optical work." (Pav 1975, p. 3102) Whereas the optics of the late seventeenth century is usefully served by reference to the Huygens-Newton exchange, the rapid descent of Huygens' *Traité* into historical and philosophical oblivion meant that "eighteenth-century scientists tended to see it as [a continuation of the earlier continuum-emission contention] between Newton and Descartes." (Shapiro 1973, p. 257) However, as Pav observes:

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[212] In an unpublished lecture, Hooke, in failing to grasp its significance to an explanation of double refraction, dismissed the process of secondary wavelet formation and interference by Huygens' principle claiming that "Every point of the propagated Ray doth also propagate a hemisphericall undulation: of which hemisphere notwithstanding, [Huygens] makes noe more use than of only one line or Ray all the Rest he supposing to be insignificant and producing noe sensible effect ..." (Hall 1951, p. 221)

[213] Although much evidence is available to conclude that Edmund Halley's "views on light ... were very near to those of Huygens [during the 1690's] and incompatible with the theories of both Hooke and Newton," (Albury 1971, p. 454) nevertheless, sometime between 1695 and 1700, Halley "switched his allegiance from a continuum theory of light to Newton's emission theory." However, Shapiro does not believe that "Huygens lost a true supporter ...When given the opportunity, Halley did not test the *experimentum crucis*; in fact, he did not express any true interest in double refraction." (Shapiro 1973, p. 251)

[214] For his part, Parent (1666-1716), despite his enthusiasm for abstract rays as the geometrical normal to a physical wave-front, discounted the facilitating procedure complaining that the secondary wave process "makes the system of luminous waves infinitely more difficult than it is without them." (Shapiro 1973, p. 254)

> Rather than a strictly scientific controversy between the wave and corpuscular theories of light, the crucial issue in eighteenth century optics was philosophical – a series of skirmishes between Cartesians and Newtonians ... at question was the deductive methodology of Descartes and his brand of mechanistic cosmology. (1975, p. 3102)

Eventually attempts to pursue a continuum modeling of light propagation suffered from direct exposure to the considerable weaknesses of its inadequate and vulnerable Cartesian advocacy and by the middle of the eighteenth century, Huygens' *Traité de la lumière* was already forgotten, and "Newton's theory of light replaced not Huygens' but Descartes' theory." (Shapiro 1973, p. 257) Exacerbating this climate of opinion, a corpuscular model was believed to offer the more simple and straightforward explanation of the discovery of stellar aberration by James Bradley (1692-1762) in 1725, wherein the position of an observed star appears changed due to the relative motion of the earth. (Römer 2005, p. 9)

Restitution of wavefront and process modeling

Almost a century after Huygens, a renewed interest in double refraction towards the end of the eighteenth century underwrote fresh investigations, initially conducted by emission theorist René-Just Haüy[215] in 1788. The Newtonian bias however, was sufficiently resilient to prevent open acknowledgment of the results which favored Huygens' interpretation while clearly disagreeing with Newton's. Nonetheless, further interest early in the nineteenth century began turning the political tide when the English physicist William Hyde Wollaston in 1802, verified results in accord with Huygens model, to be followed soon after by an additional confirmation by Étienne-Louis Malus in 1808. (Shapiro 1973, pp. 252, 257) However, reinstatement of Huygens' principle would require more than positive self-referential affirmations – the entrenched Newtonian theoretical framework, resting on its theologically sanctioned atomism, would require point by point refutation.[216] Significant change to optical theory construction would require an equally significant change in the predisposing modeling, not simply by incorporating seeming rivals into mutual complementary harness, but by recognizing the complementary, rather than dictatorial role, of the idealized model.

[215] pronounced similar to English "ar'yee"

[216] According to Buchwald, nineteenth century physicist William Whewell's opinion was "that the wave theory came to [eventual] dominance by exposing the comparative inadequacy of the emission theory." (Buchwald 1989, p. xiii)

The first significant breach of Newtonian theoretical monopoly was claimed by Thomas Young (1733-1829) whose historical ripple tank and 'double-slit' experiments, deriving from analogies to wave propagation in elastic fluids and eighteenth century acoustics, advanced powerful arguments supporting a wave model of light. Young brought pressure to bear on a primary Newtonian particle model vulnerability through the principle of interference. Further, in his Natural Philosophy lectures, Young surmised from properties of the 'Newton's rings' phenomenon in liquids, that "light has a smaller velocity ... in the medium in which it is refracted towards the normal." (Mach 1926, p. 145) In this Young preempted later experimental work by Foucault which definitively prorogued Newtonian dominion. Although Shapiro claims that he did not base his optics on Huygens' principle, (1973, p. 258) Young was probably the first in the century[217] to acknowledge its true credentials and pave the way for eventual reinstatement:

> The unusual refraction of the Iceland spar has been most accurately and satisfactorily explained by Huygens, on the simple supposition that this crystal possesses the property of transmitting an impulse more rapidly in one direction than in another; whence he infers that the undulations constituting light must assume a spheroidical instead of a spherical form, and lays down such laws for the direction of its motion, as are incomparably more consistent with experiment than any attempts which have been made to accommodate the phenomena to other principles. (Young 1845, p. 363)

Nonetheless, although Young acknowledged Huygens' propagation process early in the century, wave-based and continuum models suggesting an abstract ray as the normal to an optical wavefront, and acknowledging a more physical nature of the wavefront,[218] were "only gradually and often simultaneously rediscovered in the first third of the nineteenth century …"[219] (Shapiro 1973, p. 263) Although Young's wave model proposals were vigorously opposed by the majority of English scientists for whom the possibility of Newtonian error was unthinkable, Young further eroded the Newtonian hegemony by elaboration of his optical model in 1817. Here he explored a model in which light waves exhibited transverse vibratory excursions at right angles to the

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[217] Young's "*Course of Lectures on Natural Philosophy*" was first published in 1807 having been delivered to the Royal Society from 1801-1803, during Young's term as Professor of Natural Philosophy at the Royal Institution, London.

[218] The status of Huygen's *'quadam materia'* was yet ambiguous. Limiting his description of the Huygens' wave-front to strictly conventional continuum terms which lacked more tangible insights from process modeling, Hamilton claimed that it was "according to [Huygens], no *thing* ... which moved from the sun to the earth ... but [merely] a *state*, a *motion*, a *disturbance* ..." (Hamilton 1966, p. 1033)

[219] The first such rediscovery was that by Malus in 1807 and later became known as the "Law of Malus." In modern terms it claims that: "If a surface can be found which is orthogonal to all the rays of a system, which system is then called an orthotomic system, then after any number of reflections and refractions the rays remain an orthotomic system." (Shapiro 1973, p. 263)

direction of travel. Such a notion opposed the entrenched belief that, as with water waves, undulations were solely longitudinal. A transverse wave model paved the way for wave-based optical polarization as was subsequently, yet independently, advanced by Fresnel in 1821. Nonetheless, it was to be atomist-emissionist model weakness when applied to the arena of quantitative results that primarily betrayed it when compared with its wave-model alternative. As Buchwald explains:

> By about 1830 quantitative deduction had become extremely desirable, and so the replacement of the emission theory by the wave theory was … more a function of a change in the canons of what a theory must do than of its failing abysmally to explain some new experiment … waves could be used to obtain results, whereas it seemed clear by then that this could not be done with light particles. By the end of the 1830s only a few diehards remained committed to the emission theory,[220] which had almost completely vanished from the journals of science. (Buchwald 1989, pp. xiii-xiv)

William Whewell, (1794-1866), hailed by some as the "father of modern philosophy of science," (Fonseca 2007), portrayed the pre-transition era in terms echoed later by Thomas Kuhn, (1962) as those typically foreshadowing paradigmatic change.

> When we look at the history of the emission-theory of light, we see exactly what we must consider as the natural course of things in the career of a false theory [which] may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition, - an addition to the machinery; and as observation goes on, these incoherent appendages accumulate till they overwhelm and upset the original framework. Such was the history of the [cosmological] hypothesis of solid epicycles; such has been the history of the hypothesis of the material emission of light ... There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters; the philosopher builds the machine, but the parts do not fit; they hold together only while he presses them; this is not the character of truth. In the undulatory theory, on the other hand, all tends to unity and simplicity. (Whewell 1858, p. 107)

The transition from atomist-emissionist optical modeling to a predominantly wavefront rendering of continuum modeling was not to be, however, without its difficulty. For the science of the time, as it had also been experienced in the past, the perception was one of mortal contest. At stake was believed to be the truth concerning

[220] So dramatic was the rate of Newtonian disaffection that, writing a little earlier (1833) in the same decade, Sir William Rowan Hamilton reported that the optical "theory [of Newton] was very generally received by mathematicians during the last century, and still has numerous supporters." (Hamilton 1966, p. 1033)

the very nature of supporting realities. Such truth was of necessity exclusive and its true explanation was to be uncontested. Viewed as each purporting realist explanations of immutable truths, apparent rivalry between basic conceptions could not be tolerated. Few, if any, could appreciate a wave concept, not as a destructive invader, but as a welcome opportunity to add richness to a suite of facilitating optical models, with each different analogy providing useful insights into solving different optical concerns. Wave modeling of polarization in particular sparked conflicts and encountered resistance deriving from "deep-seated views on the nature of rays and beams of light" held by obdurate adherents to a failing Newtonian legacy. (Buchwald, p. xv)

> Although …waves replaced light particles, a deeper process also took place at about the same time …While waves in the ether were replacing particles of light as tools of explanation, wave-fronts were also beginning to replace rays as tools of analysis. This second process was at least as difficult for many people as the first one because it required them to alter many fundamental optical concepts concerning the nature of a *ray* and its relation to a *beam* … many people had immense difficulty understanding that the *wave-front* is irreconcilable with the concept of an *isolatable ray* – that rays must be abandoned entirely as physical objects in their own right in order to deploy the mathematical apparatus of the wave theory. [Some] refused to admit that one cannot use rays in the old [Newtonian] ways. In the emergence of the wave theory to dominance … one finds a profound dichotomy in understanding as well as in conception: a dichotomy between the most elementary images of the nature of a ray, on the one hand, and a dichotomy between the physical models of light as particles and light as waves in the ether, on the other. (Buchwald 1989, p. xiv)

Thus in Buchwald's analysis of the causes of early nineteenth century optical conflicts, it was not simply a disparity of optical ray models but the miss-assignment of objective realism to an idealized representation that presented as the most intractable. The Newtonian orthodoxy, prior to Young and Fresnel, defined an optical *beam* as a collection of individual rays deemed as separable, countable and irreducible physical objects. Each ray exhibited an inherent asymmetry, with the beam intensity being the numerical summation of its constituents. The beam's degree of polarization was believed an averaged directional asymmetry determined by the number of rays, as groups, that held a particular alignment. A further specific assumption by many emission adherents, that rays of a polarized beam "are selectively rotated [during optical reflections and refractions] to new azimuths depending upon the color that corresponds to the particular ray," led Thomas Young to "call those who thought about light in this way *selectionists*, and the theory they espoused *selectionism*." (Buchwald, p. xviii) On the contrary, for the wave model, a beam was to be conceived as a small area of a wavefront viewed as a rapidly expanding optical surface wherein properties possessed by contributing wavelets endowed the properties of that portion of the front. If reduced in width, a beam's geometrical portrayal by a ray - as an idealized mathematical abstraction constructed normal to a diminishing wavefront area - yields the more effective representation of optical properties at a surface location. Such a beam then is neither a collection nor group nor capable of separability and dissection. According to Buchwald:

> ...the vast gulf between these concepts made it extremely difficult for scientists who did not think about polarization in the same way to communicate their experimental results to one another without leaving an inevitable residuum of ambiguity. This ambiguity could, and often did, lead to controversies[221] that ended in frustration and anger ... (Buchwald 1989, p. xviii)

However, for Peterfreund, (1994) the optical confrontations of the early nineteenth century and the "rigid resistance" of selectionists, turn on more than a *prima facie* conflict between a discrete physical ray versus a ray as abstraction. Beyond scientific arguments as to the relative merits of the optical models on offer, the Newtonian theologically inspired ideology that underwrote the selectionist position was not only that light was of necessity a discrete matter-based phenomenon, "but also that the matter in question satisfies the primary criterion of the identity position [as proposed by Natural Theology] – that is, it was created for a specific ... purpose – and as such is a material metonym[222] of a divine first cause." (ibid., p. 70) Of primary theological concern then was that "Light as an effect, caused not by a material ray but by a hypothetical wave, points the way toward a God that is himself hypothetical." (ibid., p. 68) Thus the "legacy left by Newtonian optics to Natural Theology [became] the basis for resistance to any attempt, such as that of Fresnel, to render the basis of light hypothetical rather than material and knowable." (ibid., p. 72)

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[221] Buchwald cites in particular " a striking public confrontation between Arago and Fresnel, on the one hand, and Biot, on the other" (Buchwald 1989, p. xviii)

[222] *metonymy,* as distinguished from *metaphor,* can be defined as a figure of speech wherein "the literal term for one thing is applied to another with which it has become closely associated. Thus 'the crown' or 'the scepter' can stand for the king ... The problem with the way in which metonymy operates, however, is that substitution tends to occlude agent in favor of act, or effect in favor of cause, or vice versa. One effect of this substitution is to reify the occluding term at the expense of the occluded term. The king is reified as his crown or his scepter – and, looking ahead to optical theory, light is reified as rays." (Peterfreund 1994, pp. 64-65)

The concretization of Huygens' wavefront process model

Although Augustin Fresnel (1788-1827) was born shortly before the French Revolution and had no formal education before the age of twelve, after successful graduation as an engineer, in his spare time he pursued his passion for physics in general and light in particular. Much of his early work addressed many standing objections to the wave modeling of light and confirmed and extended the work of significant contemporaries such as Young. Nonetheless his work was largely undertaken in academic isolation and in ignorance of their scientific contributions.

> He neither knew of the wave theories that had been postulated by Huygens, Euler and Young, nor did he know of the latest developments in the corpuscular theory supported by the majority of scientists. (O'Connor & Robertson 2002a)

When the French *Académie des Sciences,* in expectation of fostering an emission theory triumph, announced that its 1818[223] Grand Prix would be awarded to the best submission on diffraction, Fresnel entered the results of his extensive experimental and theoretical work. Much of Fresnel's initial approach was similar to that of Young, but had advanced to enunciate a new mathematical formulation which independently incorporated many of Huygens' earlier insights.[224] The eminent panel of judges, chaired by Dominique François Arago, included the prominent corpuscularian proponents Siméon-Denis Poisson, Pierre-Simon de Laplace and Jean-Baptiste Biot, each hostile to optical wave hypotheses and modeling. Nonetheless, Poisson "was fascinated by the mathematical model which Fresnel proposed and succeeded in computing some of the integrals to find further consequences." Arago then commissioned an experiment designed to test Fresnel's claim using Poisson's predictions.[225] In awarding the prize to Fresnel, Arago was to admit that: "The consequence [predicted by Poisson] has been submitted to the test of direct experiment, and observation has perfectly confirmed the calculation." (O'Connor & Robertson 2002a) Historically, the result not only lent invaluable assistance to the reemergence of optical wave modeling but had the additional effect of "adding back" or "concretizing" Huygens' original minimalist

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[223] Some historians list the event as relating to 1819 as in (O'Connor & Robertson 2002a) and (Vohnsen 2004)

[224] It is significant to note that whereas the Huygensian description was framed in the time domain, the Fresnel treatment was expressed in the frequency domain with "time differences *modulo* the oscillation period." (de Lang 1990, p. 27)

[225] Poisson made the remarkable deduction from computing some of Fresnel's integrals that a bright spot should appear in the centre of the shadow cast by a small opaque disk.

wavefront model. Huygens had also, in effect, employed a Galilean idealization[226] to offset the difficulties of engagement with rectilinear propagation by assuming that the interactions between secondary waves were impotent except at the wavefront.

> In his [prize-winning "Mémoire couronné" submission] on Diffraction ... Fresnel made an important extension of Huygens' principle, in that he replaced Huygens' isolated spherical [pulses] by purely periodic trains of spherical waves and made use of [Young's] principle of interference. On this [model], light ought to appear, not necessarily on the envelope of the secondary waves, but at every point where these secondary waves reinforce one another; on the other hand, there should be darkness wherever these secondary waves destroy one another. In this way, Fresnel was able to account, not only for the rectilinear propagation of light of very short wave-length and the laws of reflection and refraction, but also for certain diffraction phenomena. (Baker & Copson 1987, p. 20)

However, Poisson's projections had additional serendipitous results. Huygens had been aware that the spherical shell by which he had modeled the expanding optical wavefront consisted of both an outside and inside surface from which secondary waves could propagate not only forwards but also backwards. Here again Huygens had, in effect, invoked a Galilean idealization which assumed that only outward propagation from the outer surface need be considered. Poisson's analytical solution of the Huygens-Fresnel wave model did not give rise to a returning wavefront,[227] and Huygens' idealization of the geometrical construction was now shown justified. Also justified by Poisson's analytical solution was Huygens' idealization that secondary waves were ineffectual except at their mutual reinforcement at the wavefront envelope. (Baker & Copson 1987, pp. 3-19)

Although successfully demonstrating radiation processes such as sound waves in air, the initial Fresnel model, as with Huygens' earlier minimalist representation, took no account of optical polarization phenomena. The discovery by Malus in 1809 of polarization in the reflection of mirrors had been hailed a further corpuscular model confirmation and created "a small crisis for the wave theory of optics." (Römer 2005, p. 9) As Baker and Copson note, "Huygens' principle in its crudest[228] form takes no

[226] In the absence of an accepted modeling theory and consistent terminology, such Galilean idealizations employed by Huygens are described by Baker & Copson as "special *ad hoc* hypotheses." (Baker & Copson 1987, p. 3)
[227] However, as Baker and Copson point out, "an initial disturbance is actually propagated in both directions unless certain [initial] conditions are satisfied" that would then give rise to a "wave which is propagated in a definite [single] direction," and that a "similar conclusion holds for electromagnetic waves in vacuum." (Baker & Copson 1987, p. 3)
[228] Such thinly disguised disparagement betrays an historical and ongoing failure to recognize Huygens' original formulation as an idealized *scientific model*, and thence demands its later development as an

account whatever of the phenomenon of polarization, although this phenomenon was discovered by Huygens himself in his experimental work on Iceland spar." (1987, p. 6)

Although Huygens had taken precaution not to utilize what he believed to be unsound analogies in modeling aspects of light, and had wisely refrained from extending his representations to include a polarizing phenomenon which he frankly admitted to not understanding, Newton had embellished his own physical rays with 'sides' in an attempt to incorporate the phenomenon. Subsequent nineteenth century selectionism held firmly to physical orientation attributable to such conjectural ray properties. Thus further concretization of the Huygens-Fresnel model to include a wave-consistent representation of polarization would prove vital to its bid for comprehensive optical modeling recognition. Fresnel collaborated with a now more confident Arago to model polarization and in 1821 published with certainty the hypothesis, earlier conjectured by Young in 1817, that light was a transverse wave. Although, in the event, a yet vacillating Arago dissented from the claim, Fresnel consolidated his assertion by further demonstrating "that double refraction could be deduced from the transverse wave hypothesis." (O'Connor & Robertson 2002a)

Subsequently, Fresnel's independent concretization of Huygens' process of wavefront construction, now incorporating by "adding back" Young's interference proposition, coupled with a true periodicity, and a successful transverse wave model of polarization, spear-headed "a succession of investigations which, in the course of a few years, were to discredit the corpuscular theory completely." (Born & Wolf 1999, p. xxvii) Subsequent investigations included those of Joseph Fraunhofer (1787-1826) who furthered Fresnel's diffraction work, and the integral formula of Hermann von Helmholtz (1821-1894) that presented an analytical conception of Huygens' principle which held for the case of a 'monochromatic' disturbance emanating from a point source. (Baker & Copson 1987, p. 36)[229]

It remained then for the German physicist Gustav Robert Kirchhoff (1824-1887), to consolidate the concretization of the Huygens-Fresnel principle by resolving most of the residual difficulties and to present it in a more tractable form. Additionally, Kirchhoff effectively incorporated particulars from investigations by such as Poisson and

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'incomplete' or 'bad' ***theory.*** (de Lang 1990, p. 21) Such commentary effectively tarnishes much otherwise useful historical review. As de Lang notes, "in order to obtain a balanced view on the physical significance of Huygens' Principle, we cannot rely on such historiographic literature."

[229] It is to be noted that the notions of a 'point source' and 'monochromism' are both idealized modeling concepts. (Baker & Copson 1987, p. 68)

Helmholtz [230] into a consistent and more general representation of scalar radiation and diffraction processes. So successful were Kirchhoff's syntheses, simplifications and diffraction insights that the "Huygens-Fresnel-Kirchhoff wave-front diffraction formulation"[231] has survived the revolutions and paradigm changes visited upon the physical sciences in the ensuing century and a half, with almost no need for further modification.[232]

Unfortunately, however, the formulation has often been characterized as "the wave theory of light," (Boorse & Motz 1966g) a description that has raised for some, expectations of an ability to explain details otherwise particular to opto-electromagnetic theory. On ocassion, Huygens' principle, has been belittled, and even dismissed, for failing an expected theory-responsible task of addressing the 'why' of luminous behavior. Cited by Kevin Brown from "*Principles of Electrodynamics*," Melvin Schwartz wrote:

> Huygens' principle tells us to consider each point on a wavefront as a new source of radiation and add the "radiation" from all of the new "sources" together. Physically this makes *no* sense at all. Light does not emit light; only accelerating charges emit light. Thus we will begin by throwing out Huygens' principle completely ... (Brown 1999d)

Although admitting that much of its history has been found in the context of optical discussions and controversy, it might prove less confusing to suggest that, as an empirically successful theory, Huygens' principle has emerged into modern science as a well-corroborated continuum *theory of propagation*. (Enders 1996, p. 226) When combined then, with appropriate elements relevant to various sciences, it has augmented modeling for predictively successful theory constructions in optics, (Born & Wolf 1999, p. 424; Kraus 1990; Möller 2003, p. 129),[233] medical ultrasound imaging (Angelsen

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[230] Baker and Copson show that Poisson's formula, which justified Huygens' principle for an isolated wave issuing from an initial disturbance, and Helmholtz's formula which models a monochromatic wave train issuing from a point source, are particular cases of Kirchhoff's more general theorem. (Baker & Copson 1987, p. 36) Further, expanding on Poisson's findings, Greene notes that Kirchhoff's formula showed that the intensity of 'backward reflected wavelets' [or more correctly 'advanced potentials'] is zero for solutions in domains of both 1 and 3 dimensions. (Greene 2004) In two dimensions, one finds "diffusion of waves. One can see it when a pebble falls in water: the front wave ...will be followed by the so-called residual waves ..." (Veselov 2002)

[231] also referenced in the literature of scalar wave diffraction theory as the "Kirchhoff-Fresnel Integral" (Möller 2003, pp. 129-130) or the "Fresnel-Kirchhoff diffraction formula" (Baker & Copson 1987, p. 73)

[232] The eminent Italian mathematician, Giacinto Morera (1856-1907), unfortunately perhaps less known outside of his native Italy, "improved the proof of the Kirchhoff formula for the Huygens principle" (Lucia 2006) in a work published in 1895 in which the solution was finally rendered quite general (Morera 1895).

[233] However, it should also be remembered that, although Kirchhoff's formula "does give results which agree very well with experiment," it is nonetheless a model employing appropriate idealizations, some of which derive from Kirchhoff's use of Helmholt's idealized point-source and 'monochromism' and yet

2000, p. 1.10), seismology (Shearer 2006), and acoustics. (Fink 1994) Pioneers such as Erwin Schrödinger and Richard Feynman also claimed to have invoked the principle as integral to their early modeling of quantum mechanics. (Feynman 1948, p. 377; Wolf 1990, pp. 15-16)

Subsequent nineteenth century optical investigations were also to include a now famous "*exprimentum crucis*," - a test of the relative speed of light after entry into a denser optical medium, and the definitive word on interpretations of Aristotle's infamous 'ball analogy.'

> Fresnel's work had put the wave theory on such a secure foundation that it seemed almost superfluous when in 1850 Foucault and Fizeau and [independently] Breguet undertook a crucial experiment first suggested by Arago. The corpuscular theory explains refraction in terms of the attraction of the light-corpuscles at the boundary towards the optically denser medium, and this implies a greater velocity in the denser medium; on the other hand the wave theory demands, according to Huygens' construction, that a smaller velocity obtains in the optically denser medium. The direct measurement of the velocity of light in air and water decided unambiguously in favour of the wave theory. (Born & Wolf 1999, pp. xxviii-xxix)

Ultimately, Newton's theory of light was not abandoned because Newton's corpuscular model was derived from theological precepts, nor that, in practice, Newton's uncompromising convictions effectively denied his theory falsifiability, nor that the wave model was believed increasingly effective, but for the fundamental reason that his theory, when finally assessed by appropriate experiment, proved to be predictively unsuccessful. Predictive failure was in turn held to demonstrate an inadequacy of innate properties of the classical particle-emission model as suitable for the construction of a valid theory of light. Atomist particle and reified ray models of light together with the emission-styled optical theories they nurtured, had sustained a near mortal blow and would not be seriously entertained again until physicists such as J.J. Thomson, Walter Ritz and Albert Einstein revived similar interpretations of light, including "directed bundle[s] of rays," (Einstein 1917, p. 892) early in the twentieth century.

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others are Kirchhoff's own Galilean idealizations employed to astutely manage complexity. Some authors claim that "the formula gives the first [usefully approximate] step in a an accurate solution by [an iterative process of] successive approximations... Other authors, notably Kottler, regard Kirchhoff's formula as an accurate solution, not of a boundary value problem, but of a 'saltus problem' [which involves prescribed discontinuities]." (Baker & Copson 1987, pp. 71-72)

Chapter 6
The Optical-electromagnetic unification

In the centuries following Newton's authoritative presentation of the fundamental principles of mechanics, physics had developed somewhat unevenly, with pursuits such as celestial mechanics being favored by prominent mathematicians over optics. Developing almost independently of optics, however, research into electrical and magnetic phenomena had become particularly fruitful during the nineteenth century. Specific modeling of electromagnetic induction and introduction of the concept of the 'field' by Michael Faraday (1791-1867) was now claimed one of the greatest of all scientific achievements.[234] It was, however, the Scottish physicist James Clerk Maxwell, (1831-1879) who eclipsed Faraday and incorporated his discoveries. In his synthesis of all prior electromagnetic understandings, Maxwell succeeded in reducing the complexities, disparate approaches and wealth of detail of so large a subject matter, to a concise system of four[235] fundamental equations. In his synthesis, Maxwell comprehensively described "electricity, magnetism, space, time and the relationships among them," together with fundamental properties of both detectable electromagnetic media and vacuum. (Wolfe & Hatsidimitris 2006b) Here, Maxwell's departure from prior optical and electromagnetic approaches is recognizable

> ... in the manner in which he introduces the electromagnetic field. Previously, physicists had considered space as empty, and spoke of lines or tubes of force between charged particles as mere mathematical fictions. Maxwell, however, imparts a reality to the electromagnetic field that goes far beyond a mere mathematical convenience. This is one of those giant steps in science that herald a revolution in scientific thinking and mark the emergence of a great genius. (Boorse & Motz 1966d, p. 332)

[234] Faraday's deficiency in mathematics "forced him to present physical phenomena in terms of physical models rather than by means of abstract mathematical formulas ... his idea of 'tubes of force' stemmed directly from the need for a model, and the model of the field with its 'tubes of force' in turn led him to the explanation of induction as a cutting of the tubes of force." (Boorse & Motz 1966b, p. 319)

[235] In their nascent form they are a set of eight first-order partial differential equations deriving from and summarizing twenty separate equations (Maxwell 1865, p. 342) and which depend on an arbitrary choice of coordinate axes as required to fully define the magnitude and direction of both electric and magnetic field vectors. By invoking 'vector field theory,' the equation group can be written both as a coordinate-free system, and as a simpler and more tractable set of four comprehensive equations of electrodynamics. (Fitzpatrick 2006a) Incorporated in the equation group is Faraday's law describing electricity generation (that changing magnetic fields produce electric fields), Gauss' and Coulomb's laws of electrostatic interaction, the Ampère-Maxwell law descriptive of the magnetic field, (that changing electric fields produce magnetic fields), Gauss' law for magnetism and the law of Biot-Savart. (Breinig 2008a; Wolfe & Hatsidimitris 2006b) Maxwell's equations are notated in different ways for use in different circumstances. The principal conventional notations employ the integral operator, $\oint$, to render the equations in integral form whereas ∇ ('del' or 'nabla') is utilized as a spatial derivative operator for expression in both differential and symmetric forms.

Nonetheless, as with many physicists before him, Maxwell mistakenly reified analogical representations as concrete identities, believing that all wave-like propagations depend on a physical transport medium. For the electromagnetic field, Maxwell conjectured a pervasive elastic aethereal substance “of small but real density,” (Maxwell 1865, p. 338) capable of energy interchange with the field and through which electromagnetic waves could propagate by traditional wave modeling. Pursuing a formulation that he believed should govern the propagation of such projected electromagnetic waves, Maxwell ‘solved’ his suite of field equations with respect to time. Properties of the resulting wave equation were to prove an axis point in the history and philosophy of science. Not only did the wave equation require that the direction of an oscillating electric field be always orthogonal to a complementary magnetic field, but that the electromagnetic disturbance was also transverse to the direction of wave propagation. Such a configuration supported modeling previously advanced first by Young and then consolidated by Fresnel as obtaining for the optical wave. Further, by 1846, Wilhelm Weber had introduced a constant now universally recognized as ‘c.’ Weber’s constant had the unit of velocity but was derived from the static ratio of the electromagnetic and electrostatic units of charge. (Assis 2003) Both were measurements of media then quantifiable by available laboratory practice, but above all they quantified a propagation speed of an electromagnetic disturbance for each medium, a fact that Kirchhoff had also independently realized. (Ellis & Uzan 2005) Inserting values for the ‘specific dielectric capacity’ and the ‘specific magnetic capacity,’ as measured for vacuum for the first time by Rudolph Kohlrausch and Wilhelm Weber in 1855, a propagation rate for vacuum emerged[236] that in Maxwell’s words was,

> ... so nearly that of light[237], that it seems we have strong reason to conclude that light itself [including radiant heat, and other radiations, if any] is an electromagnetic disturbance in the

[236] In the International System of Units (SI) notation, the electromagnetic propagation speed in physical media is denoted by ‘*c*’ deriving possibly from English ‘***constant***’ or Latin ‘***celeritis***’ [swiftness]. The ‘speed of light in vacuum’ or ‘free space’ is more specifically denoted c_0 and is defined in SI units (since 1983) as 299,792,458 $m.s^{-1}$ The ‘specific dielectric capacity’ became known as the *dielectric constant* or *electric permittivity* denoted by ‘ε’, whereas the ‘specific magnetic capacity’ became known as the *magnetic permeability* denoted by ‘μ.’ For true vacuum these electromagnetic measurements are designated the *electric constant*, ε_0 (also called the *permittivity of free space*) and the *magnetic constant*, μ_0 (also called the *vacuum permeability* or *permeability of free space*) which has been defined in SI units as $\mu_0 = 4\pi \times 10^{-7}$ N/A^2 (Mohr, Taylor & Newell 2007, p. 94) In SI units the speed of all electromagnetic radiation in free space is related to these measurements by the equation: $c_0^2 = 1/\varepsilon_0 \mu_0$ deriving from the differentiation of Maxwell’s equations with respect to time.

[237] Maxwell’s estimated wave propagation speed was ≈ 284,000 km/s. (Spring et al. 2004) The best experimental estimates at the time were the (terrestrial) ‘toothed wheel’ results of Hippolyte Fizeau in 1849 (≈ 313,000 km/s) and Leon Foucault’s improved rotating mirror method of 1862 claiming ≈ 298,000 km/s.

> form of waves propagated through the electromagnetic field according to electromagnetic laws. (Maxwell 1865, p. 343) [238]

In 1888, Heinrich Hertz (1857-1894) made the crucial discovery[239] of the electromagnetic phenomenon of radio waves and proved that such waves were both transverse and propagated at the same speed as had been established for light. Maxwell's conjectured waves had been empirically verified. The advantages to both optical and electromagnetic studies through their mutual identification were immediate and generous, with the study of physical optics being "gradually subsumed within electromagnetic theory." (Warwick 1991, p. 29) However, although a vectorial electromagnetic wave model had been added to existing optical facilitating models, such would seemingly have to be at the expense of the earlier successful scalar wave representations.

> Until the time of Young and Fresnel, light was regarded as a disturbance in a medium analogous to that of sound in air. We know now that the propagation of light is of an entirely different character from that of sound. To specify a light wave, we need to know the three components of the 'light-vector', whereas a sound wave is specified by a single quantity, the scalar velocity potential. There is, then, no precise analogy between the propagation of sound and the propagation of light. (Baker & Copson 1987, p. 6)

Nevertheless, although a more rigorous vector-based derivation of the Huygens-Fresnel-Kirchhoff propagation process from electrodynamic principles was delivered by Hendrick Lorentz, (Römer 2005, p. 10) in practice, appeal to a Galilean idealization[240] permits a continuing pragmatic employment of the more tractable scalar formulation:

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[238] Vohnsen notes that in 1867 Lorentz had also reached this conclusion on the basis of his work on electrodynamics (2004, p. 78) However, although not directly relating to vacuum, it appears that perhaps priority in the discovery that electromagnetic phenomena propagate (at least in circuits of negligible resistivity) at a velocity essentially that of light, should be awarded to Gustav Kirchhoff (1857) and independently to Wilhelm Weber, both of whom published such findings prior to the Maxwell result.

[239] According to Coey, Hertz was anticipated, at least in concept, by Irish physicist George FitzGerald, who, three years earlier had suggested the production of 'Electromagnetic Disturbances of Comparatively Short Wavelengths' by "utilizing the alternating currents produced when an accumulator is discharged through a small resistance." (Coey 2000) In Arianrhod's account, even FitzGerald was anticipated by David Hughes, who, in 1880, claimed to have found evidence of electromagnetic waves, "but the mainstream prejudice against Maxwell's theory was such that Hughes was dissuaded from pursuing his experiments." (Arianrhod 2003, p. 236) "Lodge also managed to demonstrate electromagnetic waves at the same time as Hertz – although Lodge's waves were along wires rather than in free space." (ibid. , p. 265)

[240] For electromagnetic fields defined by Maxwell's equations, each secondary source of a Huygens-Fresnel principle should give rise to an *electromagnetic* wavelet. Although the scalar Kirchhoff formula can be applied to each of the electric and magnetic vector components independently and thus adequately define the existence and location of the expanding wave-front, the secondary disturbances so obtained are not rigorous solutions of Maxwell's equations. An attempted solution by Larmor and Tedone, although again of practical value, suffered from a similar deficiency. (Baker & Copson 1987, pp. 102-114)

There is a question of why we should use a summation process based on the idea of Huygen's Principle to describe [modern] diffraction theory. Why not solve Maxwell's equations with the appropriate boundary conditions? The [scalar] mathematical formulation of Huygens' Principle was performed by Gustav Kirchhoff and Augustin Jean Fresnel before Maxwell's theory was developed. It turned out that the use of the Kirchhoff-Fresnel integral for many applications is so much easier than solving Maxwell's equations and applying boundary conditions, that one just continues to use the scalar wave diffraction theory." (Möller 2003, p. 129)

Modeling the luminiferous aether and the decline of mechanism

The classical laws of physics as formulated in Newton's *Principia* of 1687, described mechanics and kinematics relative to an idealized model of space and time – the 'inertial reference frame.'[241] Such a referencing system utilized both Galilean[242] and minimalist idealizations to examine 'mass point' representations of inertial objects in an idealized spatial domain devoid of all extraneous matter and accelerations due to forces such as gravity or rotation. The scheme facilitated location by a convenient Cartesian three-dimensional co-ordinate mapping of a geometrically 'flat' spatial universe based on the familiar and generally more intuitive Euclidean geometry. A unique reference frame was then to be assigned to each object under consideration and in which the object was deemed to be at rest. Inertial frames of any such objects in relative motion can then be related such that they translate in fixed directions and at constant speeds with respect to each other. In the classical Newton-Galilean context, time was considered to be a constant and was described relative to a universal or absolute Time. Specific also to the Newtonian "absolutist" interpretation, as distinct from "relativists" such as Leibniz and Huygens[243] who concluded any body 'at rest' to be equivalent to

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However, "a scalar wave approximation that is a mathematical expression of Huyghen's Principle (the Fresnel-Kirchhoff Diffraction Integral) is very fruitful, and dominates the discussion in optics textbooks ..." (Calvert 2000b)

[241] Although the term "inertial reference frame" as such was first employed in the nineteenth century, (DiSalle 2002, §1.1) the physical elements and philosophical intentions of the seventeenth century mechanists are implicit in such as Galileo's uniformly moving ship.

[242] Terrestrial 'laboratory' frames are not strictly 'inertial' in that they partake of various rotational and non linear motions of the earth including gravity. Systems such as Foucault's pendulum show dynamic effects on a body seen from a weakly non-inertial frame. Fortunately, for many laboratory experiments, the fact that the frame is not strictly inertial makes insignificant measurable difference and in the spirit of Galilean idealization, can be safely ignored.

[243] Where Leibniz apparently had difficulty comprehending force and inertia in a "Galilei-invariant way," in the 17th century, the acceptance of a [comprehensive] "equivalence-class structure as the fundamental spatiotemporal framework" and its required abstract view of geometry, was exhibited "only [by] Christiaan Huygens ... [who] held that not velocity, but velocity-difference, was the fundamental dynamical quantity." (DiSalle 2002, §1.4)

any other, (DiSalle 2002, §1.2, 1.4) is that of a privileged universal frame 'at rest.' Relative to this resting frame, the 'absolute' motion of any inertial object would be notionally measurable. In practical application, Newton "chose a Cartesian reference system with its origin at the center of the Sun, while its three axes were directed toward distant stars ... [and] considered such a reference system to be 'motionless'." (Logunov 2005, p. 7)

Although a 'speed of light' had been reasonably well quantified, the reference frame in which light propagated with its Maxwellian speed was still conjectural. (Vohnsen 2004, p. 79) The long perceived requirement of a universal 'aether' medium to support a variety of physical phenomena including optical waves, became identified by some, such as Oliver Lodge, with the Newtonian universal 'rest' frame.[244] (Hirosige 1976, p. 42) The universality of such an aether, however, remained unquestioned, as Maxwell claimed in his 1878 contribution to the Encyclopaedia Britannica:

> Whatever difficulties we may have in forming a consistent idea of the constitution of the aether, there can be no doubt that the interplanetary and interstellar spaces are occupied by a material substance of body. (O'Connor & Robertson 2002c)

The 'difficulties' referred to were, nevertheless, extreme. Properties required of such an aether were contradictory and incommensurate with any known material. To carry the extremely high vibrations accorded to light, the substance needed an equally extreme rigidity, yet its density needed to be such as to not hinder the motion of the planets. Moreover, different phenomena appeared to demand different properties. Cauchy, Stokes, Thomson and Planck all postulated ethers with differing properties and by the end of the nineteenth century, "light, heat, electricity and magnetism all had their respective ethers." (O'Connor & Robertson 1996) In an effort to construct a consistent electrodynamic theory, Maxwell explored the insights offered by various mechanical models. Initially it seemed that a physical representation of Faraday's field required an ambient ether as a substratum, but not a constituent, of the field. However, the electromagnetic wave model suggested an elastic aether subject to distortion, whereas the Kerr magneto-optic effect suggested rotations, analogous to vortices in a fluid, as suggested by William Thomson. In response, Joseph Larmor "posited a physical model of a fluid, rotationally elastic, and incompressible," and Maxwell translated such a system of fluid vortices into a mechanical model in which the action of particles in the ether represented the action of the field in transmitting forces. Another scientific model,

[244] It is of interest that Maxwell recognized deficiencies in all aether propositions except one. In his 1879 article, Maxwell claimed: "The only ether that has survived is that which was invented by Huygens to explain the propagation of light." (Keswani & Kilmister 1983, p. 345)

advanced by George Fitzgerald and composed of "rubber bands and wheels," was intended to "represent the connection between ether and matter by means of a mechanism, thereby providing a mechanical illustration of ether strain ..." Ultimately Maxwell abandoned attempts at mechanical modeling and embraced "methods of Lagrangian analytical dynamics to represent the electromagnetic field instead," claiming that the "problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions." (Greco 2002) However:

> By the end of the century, and largely as a result of the sweeping Maxwellian synthesis, electrodynamics had replaced mechanics in providing the conceptual foundations of physics. In the new electrodynamic ontology, the field had an independent physical reality denuded of mechanical properties. (Greco 2002)

Search for the illusive luminiferous aether

Included in his 1878 Encyclopaedia Britannica article, Maxwell advanced a plausible, but potentially for that time, impractical, means of determining the earth's velocity relative to the luminiferous aether, using light.[245] Modelled on the more recognizable analogy of two equivalent swimmers, one swimming upstream against the current of a river and returning to the start whereas the second swimmer, beginning at the same instant, traversing the same distance and swimming at the same rate, swims across the stream and also returns. A basic mathematical analysis shows that the swimmers would not meet back at the starting point at the same time, the temporal difference being mediated entirely by the rate of the river's current flow. Based on this discrimination of flow rate from all other elements, Maxwell suggested:

> Split a ray of light ... and send the two resulting rays at right angles to each other. Let one travel at right angles to the motion of the earth through the aether. Reflect the two rays back after each has travelled exactly the same distance to join up again and let them interfere. (O'Connor & Robertson 2002c)

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[245] This same article also "tells of a failed attempt by Maxwell to measure the effect of the ether drag on the earth's motion" and proposal of "an astronomical determination of the ether drag by measuring the velocity of light using Jupiter's moons at different positions relative to the earth." (O'Connor & Robertson 1996) Maxwell also described here experiments aimed at detecting an effect of the earth's motion on the sodium D1 and D2 Fraunhofer lines from starlight performed in the period 1864-7, but states: "The experiment was tried at different times of the year, but only negative results were obtained." (Keswani & Kilmister 1983, p. 345)

On the assumption that the aether was an isotropic medium wherein each optical beam would travel at the same speed, the "aether wind," analogous to the river's current, should exhibit 'anisotropy of the ray speed.' (Brown 2001, p. 3) One beam should return slightly ahead of the other and become discernable as changes in their recombined interference pattern. A quantitative assessment of the earth's speed relative to the aether could then be derived as proportional displacement of the interference fringes. Previous 'first-order' attempts to detect the earth's velocity as a direct fraction of the velocity of light (as in the 'Bradley aberration' phenomenon) had failed. (Curtis 1911, p. 220; Poincaré 1905a, intro) Fresnel, in 1818, had investigated the potential influence of the earth's motion on the propagation rate of light and developed a hypothesis of 'partial convection of the luminiferous aether by matter' which subsequently appeared confirmed by a Fizeau experiment using moving water. (Born & Wolf 1999, p. xxviii; Fizeau 1860) Future first-order null results were routinely excused by this "Fresnel drag coefficient" (Brown 2003, p. 3) supposed to exhibit for light speeds inside moving transparent media. Maxwell's proposed method, however, offered a greatly improved 'second-order' sensitivity.[246]

In pursuit of a possible implementation of such an *experimentum crucis* of aether-drift measurement, Maxwell wrote in 1879 to David Peck Todd of the U.S. Nautical Almanac Office in Washington. His request came to the attention of Albert A. Michelson, a young naval instructor who had recently been transferred to that office. Taking study leave in the Berlin laboratory of Helmholtz in 1881,[247] Michelson attempted Maxwell's experiment using his own invention now universally called the 'Michelson interferometer' which incorporated orthogonal optical return pathways and their recombined interference detection. He observed no difference in the travel time of the two rays and published a "null" result.[248] Michelson nevertheless suspected that perhaps either his apparatus lacked sufficient precision or that the earth 'dragged' the aether along with it in a relationship similar to that between the earth and its atmosphere. Both Lord Rayleigh and Thomson wrote to Michelson, urging him to repeat the experiment with greater precision. In response, Michelson teamed up with a chemist and highly skilled experimenter, Edward W. Morley. Collaborating in 1887 at

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[246] If 'v' represents the velocity of the earth relative to a 'stationary' aether frame and 'c' the constant velocity of light in that frame, then first order estimates would deal with the ratio v/c of $\approx 1 \times 10^{-4} : 1$, whereas second order estimates would deal with the ratio v^2/c^2, with a relative resolution of $\approx 1 \times 10^{-8}$.

[247] The original experiment was conducted at the Helmholtz laboratory but "the vibrations of the city traffic made it impossible to get steady fringes. The apparatus was transferred to the observatory at Potsdam," where Michelson states: "I got a zero result in Potsdam." (Michelson et al. 1927, p. 344)

[248] In Michelson's almost terse summary, "The result of the hypothesis of a stationary ether is shown to be incorrect, and the necessary conclusion follows that the hypothesis is erroneous." (Michelson 1881, p. 128) This result was dismissed in 1886 by Lorentz, who criticised the experiment and doubted its accuracy. (O'Connor & Robertson 1996) Lorentz repeated a summary of this critique in his 1895 paper. (Lorentz 1895, pp. 3-4)

the Case School of Applied Science in Cleveland Ohio, Michelson and Morley constructed a new interferometer giving a tenfold increase in path length and repeated the Maxwell experiment. The extreme precision of this new instrument, capable of discerning a difference of one part in ten billion, (Holton 1969, p. 135) was believed capable of giving a positive result of about 0.4 fringe displacement for a supposed orbital motion of earth relative to a 'stationary' aether. In the event, less than a 0.005 fringe displacement was recorded for 360^0 rotations of the apparatus repeated many times and over full rotations of the earth, and eventually for seasonal variation of the earth's orbit around the sun. (Michelson & Morley 1887) By 1885, Lorentz had convincingly shown that necessary consequences of a supposed aether 'drag' made that model implausible. The negative result could not then be accounted for by aether drag, experimental inaccuracy or equipment imprecision.[249]

Similar optical experiments to detect an aether-wind were repeated many times over the ensuing fifty years, including those by Morley & Miller with four-fold sensitivity during 1904-5; Miller at Mt. Wilson; Tomascheck using starlight (1923); Kennedy also at Mt. Wilson; Illingsworth with a claimed precision of 1/1500 fringe shift (1927); Piccard & Stahel at Mt. Rigi, and Joos, (1930) with even greater precision. In an era of improved technology, Essen (1955) performed a microwave analog of the Maxwell optical experiment, Jaseja *et. al*. (1964) compared the monochromatic infrared frequencies of two masers at right angles, and Brillet and Hall (1979) used a helium-neon laser to improve precision by a factor of 4000. Although both Miller and Illingsworth claimed small positive results in fringe displacement,[250] a majority of physicists maintained that no compelling evidence had emerged demonstrating that such data necessitated measured variations in the speed of light and that the negative, or "null," result had persistently been reaffirmed.

Although the interferometer result contributed to awarding Michelson the Nobel Prize in 1907, (Hasselberg 2005)[251] it directly confronted the absolutist Newtonian world-view and brought classical physics to the brink of crisis. Not only did the result

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[249] It is of consequence however, that, as for experimental results in general at this time, no calculated estimate of experimental uncertainty was provided for the earlier 'aether-drift' results. Ultimately, the significance, or otherwise, of a 0.005 fringe result turns on a valid estimate of such experimental uncertainty. (Roberts 2006, p. 8) Lorentz took advantage of the situation: "the experiments of Michelson and Morley, in consequence of unavoidable errors of observation, afford considerable latitude for the values ..." (Lorentz 1895, p. 7)

[250] Although claiming his result "corresponds to an ether drift velocity of about one kilometer per second," Illingworth was cautious adding: "Since in over one half the cases the observed shift is less than the probable error the present work cannot be interpreted as indicating an ether drift to an accuracy of one kilometer per second." (Illingworth 1927, p. 696)

[251] "... for his optical precision instruments and the research which he has carried out with their help in the fields of precision metrology and spectroscopy." (Hasselberg 2005)

cast doubt on the existence of a universal motionless aether and confront belief in a privileged, universal, reference frame, the result, as interpreted by most physicists, required that the speed of light was in contradiction to the fundamental tenets of the classical Galilean velocity-addition law. The vacuum velocity of light appeared to be a constant *independent of the motion of the observer*. As Whitehead assessed the counter-intuitive situation:

> The central point of the explanation is that every instrument, such as Michelson's apparatus as used in the experiment, necessarily records the velocity of light[252] as having one and the same definite speed relatively to it. I mean that an interferometer in a comet and an interferometer on the earth would necessarily bring out the velocity of light, relatively to themselves, as at the same value. This is an obvious paradox, since the light moves with a definite velocity through the ether... For example, consider two cars on a road, moving at ten and twenty miles an hour respectively, and being passed by another car at fifty miles an hour. The rapid car will pass one of the two cars at the relative velocity of forty miles an hour, and the other at the rate of thirty miles an hour. The allegation as to light is that, if we substituted a ray of light for the rapid car, the velocity of the light along the roadway would be exactly the same as its velocity relatively to either of the two cars which it overtakes (Whitehead 1938b, p. 140)

As Thomas Kuhn has noted, for perceived crises of which that created by the Michelson and Morley (MM) experiment is deemed a significant exemplar, (Holton 1969) the scientific community of the late nineteenth century reacted as predictable by all historical precedent. Faced with what was perceived as a challenge to the basics of the embedded Newton-Galilean system, a plethora of hypotheses was propounded which served to retain the *status quo* by extensive, sometimes *ad hoc*, but often ever more complex re-articulation of the aether paradigm.

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[252] More precisely, such an interferometer compares the velocity of light in one arm against the velocity in an orthogonal arm and historically found them both constant and equal under all orientations of the system and over periods of time. However, a *quantitative* value for this constant velocity was not furnished by this method. (Brown 2001, p. 10) Nonetheless, during 1878-9, Michelson, after discussion with Professor Simon Newcomb, renowned astronomer and superintendent of the navy's *Nautical Almanac,* (Oldford 2000) constructed a more sophisticated version of Foucault's rotating mirror apparatus, improving both accuracy and precision to record a terrestrially derived value of 299,909 ± 50 km/s. In 1891, Michelson created a large scale interferometer using the Lick Observatory telescope in California and later in 1926 incorporated a modified interferometer into the rotating mirror methodology to achieve an improved result of 299,798 km/s. (Spring et al. 2004)

Invariance and the modeling of electrodynamics
Emission model responses

Despite the extensive philosophical and empirical considerations that had attended the demise, if not extinction, of optical particle and emission theories as not deserving of further scientific consideration, early twentieth-century history records their phoenix-like resurrection. Hypothetical atomist and emission-like solutions began appearing, each addressed to solving enigmas surrounding the 'electrodynamics of moving bodies'. Once again, the underlying representational approach to light was to be found as exhibiting a basic dichotomy of contradictory concepts. Yet again, such concepts were almost universally regarded as explanatory depictions with some degree of realist pretension. As fiducial explanations, such concepts were again regarded as mutually exclusive, and so yet again the dichotomy nurtured antagonisms and misunderstandings. Once again, rather than additional and complementary analogies being brought to bear on resolving appropriate optical problems, dissention and analogical misapplication dogged the path to successful theory construction.

Included in the neo-atomist resurgence were the more prominent formulations advanced by Sir J.J. Thomson in 1903, Albert Einstein in 1905, Walter Ritz in1908, Richard C. Tolman (1910), and O.M. Stewart. (1911). Other notable emission-based hypotheses were proposed by Daniel F. Comstock, (Martínez 2004b, p. 15) and Jakob Kunz, who published "*An Attempt at an Electromagnetic Emission Theory of Light*" in 1914. Underlying such hypotheses in general, radiation was again found modeled as physical particles and projectiles essentially exhibiting the atomist properties of separability and 'simple location.' However, other intrinsic properties, their size and constituency, their motion relative to their emitter and their mode of generation and termination, were not equally restricted, and found a variety of expressions as did sanction sought for their virile reemergence.

Perhaps occupying a principal role in revived emission-model apologetics was the recognition that such an approach did not demand a transportation medium and could thus equitably avoid the mounting difficulties attending an aether concept.[253] Beyond this, recent identification of 'point sources' of electromagnetic disturbance such as the localized electron, suggested the possible inclusion of optics within an expanded electromagnetic theory of matter. Importantly, however, since reflective surfaces were

[253] As Brown views it "[the aether's] main role was increasingly just that of providing the inertial frame of reference relative to which the fundamental electromagnetic field equations of Maxwell were postulated to hold." (Brown 2005, p. S86)

deemed to re-emit radiation with a source dependent velocity, the model offered a simple and yet elegant solution to the MM result:

> Since the various parts of the apparatus are moving with the same speed, in the same direction, their relative motions with respect to each other are cancelled out. Therefore, the observer can notice only the Maxwellian speed of light *c* ... (Cyrenika 2000, p. 92)

Principal among variations of the general emission style was the 'ballistic' model which takes its essential characterization from analogy to the action of firearms ballistics. As Tolman explained: "All [ballistic] emission theories agree in assuming that light from a moving source has a velocity equal to the vector sum of the velocity of light from a stationary source and the velocity of the source itself at the instant of emission." (Tolman 1912) Such models are based on adherence to the tenets of Galilean kinematics and hence deemed to obey its vectorial velocity addition rule.[254] In overview:

> If only the general principles are considered, [ballistic] emission theories then constitute one single theory built upon two assumptions:
> 1. The Galilean [relativity] theorem is applicable to electromagnetic radiation
> 2. Electromagnetic radiation propagates ballistically
>
> The second assumption can be eliminated, if the addition of velocities, in the theorem, is taken to imply ballistic propagation ... To simplify the quantitative treatment ... it should be assumed that ... a basic emitter emits the fundamental elements of its radiation, with constant speed, at regular intervals of time.[255] The speed of the radiation is [then] constant with respect to the inertial frame of the basic emitter. (Cyrenika 2000, p. 90)

Such considerations appear to have occupied Thomson in the aftermath of his 1897 conclusion that the atoms of all matter contained universally similar electrified sub-atomic 'corpuscles,' later named electrons.[256] Taking advantage of the forum provided by Yale University's May 1903 Silliman Lectures, Thomson canvassed a discontinuous model of light and offered his theorem of the 'Change of Velocities' developed to accommodate optical phenomena such as reflection described from a ballistic stance.

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[254] by analogy, for a bullet fired forward with muzzle velocity v from a gun mounted on a tank travelling at velocity u, the bullet velocity relative to the earth would be $u + v$.

[255] for ballistic models, the wave model equivalent of 'wavelength' is "defined as the distance between two successive elements of radiation." (Cyrenika 2000, p. 90)

[256] The term 'electron' was coined in 1891 by G. J. Stoney as "the unit of charge found in experiments that passed electric current through chemicals." For Thomson, these corpuscles claimed a role in his 'plum pudding' (or 'raisin cake') model of the atom wherein by analogy, the atom was represented by a 'pudding' of massless positive charge in which a swarm of negatively charged corpuscles as 'plums' rotated in circular orbits. (AIP 2008)

(Cyrenika 2000, p. 91) However, the more robust and extensive ballistic treatment came from a young Swiss theoretical physicist, Walter Ritz (1878-1909) who studied at the Zurich Polytechnikum contemporaneously with Albert Einstein, and who was one year his junior but graduated a year earlier in 1900. At Leiden University Ritz attended lectures by H. A. Lorentz on electron theory and electrodynamics. However, at "Leiden and later he became increasingly antagonistic to Maxwell's theory in general and to Lorentz's electrodynamics in particular." (Martínez 2004b, p. 6) By 1908 Ritz began publishing objections and counter arguments to then prevailing approaches in electrodynamics and optics. For Ritz, Maxwell's continuum based equations required radical revision, being at the root of difficulties surrounding the electrodynamics of moving bodies. As remedy he offered an emission hypothesis of light that held consistent with classical mechanics wherein the speeds of its elementary components varied "depending on the motion of their sources at the instant of emission, as with any other projectile." (Martínez 2004b, p. 4) In summary Ritz noted:

> There are two ways to represent [optical] phenomena. Two distinct images have successfully ruled Optics: the one of emission (the light moves) and the other of ether (the light is propagated). The second one introduces absolute motion while the first leads to the movement of light in vacuum exactly as the law that requires the principle of relativity ... (Ritz 1908a, p. 210)

Where such as FitzGerald, Larmor, Lorentz and Poincaré had confidently assumed the universal applicability of Maxwell's equations and invoked inter-frame transformations to suitably modify classical mechanics, Ritz attempted to retain classical mechanics and at least modify, if not "replace the fundamental field equations of electrodynamics." (Martínez 2004b, p. 9) Although Ritz received much support for his early and often hypothetical[257] endeavors, his life, and his work, were prematurely cut short with his untimely death from tuberculosis in 1909, at the age of 31. His preliminary submissions were subjected to both philosophical and empirical criticisms and, deprived of the opportunity for redress by either considered reply or model modification and concretization, were ultimately rejected. Chief amongst evidence for the prosecution was that of Willem de Sitter (1872-1934) at the University of Leiden, whose claimed interpretation of radiation from stellar spectroscopic binaries were seen by many, at least at that time, as amounting to an *experimentum crucis* for the ballistic model and

[257] Although arguing that from the instant of their emission "the velocity of the particles remains invariable [even if they] pass through ponderable bodies or electric charges," Ritz later qualified his claim by asserting that "this hypothesis ... is only temporary; and is contrary to that of action and reaction. But the advantage of being as close as possible to the corresponding hypotheses of Lorentz makes it preferable ..." (Fritzius 2003; Ritz 1908a, p. 211)

inimical to its survival. In the de Sitter assessment: "it is to be seen, that Ritz's accepted dependence of the speed of light on the movement of the source is absolutely inadmissible." (de Sitter 1913)

Continuum model responses

Amongst the first attempted remedial responses,[258] deriving from a continuum-styled modeling and that maintained belief that electromagnetic waves were propagated within some conception of an aetheral light-bearing medium, was that from the Professor of Natural and Experimental Philosophy at Trinity College Dublin, George Francis FitzGerald. (1851-1901) His proposition, originally published in a short 20-line "Letter to the Editor" of the May 1889 issue of American journal, *Science*, was poorly known[259] until historian Stephen Brush drew attention to it in 1967. (Brown 2001, p. 13; Brush 1967, p. 231) In essence, FitzGerald envisaged a velocity dependent physical deformation and suggested that:

> … almost the only hypothesis that could reconcile this ... is that the lengths of material bodies changes, according as they are moving through the ether or across it, by an amount depending on the square of the ratio of their velocities to that of light. (FitzGerald 1889)[260]

The FitzGerald proposal of a physical shortening due to motion against the aether flow that could exactly counteract an expected optical anisotropy due to the same flow, was often criticized as *ad hoc.*[261] However, FitzGerald's contraction appears to have been an

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[258] Some distinction if not priority needs to be granted to Prof. Woldemar Voigt, director of Mathematical Physics at the Physics Institute, Göttingen University, (Georg-August-Universität 2006) who first corresponded with Lorentz in 1887 with concerns about the MM experimental result and in a paper written the same year on the Doppler principle, (Voigt 1887) appears to have been the first to write down, albeit using a different scale factor, a set of equations which entail that group now known as the 'Lorentz Transformations,' eventually to be held central to explanations by both continuum and atomist approaches in their respective solutions to the MM crisis. (O'Connor & Robertson 1996) In his 1887 paper, Voigt also advanced the concept of the universal speed of light, proposed a reference frame invariance of physical laws, (Ernst & Hsu 2001, p. 211) and a form of the relativistic 'addition of velocities' theorem, (Gluckman 1968, p. 227) and the hint of a 'time dilation.' Although generally unrecognized by the subsequent developers of relativistic theories, Lorentz belatedly acknowledged Voigt's contribution in a paper of 1909.

[259] The letter was apparently unknown to Joseph Larmor who edited the posthumous publication of FitzGerald's collected *Scientific Writings* in 1902, (Hunt 1988, p. 75) or to Alfred Bork as late as 1966. (Bork 1966, p. 199)

[260] In mathematical terms, "that an object moving through the ether changes its length in the direction of motion by a factor of $\sqrt{(1 - v^2/c^2)}$, where v is the speed relative to the ether and c is the speed of light." (Brush 1967, p. 230)

[261] FitzGerald's proposal, although reported as "a brilliant guess" (The Times 1942) was elsewhere derided as "a somewhat desperate attempt," (Brown 2001, p. 2) "a rather wild idea ... without any real theoretical justification" and "traditionally ... the very paradigm of an *ad hoc* hypothesis." (Hunt 1988, p. 67) In a letter to Lorentz, FitzGerald (1894) admitted that "I have been rather laughed at for my view over here." Oliver Lodge, at the Royal Society in 1893, although praising the idea as 'ingenious,' appeared

extrapolation from his, and his Maxwellian colleagues' hypothesis, that intermolecular forces might be electromagnetic in nature. This notion was then coupled with plausible dynamical arguments advanced by Oliver Heaviside in 1888, and soon corroborated by J.J. Thomson,[262] "that the electromagnetic field around a moving charge should shrink slightly along its line of motion." In apparent confirmation, Heaviside's conjectured second-order 'shrinkage' proved to be just that amount required to exhibit a MM null result.[263] (Hunt 1988, p. 67) Further, in what appears a noteworthy anticipation of later developments, FitzGerald also remarked to Heaviside that the formula quantifying the electromagnetic contraction "suggested the velocity of light might be a physical limit to speed." (Hunt 1988, p. 72)

Independently however, in 1892, the great Dutch physicist, Hendrick Antoon Lorentz, conceived essentially the same solution to the perplexing MM result and went on to incorporate it comprehensively into his electron theory of matter. (Hunt 1988, p. 76) However, the Lorentz formulations appealed to more than the MM result for sanction. Lorentz was now in possession of supplementary empirical evidence wherein additional experimental methods of second order sensitivity had also published 'null' results. Around 1900, Rayleigh and Brace had examined birefringent crystals to test for a double-refraction effect due to changes of aether-flow on refractive index. (Curtis 1911, p. 221) Trouton and Noble had also attempted detection of a turning couple acting on a charged capacitor fixed to the beam of a torsion-balance. Both experiments were pronounced unsuccessful. (Lorentz 1904, pp. 11-12; Trouton & Noble 1903, p. 181) [264] By 1895, having learnt of FitzGerald's idea, Lorentz inserted a footnote (Lorentz 1895, p. 4) to his major work on electron theory belatedly acknowledging FitzGerald's contribution and awarding him priority, an act of academic generosity which ultimately gave rise to the effect becoming known as the "FitzGerald-Lorentz contraction." Although now placed on a more plausible theoretical footing, the Lorentz conception was also to be branded *ad hoc* by the prominent French mathematician Jules Henri

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ignorant of FitzGerald's theoretical basis, claiming "that the length and breadth of Michelson's stone supporting block [and necessarily with it the interferometer arms] were differently affected, in what happened to be, either accidentally or for some unknown reason, a compensatory manner." (Lodge cited in Bork 1966, p. 200)

[262] Further analysis by both J.J. Thompson and G. Searle suggested that the spherical surface of equipotential surrounding a Maxwellian electric charge, underwent a distortion when moving through the aether, assuming a flattened ellipsoid shape later to be known as the "Heaviside ellipsoid". (Brown 2003, pp. 2-3)

[263] In physical terms "a change of about .005μ [5 x 10 $^{-9}$m] would be required" which, for the diameter of the Earth, would translate to a shortening of "only two and a half inches [≈ 60-65mm]" (Curtis 1911, p. 221)

[264] "There is no doubt that the result is a purely negative one." (Trouton & Noble 1903, p. 181)

Poincaré (1854-1912),[265] leaving Lorentz to complain to Einstein in a letter of January 1915, that:

> One arrives at this [contraction] hypothesis if one extends to other forces what one can already say about the influence of translation on electrostatic forces. If I had stressed this more, then the hypothesis would have given much less of an impression of having been invented *ad hoc*. (Lorentz to Einstein cited in Hunt 1988, p. 74)

Pursuing a similar approach, and one that initially emphasized the optical principle of 'Least Time,'[266] based on Huygens' modeling of optical propagation, prominent Irish physicist Joseph Larmor[267] published a series of three influential papers in the *Philosophical Transactions* between 1894-1897, entitled *A dynamical theory of the electric and luminiferous medium*. His papers also advanced his views on the 'particle' source of electric charge – the electron,[268] a perspective that gained gravitas after J.J. Thomson had experimentally identified the electron in 1897. In historical perspective:

> Larmor's contributions came at a time when there were major revolutions in physics with the passing of classical physics to be replaced by quantum theory and relativity. His contributions can be seen as a bridge between the old and the new physics. (O'Connor & Robertson 2003b)

Biographical notes accompanying the Larmor Royal Society collection add the eulogy:

> Of those who brought classical physics to the point where new methods became inevitable, H A Lorentz and Larmor were the most prominent, preparing the old physics for the advent of the new. Larmor's major contribution to this was his book *Aether and Matter*, (Larmor 1900) ... which began as a memoir published initially in the *Philosophical Transactions* between 1894-1897 and which to the student of the period was the gateway to new thought. (AIM25 2008)

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[265] "Poincaré has objected ... that, in order to explain Michelson's negative result, the introduction of a new hypothesis has been required, and that the same necessity may occur each time new facts will be brought to light. Surely this course of inventing special hypotheses for each new experimental result is somewhat artificial." (Lorentz 1904, p. 13)

[266] Larmor's explanation of the Principle of Least Time is derived from the radiation process innate to Huygens' principle wherein abstract optical rays have "direction ... determined by the wave-surface construction of Huygens." (Larmor 1900, pp. 29-32)

[267] Later to become Sir Joseph Larmor, a later successor to Sir Isaac Newton as Lucasian Professor of Mathematics at Cambridge, 1903-1932, and secretary of the Royal Society, 1901-1912. (AIM25 2008)

[268] Such "electrons could be thought of mechanically as point centres of radial strain in the ether, and they were the sole constituents of ponderable matter ... [a view that] diffuses the problem of the relationship between ether and matter, by reducing all matter to moveable discontinuities in the ether." (Warwick 1991, p. 33)

Initially Larmor approached the problem posed by the earth's motion through a 'static' aether using physical optics and thermodynamics, but increasingly incorporated a broader understanding of electromagnetic theory after contacts with a wider Maxwellian community including George FitzGerald. As Warwick summarizes the later development of Larmor's more significant proposals:

> ... following the introduction of the electron, [Larmor] began to approach the problem of motion through the ether as one in the electrodynamics of moving bodies. In this specifically electromagnetic context, Larmor confronted the problem of the null result of the Michelson-Morley experiment, adopted the famous FitzGerald-Lorentz contraction hypothesis,[269] and became the first physicist to employ what are now called the 'Lorentz' transformations. (Warwick 1991, p. 30)

It is also of note that, not only did Larmor publish the 'Lorentz' transformations in his 1897 paper some two years prior to their publication by Lorentz in 1899 and 1904, but also warrants priority in describing a relativistic 'time dilation' wherein he claimed that "individual electrons describe corresponding parts of their orbits in times shorter for the [rest] system in the ratio $(1 - v^2/c^2)^{1/2}$" (Brown 2003, p. 7; Macrossan 1986, p. 2)[270]

Irrespective of the variations of emphasis and mathematical presentation exhibited by physicists such as Voigt, FitzGerald, Lorentz and Larmor, a commonality of the Newtonian absolutist modeling of a 'motionless' universal aether harmonized their approach. However, by 1900, aether as a ponderable substance was under intense scrutiny (Brown 2005, p. S86) and Henri Poincaré broached a more relativist model earlier advocated by Huygens. In 1899, while teaching at the "Ecole Polytechnique," Poincaré declared:

> I am not satisfied with the explanation of the negative result of the Michelson experiment by the Lorentz theory, I would say that the laws of optics are only depending on the relative motion of the involved bodies. (Cited from J. H. Poincaré, 'Electricité et Optique,' 1901 in Fric 2003)

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[269] "... the effect of imparting to a steady material system a uniform velocity of translation is to produce a uniform contraction of the system in the direction of motion, of amount $\varepsilon^{-1/2}$ [where $\varepsilon = (1 - v^2/c^2)$]" (Larmor 1900, p. 176)

[270] John Bell attributes priority to Larmor referring to "the time dilation of J. Larmor (1900)." (Bell 1976, p. 71) as also does Macrossan (1986, pp. 2-3)

Further, when opening the Paris Congress in 1900, Poincaré provocatively asked "Does the ether really exist?" (O'Connor & Robertson 1996) However, although some have suggested the Poincaré aether doctrine as ambiguous, it appears probable that an equitable application of the 'principle of relative motion'[271] to include the aether frame was his primary motivation rather than to seriously question its reality. Of necessity, this disparity of underlying models brought Lorentz and Poincaré into controversy. Nonetheless, the ensuing amicable exchange was recorded as eventually becoming fruitful, at least from Poincaré's position:

> [The] contraction [proposed by Lorentz and FitzGerald] which we will call the *Lorentzian contraction,* would explain Michelson's experiment and all others performed up to now. The hypothesis would become insufficient, however, if we were to admit the postulate of relativity in full generality. Lorentz then sought to extend his hypothesis and to modify it in order to obtain perfect agreement with this postulate. This is what he succeeded in doing in his article ... [Lorentz, 1904] The importance of the question persuaded me to take it up in turn; the results I obtained agree with those of Mr. Lorentz on all the significant points. (Poincaré 1905a, p. 1)

Resulting from his extensive review of the Lorentz thesis, Poincaré presented a preliminary report, "*On the Dynamics of the Electron*," (Poincaré 1905a) to the *Académie des Sciences* meeting in Paris on June 5. His thesis, entailing a revised version of Lorentz invariance, addressed almost every aspect (Brown 1999a) of a special[272] theory of relativity:

> In his … paper, Poincaré explored the consequences of the principle of relativity in kinematics, dynamics, electrodynamics, and gravitation. This principle, that all physical laws in moving and stationary frames of reference are identical, and Maxwell's equations, are the only two premises he assumed; all other characteristics of his new relativistic physics, such as the constancy of the speed of light, follows from them. (Katzir 2005, p. 280)

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[271] although Poincaré stressed the relativist position and employed the terms, "The relativity principle," and "The Law of relativity," it appears to have been Maxwell who first referred to "the doctrine of relativity of all physical phenomena" in its modern meaning in section 102 of his 1877 *Matter and Motion*, wherein he noted: "There are no landmarks in space ... We are, as it were, on an unruffled sea ... we may compute our rate of motion with respect to the neighbouring bodies; but we do not know how these bodies may be moving in space." (Keswani & Kilmister 1983, pp. 344, 348)

[272] Such relativism is designated 'special' insofar as it is restricted to the extensive idealizations incorporated in the inertial reference-frame modeling of space and time.

Deriving from admittance of his 'Postulate of Relativity,' as a law of nature[273] that denied the Newtonian absolutes of time and space, he posited that no experiment, mechanical or *electromagnetic,* could discriminate between a state of uniform motion and a state of rest. Of singular importance, Poincaré pre-empted Minkowski with his conception of a four-dimensional chrono-geometry of space-time, (Giannetto 2009, p. 121) wherein the quadratic form $x^2 + y^2 + z^2 - c^2t^2$ remained invariant under Lorentzian transformation "and thus could be regarded as rotations in a four-dimensional space with an imaginary fourth [time] coordinate." (Darrigol 2005, p. 12) As corollaries ensuing from his premises he variously claimed the inter-frame invariance of Maxwell's equations; derived the formal symmetry group of inter-frame transformations which he charitably named for Lorentz;[274] postulated that a 'local time'[275] should be individually defined for each reference frame in relative motion together with a physical 'Lorentzian' contraction of matter in its direction of motion; proposed that the velocity of light be a limit velocity; endorsed a relativistic mass as dependent on velocity;[276] implicitly derived the 'mass-energy' relation $E = mc^2$; [277] and relinquished the validity of conventionally defined time and simultaneity.[278] Additionally, he further pre-empted Einstein by suggesting a method of synchronizing clocks at various locations in a reference frame by the use of light signals, (Poincaré 1900, p. 20) and gave in a letter to Lorentz a rendition of the now termed 'relativistic velocity-addition law,' (Poincaré 1905b) as also implied in his 1905 paper. (Darrigol 2005, p. 12; Keswani & Kilmister 1983, p. 349) Further, in seeming anticipation of future relativistic conjectures, he

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[273] "It appears that this impossibility to detect the absolute motion of the Earth by experiment may be a general law of nature; we are naturally inclined to admit this law, which we will call the *Postulate of Relativity* and admit without restriction." (Poincaré 1905a, p. 1) Earlier, in 1900, this postulate had been termed the "principle of relative motion." (Poincaré 1900, p. 19)

[274] "... if we are able to impress a translation upon an entire system without modifying any observable phenomena, it is because the equations of an electromagnetic medium are unaltered by certain transformations, which we will call *Lorentz transformations*." (Poincaré 1905a, p. 2) Poincaré also selected a 'natural' system of temporal and extensive units such that the speed of light equals 1 and claimed that "these equations admit a remarkable transformation discovered by Lorentz, which ... explains why no experiment can inform us of the absolute motion of the universe." (Poincaré 1905a, pp. 3-4)

[275] In an address to the 1904 International Congress of Arts and Science in St Louis, Poincaré noted that observers in relative motion would have clocks which would "*... mark what one may call the local time ...as demanded by the relativity principle ...*" (Poincaré cited in O'Connor & Robertson 1996)

[276] Poincaré implicitly endorsed the Lorentz discourse (1904, p. 24). Eqn. (31) of the same discourse also states an unequivocal dependence of the 'electromagnetic' mass of electrons on relative velocity and subsequently deems it applicable to "the masses of all particles". (1904, p. 30)

[277] Ives showed that the 'mass-energy' relation $E = mc^2$ is implicit in Poincaré's 1900 paper "*The Theory of Lorentz and the Principle of Reaction*," wherein the momentum of radiation, which here plays the role of mass, is defined as E/c^2. However, the first explicit claim of the mass-energy relation is attributed to F. Hasenöhrl in 1904, although Ives claims that the Planck 1907 derivation "is historically the first valid and authentic derivation of the relation," discounting "the fallacy of Einstein's [1905] argument." (Ives 1952)

[278] "We ... choose [temporality] rules, not because they are true, but because they are the most convenient ... The simultaneity of two events, or the order of their succession, the equality of two durations, are ... so defined that the enunciation of the natural laws may be as simple as possible ... all these rules, all these definitions are only the fruit of an unconscious opportunism." (Poincaré 1898 sec. XIII)

canvassed the legitimacy of the spatial domain mapped to a non-Euclidean geometry;[279] and argued that "gravitational propagation is not instantaneous, but occurs with the speed of light." (Curtis 1911, p. 220; Poincaré 1905a, p. 2)[280]

Einstein's emission electrodynamics

The enigmas clustered around electrodynamics and relative motion were to incite the interest of an increasing number of physicists in the first decade of the twentieth century. The editors of *Annalen der Physik* alone were to publish eight papers on the subject from 1902 to 1905. (Holton 1960, p. 634) However, June of 1905 was to become notable in the presentation of proposed solutions to these problems. Poincaré's report, on June 5, was to be followed on June 30 by a paper by Albert Einstein then working as a third class technical expert at the Bern patent office while pursuing his second attempt at a doctoral thesis.[281] The Einstein paper, "*On the Electrodynamics of Moving Bodies*," (Einstein 1905c) has subsequently been characterized as a return to the classical format[282] wherein deductive argument and discussion derive from a preliminary statement of axioms or postulates. Although usually presumed as having been independently engaged, Einstein's approach is notably in sympathy with the mathematical program for the axiomatization of physics then being pursued at Göttingen university by David Hilbert and his associates. (Corry 1998, p. 96) Holton notes that this format agrees with Einstein's later expressed desire to "restrict hypotheses to the most *general kind* and the *smallest number* possible, and views it as inclusive with a "number of methodological correspondences with earlier classics ... [such as Newton's] *Principia*."[283] (Holton 1960, pp. 630-631, orig. emph.) In this,

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[279] "There is no absolute space, and we only conceive of relative motion ... There is no absolute time ... we have no direct intuition of the equality of two periods ... we have not even direct intuition of the simultaneity of two events in two different places ... Mechanical facts might be enunciated with reference to a non-Euclidean space which would be ... quite as legitimate as our ordinary space ..." (Poincaré 1898 sec. III, VI; 1905c, p. 90)

[280] However, Poincaré later acknowledged "I was at first led to suppose that the propagation of gravitation ... moves with the velocity of light. This seems to be in contradiction to a result obtained by Laplace, who announced that this velocity ... is at least much more rapid than light." (Curtis 1911, p. 228)

[281] Einstein's first attempt at a doctoral thesis is recorded as being during Sept – October 1901 when a teacher in Schaffhausen. It failed acceptance. Einstein's eventual doctorate was completed on April 30, 1905, not submitted until July 20, and approved on July 27, 1905, by the University of Zurich and formally awarded 15 Jan. 1906. After necessary correction in Jan 1906, this thesis was published in *Annalen der Physik* on 8 Feb. 1906. The correction of an important mathematical error in the thesis was later published in 1911. (Pais 1982, pp. 46, 58, 88-89; The Center for History of Physics 2004)

[282] Holton sees a general trend in all of Einstein's 1905 publications: "If one has studied the development of scientific theories, one notes here a familiar theme*: the so-called scientific 'revolution' turns out to be at bottom an effort to return to a classical purity*." (Holton 1960, p. 630, orig. emph.)

[283] Holton lists his assessment of seven main parallels between the two works, and freely concedes that their "basic attitudes have in both cases more in common than appears at first reading." (Holton 1960, p. 631)

Einstein had appealed to a philosophical perspective which he also later believed constructive when brought to bear on 'the method of theoretical physics:'

> ... fundamental concepts and postulates, which cannot be further reduced logically, form the essential part of a theory, which reason cannot touch. It is the grand object of all theory to make these irreducible elements as simple and as few in number as possible, without having to renounce the adequate representation of any empirical content whatever. (Einstein 1934a)

For Einstein, two postulates provided the minimum but necessary basis for discussion of the work at hand. Commencing with

> the conjecture … that … for all coordinate systems for which the mechanical equations are valid, the same laws of electrodynamics and optics will also be valid … We shall raise this conjecture (the substance of which will hereafter be called the 'principle of relativity') to the status of a postulate and shall introduce, in addition, the postulate, only seemingly incompatible with the former one, that in empty space light is always propagated with a definite velocity *V* which is independent of the state of motion of the emitting body. (Einstein 1905c, p. 140)

Ensuing from this axiomatization of the relativity principle and the constancy of light-speed in vacuum, formal mathematical argument then derived the Lorentz transformations and their Poincaré group property, the Larmor time dilation, the Fitzgerald-Lorentz contraction, and the Poincaré clock light-signal synchronization method, all however without any academic acknowledgements.

Such treatment of the 'principle of relativity' and constancy of the vacuum velocity of light as *a priori* axioms rather than empirical dependencies, as required by Lorentz (1904) and Poincaré (1905), (Hirosige 1976, p. 51) was eventually to be hailed by a majority of physicists as housing seminal theoretical insights. Philosophical and theoretical issues held as deriving from them, were to eventually inform a relativistic 'four-vector spacetime' as a foundational pillar of modern science. On the other hand, it was also to ignite sparks of contention and calls for fundamental review that have persisted into the current century. (Chappell 2000; Mueller & Kneckebrodt 2006; Whitney 2011)

The 1905 document in historical context

In attempting to place the Einstein paper into a context useful for historical analysis and investigative research, it also becomes necessary to more carefully define it, distinguishing fact from those distortions perpetuated by legend, folklore and hero-myth. Often popularly but carelessly characterized, it has been portrayed as possibly the greatest single contribution to science, authored by an extraordinary genius among geniuses,[284] (Golden 1999; Levenson 2005) miraculously conceived in 'splendid' academic isolation, (Pais 1982, p. 26) yet the work of a virtually unknown and lowly patent clerk. The unvarnished facts of history suggest otherwise. As a first approach to a more responsible assessment, chief editor of "*The Collected Papers of Albert Einstein*" project, John Stachel, suggests differentiating between microscopic electron theories and macroscopic theories of electromagnetic and optical phenomena in moving media, as being historically meaningful when discussing electrodynamics and motion in its early twentieth century setting. As Stachel notes:

> In 1905 Einstein had only applied relativistic kinematics to Lorentz's electron theory. In 1908 Minkowski [independently] offered the first solution to the problem of formulating a relativistic electrodynamics of moving media. Since then, the nature of the proper solution to this problem has been a subject of considerable controversy, with a number of questions still in dispute.[285] (Stachel 1989b)

It is in direct accordance with such a distinction that Janssen characterizes the relationship between the Einstein contribution and Lorentz's electron theory as being best understood in terms of "competing interpretations of Lorentz invariance." (Janssen 2002, p. 421) Battimelli further adds that:

> What we today call the special theory of relativity did not spring all at once in one elegant block from Einstein's … work … A fully developed relativistic dynamics, however, was still to come. Its final formulation, and its consolidation into the canonized version which

[284] In a grossly inflated Time magazine eulogy: "…he was unfathomably profound--the genius among geniuses who discovered, merely by thinking about it, that the universe was not as it seemed." (Golden 1999)

[285] "The difference between Einstein and Minkowski's approach … is a polarity that will persist in various manifestations throughout the whole development of relativity theory. Einstein's emphasis is on the algebraic properties of the theory… its *covariance* … Minkowski ensures satisfaction of the principle of relativity by quite different means. The only structures allowed … are spacetime invariants. " (Norton 1993, p. 797)

> became paradigm for a new generation of physicists, required the prolonged efforts of quite a few people over a period of several years.[286] (Battimelli 2005, p. S115)

Stachel further reminds us that it is anachronistic to use the term "the theory of relativity" in relation to Einstein's early papers, in which only the "principle of relativity" finds reference. By 1906, Planck was referring to the identical formalisms of the Lorentz and Einstein equations of motion as "Relativtheorie," whereas, in 1910, Göttingen mathematician Felix Klein suggested the term "Invariantentheorie." It was to be 1915 before Einstein "started to refer to his earlier work as 'the special theory of relativity.'" (Stachel 1989d) Pursuing his own clarification, Janssen also rejects the less critical modern picture that views the Lorentz-Einstein comparison in terms of a "battle of paradigms"[287] and suggests that accepting this now traditional view of Einstein's work

> … as the epitome of a scientific revolution may seriously distort our perception and assessment of this fascinating episode in the history of science … in both cases the central innovation in and of itself did not constitute a wholesale replacement of one tradition or worldview by another, but is best understood as one of several factors that brought about this transition over time. (Janssen 2002, pp. 421-422)

One of these factors, and clearly the one of prime significance, was the advancement of a claim to geometrical synthesis of time with spatial dimension as a unified four-dimensional spacetime continuum. Although earlier conceived by Henri Poincaré, the concept was to be presented as an original and ground-breaking theory by Hermann Minkowski. In a lecture before the Göttingen Mathematical Society on 5 November 1907, (Minkowski 1907; 1915)[288] the Göttingen mathematician advanced the Lorentz transformations, "interpreted as rotations in a space with three real and one imaginary coordinate." (Stachel 1989b)

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[286] Beyond Minkowski's work providing the terminology of the modern special relativistic formulation, Brown notes that the "dynamics of special relativity were first given in modern form by Lewis and Tolman in 1909. Likewise, the Riemann curvature and Ricci tensors for n-dimensional manifolds, the tensor formalism itself, and even the crucial Bianchi identities, were all known prior to Einstein's development of general relativity in 1915." (Brown 1999a)
[287] A more conventionalist view equates the Einstein interlude with terms recalling the revolutionary Copernican overthrow of Ptolemaic cosmology. (Brown 1999a)
[288] The 1907 lecture remained unpublished until Arnold Sommerfeld felt impelled to submit it for general publication in the *Annalen* in 1915.

Relativity and mathematical physics

In 1886, already celebrated mathematician[289] Felix Klein, having accepted a chair at the University of Göttingen, sought "to re-establish Göttingen as the foremost mathematics research centre in the world," and one intended to serve as a world-wide academic model. (O'Connor & Robertson 2003a) In the 1890s Klein's interest turned towards mathematical physics and in 1895, David Hilbert – acknowledged even then as one of the world's foremost mathematicians – was brought from Königsberg to join Klein's research team. When assessing the Göttingen mathematical philosophy in retrospect, mathematician Morris Kline believed it to have claimed

> … that purely mathematical considerations, including harmony and elegance of ideas, should dominate in embracing new physical facts . Mathematics so to speak was to be master and physical theory could be made to bow to the master. Put otherwise, theoretical physics was a subdomain of mathematical physics, which in turn was a subdiscipline of pure mathematics … This philosophy would seem to be a carry-over (modified of course) from the Eighteenth Century [neo-Pythagorean] view that the world is designed mathematically … (O'Connor & Robertson 2005)

By 1900, Hilbert had absorbed Klein's mathematical orientation. In a lecture delivered at the Second International Congress of Mathematicians in Paris, in what is now recognized as a significant landmark of that century's mathematical history, he presented a list of twenty three problems he believed should occupy the future efforts of the mathematical fraternity. The sixth item on his list advocated the axiomatization of physics. Hilbert proposed:

> to treat in the same manner [as geometry], by means of axioms, those physical sciences in which mathematics plays an important part. (Hilbert cited in Corry 1997a, p. 84)

In 1902, Hermann Minkowski joined the Göttingen research effort, accepting a third chair specially created for him by Klein on the request of his long-time friend Hilbert. Minkowski was to become "deeply involved in all the scientific activities of Hilbert, including his current interests in the axiomatization of physics," and began devoting "all his efforts to the study of the equations of electrodynamics and the postulate of relativity." (Corry 1997b, p. 275) Minkowski's subsequent theoretical conception, often cited as being itself definitive of the special theory of relativity, equipped the standard

[289] "Klein's synthesis of geometry as the study of the properties of a space that are invariant under a given group of transformations, known as the *Erlanger Programm* (1872), profoundly influenced mathematical development." (O'Connor & Robertson 2003a)

mathematical language of all future developments of electrodynamics and relativity. (Corry 1997b, p. 281) Unfortunately, historical and philosophical analyses of its independent origins and the moulding of its theoretical development are sparse. However, and fortunately for the history of science, early quantum theoretician, Max Born, who had studied optics under Voigt, gained a position as Hilbert's private assistant at Göttingen. After defending his doctoral dissertation in 1907 and spending a short period in Cambridge with Joseph Larmor and J.J. Thomson, Born accepted a position as Minkowski's "young collaborator who knew something of physics, and of optics in particular." (Walter 1999, p. 48) As Born later recalled:

> I was at that time [circa 1905 but prior to Einstein's publication] in Gottingen and well aquainted with the difficulties and puzzles encountered in the study of electromagnetic and optical phenomena in moving bodies, which we thoroughly discussed in a seminar held by Hilbert and Minkowski. We studied the recent papers by Lorentz and Poincaré, we discussed the contraction hypothesis brought forward by Lorentz and Fitzgerald, and we knew the transformations now known under Lorentz's name. Minkowski was already working on his four-dimensional representation of space and time, published in 1907, which became later the standard method in fundamental physics. (Born cited in Brown 2009)

Born's note corrects a widespread fallacy that suggests that Minkowski developed his theory "during the period 1907-8." (Pyenson 1985, p. 80) His note also dismisses common misconceptions that Minkowski somehow derived his ideas from Einstein's paper either as a modification or some kind of reformulation. (Corry 1998, p. 96; Scribner 1964, p. 678) [290] Although it is true that Minkowski eventually examined the Einstein work before announcing his own theoretical development,[291] his chosen version of Lorentz invariance, entailed as the essential out-working of his spacetime formalism, was initially that by Lorentz alone. Minkowski's approach idealized that of the Göttingen circle, stressing the desirability of such theory construction deriving solely as it did from mathematical principles and "without recourse to any experiments." As Corry viewed it:

> Such an achievement in a theory whose essence was a deep and unexpected unification of mechanics and electrodynamics could not but reinforce the sense of a 'preestablished harmony between mathematics and physics.' (Corry 1998, p. 96)

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[290] Scribner incorrectly claims (1964, p. 678) that Einstein's "creation of a modified theory of space and time led directly to Minkowski's mathematical interpretation of relativistic kinematics in terms of a four-dimensional space-time … " and is typical of such false historical interpretations.

[291] In a letter of 9 October 1907, Minkowski informed Einstein that his 1905 paper "was to be discussed in a Göttingen seminar and requested an offprint." (Stachel 1989b, p. 504)

To this end, in its debut presentation in 1907, spacetime relativity was presented as the projection of the "*postulate of the absolute world*"[292] and employed to particularly eulogize the Göttingen neo-Pythagorean styled[293] ideals and display their prowess.

> … [these] new approaches would represent … nearly the greatest triumph that the application of mathematics has ever produced … It would obviously be to the glory of the mathematician, to the utter astonishment of the rest of humanity, that mathematicians have created purely in their imagination a large domain, which … should be given one day the most complete real existence. (Minkowski 1907)

Academic acknowledgments were not, however, neglected. The pioneering work of Michelson and Morley, Lorentz, and FitzGerald were noted with the further remark that "Einstein, Poincaré and Planck … have contributed to the elaboration of the general principle." However, in apparently attempting to establish a personal claim to priority, (Walter 1999, p. 50) Minkowski advanced his axiomatic analysis of relativistic spacetime geometry and its application to electrodynamic theory, as the innately superior approach. Poincaré's potential to claim priority, via his earlier intimation of a four-dimensional space-time synthesis with its constant space-time interval (Walter 1999, p. 56) together with his advocacy of the universal validity of Lorentz invariance and the Relativity Principle, was summarily dismissed as failing to be "particularly transparent" as regards factuality.[294] (Minkowski 1915, p. 929) Although some details from recent works by Poincaré and Planck were discussed, Einstein's contribution, although known, remained unexplored. Some remedy and a reorientation of perspective is nonetheless noticeable in his subsequent "*Raum und Zeit*," ("*Space and Time*") presentation delivered at Cologne, 21 September, 1908, to the 80th Assembly of

[292] For Minkowski, the term "relativity-postulate" seemed 'feeble' in that it permitted some independence wherein "the projection in space and in time may still be undertaken with a certain degree of freedom." According to Minkowski's definition: "The word-postulate permits identical treatment of the four co-ordinates *x, y, z, t*," and leads directly to the construction wherein "[t]he cone $c^2t^2 - x^2 - y^2 - z^2 = 0$ consists of two parts in which the (front) cone with values $t < 0$ consists of all the world-points which send light to the apex, or zero-point of spacetime, whereas the (back) cone, where $t > 0$,contains all those world-points which receive light from the apex. (Minkowski 1908, p. 83) As Corry interprets the Minkowski doctrine, "it is only after establishing the equations for empty ether, and proving the Lorentz theorem of invariance, that we can speak of the principle of relativity [which then ultimately] yields the electrodynamics of moving matter." (Corry 1997b, p. 283)

[293] Both Walter and Pyenson suggest rather "the prevalence of a belief in a neo-Leibnizian notion of pre-established harmony between pure mathematics and physics." (Pyenson 1985; Walter 1999)

[294] In 1904, 1905, 1906 and 1908 Poincaré had consistently advocated extending the domain of Lorentz invariance to all the laws of physics and that the Principle of Relativity was a general law of nature, but one that nonetheless still required confirmation or refutation by experiment. (Corry 1997b, p. 281) Eventually both Einstein and Lorentz were also dismissed from any possibility of priority contention since, in Minkowski's view, neither had "made any attack on the concept of space …" (Corry 1997b, p. 296; Minkowski 1908, p. 83)

German Natural Scientists and Physicians. (Minkowski 1908) Of note is the more subdued, empirically dependant, and conciliatory treatment of his opening remarks to a more elite, international, and professionally diverse audience:

> The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality … [However] I should like to show how it might be possible, setting out from the accepted mechanics … along a purely mathematical line of thought, to arrive at changed ideas of space and time. (Minkowski 1908)

In the event, Minkowski's concession to experimental physics amounted to little more than a perfunctory reference to the Michelson-Morley result. Walter has interpreted the revised introduction as a ploy to diffuse "the tension created by a mathematician's intrusion into the specialized realm of theoretical physics,[295] and as Minkowski's strategy to overcome disciplinary obstacles to the acceptance of his work." (Walter 1999, p. 46) Again, the principles of a Lorentzian interpretation of invariance remained fiducial and was advanced in a style sympathetic to an axiomatic and principle theory approach in which "[t]hree-dimensional geometry becomes a chapter in four-dimensional physics." (Minkowski 1908, p. 80) In his purely mathematical scheme, the Lorentz transformations represented rotations in a four-dimensional spacetime manifold:

> I will show … that the Lorentzian hypothesis is completely equivalent to [i.e. consistent with] the new conception of space and time … for the [Lorentz-FitzGerald] contraction is not to be looked upon as a consequence of resistances in the ether, or anything of that kind, but simply as a gift from above, - as an accompanying circumstance of the circumstances of motion. (Minkowski 1908, p. 81)

However, and also of particular significance in this context, was to be his first recognition of an Einsteinian contribution to Lorentz invariance. Included as little more than a brief note, Minkowski acknowledged a claim to the equality of local and systemic times as a clarifying detail of the entailed invariance scheme:

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[295] As Sommerfeld later recalled: "The entry of mathematicians into the field of relativity was described by Einstein as an invasion." (Walter 1999, p. 54)

> But the credit of first recognizing clearly that the time of one electron is just as good as that of the other, that is to say, that *t* and *t'* are to be treated identically, belongs to A. Einstein. (Minkowski 1908, p. 82)

It is in the nature of this slender acknowledgement that some historians detect betrayal of a long-standing tension between Minkowski and his former mathematics pupil. Both Walter Ritz and Albert Einstein were counted among Minkowski's students when he earlier lectured on mathematical subjects at the Federal Institute of Technology in Zurich. That encounter had found Einstein somewhat indifferent to his teacher's enthusiasm for the subject and the master's appraisal of his student as that of a "lazy dog." (Pyenson 1977, p. 72) Unlike his classmates, after obtaining his diploma, Einstein did not undertake graduate studies in mathematics, a choice that further contributed to Minkowski's later claims that Einstein's mathematical knowledge was incomplete and his mathematical skills open to criticism. (Walter 1999, p. 47) On the other hand, however, Einstein's work was appearing to receive a more favorable attention from physicists such as Planck and Lorentz, and, given what Walter assesses as Minkowski's own "perceived deficit of credibility" in the domain of theoretical physics, (1999, p. 46) could no longer be lightly dismissed.

For his part, Einstein was initially openly dismissive of spacetime relativity, rejecting "out of hand the four-dimensional apparatus of Minkowski's paper" (Walter 1999, p. 49) and deriding his thesis as "no more than 'superfluous erudition'." (Corry 1998, p. 96; Pais 1982, p. 152) Responding in two papers published in collaboration with Johann Jakob Laub,[296] (Einstein & Laub 1908b; 1908c) Einstein attempted to argue both its unnecessary mathematical and philosophical complexity and inadequacies. Ironically, these authors found it necessary to follow-up with another two papers correcting both their own mathematical errors[297] and inaccuracies. (Einstein & Laub 1908a, 1908d) Their misfortune was to be compounded. Their objection that Minkowski's equations for a dielectric with significant magnetic permeability predicted a different charge density than did those produced by Lorentz, was to be eventually discredited when the "Minkowski prediction was confirmed several years afterward." (Stachel 1989b, p. 505)

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[296] Laub was assistant to Wilhelm Wien, then Professor of Physics at the University of Würzburg. (Stachel 1989b)

[297] "The discovery of several incorrect factors of 1/c in their equations led Einstein and Laub to publish a correction to the paper. Einstein wrote consolingly to Laub that 'well patched is always still better than full of holes'." (Stachel 1989b, p. 505)

Reception of the Einstein paper

In 1905, Einstein's Lorentz invariance proposal was not initially the success that its later pre-eminence and acclaimed position as an axis-point of modern science, might suggest.[298] An academic silence followed the publication (Pais 1982, p. 150) – a silence broken only then by Kaufmann in 1906, who, in the first article in *Annalen der Physik* to cite the Einstein work, categorically claimed an experimental disproof wherein "[*t*]*he measurement results are not compatible with the Lorentz-Einsteinian fundamental assumption*." (Kaufmann, cited in Holton 1960, p. 634 italics in orig.) Criticism was then to follow from many of his contemporaries of both continuum and atomist inclination. Sommerfeld was openly sceptical. Larmor, after much deliberation, finally rejected the special theory. (O'Connor & Robertson 2003b) Michelson raised objections. According to Shankland, "Michelson said to Einstein that he was a little sorry that his own work had started this 'monster'" and "told [Einstein] more than once that he did not like the theories that had followed from his work!" (1963, pp. 56, 57) 1908 Nobel laureate Ernest Rutherford, now honored as a 'father of nuclear physics,' was derisive. After Wilhelm Wien had unsuccessfully tried to explain Einstein's contribution to a reluctant Rutherford and had despairingly exclaimed: "No Anglo-Saxon can understand relativity!" an infuriated Rutherford retorted: "No! they've got too much sense!" (Brown 1967) Prominent physicist Oliver Heaviside reportedly dismissed Einstein's thesis as a practical joke:

> I don't find Einstein's Relativity agrees with me. It is the most unnatural and difficult to understand way of representing facts that could be thought of ... and I really think that Einstein is a practical joker, pulling the legs of his enthusiastic followers, more Einsteinisch than he. (Heaviside cited from Bjerknes 2002, chap. 6)

In what must have been a heavy disappointment for Einstein, Ernst Mach, whom he greatly admired (Born 1965, p. 3) and from whom he had earlier sought guidance, asked not to be associated with such "dogmatic and paradoxical nonsense." (Bjerknes 2002) Highly dismissive criticisms were to come later from both Nobel laureate Frederick Soddy[299] and renowned physicist Louis Essen,[300] inventor of the caesium atomic clock

[298] Martínez notes that "at first, Ritz received more appreciation and support from the established physics community than Einstein." (2004b, p. 5)

[299] Renowned English chemist and recipient of the 1921 Nobel Prize for Chemistry for investigating radioactive substances and for elaborating the theory of isotopes.

[300] Essen, fondly remembered as "Old Father Time" and as a "Time Lord" spent his working life as a scientist at the UK's National Measurement Laboratory (NPL) where "his research into the physics of frequency generation and measurement ... changed the way the world measures time ... and eventually designed the quartz ring clock ...[In 1950] he derived a value of 299,792.5 km/sec [for the speed of light] – within two metres per second of the laser based figure adopted in 1975 ... [in 1955] he designed and

and in his time a celebrated world expert in the measurement of length and time. Essen studied Einstein's paper and stated that his "reading only confirmed [his] belief that the theory was marred by its own internal contradictions[301] ... All [Einstein] did was to introduce irrational ideas into physics ..." (Essen 1996) Essen detected in the Einstein approach, not simply a rehearsal of the Lorentz theory with an intention to address and revise Newtonian dynamics, but a critique of the Newtonian theory of measurement. (Goldberg 1987, p. 3)Essen then claimed the Einstein doctrine of the constancy of the velocity of light erroneous stating that:

> Even though [the velocity of light] was found to be constant under certain conditions, it was quite wrong to make it a constant by definition under all conditions. Only the unit of measurement can be made constant by definition. (Essen 1996)

As Essen assessed it, Einstein's treatment had "constituted a duplication of units," which then led to counter-intuitive and contradictory results rather than any supposed "profundity" of the theory. (Essen 1996) In an even more blatant rebuff[302] entitled: "*Relativity – Joke or Swindle?*," Essen claimed:

> Insofar as the [Einstein] theory is thought to explain the result of the Michelson-Morley experiment I am inclined to agree with Soddy that it is a swindle; and I do not think Rutherford would have regarded it as a joke had he realised how it would retard the rational development of science. (Essen 1988, p. 127)

Amongst aether theorists, Ernst Gehrcke (1878-1960) played a prominent role as Einstein critic, claiming any pronouncement that "the aether is dead" to be premature.

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operated a novel caesium clock [claiming] accuracy of one part in a million, million, launching the world in to an era of practical atomic clocks." (National Physical Laboratory 2005b) Essen became the only British physicist to be honoured for his scientific contributions by both the USA and USSR [1959 Popov Medal] during the Cold War. (Essen 1996)

[301] Essen carefully examined the 1905 paper whose claims made it relevant to his own area of scientific expertise. Further, he had noted that relativistic effects, "although very small were not becoming significant in the definition of the atomic second and the use of atomic clocks … I, therefore, studied Einstein's famous paper, often regarded as one of the most important contributions in the history of science. Imagine my surprise when I found that it was in some respects one of the worse papers I had ever read. The terminology and style were unscientific and ambiguous; one of his assumptions is given on different pages in two contradictory forms, some of his statements were open to different interpretations and the worst fault in my view, was the use of thought-experiments. This practice is contrary to the scientific method which is based on conclusions drawn from the results of actual experiments ... Relativists often state that the theory is accepted by all scientists of repute but this is quite untrue. (Essen 1996)

[302] Essen "retired in 1972 after being quietly warned not to continue his contradiction of Einstein's law of relativity." (National Physical Laboratory 2005b) Essen recalled: "I was ... dropped some pretty broad hints that if I continued to criticise the theory my reputation and career prospects were likely to suffer." (Essen 1996)

In a twist to aether modeling, Gehrcke proposed that the "aether is so constituted that it would *not* be dragged by trivial masses of trivial velocity, but rather by large and rapidly moving masses such as the earth." In terms of such an aether, he claimed: "Now every difficulty disappears," (Gehrcke 1918) and the 'aether drag" conjectured by Fresnel's interpretation was harmonized with the aether propositions advanced by others such as Stokes. However, in a brief note Einstein countered Gehrcke's proposal by noting that "... the hypothesis of an aether which moves with space-bodies is incompatible with the aberration-law." (Einstein 1918)

Although Lorentz showed clearly to have understood Einstein's paper, his initial response was both guarded and qualified with a hint of rebuke:

> [Einstein's 1905] results agree in the main with those which we have obtained ... the chief difference being that Einstein simply postulates what we have derived, with some difficulty and not entirely satisfactorily, from the fundamental equations of the magnetic field. (Lorentz cited in Curtis 1911, p. 227)

Lorentz was, however, to continue this assessment with recognition of some "marked advantages" of the Einstein approach as against his own, and awarded such honour as he believed due in that Einstein "may certainly take credit for making us see in the negative results of experiments like those of Michelson, Rayleigh and Brace, not a fortuitous compensation of opposing effects, but the manifestation of a general and fundamental principle." (Curtis 1911, p. 227) Nonetheless, after some further reflection, his comments in a 1913 lecture would appear to display a politely clothed rejection:

> As far as this lecturer is concerned he finds a certain satisfaction in the older interpretation according to which the ether possesses at least some substantiality, space and time can be sharply separated, and simultaneity without further specification can be spoken of ... (Lorentz, 1913, cited in O'Connor & Robertson 1996)

When comparing Einstein's reception by different countries, Goldberg notes that in Germany, the Einstein approach was often confused with the Lorentz theory with which it shared an identical formalism. However, its differing theoretical implications were hotly debated. In England, three years passed before the Einstein work appeared in the literature, by which time "a way had been found to make it compatible with British concepts of the ether." (Goldberg 1987, p. 4) On the other hand, in France during the same period, there was "utter silence." Goldberg further advises that "the French physics curriculum was virtually silent as well through the sixth decade" of the century, a silence he equates with the silence held on the matter, by France's leading theorist, Henri Poincaré.

Beyond the range of views expressed by his contemporaries, the paper itself, and his 1905 work in general, was to be subjected to a variety of adverse criticisms bearing on issues including priority, independence, mathematical competence,[303] logical consistency, and authorship. (i) (ii) Prominent amongst academic criticism of the document was Einstein's conspicuous failure to specifically acknowledge the prior works of others.[304] Contemporary and colleague, Max Born, was to query this strange lack of acknowledgment. After personally rehearsing the prior and extensive contributions of both Poincaré and Lorentz, Born noted that:

> The striking point is that [Einstein's 1905 paper *On the Electrodynamics of Moving Bodies*] contains not a single reference to previous literature. It gives you the impression of quite a new venture. But that is, of course, as I have tried to explain, not true. (Born, cited from Brown 2009)

As possible explanation, some have suggested that acknowledgement of sources in the mode contemporarily mandated were not usual in works of this time. However, it is to be equally noted that in his immediately prior publication, (Einstein 1905b) which discussed his interpretation of the light quanta and radiation, Einstein had routinely acknowledged the prior contributions of Maxwell, Planck, Wein, Boltzmann, Boyle, Gay-Lussac, Stokes, Lenard, and Stark in a manner not significantly differing from modern practice. A further excuse, maintained by many Einstein scholars and occasionally claimed in his own defense, suggests that Einstein, as lowly patent clerk,

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303 Herbert Ives, (1952) after a detailed analysis of Einstein's 1905 paper, "*Does the Inertia of a Body Depend upon its Energy Content*?" (Einstein 2005) concludes that the relation $E = mc^2$, which does not explicitly appear in the paper, "was not derived by Einstein." Ives notes that Planck, after correctly deriving the relation in 1907, remarked that Einstein's attempt was advanced under an "assumption *permissible only as a first approximation*." According to Ives, the relation Einstein's derivation was intended to yield, is "slipped in by the assumption Planck questioned." (Ives 1952, pp. 542-543) Ives defended his conclusion in (Ives 1953). In further comment Brown states: "Planck also critiqued Einstein's original deduction of mass-energy equivalence, and gave a more general and comprehensive argument. (This led Johannes Stark in 1907 to cite Planck as the originator of mass-energy equivalence, prompting an angry letter from Einstein saying that he "was rather disturbed that you do not acknowledge my priority with regard to the connection between mass and energy". In later years Stark became an outspoken critic of Einstein's work.)." (Brown 1999a)

304 Rothman suggests that in "being less than fastidious about providing references," Einstein has sponsored a myth such that "since then credulous scientists have equated absence of evidence with evidence of absence." (Rothman 2006, p. 112) Further, in claiming that "The secret to creativity is knowing how to hide your sources," ('Albert Einstein Quotes' 2009) Einstein appeared to confirm a suspicion, openly expressed by some, of plagiarism. (Bjerknes 2003; Moody 2003) In supporting his view, Moody notes *Webster's New International Dictionary* definition of "to plagiarise," which includes: "to use without due credit the ideas, expressions or productions of another."

worked in effective academic isolation, unaware of the extensive and pertinent work of his contemporaries. However, beyond an accumulation of evidence that now seriously undermines the claim, such an appeal to independence through isolation runs the attendant risk of a charge of indefensible ignorance and dereliction of that level of supporting research properly to be expected of a paper seeking to be held in academic regard. On the other hand, in a more candid moment in his biographical review, and in which he admits Einstein had access to relevant literature had "he set his mind to it," Pais classifies such an indifference to academic standards as "a notable lack of taste." Here Pais concedes: "The truth of the matter is that [Einstein] did not much care," and cites a 1906 paper where Einstein was to comment:

> It seems to me to be in the nature of the subject, that what is to follow might already have been partially clarified by other authors. However, in view of the fact that the questions under consideration are treated here from a new point of view, I believed I could dispense with a literature search … especially since it is hoped that other authors will fill this gap, as was commendably done by Herr Planck and Herr Kaufmann on the occasion of my first paper on the principle of relativity. (Einstein, 1906, cited in Pais 1982, p. 165)

For others, however, this lack of specific acknowledgement and references has suggested the less palatable possibilities of 'selective memory' and has cast aspersions on claims to independence. In response to Einstein's claim to have known only the prior Lorentz paper of 1895, and not to have known his 1904 exposition in which the Lorentz transformations were specifically published, Rothman suggests that "[m]emory is often too good to be true" and notes that Einstein's 1905 general recognition that "... the electrodynamic foundation of Lorentz's theory ... agrees with the principle of relativity," appears to be a direct reference to the 1904 paper. (Darrigol 2005, p. 16; Rothman 2006, p. 113)

More particularly, Einstein's indifference to, and claimed ignorance of, the extensive, detailed, and prior expositions by Henri Poincaré on the same subject matter, even to his use of the same phraseology, has inspired unending acrimonious debate, and unhelpful diversions away from the more serious scientific questions at issue. Detractors from claims to Einstein's independence have underscored their case by reference to remarks made by his close friend Maurice Solovine, who with Conrad Habicht and, after their marriage, Mileva Einstein-Marić, gathered at the Einstein residence for more than three years,[305] beginning in 1902, for frequent evenings of

[305] Solovine records the beginnings of the "Academy" as around Easter 1902 and concluding with the departure first of Habicht and then himself from Berne during 1906. Solovine also claimed "that they read many other works on similar themes, as well as a number of literary works." (Stachel 1989e)

animated scientific and philosophical discussions jokingly dubbed the "Olympia Academy." (Stachel 1989e, p. xxiv) In describing their activities Solovine recalled:

> ... we shared an uncommon penchant for studying and explaining the most difficult problems of science and philosophy. Together we read ... Mach ...Mills ... Hume ... Helmholtz ... Reimann ... Poincaré's *Science and Hypothesis*, which engrossed us and held us spellbound for weeks ... (Introduction by M. Solovine to Einstein 1987, pp. 8-9)

Commentators have pointed out that even if it were to be conceded that Poincaré's earlier work on the Lorentz theory (Poincaré 1900) and the comprehensive Poincaré exposition of a relativity theory in early 1905,[306] were in fact unknown to him, much of what informs Einstein's position and is claimed as essential to its enunciation, was unambiguously developed by Poincaré in his popular 1902 book so enthusiastically read by Einstein and his friends. As commentators such as Rothman note: "... it is hard to imagine that they did not have a profound effect on Einstein's thinking," or that he failed to grasp their significance for his own acute interests in the same matters which were then at a stage of development. (Rothman 2006, p. 113)

The fate of Einstein's version of Lorentz Invariance

Despite the active criticism and reserve that early greeted the Einstein paper, for others his contribution was regarded as a useful elaboration or extension to the Lorentz foundation, an assessment even Einstein, on later occasions, appeared to endorse. (Pais 1982, pp. 169, 171) Holding this opinion, Nobel laureate Wilhelm Wein proposed that the Nobel prize of 1912 be awarded jointly to Lorentz and Einstein,[307] suggesting:

> The principle of relativity has eliminated the difficulties which existed in electrodynamics and has made it possible to predict for a moving system all electrodynamic phenomena which are known for a system at rest... From a purely logical point of view the relativity principle must be considered as one of the most significant accomplishments ever achieved in theoretical physics... While Lorentz must be considered as the first to have found the mathematical content of relativity, Einstein succeeded in reducing it to a simple principle.

[306] "In a letter of 14 April 1952 to Carl Seelig (SzZE Bibliothek, Hs 304:1006), Solovine includes *Poincaré 1905a* in a similar, but otherwise less complete list of the readings of the Olympia Academy." (Stachel 1989e, p. xxiv)

[307] "As it happens, the physics prize for 1912 was awarded to Nils Gustaf Dalen (for the "invention of automatic regulators for lighting coastal beacons and light buoys during darkness or other periods of reduced visibility"), and neither Einstein, Lorentz, nor anyone else was ever awarded a Nobel prize for either the special or general theories of relativity." (Brown 1999a)

> One should therefore assess the merits of both investigators as being comparable. (Wein cited in Brown 1999a)

However, less predictable events were to shape the future. Regrettably, Hermann Minkowski fell victim to acute appendicitis and died in 1909, and Henri Poincaré died in 1912. Two of Einstein's more important potential critics fell silent, and their untimely departure deprived the early development of relativity theory of two of its most notable contributors. On the other hand, Einstein's paper gained in academic approval from two unlikely directions. Ironically, considering his initial reserve, it was to be Max Planck, whose subsequent 1907 article "*Zur Dynamik bewegter Systeme*," consolidated and extended an emerging relativistic theory. Planck offered his revised opinions in favor of an Einstein approach after meeting Einstein at the first Solvay Conference in 1911, and is seen by some historians as providing a crucial 'breakthrough,' that virtually guaranteed its academic acceptance into Germany. (O'Connor & Robertson 1996)

Subsequently however, and possibly of comparable importance, Lorentz appears, or, as some historians assess it, at least wished to appear, as having come to terms with the Einstein approach and to have eventually even acknowledged it as superior.[308] The reproof evident in his earlier charge of an artificial Einstein having "simply postulated," later mellowed to a virtue. Lorentz now considered that, "[t]o the experimental evidence that we already had, the charm of a beautiful and self-consistent theory was then added." (Michelson et al. 1927, p. 349) Nevertheless, Max Born, who had first-hand discussions with Lorentz and was well aware of his earlier scepticism, reported that:

> When I visited Lorentz a few years before his death, his skepticism had not changed ... he probably never became a relativist at all, and only paid lip-service to Einstein at times in order to avoid argument. (Born cited in Brown 2009)

On another hand, Richard C. Tolman had initially noted that acceptance of the postulate "that the velocity of light and the velocity of the source are additive" in agreement with a direct reading of the Galilean relativity principle,[309] would avoid "the apparent paradoxes of the Einstein theory." (1912, p. 136) However, his detailed analysis of the prominent electrodynamic approaches fielded by Lorentz, Einstein, and

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[308] In later reviewing the role time was given in his own work, in 1927, Lorentz admitted that "real time for me was still represented by the old classical notion of an absolute time, which is independent of any reference to special frames of co-ordinates … I considered my time transformation only as a heuristic working hypothesis. So the theory of relativity is really solely Einstein's work." (Michelson et al. 1927, p. 350) On the other hand, others claim that Lorentz "did not ever seem to accept [the Einstein paper's] conclusions." (O'Connor & Robertson 1996)

[309] Tolman also noted that "this [Galilean] assumption does directly explain the Michelson-Morley experiment," and "if true, would lead to the simplest kind of relativity." (1910a, p. 29)

Ritz was eventually to lend support to Einstein's second postulate. Despite some initial reluctance to unreservedly accredit the Einstein interpretation, variously described as "remarkable," and "peculiar," (1910b) "strange," "extraordinary" and "[c]ontrary to what seem the simple conclusions of common sense," (1910a, p. 27) Tolman drew on astronomical evidence prior to that of de Sitter. Tolman's evidence purported to demonstrate the velocity of light to be the same whether measured from the approaching or receding limbs of the sun. Although the constancy of light-speed was integral to the Lorentz mechanics, its claimed verification using a dynamic source was now to be found justifying the Einstein axiomatic approach.

Sanctified then by factors such as Planck's approval, Lorentz's apparent complicity, Tolmam's empiricism, and together with its own congruous axiomatic approach, eventually Einstein's thesis seamlessly replaced that of Lorentz as the preferred "Lorentz invariance" ingredient of a maturing spacetime formalism. Further, and to some extent responsive to a growing media-driven Einstein adulation, (Pais 1994) and now lacking Minkowski's potential restraint, the invariance component gained in dominance. Eventually the Einstein contribution subsumed its parental spacetime apparatus relegating it to the status of a derivative and subsidiary, to be referenced at most as "Minkowski spacetime." Ironically, it was to be Minkowski's thesis that was to eventually be supposed as no more than a dependent elaboration of an Einsteinian groundwork. Later, and giving approval to the evolving changes in theoretical status and tacitly abetting a rapidly growing 'lone-hero' mythology, Einstein would unashamedly lecture on the world stage on "*How I Created the Theory of Relativity.*" (Einstein 1922) In this new dispensation, the complex historical and philosophical development of the special theory would be claimed uniquely,[310] devoid of any acknowledgement of, or even reference to, the extensive, prior, and independent contributions of Larmor, FitzGerald, Poincaré or Minkowski.

The 1905 document in philosophical perspective.

Assessment and analysis of the philosophical and ideological circumstances which led Einstein to advance his particular axiomatic or 'postulate' approach has been the subject of intense scrutiny and voluminous works, yet with no clear consensus. (Stachel

[310] On the other hand, it must be fairly noted that in response to a *New York Times* correspondent's question in December 1920, concerning the origins of relativity theory, Einstein stated that early difficulties reconciling rapid motions in electrodynamics with Galilean invariance "led the Dutch professor Lorentz and myself to develop the theory of special relativity." (Pais 1982, p. 171) Also, in a memorial speech after Lorentz's death, Einstein would concede: "The special theory was a more detailed exposé of those concepts which are found in Lorentz's research of 1895." (Pais 1982, p. 169)

1989d) Even questions as to which influences had played the prominent roles in molding his formative thinking remains a subject of intense fascination for both philosophers and historians of science. In this, the place Einstein accorded to aether-drift experiments, or the prior writings of contemporaries such as Lorentz and Poincaré, are often conceded as ambiguous or even contradictory and irreconcilable.

For his part, John Stachel concluded[311] after a detailed analysis, that Einstein's theoretical approach resulted from his relinquishment of the aether concept prior to 1905. For Stachel, "Einstein's omission of the ether was deliberate and crucial: by the time he formulated [his theory] he did not believe in its existence" and that "... abandonment of the concept of the ether was a most important act of liberation for Einstein's thought ..." (Stachel 2002) Nonetheless, although such an assessment would find little discouragement as reviewed by modern versions of the Einstein narrative, its accuracy and motivational primacy appears questionable when appraised by Einstein's own statements and those of his contemporaries and critics. Compounding the issue, Brown notes that "the definitive meaning of the word 'aether' has never been given … so claims about the existence [or non-existence] of aether are always somewhat ambiguous." (Brown 2009) For Einstein, omission of an aether may have been less a dismissal due to non-existence than its inconsequence due to reformulation. By his own later account, Einstein firmly believed in an aether and held its existence essential.[312] Nonetheless, Einstein's aether was, as was that of almost all his contemporaries, patterned after his own needs. Speaking in 1920 at the University of Leyden, Einstein declared:

> … careful reflection teaches us… that the special theory of relativity does not compel us to deny ether [but rather to modify] the conception … taking away from the ether its last mechanical quality, namely, its mobility … a weighty argument [is] to be adduced in favour of the ether hypothesis. To deny the ether is ultimately to assume that empty space has no physical qualities whatever[313]… Only we must be on our guard against ascribing a state of motion to the ether… According to the general theory of relativity space without ether is unthinkable … (Einstein 1920)

[311] It must, however, be conceded that in view of what Stachel calls "the scanty evidence we possess," he regards his conclusion as more correctly "a plausible conjecture." (Stachel 2002)
[312] Einstein was familiar with contemporary thought that questioned its physical existence as a light-bearing medium including works by Poincaré (1902), Mill (1872), and Ostwald (1893). (Stachel 1989d, p. 261) Granek sees the later review as a vacillation in that "Einstein's 1920 reasoning hardly differs from the one Poincaré had presented prior to 1905. Thus while Einstein was a hero because he did away with the ether, this situation lasted a few years only." (Granek 2001)
[313] The mathematician Edmund Whittaker took the same view in his 1910 "*A History of the Theories of Aether and Electricity*," suggesting that: "It seems absurd to retain the name 'vacuum' for an entity so rich in physical properties, and the historical word 'aether' may fitly be retained." (Brown 2009)

For others the paper is remarkable "for the different approach it takes. It is not presented as an attempt to explain experimental results, it is presented because of its beauty and simplicity." (O'Connor & Robertson 1996) Yet others disagree. For Millikan, Einstein's contribution "may be looked upon as starting essentially in a generalization from Michelson's experiment" and exemplifying the "essentially empirical" modern scientific approach by commencing with "well-authenticated, carefully tested *experimental* facts, no matter whether [they] seem at the moment to be reasonable or not." (Millikan 1949, p. 343) Nevertheless for Holton, such an empiricist view[314] "may also be the stuff of which fairy tales are made," and reminds us that:

> … the belief that Einstein based his work leading to his 1905 publication … on Michelson's result has long been a part of the folklore. It is generally regarded as an important event in the history of science, as widely known and believed as the story of the falling apple in Newton's garden and of the two weights dropped from the leaning tower in Galileo's Pisa [but] it is probably … too late to stop the spread of a fable which has such inherent appeal.[315] (Holton 1969, p. 135)

Unfortunately compounding historical myth-making with disinformation, Einstein himself made numerous conflicting statements concerning the influence of aether-drift experiments, including, "there is no doubt that Michelson's experiment was of considerable influence on my work …" to "the Michelson-Morley experiment had negligible effect …." (Holton 1969, p. 134) In other reports he suggested that in his student years he "came to know the strange result of Michelson's [1881] experiment" claiming that "[t]his was the first path which led me to the special theory of relativity." (Einstein 1922) Nonetheless, by one later report, Einstein denied knowledge of the much more famous and definitive 1887 result prior to 1905. Many have found the claim implausible considering the turmoil this event had engendered taken together with the plethora of published responses in a field of science so pertinent to Einstein's own areas of research interest.[316] Beyond this however, the notion has also proven to be inconsistent with many of his other recollections and claims.[317]

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[314] Darrigol also considers the empiricist view which features the MM experimental result as having led to the second postulate, as myth making. (Darrigol 2005, p. 1)

[315] Typically, modern popular texts such as Crilly (2007, p. 184) reiterate and thus reinforce the myth: "The way Einstein dealt with Michelson's findings about the speed of light was to adopt it as a postulate."

[316] References in his correspondence to articles in the *Annalen der Physik* during 1898-1901 show his keen interest in that journal. Although complaining in 1907 that the libraries were closed during his free time, his letters again cite several recent journal articles. As Stachel notes: "It is reasonable to suppose that he continued to [read the journal] between 1902 and 1905." (Stachel 1989d, p. 260)

[317] Pais admits that Einstein "knew the 1895 paper of Lorentz in which the Michelson-Morley experiment is discussed at length." (Pais 1982, p. 133) Shankland recalls: "When I asked [Einstein] how he had learned of the Michelson-Morley experiment, he told that he had become aware of it through the writings

By contrast, and in later recall with Shankland, the experimental results that had influenced him most "were [Bradley's] observations on stellar aberration and Fizeau's measurements on the speed of light in moving water." (Shankland 1963, p. 48)[318] Elsewhere, Einstein held as paramount his questioning of the apparent need to construct asymmetrical electromagnetic explanations for the relative motions of a conducting coil and magnet which seemed illogical in the face of the relativity principle: "My direct path to the special theory of relativity was determined above all by the conviction that the induced electromotive force in a conductor moving in a magnetic field was nothing else but an electric field." (Einstein, 1952, cited in Rynasiewicz 1998, p. 171) Yet elsewhere, this primary motivational status was awarded to an almost dramatic epiphany that required that fundamentals of time cannot be absolutely defined, leading to a Poincaré-like insight as to the relativity of simultaneity. (Rynasiewicz 1998, p. 162) The 'germ' of this notion he claimed already existed in his *Gedankenexperiment*, conducted at the age of sixteen, in which he conjectured the paradoxical way a beam of light might appear to an observer travelling with it. (Boorse & Motz 1966c, p. 559; Pais 1982, p. 131) Alternatively, for Hirosige, correctly understanding the origins of Einstein's approach to theory construction "requires a reevaluation of the great influence of Mach on Einstein's thought," wherein Ernst Mach's "devastating criticism of the mechanistic world view … was of crucial importance for the formation of the theory …"[319] (Hirosige 1976, p. 6) Elsewhere, Stachel claims that the crucial "axiom of the absolute character of time, or rather of simultaneity," emerged from "extensive readings and discussions in the area now called the philosophy of science." Stachel based his view on Einstein's note that: "The critical reasoning required for the discovery of this central point was decisively promoted, in my case, especially by the reading of

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of H. A. Lorentz, but *only after 1905* had it come to his attention!" (Shankland 1963, p. 48, orig. emph.) Stachel discounts this claim when suggesting that "Lorentz's explanation of the M-M experiment seemed to Einstein so artificial that he [opted] ... for the physical relativity principle," and "Lorentz could explain away the failure to detect motion of matter relative to the ether convincingly to Einstein in all cases but one: the M-M experiment." (Stachel 2002) Here Einstein's note that there "was only one single test that seemed incompatible with Lorentz's [1895] theory, namely the interference experiment of Michelson and Morley" (Einstein 1909a, p. 382) may be cited as evidence of prior knowledge. Further evidence seems implicit in Einstein's correspondence with Mileva Marić, later to become his wife. In a letter of September 1899, he notes having read "a very interesting paper by Wien from 1898" in which Wien had listed the most important experimentation attempting aether detection including the negative MM result. (Holton 1994a) By 1908, however, Einstein rated the MM result as of prime significance: "If the Michelson-Morley experiment had not put us in the worst predicament, no one would have perceived the relativity theory as a (half) salvation." (Einstein to Sommerfeld, Jan. 1908, cited in Brown 2005, p. S85)

[318] It is of note however that "[Einstein] again and again cites the aberration of starlight and the results of Fizeau's experiment on the velocity of light in flowing water as *decisive* evidence in favor of Lorentz's interpretation of Maxwell's equations," (Stachel 2002, orig. emph.) at a time when he viewed the Lorentz 1895 solution as pre-eminent and conclusive in establishing the velocity of light as an empirical constant.

[319] As Hirosige views it: "… Mach's criticism of the concepts of absolute space and time, holding that determinations in space and time are no more than the determinations of an event by other events, must have been suggestive to Einstein." (Hirosige 1976, p. 81)

David Hume's and Ernst Mach's philosophical writings."[320] (Stachel 1989e, p. xxiii) On other occasions, however, he confessed to Abraham Pais more than once, that "without Lorentz he would never have been able to make the discovery of special relativity." (Pais 1982, p. 13) In summary, Stachel laments: "Einstein's replies are not always self-consistent, it must be noted" (Stachel 2002) – a failing Einstein readily conceded.[321] Pleading in his own defense, Einstein elsewhere recalled that "there were so many hidden complexities to motivate my thought, and the impact of each thought was different at different stages in the development of the idea." (Einstein 1922) Again, recognizing his lack of historical consistency when in conversation with Bernard Cohen shortly before his death, Einstein admitted that:

> He had always found himself a very poor source of information concerning the genesis of his own ideas. Einstein believed that the historian is likely to have a better insight into the thought processes of a scientist than the scientist himself. (Cohen 1955, p. 71)

In respect of this, an enquiry into the preliminary period Einstein variously claimed to have spent on theoretical development,[322] but that also includes an understanding of scientific modeling as an adjunct to theory construction, may well suggest enlightening but alternative interpretations.

Einstein and the 'principle theory' approach

The photoelectric effect had been quite accidentally discovered by Heinrich Hertz as part of his celebrated 1887 work that had empirically demonstrated Maxwell's conjectured electromagnetic waves.[323] Although its full significance was initially far

[320] Despite Hirosige's argument, in a letter to Michael Besso, 6 January 1948, Einstein professed that Hume's criticism of the notions of substance and causality had exerted more direct influence on his work than had his study of Mach. (Hirosige 1976, p. 57)

[321] Shankland pleads mitigation for Einstein's later years: "... the reader will find certain repetitions, and perhaps a few discrepancies. These, however, only emphasize the fact that all Professor Einstein's comments are made from memory, often on events that occurred 50 years before our meeting." (Shankland 1963, p. 47)

[322] In an answer to Shankland, Einstein stated that "he had started [this work] at age 16 and worked for ten years [at first only part-time]" and that he had abandoned many fruitless attempts. (Shankland 1963, p. 48). Elsewhere, this period is described as "the seven years that relativity had been my life." (ibid., p. 55) Conversely, his boyhood friend and mentor, Dr. Max Talmey, recalls that "[h]is special relativity theory was worked out between 1902 and 1905." (Talmey 1932, p. 170) In another account, Einstein recalls that he had sudden inspiration on the concept of time immediately after detailed discussions with his long-time friend Michael Besso in Berne regarding the contradiction apparent between the invariance of light velocity and the mechanical velocities addition rule. "Within five weeks the special theory of relativity was completed." (Einstein 1922; The Center for History of Physics 2004)

[323] Hertz observed that the voltage required for sparks to jump the metallic electrode gap of his induction coil operated emitter was lower when ultraviolet light was shone onto the electrodes. J. J. Thomson in 1899, and independently Hertz's student Philipp Lenard in 1900, showed that this photoelectric effect was

from understood, additional findings were to eventually suggest a possible inadequacy of the Maxwell continuum modeling of radiation, conceived by then as a comprehensive theory. Suspicion that the Maxwellian paradigm had been violated came from consideration of the spectral distribution characteristics of thermal radiation at equilibrium, inside the walls surrounding a cavity - more usually termed a 'black body.'[324] Concerted efforts to construct an adequate Maxwellian continuum model of radiation facilitated solely by the modeling of electromagnetic fields for free space, but one that also included interaction with matter, finally proved futile.

Physicist Max Planck, (1858-1947) who had studied under both Helmholtz and Kirchhoff, built on initial work by Kirchhoff that had shown this spectral distribution to be independent of the constitution of the black body and a function only of temperature. Then, incorporating the not unproblematic work of others, Planck derived a composite radiation formula. To achieve his synthesis Planck 'added' a spectral distribution law previously published by Wilhelm Wien, (1864-1928) that had proven useful for short waves and low temperatures, together with a formula, later known as the 'Rayleigh-Jeans' law[325], that was shown successful for long waves and higher temperatures. According to Max Born, "[t]his adding up was one of the most fateful and significant interpolations ever made in the history of physics ..." (Born 1948, p. 471) Integration of this composite conception led directly to a new radiation formula that Planck submitted for publication in October 1900. Empirical verification ensued wherein subsequent experimental results were "found to become more and more perfect with the improvement of methods of measurement." Nonetheless, as Born remarks, "it was only an interpolation, a real physical meaning had to be found." (Born 1948, p. 472) By December of 1900, Planck had made a crucial analysis of the new radiation law that drew on his extensive knowledge of thermodynamics. Employing a relationship between entropy and probability[326] previously advanced by his senior colleague Ludwig Boltzmann, Planck's analysis revealed a new constant with dimensions (energy × time) and which he called the "elementary quantum of action." Denoted by h, it is now always

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due to Thomson's recently discovered electrons being ejected from the metal when struck by light. (O'Connor & Robertson 2002c)

[324] termed 'black' insofar as in a perfectly efficient hollow enclosure in thermal equilibrium, the walls absorb all radiant energy falling on them.

[325] In 1900, Lord Rayleigh demonstrated that the energy per frequency interval, for this region of the spectrum, is proportional to the temperature and a necessary consequence of ordinary statistical mechanics applied to radiation, a view supported by physicist James H. Jeans again in 1909. (Born 1948, p. 471)

[326] Boltzmann's fundamental relation was $S = k \log P$, between S as the measure of the entropy of a system in a given state and P, the statistical probability of the system's being in that state. The proportionality constant, k, denoted "Boltzmann's constant" was however, neither introduced nor quantified by Boltzmann, this work having been left to his colleague Loschmidt. (Born 1948, p. 468)

quoted as “Planck’s constant.” (Born 1948, p. 472)[327] Combining this result with his prior work relating radiation to the entities responsible for its emission and absorption, modeled then as simple mechanical harmonic oscillators, Planck derived an expression for ‘black body’ radiation density. This grand synthesis now incorporated all previously known radiation laws of interaction with matter – the Stephan-Boltzmann law for total radiation and Wien’s displacement law[328] together with the two limiting laws of Wien for lower radiation temperatures and shorter wavelengths and the Rayleigh-Jeans law for higher temperatures and longer wavelengths.[329] Outstanding as this synthesis proved to be and reminiscent of prior great syntheses due to Kirchhoff and Maxwell, it was however his discovery of the ‘quantum of action’ inherent in the new statistically based formulation that proved the more significant event. As Born noted, it “has been generally acknowledged that the year 1900 of Planck’s discovery marks indeed the beginning of a new epoch in physics.” (Born 1948, p. 473)

Initially Planck tried hard to fit his quantum of action into the framework of a classical continuum approach, but with no success. It became apparent that a Maxwellian continuum model could not directly incorporate a ‘discontinuous’ quantum conception without destruction of its theoretical integrity and loss of the close agreement this new formulation had found with empirical data.

> [Planck] finally, and quite reluctantly, concluded that this new constant represented a drastic departure from classical physics and would have to be explained in terms of an atomism in radiation that was quite revolutionary. (Boorse & Motz 1966f, p. 489)

On reflection however, for Planck, the new phenomena should not invalidate empirically well-verified, and highly successful understandings offered by the Maxwellian electromagnetic paradigm. Although the terminology and philosophy of scientific modeling were not then clearly understood, Planck nonetheless successfully negotiated the potential failing that Whitehead had previously ascribed to Newton. In effect, Planck recognized the Mawellian scheme as a valid modeling of radiation yet potentially limited in scope. Failure to address one aspect, albeit a new and fundamental aspect, should not demand repudiation of an otherwise well-validated scheme. Integration into a wider electromagnetic world was obviously necessary, but a

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[327] the universal constant *h*, has the extremely small numerical value of ≈ 6.626 x 10^{-34} J.s (Mohr, Taylor & Newell 2007, pp. 40-41)
[328] Wien was awarded the 1911 Nobel Prize for Physics for his displacement law descriptive of radiation emitted from a perfectly efficient blackbody.
[329] from this consolidated formula, Planck quantified values for both *k* and *h* together with the then most accurate determinations of Avagadro’s (or Loschmidt’s) number, *N*, being the number of atoms (or molecules) per gram mole of any chemical substance, and also, utilizing Faraday’s law, determined the elementary electric charge, *e*. (Born 1948, p. 473)

designation of *domains of applicability* might allow such integration to be non-destructive:

> ... I think that first of all one should attempt to transfer the whole problem of the quantum theory to the area of *interaction* between matter and radiation energy; the processes in pure vacuum could then temporarily be explained with the aid of the Maxwell equations. (Planck et al. 1909, p. 396, orig. emph.)

For his part, Einstein was by no means immune from this turn-of-the century crisis in physics, which had found incitement in apparently paradoxical discontinuous phenomena. Common to much recent historical research into Einstein's scientific career has been the proposed identification of, and rationale behind, what is described as "the deep changes that affected some of his most basic scientific conceptions along the years." (Corry 1998, p. 95) Such changes, apparent in diametrically opposed assertions, are believed understandable in terms of an evolutionary but staged development. On this view, Einstein's student years and early period

> … was characterized by a cavalier approach to mathematics: he saw mathematics as a tool in the service of physical ideas and sought to command only as much mathematical knowledge as needed for his immediate purposes. He distrusted mathematical sophistication as such and repeatedly manifested his distaste for 'formal approaches' and 'pure speculation' as opposed to 'real physics.' (Corry 1998, p. 95)

Supporting this view, when writing in 1908 to Arnold Sommerfeld, Einstein expressed what may appear as a foundation stone of an earlier philosophy of science when stating: "It seems to me ... that a physical theory can be satisfactory only when it builds up its structures from elementary foundations." (Einstein cited in Brown 2005, p. S85) Such a conviction then seems to quite naturally support his contemporary and uncompromising empirically-based assertion that:

> When we say we have succeeded in understanding a group of natural processes, we *always* mean by this that a *constructive* theory has been found, which embraces the processes in question." (Einstein 1919, cited in Stachel 1989e, p. xxi emph. added) [330]

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[330] As Brown sees it, Einstein conceived of formulations such as thermodynamics as the archetypical example of a 'principle theory' in physics, as ones which, although necessarily supported by empirical results, nonetheless advance "unexplained observable regularities. On the other hand, statistical mechanics, or more specifically the kinetic theory of gases, was for Einstein the prime example of a 'constructive theory', one built on the 'elementary foundations' mentioned in his 1908 [Sommerfeld] letter. (Brown 2005, p. S86)

By contrast, and revealing the philosophical underpinnings of a supposed 'later' period, Einstein was to proclaim in his Herbert Spencer lecture on '*The Method of Theoretical Physics*:' "In a certain sense ... I hold it true that pure thought can grasp reality, as the ancients dreamed," - a belief expanded in the declaration:

> If… the axiomatic basis of theoretical physics cannot be extracted from experience but must be freely invented, can we ever hope to find the right way? … I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it … I am convinced that we can discover by means of pure mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. … the creative principle resides in mathematics. (Einstein 1934a)

Pivotal circumstances alleging an explanation for such a dramatic reversal of opinion has been suggested by various historians. For Pyenson, philosophical reorientation derived from his discovery of Immanuel Kant's idealism and its imputed capacity for realism, and that for Einstein, affirmed that truth could be discovered "from the aesthetics of mathematical expressions." (Pyenson 1985, p. 73) On the other hand, for Corry:

> … a major factor behind this fundamental change in Einstein's conception was the difficult struggle he maintained between 1912 and 1915 in the quest after a relativistic theory of gravitation [that] was characterized by a complex interaction between physical and mathematical considerations … The unexpected application of Riemannian geometry and of the so-called 'absolute differential calculus' of Gregorio Ricci-Curbastro and Tullio Levi-Civita to the solution of this quintessential physical problem obviously impressed Einstein very much.
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> … In later years, [Einstein] came to consider mathematics as the very source of scientific creativity. A main motive behind this change was the influence of two prominent German mathematicians: David Hilbert and Hermann Minkowski. (Corry 1998, p. 95)

However, Corry concedes that it was to take some time before Einstein fully succumbed to the notion that pure mathematics could "dominate the development of physical theory," – a dogma long held as central to the program of mathematical physics at Göttingen, and to which Einstein had been eventually and favorably exposed.

Nevertheless, however indicative such analyses may seem, placing the various Einstein pronouncements onto an historical time-line proves less suggestive of an orderly evolution of thought. Frequent changes of mind appear more as resulting from ongoing ideological indecision displaying as a dialectical vascilation responsive to successive crises, dilemmas and their hopeful resolutions. In support of this, in terms of a proposed evolutionary progression, his formal axiomatic approach of 1905 appears anachronistic when assessed against his 1917 rebuke of Klein's formal mathematical treatment of the equations of general relativity:

> It seems to me that you strongly over-rate the value of formal points of view. These may be valuable when an already *found truth* needs to be formulated in a final form, but they fail almost always as heuristic aids. (Einstein to Klein, cited in Corry 1998, p. 95)

Further, Einstein's eventual unqualified embrace of the Göttingen mathematical ethos as integral to his own pursuit of a formulation of a general theory of relativity, stands in chronological discord with yet later pronouncements wherein he claimed that: "all knowledge of reality starts from experience and ends in it. Propositions arrived at by purely logical means are completely empty as regards reality," (Einstein 1934a) and: "As far as the laws of mathematics refer to reality, they are not certain; and so far as they are certain, they do not refer to reality." (Einstein 1921a, 1940) It is to be noted that such later disparagements of a possible mathematical realism find reflection in his earliest thoughts, and in this sense his philosophy had come 'full circle.'

In what appears to have preconditioned his more profound dilemmas, in conversations with Robert Shankland in the period 1950-1954, Einstein revealed that he, like Ritz, had also entertained an electromagnetic ballistic hypothesis[331] prior to 1905, with such notions supplanting even earlier more conventional, but equally ill-fated, aether-based adventures. Albeit, after intense struggle "he could think of no form of differential equation which could have solutions … whose velocity depended on the motion of the source." (Shankland 1963, p. 49) Eventually, he had "rejected this [ballistic] hypothesis ... because it leads to tremendous theoretical difficulties."[332]

[331] Much unnecessary confusion has befallen commentary on Einstein's relationship with 'emission' theory wherein emission is mistakenly believed to necessarily demand a 'ballistic' dynamics in addition to some form of corpuscularity of the emanation. Many commentators fail to make this necessary distinction, such as Norton (2004, p. 42) and Hirosige (1976, p. 7) Although rejecting a ballistic dynamics, Einstein openly embraced an emission theory *exactly* as found in the corpuscularity of Newtonian emission theory as "independent entities emitted by the sources of light." (Einstein 1909a, p. 383)

[332] Martínez suggests that "Einstein rejected the [ballistic] hypothesis prior to 1905 not because of any direct empirical evidence against it, but because it seemed to involve too many theoretical and mathematical complications." (2004b, p. 11)

(Einstein to Viscardini, April 1922, cited in Martínez 2004b, p. 10) Also of note in his earlier years, Einstein "spent some time trying to modify Maxwell's equations," believing that: "If the Maxwell equations are valid with regard to one system, they are not valid in another. They would have to be changed." For years Einstein tried to change the Maxwell equations without succuss, and in consequence "had to spend nearly one year with fruitless thinking." (Hirosige 1976, p. 54) It was then against this already fractured and disturbing background, that in attempting to explain the photoelectric phenomena, Einstein now believed that not only the validity of the recently well-validated Maxwellian field continuum, but also fundamental Newtonian mechanics, to have been directly challenged. (Einstein 1905b, p. 99)

> Reflections of this type ... made it clear to me as long ago as shortly after 1900, i.e., shortly after Planck's trailblazing work, that neither mechanics nor electrodynamics could (except in limiting cases) claim exact validity. (Einstein 1949b, p. 568)
> All my attempts ... to adapt the theoretical foundation of physics to this [new type of] knowledge failed completely. It was if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built. (Einstein 1949b, p. 566)

Resolution dispelling such intolerable uncertainty was to eventually find expression in what appears as a significant but volatile departure (Pais 1982, pp. 172-173) [333] from his elsewhere evidence-based convictions:

> The longer and more despairingly I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results." (Einstein cited in Brown 2005, p. S88)

In assessing this judgment, it would be a distortion of the available evidence to suggest that Einstein, comparable to Newton, embraced theological or other extra-scientific notions detrimental to an appropriate scientific objectivity. It is nonetheless true that he also entertained strong personal convictions nourished by a philosophical orientation that took an optimistic view of man's place in an approachable, even benevolent universe. In this Einstein might be thought to portray an uncomplicated

[333] A solution with some similarities is offered by Pais in that "the origins of Einstein's later attitude toward the discovery of concepts by purely mathematical thinking may go back to 1905. The kinematic part of his June paper has the ideal axiomatic structure of a finished theory … which had abruptly dawned on him after a discussion with Besso. Is it possible that this experience was so overwhelming that it seared his mind and partially blotted out reflections and information that had been with him earlier…?" (Pais 1982, pp. 172-173)

realist.[334] On the other hand however, his 1905 grasp for the comfort afforded by unexplained universal principle appears as a specific arbitration in what was to become a life-long ideological struggle between competing approaches to realism. For Einstein, the repercussions of the initial quantum crisis had rendered the potential for sound theory construction extremely precarious, leading to the admission: “By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts.” (Einstein 1949b, p. 568) Born of this despair, an emboldened attempt to retrieve realist certainty then seems to have necessitated adopting a brief but otherwise aberrant departure from his avowed adherence to evidence-based science and its constructive approach to theory – a provisional ‘principle theory’ approach that found virtue in ascribing realism to an essentially neo-Pythagorean proposition in which physical fact was to be extracted directly from abstraction. (Pais 1982, p. 13) It then appears in keeping[335] with such a philosophical adventure that, according to his own claim, he adopted the independence of the velocity of light from its source, due to “its simplicity and easy practicability,”[336] (Einstein to Ehrenfest, June 1912, cited in Martínez 2004b, p. 10) - an idea he would eventually return to again embrace with the claim: “Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas.” (Einstein 1934a) Commenting in a moment where biographer turns critic, Pais has cautioned that:

> It is true that the theoretical physicist who has no sense of mathematical elegance, beauty, and simplicity is lost in some essential way. At the same time it is dangerous and can be fatal to rely exclusively on formal arguments. It is a danger from which Einstein himself did not escape … (Pais 1982, p. 172)

Thus, when counting as paramount the philosophical notion of ‘simplicity’ as justifying the axiomatic independence of his electrodynamics proposal from direct empirical reliance,[337] Einstein also seeded future conjecture that his procedure was possibly less a

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[334] Pais notes that Einstein “lived by a deep faith – a faith not capable of rational foundation … His realism and his optimism are illustrated by his remark: ’Subtle is the Lord, but malicious He is not’.” (Pais 1982; Shankland 1963, p. 57)

[335] Stachel provisionally agrees: “There is no contemporary evidence showing when Einstein adopted this [principle theory] point of view (he first indicated it in print as early as 1907). I believe he had done so by 1905. The structure of the 1905 SRT paper is certainly compatible with his having done so.” (Stachel 2002) Brown also notes that Einstein’s 1908 correspondence with Sommerfeld positively compares the status of his electrodynamics with that of thermodynamics thereby denoting it as a principle theory. (Brown 2005, p. S86) According to Pais, Einstein stated more conclusively in 1919 that: “The theory of relativity is a theory of principle.” (Pais 1982, p. 27)

[336] Pais claims that: “Einstein was driven to the special theory of relativity mostly by aesthetic arguments, that is, arguments of simplicity.” (Pais 1982, p. 140)

[337] [the scientist] “may even appear as a *Platonist* or *Pythagorean* in so far as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research.” (Einstein cited in Pais 1982, p. 13)

carefully considered but inscrutable work of intellectual virtuosity, than a precipitous and philosophically vulnerable reaction to calamitous circumstances.

Poincaré and Einstein – differing philosophies of science

Far more important, however, than conjectures suggesting possible motivation for engaging a postulate or axiomatized approach to Lorentz invariance, is the import of its adoption in terms now resurfacing as significant for the philosophy of science. Historians such as Darrigol[338] and Rothman[339] have laudably pleaded that concerns with 'single authorship' and awards of priority, are to be held as misleading and misguided in what was manifestly a multi-faceted and complex group development. Essentials of the questions raised are nonetheless pertinent to the resurgence of a particular issue of priority, wherein Poincaré's extensive presentations on relativistic topics, prior to June 1905, have found champions openly challenging entrenched mainstream claims elsewhere made on Einstein's behalf.[340] Prominent in the rise of disputational literature has been the 1953 second volume of mathematician Sir Edmund Whittaker's 1910 publication "*A History of the Theories of Aether and Electricity.*" The revised edition is notable for his attribution of "the special theory of relativity almost exclusively to Lorentz and Poincaré." (Brown 2009; Whittaker 1953, pp. 27-77) As Holton summarizes:

> … commentaries on the historical origins of the theory of relativity have tended to fall into two classes, each having distinguished proponents: the one views it as a mutant, a sharp break with respect to the work of the immediate predecessors of Einstein; the other regards it as an elaboration of then current work, e.g., by Lorentz and Poincaré. (Holton 1960, p. 628)

Certain historical facts appear, however, not to be in dispute, such as:

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[338] "It seems wiser to acknowledge that Lorentz, Poincaré, [Minkowski] and Einstein all contributed to the emergence of the theory of relativity, that Poincaré and Einstein offered two different versions of this theory" and that Einstein advanced the form that was to become preferred. "This attitude avoids biases in the assessment of respective contributions, and it is better adapted to historical studies of the reception and later evolution of relativity theory." (Darrigol 2004, p. 619)

[339] "...if the history of science has any relevance to the doing of it, surely it is to remind us that science is a collective enterprise and to engender in us a humble awareness that the landscape of science would appear very different had the vast unrecognized majority never existed." (Rothman 2006, p. 113)

[340] For Goldberg, these are represented by "a stubborn and persistent few [who] continue to champion Henri Poincaré and H. A. Lorentz, either as the sole creators of the theory or [as those] who provided Einstein with the necessary insights to construct a finished piece of work." (Goldberg 1967, p. 934) Together with T. Whittaker, Katzir includes Jerzy Giedymin, Eli G. Zahar, Oliver Darrigol and Peter Galison. (Katzir 2005)

> The term "principle of relativity"[341] was used by Poincaré for the first time in September 1904 in his address at the International Congress of Arts and Science at St. Louis [where he defined it as] 'The laws of physical phenomena [mechanical and electromagnetic must] be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation' … [however, a principle which now requires] an entirely new kind of dynamics which must be characterized above all by the rule, that no velocity can exceed the velocity of light. (Cerf 2006, p. 819; Hirosige 1976, p. 49; Whittaker 1953, p. 30)

> The first to establish the explicit connection between the terminology and the ideas of group theory and the Lorentz covariance of the equations of electrodynamics was Poincaré, in his 1905 article.[342] (Corry 1997b, p. 308)

> [Poincaré was] the first to use four-dimensional [space - time] coordinates in connection with electrodynamics and the principle of relativity. (Corry 1997b, p. 308)

Poincaré's conventionally advertized failure then, at least in terms of a claim to priority, appears to turn on criteria other than the 'plain text' of historical pronouncements. Much depends on meanings intended for terms such as 'priority' and the 'theory of relativity.' Is the theory of relativity (and here of necessity only as the special theory) primarily a claim made for the 'principle' or 'law' of relativity as being universally applicable to all phenomena, mechanical and electromagnetic, that then entails local Lorentz invariance, or is it primarily a theory of invariance interpreted by relativistic kinematics that then entails the equivalence of mechanical and electromagnetic phenomena, or is it primarily a claim to a geometry unifying space and time and presenting as a four-dimensional spacetime continuum whose formalism entails the principle of relativity and inertial frame invariance?[343] In addition, much that informs the contentions and ongoing debates depends not only on the choice of a primary theoretic but whether differences in

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[341] According to Keswani & Kilmister, it was James Clerk Maxwell in 1877 who first used the phrase "the doctrine of relativity of all physical phenomena" in that sense equivalent to the relativity principle of Poincaré and postulate of Einstein. (Maxwell 1920, p. 80) Previously, in 1899, Poincaré had referenced the "law of relativity," and in 1900, the "principle of relative motion." (Keswani & Kilmister 1983) In his "*Sur la dynamique de l'electron*" of 5 June, 1905 he defined his "Postulate of Relativity" – "It seems that the impossibility of experimentally detecting the absolute motion of the earth is a general law of nature; we naturally incline to assume this law, which we shall call the Postulate of Relativity, and to do so without any restriction." (Darrigol 2005, p. 12)

[342] In his pursuit of an exclusive Einstein priority claim, Pais unfortunately blurs the order of events: "In 1905 Einstein and Poincaré stated independently and almost simultaneously (within a matter of weeks) the group properties of the Lorentz transformations and the addition theorem of velocities." (Pais 1982, p. 21)

[343] Logunov (2005, p. 4) is typical of many for whom, following Poincaré and Minkowski, "the essence of relativity theory consists in the following: the special theory of relativity is the pseudo-Euclidean geometry of space-time. All physical processes take place just in such a space-time."

formulations and approach are to be viewed as constituting different theories or simply different competing, or complementary versions of the same theory.[344]

Thus, the distinction between an 'immobile' aether[345] and an aether deprived of this last vestige of mechanics (Cerf 2006, p. 821) or the description of separate 'local' and 'true' times as against their mutual identification (Cerf 2006, p. 820) are potentially seen as denoting essentially distinct but competing invariance formalisms and for many commentators, constitute the criteria upon which decisions of validity and priority must turn. However, these issues are further complicated if priority is to be awarded. For some, precedent significance appears to rest in a published 'first mention' with less weight attached to the context or perceived understandings and intentions of the author. For others such understandings and intentions are paramount – the idea must conform substantially to its meaning as now understood or as now held to be "correct." Thus For Hirosige,

> Lorentz' and Poincaré's theory was *not* equivalent to the theory of relativity *as properly understood*" [wherein Poincaré's discussion] "quite differs in spirit from Einstein's … In Poincaré's conception the validity of the principle of relativity, which has been inferred from experience, must be given an explanation … it is an 'empirical truth' which might some day be denied by an experiment." (Hirosige 1976, pp. 5, 49, 50 some emph. added)

In similar vein, Poincaré's 1900 exposition that a body emitting radiant energy E thereby loses inertia $m = Ec^{-2}$ (Ives 1953, p. 541) as an equivalent algebraic form[346] of the more usual $E = mc^2$, is dismissed due to "Poincaré's lack of insight into certain aspects of the physics involved," wherein he supposedly shows "his unawareness of the equivalence between inertial mass and energy." (Cerf 2006, p. 821)

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344 For Herbert Dingle: "… the 'principle of relativity' had various meanings, and the theories associated with it were quite distinct; they were not different forms of the same theory." (Dingle 1965) On the other hand, for Keswani, (1966) such theories are to be accounted as substantially the same. However, elsewhere Dingle suggested that "Until the first World War, Lorentz's and Einstein's theories were regarded as different forms of the same idea …" (Dingle 1967). Giannetto further reminds us that the history of physics "can be considered as a conflict among different worldviews. Every theory, as well as every different formulation of a theory implies a different worldview." (Giannetto 2009)

345 For Poincaré, the aether was equally a convention. In a lecture of 1887/8 he noted: "It matters little whether the ether really exists … this hypothesis is convenient for us for the explanation of the phenomena … no doubt, some day the ether will be thrown aside as useless." (Darrigol 2005, p. 9)

346 Prof. Umberto Bartocci, University of Perugia, has called attention to the prior publication of the equation $E = mc^2$, by Olinto De Pretto, an industrialist from Vicenza, in the scientific periodical, *Atte*, in 1903, and republished, but with little understanding of its wider significance in 1904, by Veneto's Royal Science Institute. (Carroll 1999) Priority for the equation is also awarded elsewhere to S. Tolver Preston in 1875. (Moody 2003)

Nevertheless, the predominant case for theoretical distinction and the principal claim to novelty for Einstein's work, assuring both his eventual iconic place as "Person of the Century"[347] and classification as "genius," is ubiquitously claimed to stem from his unique insights wherefrom universal formal principle was secured "by elevating the principle of relativity from a heuristic conjecture to a fundamental proposition."[348] (Holton, 1988, cited in Cerf 2006, p. 821) Despite Einstein's later personal equivocations,[349] in exploiting his "new point of view," (Scribner 1964, p. 672) great store is placed in Einstein's axiomatization of physics. On this view, Poincaré's empirical analysis "indicates a hesitation," and thus his approach stands in crucial contrast to that of Einstein "who elevated this principle to an *a priori* postulate which stood at the head of his theory." (Goldberg 1967, p. 937) Thus, elevation of then familiar propositions to the status of empirically independent and unexplained postulates, is extolled as the crucial step that provides an essential disparity between the "hypothetical," and thereby provisional, nature of Poincaré's 1905 exposition, (Cerf 2006, pp. 819-820) and the acclaimed certainty and unequivocal verities of a new kinematics. In agreement, Karl Popper claimed: "Though Einstein appears to have known Poincaré's *Science and Hypothesis* prior to 1905, there is no theory like Einstein's in this great book … in [Poincaré's] article there is only a most inspiring programme sketched for a relativity theory – not the theory itself" (Popper 1966, p. 332) For Pais, "In all his life … Poincaré never understood the basis of special relativity." (Pais 1982, p. 21) In Scribner's assessment, Einstein formalized what Poincaré held conditional and tenuous and thus, although Poincaré was the first to advocate "that mechanical relativity should be generalized into a universal principle embracing all physical laws, the fact remains that he did not construct a *theory* of relativity … Properly speaking [Einstein's decisive axiomatic treatment] was the first *theory* of relativity." (Scribner 1964, pp. 674,676, orig. emph.) In further condescending detail, Cerf claims that:

> The conjectural nature of Poincaré's relativity principle … is very likely the reason why Poincaré did not find the ultimate consequences of the relativity principle. It has often been said that Poincaré's handicap was his 'conventionalist epistemology' which granted the laws of geometry and physics at most the nature of a useful convention, without a meaning

[347] Time magazine cover, 31 Dec. 1999, (Golden 1999) and cover picture 1 July 1946. (Pais 1994)

[348] As Einstein, in later correspondence, saw it: "The new feature was the realization of the fact that the bearing of the Lorentz-transformations transcended their connection with Maxwell's equations and was concerned with the nature of space and time in general." (Einstein to Carl Seelig, cited in Cerf 2006, p. 821)

[349] In interviews with Einstein, conducted by gestalt psychologist, Max Wertheimer beginning in 1916, Einstein was to claim: "… it was not the axioms that came first … 'No really productive man thinks in such a paper fashion' … [and it is] not at all the way things happened in the process of actual thinking." (Rynasiewicz 1998, p. 169)

of deeper reality. For Poincaré differing mathematical representations of the universe could constitute equivalent conventions. (Cerf 2006, p. 819)

In such evaluations, the innate caution intrinsic to a hypothetico-deductive methodology and the ultimately ancillary and thus subordinate role assigned to mathematics as an adjunct modeling tool, as consistently advocated by Poincaré, are assessed as cardinal impediments to his achieving both correct conceptualizations and successful theory construction.[350] In such considerations at least, issues pertinent to the earlier Newton-Huygens contention again become apparent, both in terms of opposing approaches to the conduct of science and their ensuing repercussions. Poincaré, as with Huygens before him, conducted his science in the belief that "[e]very generalization is a hypothesis," and that, although some precautions must attend its employment, "[h]ypothesis … plays a necessary role." Nonetheless, in the cause of theory construction, whether adopted as "artifices for calculation, or to assist our understanding by concrete images, [hypotheses] must be confirmed or invalidated by experiment … [and yet] Whether verified or condemned, they will always be fruitful." (Poincaré 1905c, pp. 150, 153) Elsewhere, both praising the employment of hypotheses on historical grounds and in terms which may be a thinly veiled derision of Newton's imperious approach, with its aversion to the provisional and robust expectations of theoretical realism, Poincaré suggested:

All our masters, from Laplace to Cauchy, proceeded along the same lines. Starting with clearly enunciated hypotheses, they deduced from them all their consequences with mathematical rigour, and then compared them with experiment.

A mind accustomed to admire such models is not easily satisfied with a theory. Not only will it not tolerate the least appearance of contradiction, but it will expect the different parts to be logically connected with one another, and will require the number of hypotheses to be reduced to a minimum.

This is not all; there will be other demands which appear to me to be less reasonable. Behind the matter of which our senses are aware, and which is made known to us by experiment, such a thinker will expect to see another kind of matter —the only true matter in its opinion—which will no longer have anything but purely geometrical qualities, and the atoms of which will be mathematical points subject to the laws of dynamics alone.. And yet

[350] In what appears as an extraordinary misrepresentation of Poincare's position, Scribner suggests that "the form in which Einstein was to develop such a theory was plainly in accord with Poincaré's own philosophy of science and with his views about the nature of scientific explanation." (Scribner 1964, p. 674)

> he will try to represent to himself, by an unconscious contradiction, these invisible and colourless atoms, and therefore to bring them as close as possible to ordinary matter. Then only will he be thoroughly satisfied, and he will then imagine that he has penetrated the secret of the universe. (Poincaré 1905c, pp. 213-214)

In contrast, Scribner notes that Einstein "nowhere attempted to account for the principle itself in terms of other physical hypotheses. In [this respect] Einstein's scientific strategy may be compared to Newton's …" (1964, p. 676) In overview, a methodology chosen specifically to consolidate both a pre-determined certainty and a conjoined robust realism, whether theologically or metaphysically incited, can be argued as informing both Newton's and Einstein's approaches, and may go far towards accounting for the extra-scientific vigor and emotional content often to be found associated with their contentions.[351] In Poincaré's opposing view, the role of mathematical physics is that of an "auxiliary," and although it is credited with rendering undeniable service as when suggesting areas for future investigation, neo-Pythagorean pretensions as to being a source of theoretical certainty are denied:

> Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty. These are two points that cannot be questioned … However solidly founded a prediction may appear to us, we are never *absolutely sure* that experiment will not prove it to be baseless if we set to work to verify it. (Poincaré 1905c, pp. 140, 144)

An understanding of the nature and role of "principles" in physics, as interpreted by Poincaré, then becomes equally important. In some respects, in direct contrast to Einstein's approach, Katzir notes Poincaré's

> … general conviction of the central role of principles in physics, which led him to attribute increasing importance to the principle of relativity. To Poincaré, the principles of physics are abstractions and generalizations of physical laws, and are both the basis and aim of physical research. [Nevertheless] [t]hey are [only] constraints on all physical theories, rather than independent axioms as in geometry … [and] a principle that is immune to contradiction is useless (Katzir 2005, pp. 275, 276)

In expressing this view, Poincaré kept faith with a philosophy of science that has been ably defended by others. For Whitehead, the procedure of rationalism – the central tenet

[351] Unfortunately, invective and irresponsible nationalism have often entered the debate as with: "There have always been large numbers of crackpots of varying levels of competence irrationally attacking Einstein's physics from the very beginning. This is a form of anti-Semitism coupled with weak intellect …" (Sarfatti 2002)

of scientific endeavor – was to be recognized in the pursuit of fertile analogies. (Whitehead 1938a, p. 98) Nevertheless, such meaningful patterns are inherently partial. Each can capture no more than a useful insight into a restricted domain of phenomena leaving an irremediable tension between perceivable relationships and diversity. Thus, for Whitehead, the limitation of rationalism, and of necessity the denial of claims to an absolute certainty, is "the inescapable diversity." On his view, human knowledge in general, and physical theories just as surely, remain as works in progress – an ongoing "adventure in the clarification of thought," capable of characterization as "a baffling mixture of certainty, ignorance, and probability" (Whitehead 1929, pp. 9, 205)

In further arguing his philosophy of science, Poincaré was critical of a naive belief in a supposed pervasive simplicity of natural law: "It is not certain that Nature is simple,"[352] and thence prompting the question: "Can we without danger act as if she were?"

> If we study the history of science we see produced two phenomena which are, so to speak, each the inverse of the other. Sometimes it is simplicity which is hidden under what is apparently complex; sometimes, on the contrary, it is simplicity which is apparent, and which conceals extremely complex realities … [here] the simplicity is only apparent, and the coarseness of our senses alone prevents us from seeing the complexity. (Poincaré 1905c, pp. 147-148)

In contemporaneously advancing such differences of approach, it may be felt that, yet again, history had presented a fertile opportunity for the potential resolution of perplexing problems by open discussion and amicable exchange between able antagonists. Yet again, however, the opportunity was lost. As Darrigol notes: "Having dismissed the single-author myth, we are left with the intriguing similarity of two theories whose authors ignored each other's contributions." (2004, p. 619) Whereas the Lorentz reaction, although earlier openly contentious, had given over time an appearance of increasing acquiescence, it was certainly public. On the other hand, Henri Poincaré's reaction has presented historians of science with an enigma. Poincaré's 1909 Göttingen lecture on relativity made no mention of Einstein,[353] a trait that continued

[352] More recently, Maudlin has fielded a view contrary to Einstein's optimism: "One way or another, God has played us a nasty trick. The voice of Nature has always been faint, but in [the case of theory inconsistency] it speaks in riddles and mumbles as well … the real challenge falls to the theologians of physics, who must justify the ways of a Diety who is, if not evil, at least extremely mischievous." (Maudlin 2002, p. 242)

[353] Poincaré gave a series of six lectures in Göttingen in April of 1909. In the last lecture, entitled "*La Mécanique Nouvelle,*" the new relativistic mechanics was discussed on the basis of three hypotheses: the velocity of light as a limiting velocity; the general applicability of the relativity principle thence requiring

with all his subsequent discussions and published papers on the subject, and in which references were always to the "Lorentz mechanics."[354] This seemingly pointed omission was also to be noted in Einstein's behavior with Poincaré being only mentioned once, and that not in the context of relativity.[355] (Darrigol 2004, p. 624; O'Connor & Robertson 1996) To what extent Einstein may have recognized in Poincaré's philosophy, a detailed yet scathing repudiation of his own chosen path to theory construction and to what extent a recognition of incongruence of philosophical orientation may have jointly contributed to their silent standoff, we may never know, their mutual silence precluding resolution through further enquiry. However, claiming that significant progress has more recently been made in defining and delineating the respective Poincaré and Einstein roles in the formation of special relativity theory, Darrigol believes that both historians and philosophers of science now have "a more precise idea of the similarities and differences between the two thinkers' contributions ... [and yet] the historical connection between these contributions remains highly mysterious." (2004, p. 614)

Einstein's later misgivings

Significantly less recalled in the literature eulogizing a claimed Einsteinian theoretical supremacy, are Einstein's own later expressions of uncertainty and misgivings, if not personal recrimination. By 1907, in response to a critique by his friend Ehrenfest, Einstein displayed great caution, only portraying his relativistic kinematics in the most provisional terms - a tentative, diminished, and incomplete[356] status which contrasts conspicuously with that accorded to it by later generations of physicists:

Lorentz inertial frame invariance; and the FitzGerald-Lorentz contraction. Einstein's paper, now four years previous, was not mentioned. (Pais 1982, pp. 167-168)

[354] Poincaré "often mentioned Einstein [in] connection with statistical mechanics and quantum theory, but never in connection with relativity. The Nobel prize committee adopted the same attitude." (Brown 2009)

[355] Einstein's sole academic reference to Poincaré is found in a 1921 lecture on non-Euclidean geometry. Einstein never academically acknowledged Poincaré's contribution to special relativity, but, not long before his death, when discussing the Lorentz contribution admitted to his biographer Carl Seelig: "Lorentz had already recognised that the transformation named after him is essential for the analysis of Maxwell's equations, and Poincaré deepened this insight still further ...." (Einstein to Seelig, 19 Feb. 1955, cited in Darrigol 2004, p. 624)

[356] As Brown sees it: "… as late as 1909 Poincare was not prepared to say that the equivalence of all inertial frames combined with the invariance of (two-way) light speed were sufficient to infer Einstein's model. He maintained that one must also stipulate a particular contraction of physical objects in their direction of motion. This is sometimes cited as evidence that Poincare still failed to understand the situation, but there's a sense in which he was actually correct. The two famous principles of Einstein's 1905 paper are *not* sufficient to uniquely identify special relativity, as Einstein himself later acknowledged. One must also stipulate, at the very least, homogeneity, memorylessness, and isotropy." (Brown 1999a)

> The principle of relativity, or, more exactly, the principle of relativity together with the principle of the constancy of velocity of light, is not to be conceived as a "complete system," in fact, not as a system at all, but merely as a heuristic principle which, when considered by itself, contains only statements about rigid bodies, clocks, and light signals. (Einstein 1907, p. 236)

Even by 1911 at a scientific meeting in Zurich, he still referred to his relativistic kinematics only heuristically as "a principle that narrows the possibilities." (Brown 2005, p. S85) and collectively only as the "principle of relativity" rather than in the terms of a maturing theory as was by then already claimed by others. Moreover, in his acceptance speech for the 1921 Nobel Prize in Physics,[357] after a rehearsal of key points of the special theory, Einstein was to note "in passing ... the logical weakness of this exposition ... [being] the lack of an experimental criterion for whether a material point is force-free or not; therefore the concept of the inertial frame remains rather problematical." (Einstein 1923, p. 483) In addition, as Brown saw it, the

> price to be paid for the [original] strategic retreat to a principle theory approach was not just loss of insight; Einstein became increasingly uneasy about the role played by rods and clocks in this approach. (Brown 2005, p. S89)

Einstein admitted, in respect of the 'rigid body' upon whose objectivity so much of the exposition depended, "there is some justification for challenging." Such a body was now conceded to be "only approximately achieved in Nature, not even with desired approximation; [such that] this concept does not therefore strictly satisfy the 'stipulation of meaning'." (Einstein 1923, p. 483) Nonetheless, in attempts which have been assessed as displaying "a peculiar ambivalence," (Uchii 2001) and which were to occupy a significant role in his later apologetics, Einstein elsewhere tried to define and defend a tangible relationship between the mathematical abstraction of a Euclidean-based coordinate geometry and the ontological entity denoted as the "practically-rigid body." Central to these deliberations and nurturing the charge of "ambivalence," appears the ancient tension apparent between a neo-Pythagorean evaluation of the substantive role of axiomatic geometry in physics and the more tempered view based on its acute representational power mediated by analogy.

[357] "During the selection process in 1921, the Nobel Committee for Physics decided that none of the year's nominations met the criteria as outlined in the will of Alfred Nobel. According to the Nobel Foundation's statutes, the Nobel Prize can in such a case be reserved until the following year, and this statute was then applied. Albert Einstein therefore received his Nobel Prize for 1921 one year later, in 1922." (Nobel Foundation 2011)

In his 1921 address on "*Geometry and Experience*" before the Prussian Academy of Sciences in Berlin, and building on a note originally advanced in 1916,[358] Einstein attempted two different forms of redress, with what might be argued as exhibiting differing potentials for success. Initially, he reiterated a 1916 observation that it "is clear that the system of concepts of axiomatic geometry alone cannot make any assertions as to the relations of real objects of this kind, which we will call practically-rigid bodies." However,

> ... To accomplish this, we need only add the proposition: - Solid bodies are related, with respect to their possible dispositions, as are bodies in Euclidean geometry of three dimensions. Then the propositions of Euclid contain affirmations as to the relations of practically-rigid bodies. Geometry thus completed is evidently a natural science; we may in fact regard it as the most ancient branch of physics. ... We will call this completed geometry "practical geometry," and shall distinguish it ... from "purely axiomatic geometry." (Einstein 1921b, p. 211)

It remains to be answered, however, how the mere pronouncement of such a proposition, even by a physicist turned priest, might create the necessary miracle whereby those practical impediments, previously judged to exclude the rigid body from conformity to a rigorous 'stipulation of meaning,' might thereby be expunged. The desired correlation of rude physical entities with pristine abstract concepts is, at foundation, more correctly found by employment of representation through analogy – not as Einstein appears to have wished – as identity through alchemical transfiguration.

The import of such an apologetic also finds its counter in Poincaré's thinking:

> If geometry were an experimental science, it would not be an exact science. It would be subjected to continual revision. Nay, it would from that day forth be proved to be erroneous, for we know that no rigorously invariable solid exists. [Further,] it would be a mistake to conclude ... that geometry is, even in part, an experimental science. If it were experimental, it would only be approximative and provisory. And what a rough approximation it would be! (Poincaré 1905c, pp. 49-50, 70)

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[358] "... we now supplement the propositions of Euclidean geometry by the single proposition that two points on a practically rigid body always correspond to the same distance (line-interval), independently of any changes in position to which we may subject the body, [thus] the propositions of Euclidean geometry then resolve themselves into propositions on the possible relative position of practically rigid bodies. (Einstein 1916, Part I-01)

Einstein's second form of defence devolves ultimately to a capitulation but includes an appeal for compromise. Of interest to the historian and philosopher alike, given his conspicuous aversion to mention of Poincaré's prior relativity, is Einstein's attempt to reconcile with his understanding of Poincaré's critique. Once again, the alchemical marriage of abstraction and experiential reality is attempted as efficacious yet strangely within an envelope that appears to concede its failure yet with a plea for clemency based solely on pragmatism:

> Why is the equivalence of the practically-rigid body and the body of geometry which suggests itself so readily-denied by Poincaré and other investigators? Simply because under closer inspection the real solid bodies in nature are not rigid, because their geometrical behaviour, that is, their possibilities of relative disposition, depend upon temperature, external forces, etc. Thus the original, immediate relation between geometry and physical reality appears destroyed, and we feel impelled toward the following more general view, which characterizes Poincaré's standpoint. Geometry (G) predicates nothing about the relations of real things, but only geometry together with the purport (P) of physical laws can do so. Using symbols, we may say that only the sum of (G) + (P) is subject to the control of experience. Thus (G) may be chosen arbitrarily, and also parts of (P); all these laws are conventions. All that is necessary to avoid contradictions is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience. Envisaged in this way, axiomatic geometry and the part of natural law which has been given a conventional status appear as epistemologically equivalent.
>
> Sub specie aeterni Poincaré, in my opinion, is right. The idea of the measuring-rod and the idea of the clock co-ordinated with it in the theory of relativity do not find their exact correspondence in the real world. It is also clear that the solid body and the clock do not in the conceptual edifice of physics play the part of irreducible elements, but that of composite structures, which may not play any independent part in theoretical physics. But it is my conviction that in the present stage of development of theoretical physics these ideas must still be employed as independent ideas; for we are still far from possessing such certain knowledge of theoretical principles as to be able to give exact theoretical constructions of solid bodies and clocks. (Einstein 1921b, p. 212)

A number of commentators since 1905 have commented on such qualms including Pauli and Eddington in the 1920s, Swann in the 1940s, and Lajos Jánossy and John Bell in the 1970s. "All of these authors called for a more constructive version of special relativity." (Brown 2005, p. S89) Such a constructive revision was not, however, forthcoming. Yet, for Einstein, acknowledgement of the realist difficulties deriving from his employment of the practically-rigid body in the context of abstract idealism were not exhausted. He further acknowledged a logical inconsistency wherein his original 1905 argument form was demonstrated to be self-referencing, and in an admission that finds some agreement with critics such as Essen, conceded:

> It is ... logically unjustifiable to base all physical consideration on the rigid or solid body and then finally reconstruct that body anatomically by means of elementary physical laws which in turn have been determined by means of the rigid measuring body." (Einstein 1923, p. 483)

This logical inconsistency,[359] finds a positive resonance with Essen's objections to Einstein's claimed cavalier treatment of primary dimensional definition. This logical vulnerability was to remain unresolved, and in later review, a fundamental aspect of the problem resurfaced in his 1949 *Autobiographical Notes*:

> One is struck [by the fact] that the theory [of relativity] ... introduces two kinds of physical things, i.e. (1) measuring rods and clocks, (2) all other things, e.g., the electromagnetic field, the material point, etc. This, in a certain sense, is inconsistent; strictly speaking measuring rods and clocks would have to be represented as solutions of the basic equations ... not, as it were, as theoretically self-sufficient entities … it was clear from the very beginning that the postulates of the theory are not strong enough to deduce from them sufficiently complete equations [360]... in order to base upon such a foundation a theory of measuring rods and clocks ... (Einstein, Autobiographical Notes, pp 59, 61, cited in Brown 2005, p. S89)

However, beyond Einstein's own damaging reviews, it is to be noted that the world-wide research program, wherein the claims made by Einstein's version of local Lorentz invariance are submitted to acute experimental re-evaluation, exhibits the belief that such invariance is now to be held in the nature of a 'law' or 'empirical truth,'and as such, vulnerable to experimental reassessment as Poincaré required. Such a research program intrinsically regards the constancy of light-speed as a regularity perceived to some degree of precision and for some range of measurement, the limits of whose domain of applicability should eventually yield quantitatively to an appropriate

[359] The inertial reference frame, as integral to Einstein's mathematical development, rests on "spatial coordinates which are right-handed rectilinear Cartesians based on a standard unit of length, and time-scales based on a standard unit of time." (Fitzpatrick 2006c) In his early discussions on relativity, Poincaré had already argued: "How do we perform our measurements? By superposing objects that are regarded as rigid bodies, would be one first answer ... but this is no longer true in the present theory, if one assumes the Lorentz contraction." (Poincaré cited in Darrigol 2005, p. 14)

[360] Brown sees early evidence of this recognition in Einstein's 1908 letter to Sommerfeld, wherein his theoretical approach is characterized as offering only a "half" salvation. In its comparison with the principle of thermodynamics, "Einstein was emphasizing the limitations of SR, not its strengths." (Brown 2005, p. S85)

empirical enquiry. Here, it is Poincaré's *philosophy of science* [361] that finds some vindication. It was that philosophy that held those formulations relating relative motion and electrodynamics as provisional, and encouraged further experimental programs hopefully leading to a more constructive explanation.

The 'second postulate' and its predisposing model of light

Of all the mysteries encountered in the convoluted history of optics, the apparent constancy, under all dynamic conditions, of that measure denoted the 'speed of light,' (meaning, more accurately, the 'propagation rate of electromagnetic phenomena in vacuum') was to prove the most enigmatic. Given its proclaimed failure to conform to the classical, and universally agreed common sense, edict of a Galilean velocity addition rule, it has even been asserted in the genre of popular science writing that: "The speed of light just happens to be the most nonsensical thing ever discovered." (Zukav 1980, p. 127) However, for a majority of the early twentieth-century physics community, practical justification for the "constancy of light-speed" proposition rested on what appeared as unimpeachable empirical grounds. On the other hand, in the mathematical abstractions of Minkowski's spacetime proposal, entails of a constant spacetime interval required that a unique, but non-theoretically quantifiable, velocity, should exhibit as a constant despite relative motion.[362] It then remained for an empirically based investigation to identify this velocity as that quantified from a dynamic interpretation of Maxwell's equations. Then, raised to the level of postulate, this velocity was to become enshrined in the now preferred Einstein approach to invariance as fundamental to one of the foundational theorems of modern science.

[361] Claiming Lorentz and Poincaré, not only as valued scientific pioneers but as physicists retaining an effective relevance in the modern context, has been advocated by quantum theorist John Bell. It was Bell's assessment that a sound classical education, infused with a knowledge of the reasoning used by such giants of scientific history, together with that of Einstein, confers "stronger and sounder instincts." Although acknowledging differences in approach in both the philosophy and style adopted by Lorentz and Einstein, in stressing his view he argued that: "The facts of physics do not oblige us to accept one philosophy rather than the other. And we need not accept Lorentz's philosophy to accept a Lorentz pedagogy. Its special merit is to drive home a lesson that the laws of physics in any one reference frame account for all the phenomena, including the observations of moving observers ... in my opinion there is also something to be said for taking students along the road made by Fitzgerald, Larmor, Lorentz, and Poincaré." (Bell 1976, p. 77)

[362] As an exercise in mathematical abstraction, the interval $x^2 + y^2 + z^2 - kt^2$ was demonstrated to remain constant under rotations in a four-dimensional spacetime. The included constant, k, which must have the dimensions of a velocity squared, cannot be quantified by further mathematical enquiry alone. It is then empirical data that suggests that $\sqrt{k} = c$ as the "speed of light in vacuum." (Mansouri & Sexl 1977a) In Minkowski's initial presentations of spacetime relativity, it was his inclusion of invariance by the "Lorentz mechanics" that achieved this end. Odenwald (1995) suggests that only the Maxwell electrodynamics theory indicates that the speed of light is a quantity derivable from other physical quantities.

It is in this latter context that Einstein's "second postulate" of the constancy of light-speed and its independence from the source, must be understood as claiming to be sufficient and necessary to sustain the complete incorporation of the domain of electromagnetic phenomena into successful harmony with an otherwise mechanically based principle of relativity. In particular, it must, in the absence of any disclaimer, be deemed to say all that is necessary to fully and correctly represent the interests of the Maxwellian paradigm, including the dynamics and constitution of electromagnetic radiation. Nonetheless, stripped as it is to an irreducible minimum in the metaphysical pursuit of simplicity, a first reading of the postulate statement might suggest little more than a direct accordance with Maxwell's electromagnetic wave theory of light. Such a reading might then reasonably imply simple accord with a continuum interpretation of electrodynamic radiation as distinct from its emission rivals. However, Einstein's postulate has often been considered as gaining clarity from either a rephrasing, paraphrasing, or amplification with a stronger statement, in the cause of revealing its wider intimations.[363] In Jackson's view: "Because special relativity applies to everything, not just light, it is desirable to express the second postulate in terms that convey its generality," and suggests including that the speed *C* is the "finite universal limiting speed" for physical entities. (Jackson 1999, p. 518) For others, that the speed of light is independent of all *observers*, and measured as the same constant *in all inertial frames*, is as important to overtly state as is its independence from the source. (Burke & Scott 1991; Herter 2005; Kobes & Kunstatter 1999; Quinn 2003) For yet others, additional propositions are thought valuable, and for others essential, in the pursuit of clarification: the validity of Maxwell's theory of electromagnetism (Kobes & Kunstatter 1999); that space and time form a 4-dimensional continuum (Hamilton 1998); homogeneity, memorylessness, and the isotropy of vacuum. (Brown 1999a) Nonetheless it was Einstein himself who appears to have first felt the need to repeat[364] the postulate in a rephrased form:

> Each ray of light moves in the coordinate system [for the sake of an easier description denoted] "at rest" with the definite velocity *V* independent of whether this ray of light is emitted by a body at rest or a body in motion. (Einstein 1905c, p. 143)

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[363] Rynasiewicz advises that: "The light postulate is easily confused with the stronger statement that the velocity of light is a fixed constant *c* in all inertial frames … the Light Postulate is a necessary consequence of the wave theory of light, and hence of Lorentz's theory, since the velocity of propagation with respect to the aether is a fixed constant independently of whether or not the light source is in motion. In fact, Einstein often cited the experimental success of Lorentz's theory in support of the Light Postulate." (Rynasiewicz 1998, p. 167)

[364] By 1907 Einstein thought it necessary to modify the postulate: "The velocity of light in free space is a constant *c* irrespective of the velocity of source or receiver in any inertial coordinate system." Einstein, A. "Uber das Relativitatsprinzip und die aus demselben gezogenen Folgerungen", Jahrbuch der Radioaktivitat, IV, p.422-462, V, p.98-99 (cited in Spencer & Shama 1996, p. 476)

Considering that historically, fundamentally incompatible models of optics have employed the terminology of "rays" with equal facility, yet lending support to incommensurate theoretical constructions, Einstein's choice of a "ray of light" to fully represent electromagnetic phenomena is of immediate significance and calls for clarification.[365] As Giannetto views it: "Every theory, as well as every different formulation of a theory implies a different worldview." (2009) For McMullin: "Every physical theory involves a model of the physical object(s) whose behaviour the theory is expected to explain." (McMullin 1985, p. 257) In this respect, in later review, Einstein was to claim that: "The space-time theory and the kinematics of the special theory of relativity were modelled on the Maxwell-Lorentz theory of the electromagnetic field." (Einstein 1920) Nonetheless, in his entailed 1905 kinematics development of the second postulate, both a facilitating model of light and the philosophical orientation that would nurture its employment in the cause of theory construction, are far from unambiguously apparent. It then becomes significant that Einstein's relationship with Maxwell's paradigm, by his own reports, was contemporaneously portrayed as troubled.[366]

It appears to be more usual when categorizing the Einstein anthology, to collect papers relating to quantum theory and its genesis, into a classification separated from those on invariance and relativity. Such a criterion appeals as logical given these subject's apparent philosophical separability and independent historical development.[367] However, analysts such as Holton claim to discern commonalities behind the 1905 collection,[368] suggesting that the exercise of a strict editorial categorization may possibly obscure valuable insights. The possibility exists that Einstein's facilitating optical modeling and personal world-view might prove more transparent from a less categorical survey of the 1905 material. It is in this respect that a relevance may be attached to his immediately prior 'quantum' publication, "*On a Heuristic Point of View Concerning the Production and Transformation of Light*," (Einstein 1905b) in which he

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[365] In Torretti's view: "For greater clarity, one ought perhaps to replace … Einstein's expression 'a light ray moves,' which anyway sounds peculiar, by 'a light point moves'." (Torretti 1983, p. 55) – an interpretation that clarifies the locality and separability attributes of the sponsoring radiation model.
[366] Such claims appear to have distorted modern perceptions of Einstein rejection of the correctness of Maxwell's theory as in: "Einstein was one of the second generation of Maxwell's supporters, who helped demonstrate the soundness of his new methodology…" (Arianrhod 2003, p. 264)
[367] "The development during the [twentieth] century is characterized by two theoretical systems essentially independent of each other: the theory of relativity and the quantum theory." (Einstein 1940)
[368] "When one studies the relativity papers in the larger contextual setting of Einstein's other scientific papers, particularly those of the quantum theory of light and on Brownian motion ... [which] seem to be in entirely different fields, closer study shows that they arose in fact from the same general problem, namely, the fluctuations in the pressure of radiation ... This connects on the one hand with the consideration of Brownian motion as well as to the quantum structure of radiation, and on the other hand with Einstein's more general reconsideration of "the electromagnetic foundations of physics" itself. (Holton 1960, p. 629)

first broached the subject of light quantization. In some measure appealing to a claimed analogy between the statistics of gas molecules in a container and the statistics of 'black-body' radiation and absorption, Einstein here pursued both an elucidation of the photoelectric effect and proposed a granular constitution of radiation.[369] In Torretti's view:

> Though it would not be easy to guess from either [the radiation or relativity] text that they were written by the same man … the insight leading to the paper on radiation inevitably influenced Einstein's peculiar approach to electrodynamics." (Torretti 1983, p. 48)

Of further significance, and by its author's own assessment, the radiation paper was the only one produced that year to be claimed as truly 'revolutionary'.[370]

Einstein's 1905 modeling of electromagnetic radiation

Einstein's self proclaimed 'revolutionary' paper can be viewed as significant in many respects. Whereas statistical mechanics had previously only been applied to systems of particles, the Einstein approach applied the same approach to radiation. Application was not however limited to the pragmatic and less confrontational emission-absorption role suggested by Planck, but by "assuming that Planck's energy quanta exist as real light-particles, called 'light quanta' ..." (Born & Wolf 1999, p. xxxi)[371] Repudiation of a ballistic dynamics had not dissuaded Einstein from retaining the most definitive element of an emission model[372] – the localized separable particle. As for Newton before him, Einstein was to claim a real corpuscular existence with a discrete character for all aspects and all conditions of optical radiation. Given the extensive empirical support then enjoyed by wave and wavefront formulations, such an

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[369] "My own interest in those years was less concerned with the detailed consequences of Planck's results, however important these might be. My major question was: What general conclusions can be drawn from the radiation-formula concerning the structure of radiation and even more generally concerning the electro-magnetic foundation of physics?" (Einstein 1949b, p. 566)

[370] "In describing four of his 1905 papers, Einstein characterized only the one on the quantum hypothesis as revolutionary." (Stachel 1989a, p. 134) Einstein, in correspondence with Conrad Habicht claimed: "The paper deals with radiation and the energy properties of light and is very revolutionary." (Einstein to Habicht, May 1905, cited in Hentschel 2007, p. 10) On the other hand, of the special theory he claimed: "We have here no revolutionary act but the natural continuation of a line that can be traced through centuries." (Einstein 1934b)

[371] "We should note here that Einstein's explanation of the photoelectric effect was incomplete, for the temperature of the metal surface was seen experimentally to affect the energy of the emitted electrons yet this was not covered by Einstein's model. A fuller model incorporating this feature was discovered by Fowler in 1931." (O'Connor & Robertson 2002c)

[372] Some confusion of terms and definitions is possible. Historically "emission" has also been used to refer specifically to its narrower "ballistic" interpretation such as: "Relativity theories based on such a postulate … that the velocity of light and the velocity of the source are additive … may well be called emission theories. (Tolman 1912)

exclusive embrace of a descrete formula might be thought to lend weight to such as Whitehead's opinions concerning simple location. In justification, Einstein suggested:

> It appears to me ... that the observations on "black-body radiation," photoluminescence, the generating of cathode rays with ultraviolet radiation [as in the photoelectric effect] ... [when] related to the generation and transformation of light can be understood better on the assumption that the energy in light is distributed discontinuously in space ... [from which it would follow that] the energy in a beam of light emanating from a point source is not distributed continuously over larger and larger volumes of space but consists of a finite number of energy quanta, localized at points of space, which move without subdividing and which are absorbed and emitted only as units. (Einstein 1905a, p. 545)

In contradiction to Planck's approach, the 'quantum' was not to be restricted or limited to a relevant sub-domain of applicability. Somewhat in the mode of Newton, the Einsteinian quantum was to be a reality demanded by the evidence. Recalling an historical precedent, by Dorling's account, Einstein advanced his argument "as a 'deduction from the phenomena' in the sense of Newton."[373] (Dorling 1971, p. 1) As an immanent but rival certainty then, Einstein believed the quantum inimical to the prevailing continuum interpretation of Maxwellian electromagnetism:

> … realization of the fact that the bearing of the Lorentz-transformations transcended their connection with Maxwell's equations [in that] the Lorentz invariance is a general condition for any physical theory. This was for me of particular importance because I had already previously found that Maxwell's theory did not account for the micro-structure of radiation and could therefore have no general validity. (Einstein to Seelig, 1955, cited in Cerf 2006, p. 821)

In his analysis of the evolutionary development of early light and energy quantum speculations towards their becoming the 'photon' of a later maturing quantum theory, Hentschel further perceives Einstein's 1905 approach in part as a recapitulation of historical circumstance. Attention is drawn to Newton's earlier cryptic publication strategy when shielding an underlying emission modeling of light:

> His early papers in the Royal Society's *Philosophical Transactions* do their best *not* to reveal his basic conception of light as a corpuscle. Nevertheless, his *Principia* from 1687 as

[373] "The structure of this paper ... does not fit in easily with popular twentieth-century conceptions of scientific method. The traditional way of forcing it into the hypothetico-deductive mould is to avoid any mention of the first two-thirds of the paper ... the appearance of argument by analogy has proved to be only a superficial blemish" (Dorling 1971, p. 6)

well as the queries in his *Opticks* from 1704 provide clear hints at this projectile model. (Hentschel 2007, p. 2, orig. emph.)

In Hentschel's account, the Newtonian motivation is portrayed as transparent. Newton, well aware of both his concept's vulnerability to criticism and its potential to distract his reader's attention toward proscribed speculations and loathed controversy, carefully contrived what he imagined would prove a theory neutral course. In Einstein's early cautious wording, evident even in his use of *"heuristic"* in the title,[374] Hentschel detects a similar deference to Wien and Planck who were then members of *Annalen's* editorship and whose views were potentially obstructive to an overt neo-atomist stance.

> Although Planck had introduced the quantum of action to account for the spectral distribution of black-body radiation, the concept of the [quantum] as a real entity was not a very popular one and was rejected by most physicists at the time. Planck himself felt very uncomfortable about the [quantum], and considered it more or less as a useful device to derive the correct radiation equations. He was inclined to picture [the quantum] as having reality (if it had any at all) only during the processes of absorption and emission. At all other times ... radiation had only a [continuum] wave structure and character. Einstein departed completely from this tentative position that sought a compromise between classical physics and the new quantum hypothesis ... [by going] over entirely to a quantum theory. (Boorse & Motz 1966c, p. 539)

Four more major contributions to the quantum hypothesis of energy and radiation followed in the period to 1909 in which his earlier 1905 renunciation of the Maxwellian continuum appeared ever more transparent. With time, caution became less conspicuous. Einstein now openly claimed warrant from analogy for an extension of the atomism of electrical charge to justify a discrete characterization encompassing all radiation phenomena. But, above all other considerations, his position was now openly identified with the earlier, but long since empirically repudiated, Newtonian corpuscular modeling of light:

> ... the electromagnetic fields that constitute the light ... no longer appear to be states of a hypothetical medium, but rather independent entities emitted by the sources of light**, exactly as in the Newtonian emission theory of light.** (Einstein 1909a, p. 383, emph. added)

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[374] Hentschel notes definitions of '*heuristic*' as extending to "tentative," "uncertain," and even "unverifiable." *Webster's Dictionary* definition is: "providing aid and direction in the solution of a problem but otherwise unjustified or incapable of justification." (2007, p. 11)

> It is ... undeniable that there is an extensive group of facts concerning radiation that shows that light possesses certain fundamental properties that can be understood far more easily from the standpoint of Newton's emission theory of light than from the standpoint of the wave theory (ibid. , p. 379)

> ... the Newtonian emission theory of light seems to contain more truth than does the wave theory, since according to the former the energy imparted at emission to a particle of light is not scattered throughout the infinite space but remains available for an elementary process of absorption. (ibid., p. 387)

> ... the most natural interpretation seems to me to be that the occurrence of electromagnetic fields of light is associated with singular points just like the occurrence of electrostatic fields according to the electron theory. It is not out of the question that in such a theory the entire energy of the electromagnetic field might be viewed as localized in these singularities ... (ibid., p. 394)

Nonetheless, Einstein's model selection and model properties are quite recognizable as the construction of a preliminary model appropriate to a physical research question seeking a realist theoretical explanation of the phenomenon of Lorentz invariance. The model exhibits idealized abstractions – its particles are dimensionless points pictured as localized singularities. Further, he gathers into his initial mix selected elements of the Maxwellian electrodynamics. To these are added elements essential to a mechanistic treatment - the cartesian coordinate system and its associated inertial reference frames together with the background theory of the Newtonian laws of motion. By all accounts of a model-inclusive scientific method, the process of model customising should proceed next. The model should be concretized and deidealized such that model properties and functioning represent a closer and more suitable approximation of real physical objects and happenings. For Einstein, concretizing of his model appears to have been particularly important. When considering his modeling properties, wherein "radiation energy consists of individual point-like localized quanta … which are reflected undivided, in a drastic and direct way … a type of immediate reality has to be ascribed to Planck's quanta" (Einstein to von Laue, 1952 cited in Cerf 2006, p. 822) It is here, however, in pursuing Einstein's claim that his model is composed of individual entities "*exactly* as in the Newtonian emission theory of light," that a difficulty becomes apparent.

Where the point-particle representation of molecules in an idealized gas can be readily concretized by restoring their physical extension to become a real volume, a difficulty exists for Newtonian light particles idealized as point-like entities. Certainly, as geometrical points, they may be accounted as ‘massless’, but equally must be accounted as physically inert – as physical fictions. As usually understood, concretization of a point-like idealization should confer cartesian extension. Newtonian-like optical particles, at least in a classical setting, should then be expected to exhibit similar real physical properties to those that proved to be the historical downfall of Newton’s theory of light and colours. By stages, the primitive Aristotelian analogical “ball,” cast in a later Cartesian guise, had shrunk to an imperceptible, yet still essentially dimensional, corpuscle under the Newtonian paradigm. It was this undeniable localized dimensionality that had told heavily against it in Huygens’ superposition argument and had finally proven to be its nemesis in 1850 at the hands of Foucault’s, Fizeau, and Breguet’s independent, but crucial, refraction investigations. Any attempted revival of a real light particle would have to confront and surmount this substantial obstacle. Ritz, in advancing his rival ballistic-emission modeling of relativistic electrodynamics, had openly acknowledged its conjectural status by characterizing his “infinitely small” luminous particles as a convenient “fiction.”[375] Whether Ritz recognized the problems inherent in conferring some practical form of realism on his essentially dimensionless particles, or whether he hoped to contrive some method of avoiding the difficulty, is not recorded in his brief work. Einstein appears equally silent on the potential for dilemma. Fortunately for Einstein, his particles were *not* exactly as in Newton’s theory. Einstein’s particles were not classical particles, rather they were energy quanta. Here, concretization need not of neccessity lead to classical model properties. Beyond this, however, the entity intended for realisic concretization, even at early stages of investigation, was believed to exhibit disturbingly ambiguous properties. In a selected domain of interest such as the photo-electric effect, when pictured by analogy to characteristize attributes common to the separable and the simply locatable, the quantum was found to exhibit recognizable particle properties. In other circumstances, the quantum was found to display properties more appropriately characteristic of waves. Eventually the quantum was to be crafted with properties characteristic of both seemingly antagonistc modes of behavior. Historically, the designation of the ‘light quantum’ or ‘light particle’ by the term ‘photon,’ was first introduced in late 1926 by the professor of physical chemistry at the University of California at Berkeley, Gilbert N. Lewis. (Hentschel 2007, p. 2) The proposed definition by Lewis, which appeared in a letter to the editor of Nature magazine, announced: “I therefore take the liberty of

[375] “Needless to add that these particles must be considered uniquely as a fiction, furthermore convenient ...” (Ritz 1908a, p. 210) and elsewhere remarks on “the fictitious particles emitted by an ion ...” (Ritz 1908b) Commenting on this, Rynasiewicz appears to recognize the Ritz exegesis as a heuristic model and notes: “Ritz had also suggested, as an aid to the imagination, the mediation of electromagnetic interactions via fictive particles. Einstein might possibly have considered a picture such as this, only with real particles (light quanta?) instead of fictive ones.” (Rynasiewicz 1998, p. 189) On the other hand, Fox suggests that Ritz used “fictitious … to emphasize that logically it was not absolutely necessary to have a mental picture although there was no harm in it; in fact ... there were advantages.” (Fox 1965, p. 2)

proposing for this *hypothetical* new atom, *which is not light* but plays an essential part in every process of radiation, the name *photon*." (Lewis 1926, emph added)

Nonetheless, a more realistic presentation of quantum properties was not pursued in Einstein's early model construction. On this basis, the status to be awarded to the ray of light as an object in the special theory, and claims to realist explanation by the theory, might be considered as not having been fully developed.

Others have added their suggestions towards understanding the Einstein motivation to embrace a principle tenet of the Newtonian optical paradigm. Hirosige notes that, insofar as he claimed inspiration from his reading of Hume, Einstein's concept of space populated by real optical atoms immediately reminds us of Hume's assertion: *'the idea of space or extension is nothing but the idea of visible or tangible points distributed in a certain order'* (italics original) , and that 'we have no idea of any real extension without filling it with sensible objects.' (Hirosige 1976, p. 58) Further, Hirosige reminds us that Einstein's compounding early dilemmas also included his recognition of "formal incongruities between physical theories."[376] (Hirosige 1976, p. 56)

> [Einstein's] theory of light quanta, was also intended to remove 'a fundamental formal difference' between mechanics and electromagnetism, namely, the difference of having a discrete fundamental entity, the mass point, in mechanics and a continuous one, the field, in electromagnetism. (Einstein 1905b; Hirosige 1976, p. 56)

It is in this respect that an observation by Goldberg gains in importance:

> …one of the interesting features of studying the reception of the theory of relativity is that we are presented with two competing theories – the Lorentz theory of the electron and Einstein's special theory of relativity – which have identical formalisms but whose meanings are as different as night and day.[377] The Lorentz theory was a dynamical theory intended to account for the behavior of all matter, radiation and their interactions, using the overarching premises of what has been termed the electromagnetic world view.
>
> Einstein's theory, on the other hand, was a kinematical theory, not wedded to the electromagnetic world view at all … (Goldberg 1987, p. 3)

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[376] "Holton has pointed out, Einstein also found a formal incongruity between [mechanics and electromagnetism] in their respective fundamental entities." (Hirosige 1976, p. 56)

[377] From a semantic perspective, this "identical formalism" would be the fundamental uninterpreted theory of special relativity. Applications by such as by Voigt, Larmor, Lorentz, Poincaré and Einstein would be a family of models which variously interpret the theory in terms of real phenomena.

Goldberg's observation cuts directly to the heart of the matter. For Einstein, the underlying model, which is held as an integral step to theory construction, is not derived from the prevailing continuum electromagnetic paradigm. A mechanistic approach filled both a perceived need for objectivity, and provided a characterization of radiation as discrete and locatable, permitting an equitable theoretical dealing with both electrodynamics and mechanics. Impediments perceived to stem from continua and processes were thought removed. In respect of this, despite his apparent admiration of Mach's philosophical rejection of mechanism, Einstein's embrace of the particle of light is viewed by some, as a mechanical paradigm recidivism. (Giannetto 2009, p. 120)[378]

By 1911, Einstein's realist particle conviction was to provide grounds for open academic debate and one that opened a division between Einstein and his colleagues. As Born recollected the first Solvay Conference:

> Einstein's relativity theory had, to some extent, been accepted. H. A. Lorentz, recognized by all as the leading physicist presided, and Planck, Einstein, Nernst, and other top scientists were present. Most of the discussion was devoted to the [light quantum]; Einstein insisted that it be recognized as a real physical particle, but the majority was willing to accept it only as an artificial device for clarifying certain radiation phenomena. (Born cited in Boorse & Motz 1966h, p. 1046)

It is then, a neo-Newtonian emission model of light and radiation, once again intended as an exhibition of real physical particles, that is discovered as giving its meaning and expositional unction to the rays of the constancy of light-speed postulate. Of particular significance, it then becomes apparent that it was this corpuscular model that was responsible for injecting the mechanically compatible properties of separability and locality into the fabric of an Einsteinian electrodynamics. It was then this particle emission model that directly sponsored the construction of the special theory's unified relativistic kinematics. It then also becomes of particular significance, that it is properties of this specific neo-atomist facilitating model, finding expression in this specific theoretical construction, that has now emerged as underwriting a provable incompatibility between prevailing relativity and quantum theories.

[378] "Concerning relativity, the … papers written by Poincaré show that special relativity dynamics derived from, and was the first realization of, the electromagnetic conception of Nature. Albert Einstein's (1879-1955) special relativity formulation was only an incomplete (without a gravitation theory) semi-mechanistic version of this new dynamics." (Giannetto 2009, p. 120)

Part 3

Chapter 7
Models and theory confirmation and disconfirmation

For Einstein, a further history of radiation theorized as light-particles and cast as independent localized singularities became increasingly troubled. In various later correspondence, Einstein admitted recognition of impediments to realization of his atomist modeling. In correspondence with Lorentz in 1909 he admitted difficulties explaining interference, and elsewhere acknowledged difficulties interpreting partial reflection, given that the splitting of light quanta, as for any 'atom,' was ruled fundamentally impossible. Additionally, the transmission of energy and exhibition of radiation pressure by such particles appeared to imply mass – a property that conflicted with their assigned universal maximum velocity at which no massive particle could travel.[379] (Hentschel 2007, p. 12) In what may be construed as an admission of mounting difficulties, Einstein remarked: "I am sure it need not be particularly emphasized that no importance should be attached to such a picture as long as it has not led to an exact theory." (Einstein 1909a, p. 394) In his private correspondence with Lorentz as early as 1909, Einstein had already begun distancing himself from his more resolute published stance: "From the outset let me aver that I am not the orthodox light-quantum-man [*Lichtquantler*] that you hold me for. That may have come from the vague form of expression in my papers," (Einstein to Lorentz, 23 May, 1909 cited in Hentschel 2007, p. 13) In a subsequent letter to his life-long friend and confidant Michele Besso in 1911, Einstein was to suggest the possibility of modifying the quantum's domain of applicability :

> I don't ask myself anymore whether these quanta really exist. I don't try to construct them anymore either, because I now know that my brain cannot come through that way. But I am systematically examining the consequences carefully in order to find out about the area of applicability of the idea. (Einstein to Besso, 13 May 1911, cited in Hentschel 2007, p. 13)

Possible emendation of his light particles' constitution and domain of applicability was soon followed by what appears as a positive relinquishment of realist pretensions. In a subsequent letter to his mathematician friend Ludwig Hopf, Einstein claimed: "The quanta do what they are supposed to, but they don't exist, like the light aether at rest."

[379] As Hentschel notes, eventual resolution of the quantum-mass enigma was to assume the quantum's vanishing rest mass as part of a maturing quantum mechanical theory whereas the other problems proved more intractable "as they were intimately linked with the [thorny] issue of wave-particle duality." (Hentschel 2007, p. 12)

(Einstein to Hopf, February 1912, cited in Hentschel 2007, p. 13) Then, six years before his death, Einstein confided to his long-time friend Maurice Solovine:

> You imagine that I look back on my life's work with calm satisfaction. But from nearby it looks quite different. There is not a single concept of which I am convinced that it will stand firm, and I feel uncertain whether I am in general on the right track. (Gilmore 1979, p. 58)

Perpetuation of this more pessimistic view remains evident in 1954, not long before his death in 1955. At that time 'light particles' had long since been denoted 'photons' and were being treated by a growing consensus as valid components of a maturing quantum theory. Einstein again expressed both his on-going frustration and pessimism to Michael Besso:

> All these fifty years of conscious brooding have brought me no nearer to the answer to the question, 'What are light quanta?' Nowadays every Tom, Dick and Harry thinks he knows, but he is mistaken. (Pais 1982, p. 467)

Again, in candid discussions with an aging Einstein, Bernard Cohen recalls:

> I asked Einstein whether Planck had ever fully accepted the "theory of photons," or whether he had continued to restrict his interest to the absorption or emission of light without regard to its transmission. Einstein ... smiled and said: "No, not a theory. Not a *theory* of photons." (Cohen 1955, p. 72, orig. emph.)

The twentieth-century depreciation of Huygens' Principle

In early disavowing a conventional Maxwellian continuum, Einstein asserted that "Maxwell's equations for empty space, taken by themselves, do not say anything ...[but] only represent an intermediary construct." (Einstein 1909b, p. 357) Elsewhere in later interview he maintained: "Maxwell's equations are not reality," (Shankland 1963, p. 54) However, it was not specifically the undulatory model of light, still viewed by Einstein as a fundamental component of optical theory, that was found fatally flawed in his quantized radiation formulation. Certainly, for Einstein, it would require some reinterpretation such as being correct only on average rather than in particular, but its history of experimental successes nevertheless guaranteed it a place in a reformed but primarily neo-atomist electromagnetic paradigm:

> The wave theory, operating with continuous spatial functions, has proved to be correct in representing purely optical phenomena [such as] the theory of diffraction, reflection, refraction, dispersion, … and will probably not be replaced by any other theory. [However] optical observations are concerned with temporal mean values and not with instantaneous values, and it is possible … that the theory of light that operates with continuous spatial functions, may lead to contradictions with observations if we apply it to the phenomena of generation and transformation of light. (Einstein 1905a, pp. 544-545)

On the other hand however, the formulation that presented the most profound contradiction to an atomist rendition of radiation was to be recognized in the Fresnel-Kirchhoff concretization and de-idealization of Christiaan Huygens' process modeling of the wavefront. For Einstein, as for Newton before him, such a wavefront modeling of light was not understood as an idealized model of a selected domain of unobservable optical dynamics. The notion was perceived as a *realist theory* of light, and thus a theory in mortal competition for recognition as the unique explanation of the electromagnetic domain. Whereas some undulatory exhibition of electromagnetic phenomena would need to be accommodated in some form, only one *theory* – one real and correct explanation - of the foundational nature of light could be tolerated. Thus, for Einstein, as for Newton before him, real light singularities emitted so as to remain localized and undivided would stand inimical to the possibility of any post-emission evolutionary processes and thus be, in particular, hostile to Huygens' principle. Further compounding his antagonism, Born recalls that:

> [Einstein] followed the leading principles of scientific research, objectivization and relativization, and in addition used another principle which ... was used mainly for logical criticism and not for scientific construction – for instance by Ernst Mach, the physicist and philosopher whose work had made a strong impression on Einstein ... This [positivist] principle said that concepts and statements which are not empirically verifiable should have no place in a physical theory." (Born 1965, p. 3)

Thus for Einstein, as for Newton before him, the idea of an expanding wavefront, pictured as sustained by unobservable processes, was to be rejected in the cause of constructing a realist explanation. A wave*front,* proposing a non-local, non-separable, and yet a somehow physical presentation of an unobservable spreading process, was to be denied an objective role. Such a notion was condemned as both empirically unverifiable and contradictory to a factual microstructure of field radiation:

> ... the elementary radiation process seems to proceed such that it does not, as the wave[front] theory would require, distribute and scatter the energy of the primary electron in a spherical wave propagating in all directions. (Einstein 1909a, p. 388)

Such a renunciation did not however, preclude some purely pragmatic mathematical utility. The Einstein 1905 mathematical analysis considered a spherical geometry[380] of the form: $x^2 + y^2 + z^2 = c^2t^2$ (Einstein 1905c, p. 149) and further discussed it in an equivalent form in the development of kinematical argument. (Torretti 1983, pp. 55, 67) Here, the casual reader might feel justified in perceiving a patent endorsement of Huygens' principle. Nonetheless, elsewhere Einstein made his intended meaning clear: "I regard the forms containing retarded functions[381] as merely auxiliary mathematical forms ... [that] do not subsume the energy principle ..." and went on to elaborate his call for a practical process-sponsored wavefront censure:

> What distinguishes the Maxwell-Lorentz differential equations from the forms that contain retarded functions [i.e. the Huygens-Fresnel-Kirchhoff principle] is the circumstance that they yield an expression for the energy and the momentum of the system under consideration for any instant of time, relative to any unaccelerated [i.e. inertial] coordinate system. With a theory that operates with retarded forces it is not possible to describe the instantaneous state of a system at all without using earlier states of the system for this description." (Einstein 1909b, p. 357)[382]

Particularly galling for Einstein, in view of his realist and objectivist[383] conception of an electromagnetic domain studded with atomic entities, was the lack of an instantaneously quantifiable corporeality detectable for a wavefront defined by unobservable

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[380] Keswani has called attention to an apparent inconsistency in Einstein's treatment of such a wavefront, noting that he refers to it as "a spherical wave with velocity of propagation *c* when viewed in the moving system," but later this same surface "viewed in the moving system – is an ellipsoidal surface." (Levinson 1965, p. 246)

[381] For a propagating Markov process, such as the vectorized form of the Huygens-Fresnel-Kirchhoff model, determination of electric and magnetic potentials at a position ***r*** and time ***t*** requires performing integrals of both charge density and current density over all space. However, to calculate the contribution of charges and currents to these integrals at a specific location ***r′***, an earlier time defined as $\boldsymbol{t'} = \boldsymbol{t} - |\boldsymbol{r} - \boldsymbol{r'}|/\boldsymbol{c}$ needs to be employed which represents the latest time at which a light signal emitted from position ***r′*** would arrive at position ***r*** before time ***t***. By convention, time ***t*** is then denoted the *retarded time* and the potentials at (***r, t***) are termed *retarded potentials*. (Fitzpatrick 2006b)

[382] Descriptions of Markov processes are prone to misconception. As a general definition, a Markov process is a mathematical model for the random evolution of a "memoryless" system wherein the probability of a given future state, at any given moment, depends only on its present state, rather than on any past states.

[383] As Born explains, in an objectivist approach, "electromagnetic fields ... which are not directly accessible to any human sense, could be introduced by reducing them to mechanical quantities measurable in space and time." (Born 1965, p. 2)

processes.[384] Most significantly in this respect, an expanding spherical process failed to fulfill a perceived need for a direct correspondence with a mechanical construct in terms depending for quantification on separability and locality. In undisguised disparagement Einstein was to write:

> [Prior to a location of detection,] according to theories operating with retarded forces, the light complex is represented by nothing except the processes that have taken place in the emitting body during the preceding emission. Energy and momentum – if one does not want to renounce these quantities altogether – must then be represented as time integrals. (Einstein 1909b, p. 358)

Thus, in frustrating definition of primary properties in terms of a single or simple location at a stated instant of time, the Huygens-Fresnel-Kirchhoff model of radiation necessarily failed in Einstein's estimation as one that could sustain a unified treatment of electrodynamics and mechanics by use of a standard inertial reference frame methodology.

The predisposing model and theory disconfirmation.

In respect of the view advanced in that literature advocating recognition of a distinct scientific modeling role, its participation is further portrayed as contributing beyond an essential phase in the process of theory construction. As conceived by such as McMullin and Nowak, the modeling role also bears on theory disconfirmation. On this view, if a theoretical development is found to predict results that conflict either with further experimental enquiry or with the greater body of historically established mature theories, then, provided that the theoretical construct is believed self-consistent, such discrepancy is held to be against the propriety of its predisposing model. (McMullin 1985, p. 261) When consciously adopted, such a model might then, as writers such as McMullin, (1985) Box, (1979) and Nowak, (1972) have suggested, be further concretized or de-idealized before further theory reiteration, or if deemed necessary, abandoned.

From the time of its inception, the special theory has never been at a loss for criticism. (Mueller & Kneckebrodt 2006) [385] Charges of theoretical inconsistency,

[384] In this, Brown's note is germane: "In a sense, Huygens' Principle is more significant for what it says about what happens *behind* the leading edge of the disturbance." (Brown 1999d) Insofar as such an interference-superposition interplay leaves no physical trace at such locations, it is the natural prey of a Machian-styled positivism.
[385] The 2001 edition of this German based research project documented 2896 publications appearing in all languages and all countries since 1908, and which displayed criticism of special relativity and some of

mathematical or logical anomaly, or rejection of its perceived neo-Pythagorean basis,[386] have inspired the writing of innumerable articles and books and kindled sometimes protracted disputations. Some have become quite public, such as the high-profile debate maintained in the pages of *Nature* during 1967-1968, between Professor of History and Philosophy of Science, Herbert Dingle,[387] and mathematician and cosmologist, Sir William Hunter McCrea. (Ballard 2002a, 2002b, 2002c) However, although many cases have been mounted, and continue to be mounted, against the internal consistency of the theory, no account has yet found a general acceptance. On the other hand, although not generally articulated in terms of modeling as an adjunct to theory construction, the possibility of a sponsoring model's inadequacy, or an inappropriate model choice, has in principle, spawned a plethora of debates and alternative schemes over the past century.

Perceptions of conflict with 'common sense'

Chronically fostering reproach, has been a perceived confrontation of theoretical claims with common sense – a claim that if accepted might be thought to reflect particularly on the propriety of the theory's predisposing modeling. Confrontation with common sense was early held conspicuous by some, when assessing the theoretical justification offered for a particle that failed to obey the Galilean velocity addition rule, together with other concomitants of the theory such as the now notorious "twins" or "clock" paradox. (Weiss 2006; Whitney 1997) Perennial contentions have continued to fuel debates as to whether length contraction and time dilation are real physical effects or merely apparent, with acclaimed experts continuing to give seemingly obscure and often conflicting answers to such questions. (iii) Recognition of such impediments to an unproblematic general acceptance have prompted physicists such as Hermann Bondi to attempt more accessible discussions such as his, "*Relativity and Common Sense: A New Approach to Einstein.*" (Bondi 1965) It was also in this respect that, in their early appraisal of the special theory, some philosophers found it expedient to attempt a

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general relativity. The latest update (ver. 1.2, 2004) added some 900 documents with a total listing of 3789 critical appraisals. (Mueller & Kneckebrodt 2006, p. 4)

[386] In advancing four basic misunderstandings Dingle believed wrongly maintained the acceptance of the special theory, was "the false conception of the relationship between mathematics and physics." (Ballard 2002a) As Dingle saw it: "The habit has developed of assuming that a physical theory is necessarily sound if its mathematics are impeccable: the question of whether there is anything in nature corresponding to the impeccable mathematics is not regarded as a question; it is taken for granted." (Ballard 2002b)

[387] The Encyclopaedia Britannica entry for "Special Relativity" was written for many years by Prof. Dingle as Professor of History and Philosophy of Science, University College London, and President of the Royal Astronomical Society, until he "notoriously recanted" (Thornhill 2002) and published his objections both to the theory and its uncritical acceptance by mainstream physics in his book, "Science at the Crossroads," (Dingle 1972) and journal articles. (Dingle 1967) Dingle maintained correspondence with such as Born, Eddington, Schrödinger and Whittaker. (The Archives Hub 2000)

mitigation, if not exoneration, of the counter-intuitive world-view that the theory was claimed to necessitate. Philosophers attempting to interpret a new 'relativistic' landscape, typically emphasized the necessarily limited perception mortals must have of a mostly unfamiliar universe. Nonetheless, Whitehead displayed a deep concern for that character of the new science that appeared to run counter to concepts that had historically serviced a sense of security and logical connectivity:

> The note of the present epoch is that so many complexities have developed regarding material, space, time, and energy, that the simple security of the old orthodox assumptions has vanished. ... The new situation in the thought of today arises from the fact that scientific theory is outrunning common sense. The settlement as inherited by the eighteenth century was a triumph of organized common sense. It had got rid of medieval phantasies, and of Cartesian [aetherial] vortices .... The eighteenth century opened with a quiet confidence that at last nonsense had been got rid of. To-day we are at the opposite pole of thought. Heaven knows what seeming nonsense may not to-morrow be demonstrated truth ... The ground of the explanation is that the ideas of space and of time employed in science are too simple-minded, and must be modified. This conclusion is a direct challenge to common sense, because the earlier science had only refined upon the ordinary notions of ordinary people. (Whitehead 1938b, pp. 136-137, 140)

In his more phlegmatic review, philosopher-mathematician Bertrand Russell admitted that the "theory of relativity has altered our view of the fundamental structure of the world," but claimed, "that is the source both of its difficulty and of its importance." (Russell 1925, p. 86) He was not unsympathetic, however, to the disturbing role these "difficulties" played for the ordinary man and his formerly practical view of the world:

> Many of the new [relativistic] ideas can be expressed in non-mathematical language, but they are none the less difficult on that account. What is demanded is a change in our imaginative picture of the world – a picture which has been handed down from remote ... ancestors, and has been learned by each of us in early childhood ... but for our generation a certain effort of imaginative reconstruction is unavoidable ... The theory of relativity depends, to a considerable extent, upon getting rid of notions which are useful in ordinary life ...
>
> Indeed, one of the main motives of this whole theory is to secure that the velocity of light shall be the same for all observers, however they may be moving. This fact, established by experiment, was incompatible with the old [Newton-Galilean] theories, and made it absolutely necessary to admit something startling. (Russell 1925, pp. 2-3, 5, 84-85)

When confronted by his theory's claimed affront to common sense, Einstein disparaged common sense as simply "the collection of prejudices acquired by age eighteen." (Harris 1995; Horgan 2005) However, science journalist John Horgan reminds us, with perhaps a little delight, that it was Einstein's own prejudice that "God does not play dice with the universe" that prompted him to hold the disconcerting implications of quantum probabilities as adequate grounds for disconfirmation of the prevailing account of quantum theory. (Horgan 2005) However, many have agreed with the Einstein assessment. For author W. Somerset Maugham:

> Common-sense appears to be only another name for the thoughtlessness of the unthinking. It is made of the prejudices of childhood, the idiosyncrasies of individual character and the opinion of the newspapers. ('Common Sense' 2011)

Nonetheless, some caution is perhaps indicated. Common sense, seen at least in part as the result of 'common science' – as that historically accumulated reservoir of knowledge corroborated by a wealth of human experience – boasts some support from such as Huxley, Emerson, Goethe, and Whitehead. [388] For such thinkers it cannot be safely dismissed as nothing more than juvenile prejudices, or popular consensus sustained by general ignorance or mass delusion. For Whitehead, theoretical justification amounted to more than simply empirical justification. In his assessment, there should be two "gauges" against which acceptance of theoretical competence should be evaluated. Beyond the "narrow gauge" of experimental compliance there should be the broader criterion "which tests its consonance with the general character of our direct experience." (Whitehead 1922, p. 4) It is in the interpretation and implications of this view that scientific opinion has become sharply divided. Nevertheless, criticism citing a confrontation with common sense, even as interpreted in its most favorable form, has not by itself, been found sufficient to incite a mainstream reappraisal of the special theory's predisposing model, or to instigate model modification or replacement. Claims of extensive empirical compliance to theoretical prediction are routinely invoked as adequately countermanding any call for review.

[388] "Science is nothing, but trained and organized common sense." and "All truth, in the long run, is only common sense clarified." - Thomas H. Huxley. "Common sense is genius dressed in its working clothes." - Ralph Waldo Emerson. "Common sense is the genius of humanity." - Johann Wolfgang Von Goethe. "Common sense is genius in homespun." - Alfred North Whitehead. ('Common Sense' 2011)

Perceptions of conflict with established theory and scientific method

However, in the case of the special theory, additional factors have come into play. Acceptance of the implications of the theory is generally acknowledged as having demanded a rewrite of accepted scientific fact and the embrace of a radically new world-view. Fundamental reconstructions of the relationship between space and time, together with a belief that the classical system of measurement must be recast as relative to an observer, are held by some as standing in confrontation to both classical scientific methodology[389] and a long history of otherwise well-established and settled theory. In as much as most would agree that a proper deference to the accumulation and consolidation of the history of past achievement should relegate revolution to an act of last resort, for some physicists, response to the call to a pervasive paradigmatic insurrection has been too precipitous. Certainly, revolutions in science do occur, long-held paradigms are relinquished, and respected world-views abandoned. Nonetheless, according to one minority opinion, all other plausible options have not yet been exhausted and less iconoclastic alternatives have not been adequately explored. It is with such concerns that some physicists have expressed doubts as to the validity of relativity theory as conventionally accepted and have attempted an eclectic assortment of alternative propositions.

Prominent among these, and almost perennial ever since Poincaré depreciated its theoretical significance, and Einstein reformulated its primary properties, has been a call for reinstatement of that optical model in which electromagnetic radiation in vacuo is supported by an aethereal medium of some physical consequence. Despite its acclaimed disconfirmation by a majority of careful and repeatable experiments, the model has experienced periods of rejuvenation, as Decaen has remarked:

> Rumors of its death … have been greatly exaggerated, and a closer look at the content and history of the theories that have been accepted in twentieth-century science makes this manifest. Not the least among these theories is Einstein's generalized theory of relativity, just as not the least among those who interpret this theory in terms of aether was Einstein himself. (Decaen 2004, p. 407)

[389] According to Louis Essen: "The essential feature of science is its dependence on experiment. Results of experiment are expressed in terms of units, which must not be duplicated if contradictions are to be avoided and units of measurement are the only quantities which can be made constant by definition. When Einstein wrote his paper, two of the units were those of length and time. Velocity was measured in terms of these units. Einstein defined the velocity of light as a universal constant and thus broke a fundamental rule of science." (Essen 1996)

Apart from an aether as empty Space endowed with specific physical qualities in the style of a Whittaker or an Einstein, any consistent definition of a revitalized concept remains illusory. In a now burgeoning dissident literature,[390] amongst many, Lévy has revisited an aether-based Lorentzian relativism, (Lévy 2003) Hatch has pursued "An Ether Gauge Theory" also referred to as a "Modified Lorentz Ether Theory," (2000; 2001; 2006) Bourassa has advanced a "Quantum AetherDynamics," (2000) and Norgan has conceived an "Aether Theory of Velocity Effects." (2005a; 2005b) An "Aethro-Kinematics", based on "the ideal gas model of the aether,' and which attempts reinstatement of Descates' vortex model, is sponsored by Rado, (1994) and Nascimento has revisited an aether partial-dragging hypothesis as explanation for a 1969 experiment by Shamir and Fox claiming a 6.64 km/s aether wind. (1998)

An objective medium somewhat in the classical Maxwellian mould is intended by many, particularly as resulting from positive claims made for the results of a minority of interferometry experimenters. Prominent in early claims purporting new proofs of a light-bearing medium were those made for the extensive and meticulous work of American physicist Dayton C. Miller of the Case School of Applied Science, Cleveland, Ohio. In early work with Morley, and using the same techniques as had been pioneered by Michelson, Miller published a null result in 1904. However, further refinement of his instruments to provide the most sensitive ever built, together with over 200,000 observations over many years both at Cleveland and at high altitude on Mount Wilson, California, convinced Miller of a small positive result indicating a real aether-drift. (Miller 1933) Various trials were made with a range of light sources; electric arc, incandescent lamp, mercury arc, acetylene lamp, and also with sun light. (Miller 1925, p. 310) When Miller published in 1925, his controversial results claimed consistent detection of an aether wind:

> The ether-drift experiments at Mount Wilson during the last four years, 1921 to 1925, lead to the conclusion that there is a relative motion of the earth and the ether at this Observatory, of approximately nine kilometers per second, being about one-third of the orbital velocity of the earth. By comparison with the earlier Cleveland observations, this suggests a partial drag of the ether by the earth, which decreases with altitude. (Miller 1925, p. 314)

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[390] Erik Francis has created a somewhat pejorative website (Francis 2011) listing many dissident, and alternative physics internet resources where, by his personal claim, "sites are rated in terms of their crankitude." However, the site disclaimer adds: 'Not all Web sites featured on Crank Dot Net are indeed the work of cranks. Some are fringe science material, some are humor and parody. It is ultimately up to the astute reader to decide for themselves. In particular, sites marked as parody, fringe, or bizarre are not cranky."

Einstein found the discussion confronting, and in a 1925 letter to Edwin Slosson (cited in DeMeo 2002) admitted that: "Should the positive result [of Miller's experiments] be confirmed, then the special theory of relativity … would be invalid." The *Science News-Letter* of November 9, 1929 advised a science-literate public of the serious implications of Miller's claims:

> … despite the many feats of the Einstein theories of relativity … they are seriously menaced by having one of their foundations pulled out from under them.
>
> Prof. Dayton C. Miller has reported to the Optical Society of America that he has during the past year laboriously repeated the ether drift experiments that he has been making during the last nine years in a Cleveland laboratory and on high Mount Wilson in California. Again he finds an observed effect in the light path of his apparatus such as would be produced by a relative motion of the Earth and the ether of about ten kilometers (six miles) per second … In 1925, his paper on this work won the annual prize of the American Association for the Advancement of Science … [and] continued ability to obtain the same results … makes Dr. Miller's results all the more important. (Science News Online 1999)

Miller had consistently insisted that his results were 'significant,' leading ultimately to an intensive post-mortem investigation by a team headed by Miller's former student, Robert S. Shankland of Case Institute, and personally encouraged by an anxious Einstein. Their report, (Shankland et al. 1955) blamed subtle environmental variations, which Miller, a meticulous and experienced experimenter, had consistently and publicly denied.[391] Their analysis claimed that the persistent seasonal cosmic 'signal' that Miller believed inherent in his voluminous data, was such that "the second harmonics in the Mount Wilson data [are shown to be] associated with a [changing] temperature pattern in the observation hut," and concluded that "the observed harmonics in the fringe displacements are not due to a cosmic phenomenon (aether drift) ... but rather to temperature effects on the interferometer." (Shankland et al. 1955, p. 178)

According to a recent review by Roberts, the Shankland team failed "to fully resolve the issue [in failing to] give any argument or discussion of how [their claimed interpretation] could generate such a remarkable result" (Roberts 2006) For others, the report was believed tainted by Einstein's involvement and biased to support his views.

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[391] "[preliminary] experiments proved that under the conditions of actual observation, the periodic displacement could not possibly be produced by temperature effects." (Miller 1925, p. 311) Einstein wrote to Miller suggesting temperature variations as a source of error. An irritated Miller responded: "The trouble with Prof. Einstein is that he knows nothing about my results … He aught to give me credit for knowing that temperature differences would affect the results. He wrote to me in November suggesting this. I am not so simple as to make no allowances for temperature." (Miller quoted in DeMeo 2000)

The claimed positive Miller result has further been held to undermine the prior Michelson-Morley null result: "It is believed that a reconsideration … will lead to the conclusion that the Michelson-Morley Experiment does not give a true zero result." (Miller 1925, p. 314) A variety of reformulations have now been proposed in which an aether-drift based on positive claims for re-analyzed historical data is featured. (Consoli 2003; Consoli & Costanzo 2004, 2007; Múnera 1997, 1998; Selleri 1997; Vigier 1997). Amongst others adding encouragement for an objective aether renewal, Maurice Allais, French physicist and Nobel laureate, (Economics 1988) also supported Miller's results as significant and as confirming a space anisotropy.[392] (Allais 1999a, 1999b, 2000, 2003b). Experiments using a paraconical pendulum and predicting an "Allais Effect" have subsequently been claimed as having further confirmed his dissident approach. (Aujard 2001)

Sponsoring more recent reports, Australian physicist Reginald Cahill and his associates at Flinders University, have made a series of forthright claims, one deliberately coinciding with the hundredth anniversary of Einstein's 1905 'miracle' year, claiming that "the centenary of Einstein's *Special Relativity* turns out to be also its demise." (Cahill 2005) Such a claim is consistent with earlier papers (Cahill & Kitto 2002; 2003) in which a 1998 re-analysis of the original 1887 Michelson-Morley interferometer data by Múnera, was used to support a further re-analysis of the later 1927 Illingworth interferometer data. Claiming a new interpretation of an interferometer operating in gas mode,[393] such as air, or helium as in the Illingworth case, the Cahill re-evaluation, in "correcting for the refractive index" of the dielectric, claims to reveal

> … an absolute speed of the Earth of $v = 369 \pm 123$ km/s, which is in agreement with the speed of $v = 365 \pm 18$ km/s determined from the dipole fit, in 1991, to the NASA COBE satellite Cosmic Background Radiation (CBR) observations [which results] refute Einstein's assertion that absolute motion through space has no meaning. (Cahill & Kitto 2002)
>
> So far another six experiments have been found that also revealed a similar [absolute] speed. It means that space has a substructure, that absolute motion is observable, that the

[392] Although Allais claims "very significant regularities" in Miller's interferometric observations not attributable "to some perverse effects like temperature," he nonetheless claims Miller's interpretation to be "*totally erroneous*." In the Allais reinterpretation, Miller's experiments show a "variation of the speed of light due to a local anisotropy of the aether." (Allais 2003a)

[393] "In reviewing the operation of the Michelson-Morley interferometer … it was noticed that the Fitzgerald-Lorentz contraction explanation only implies a null effect if the experiment is performed in vacuum. In air, in which photons travel slightly slower than in vacuum, there should be a small fringe shift effect when the apparatus is rotated, even after taking account of the Fitzgerald-Lorentz contraction … [such] interferometer experiments do indeed reveal small but significant non-null results [but] only when they are operated in a dielectric … The correction is in fact large, being some two orders of magnitude." (Cahill & Kitto 2002, p. 3)

> spacetime construct has no ontological meaning … and that it is absolute motion that causes the various relativistic effects, as first suggested by Lorentz … modern-day vacuum Michelson interferometer experiments are badly conceived, and their null results continue to cause much confusion: only a Michelson interferometer in the gas-mode can detect absolute motion … (Cahill 2005)

Nonetheless, in a recent detailed but critical review utilizing the acutely analytical powers of digital signal processing, Roberts claims that Miller's signal "is an order of magnitude smaller than the resolution with which his raw data points were recorded." (Roberts & Schleif 2007) Roberts further claims that an error analysis of Miller's data[394] shows that "the errorbars are enormous and his stated results are not statistically significant." (Roberts 2006, pp. 2, 7) Further, in reviewing papers by such as Allais, Cahill, Cahill and Kitto, Consoli, DeMeo, Múnera, Selleri, and Vigier, all of whom build on an assumption that the Miller claim is valid, Roberts dismisses them all as "worthless" on the basis that:

> … none of these authors include a comprehensive error analysis, including the few who claim to do so (every one ignores the huge systematic drift, and none performs the simple [digital signal processing] error analsis … [on the other hand] The mainstream of physics assumes [Miller's] results are invalid. Neither group has had solid, objective criteria to support their position … (Roberts 2006) [395]

Applying similar criteria to Cahill's more recent claims to have detected "absolute motion," (Cahill 2006) Roberts again claims that, although initially reasonable efforts were made to minimize temperature effects on his co-axial cable configuration, Cahill failed to "monitor the environmental factors (temperature, barometric pressure) that can affect his apparatus." Further, Cahill's "comparisons are without error bars and are therefore worthless." Thus Roberts again claims that a competent uncertainty analysis reveals that "*NONE* of these variations are statistically significant." (Roberts & Schleif 2007) As Roberts reviews it:

> A key point is: if one is performing an experiment and claiming that it completely overthrows the foundations of modern physics, one *must* make it bulletproof or it will not be

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[394] "Error" here does not indicate falsehood, discrepancy, or experimental failure, but intends a measurement of *uncertainty* as a quantitative parameter indicating the level of confidence that a claimed result actually lies within the range defined by an 'uncertainty bound' or 'error bar' interval. (FloodRiskNet 2007; UKAS 2007) Standard methods of computation and expression of experimental uncertainties are now defined by the *National Institute of Standards and Technology*. (NIST 2001)

[395] "Even in 1955, Shankland *et al* did not have knowledge of these aspects of Miller's analysis." (Roberts 2006)

> believed or accepted. At a minimum this means that a comprehensive error analysis *must* be included, direct measurements of important systematic errors *must* be performed, and whatever "signal" is found *must* be statistically significant. None of these experiments come anywhere close to making a convincing case that they are valid and refute SR. (Roberts & Schleif 2007)

Nonetheless, investigators, unconvinced by rival claims, and suspicious of reported data and the potential for both incompetent[396] or politically motivated analysis, continue to explore potential alternatives to an establishment physics with its perceived dogma and intransigence. Much contention is incited by the appearance of editorial embargo. Approaches suggesting any digression from conventional lines are claimed by many authors to meet with publication rejection by mainstream journals. In a reaction intended to redress injustice, safeguard scientific integrity, and provide a true freedom of access to information, scientific journals have been astablished, such as "Galilean Electrodynamics," founded by Petr Beckman in 1989. Such publications now offer open fora for quality scientific and philosophical papers and refereed by professional scientists. Yet again, however, appeal to a strong empirical compliance conventionally holds alternative interpretations as both unnecessary and fallacious.

Conflict with empirical predictions

Of more immediate concern however, it is now evident that Einstein's original quantum model of field radiation endorsed a representation of light in a form compatible with point-mass modeled counterparts of a mechanically formulated inertial reference frame system. As the supporting model for his approach to theory construction, it became the entry point into optical theorizing of such mechanically compatible propositions as separability and locality. Given that it now appears an unavoidable conclusion resulting from competent empirical investigation, that the world is truly non-local in contradiction to the predictions of special relativity, a more cogent case now exists for replacement of the theory's predisposing optical model. In casting doubt on the propriety of the facilitating modeling, the original atomist model is not thereby decreed to be "wrong," but rather as unsuitable, or as no longer providing an appropriate choice of representations for the particular phenomenon under

[396] In his report on Data Models and data manipulation, Harris notes both that data "models represent inexactly" and "that many cases of data acquisition and processing in science are poorly described in terms of the treatment of raw, uninterpreted data." (Harris 2003) On yet another front, in his 1933 variation on the aether theme, Arthur Lynch, in his "*Case Against Einstein*," having "criticised [the special theory] in a close and minute discussion of the crucial questions in the psychological, physical, and mathematical domains," (Lynch 1933) claimed that Einstein had done little more than replace aether with a "mathematical expression" of dubious accuracy.

consideration. As an *experimentum crucis*, the 1850 Foucault, Fizeau and Breguet exprimental approach was intended to discriminate between classical properties of two contending classical models. There, solid bodies should display a different refractive angle to that of a wavefront under the same experimental circumstance. The resulting verdict found in favour of a continuum wavefront model, and is held to have ruled against a future employment of *classical* particle properties in the modeling of optical radiation. However, the empirical program evincing from Bell's inequalities can be seen as an experimentum crucis centered on a different criterion. Here, the models are not classical but representative of quantum phenomena. The new criteria for discrimination now selects for observable differences based on locality and separability. Insofar as a verdict is now accepted as having been reached, the results are believed to find in favour of the continuum rather than the discrete, and might be held to rule against the future employment of emission modelings of radiation *per se*. The model supporting Einstein's invariance theory construction is then not to be rejected due to its claimed correspondence to the prior failed Newtonian model, nor as failing to incorporate wave-like modeling properties, but for the sole fact that it sponsored a theory that proved to be predictively unsuccessful.

A useful candidate for replacement is not difficult to discover. It is here of value to note that where a subject matter, such as electromagnetism, is serviced by a multiple-models idealization, the possibility also exists that a current, but now censured model, might be deemed to have been less appropriately chosen for the specific domain under consideration. Such a model might then be replaced by another model from the same modeling community, but one whose insights might now be considered more appropriate to an empirically successful theory construction. Given the criteria for adjudication by Bell inequality tests, such a model would need to exhibit properties of nonlocality and inseparability.

Contention between aspiring modeling concepts had been evident from early in the "new epoch in physics" inaugurated by Planck's unprecedented discovery of an 'elementary quantum of action'. (Born 1948) Planck himself recognized the potency of emerging contentions. A realist pretension appeared evident in the neo-atomist characterization of the quantum and he wrote to Einstein in 1907, clearly expressing his concerns:

> ...Does the absolute vacuum (the free aether) possess any atomistic properties? ... [You] seem to answer the question in the affirmative, while I would answer it, at least in line with my present view, in the negative. For I do not seek the meaning of the quantum of action

(light quantum) in the vacuum but at the sites of absorption and emission, and assume that the processes in the vacuum are described *exactly* by Maxwell's equations. (Planck to Einstein, July 6, 1907, cited in Gearhart 2002, p. 192)

A noticeable trend towards an atomist ascendency in the ensuing radiation debate, and its attendant and practical hostility to Huygens principle, was to continue to trouble Planck. In his acceptance speech for the Nobel Prize in Physics in 1919, Planck conceded that "the tables have been turned." In what he assessed as "this desperate struggle," he recalled as pertinent the historical Huygens-Newton conflict, observing that:

> While originally it was a question of fitting in with as little strain as possible a new and strange element into an existing system which was generally regarded as settled, the intruder, after having won an assured position, now has assumed the offensive; and it now appears certain that it is about to blow up the old system at some point ...
> There is one particular question the answer to which will, in my opinion, lead to an extensive elucidation of the entire [radiation] problem. What happens to the energy of a light quantum after its emission? Does it pass outward in all directions, according to Huygens's wave theory, continually increasing in volume and tending towards infinite dilution? Or does it, as in Newton's emanation theory, fly like a projectile in one direction only? In the former case the quantum would never again be in a position to concentrate its energy at a spot strongly enough to detach an electron from its atom; while in the latter case it would be necessary to sacrifice the chief triumph of Maxwell's theory – the continuity between the static and the dynamic fields – and with it the classical theory of the interference phenomena which accounted for all their details, both alternatives leading to consequences very disagreeable to the modern theoretical physicist. (Planck 1919, pp. 500, 501)

In Planck's assessment, the contention between discrete and continuum concepts of radiation was best represented by the historical contention between a Newtonian-styled corpuscularism and a Huygens-styled wavefront, and he cast a resolution of the more modern contest as the basis of a very perplexing but essential scientific arbitration. However, such concepts, reinterpreted as scientific models within the scope of a *multiple-models idealization*, goes at least part way to addressing Planck's concerns. Further in this respect, recall of Whitehead's warning concerning "unimaginative empiricism" is also germane. Whitehead warned of "the danger of refusing to entertain an idea because of its failure to explain one of the most obvious facts in the subject-matter in question." (Whitehead 1938b, p. 63) A Huygens-styled wavefront, de-idealized by Fresnel and Kirchhoff to more realistically represent aspects of a Maxwellian-styled electromagnetic continuum, is nonetheless a model drawing on the representational power of analogies. Each analogy is capable of offering only a partial insight into a whole subject matter. In the event, the model aspires to usefully represent the unobservable dynamical processes of radiation in free space, and fulfills a brief in which modeling an interface event with matter was not a part of its original job description. It then appears plausible to submit that, insofar as the special theory's second postulate at least purports to engage with light-speed on the basis of an unmodified Maxwellian electromagnetic field[397] and equally fails mention of an interface with matter, a predisposing Huygens-Fresnel-Kirchhoff wavefront model of radiation in free space appears to appropriately represent the particular domain under investigation, and, in terms of its properties, to be well suited.

[397] Despite a formerly afflicted Maxwellian relationship, Einstein was to eventually summarize: "The special theory of relativity is an adaptation of physical principles to Maxwell-Lorentz electrodynamics." (Einstein 1923)

Chapter 8
Considerations of theory construction from a wavefront model

The “speed of light” in vacuum

The central role the vacuum speed of light plays in modern theoretical physics as a universal constant, the complex history of its quantification and ultimate elevation as a primary criterion for scientific measurement, has demanded close attention to unambiguous definition. It then becomes equally necessary to determine the possible emendation of these ideas in terms consonant with an alternative sponsoring wavefront model.

For centuries light was held to be an instantaneous phenomenon, wherein its “speed” would be necessarily infinite. Musings of a possibly finite rate of travel are recorded as early as around 450 BC with the philosophy of Empedocles of Acragas. Resistance to change on a variety of grounds, as with most advances in human thought, was evident and even punitive.[398] Nonetheless, with the dawning of the modern scientific era, the first faltering attempt at quantification is famously attributed to Galileo, who used the manual covering and uncovering of widely separated lanterns. With improvement in techniques and commencing with Römer’s reasonably accurate determination using Jupiter’s moons in 1676,[399] (Cohen 1940) over the next 300 years the speed of light was to be measured some 163 times utilizing a wide variety of techniques and by more than 100 investigators. (Spring et al. 2004) Although many including Arago, Fizeau, and Foucault, resorted to mechanical devices utilizing rotating mirrors and toothed wheels, Bradley employed stellar aberration, (Bradley 1727) whereas Maxwell, and Rosa and Dorsey in 1907, used theoretical calculations.[400] With the discovery of electromagnetic waves, a variety of techniques - radio, electrical,

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398 “… around 525 AD, Roman scholar and mathematician Anicius Boethius attempted to document the speed of light, but after being accused of treason and sorcery, was decapitated for his scientific endeavors …” (Spring et al. 2004)

399 Danish astronomer, Ole Römer, on a visit to Paris, noted a variable delay in the eclipse data for the first Jovian moon, Io, whose orbital period of 1.769 days was then known accurately from both Borelli’s and Cassini’s tables. The delay in eclipse times increased as Jupiter moved further away from Earth amounting to ~ 10 mins. with Jupiter near conjunction. (Cohen 1940, p. 353) Römer hypothesized "mora luminis," a delay due to light travelling an extra Earth orbital diameter, (Roemer 1677) A light-speed estimate of ~200,000 **miles/sec**. (≈ 300,000 km/s) is suggested by some accounts. Unfortunately, some modern texts may malign Römer’s reasonable accuracy, failing to convert this imperial value to metric and quoting ~200,000 **km/s.** (Gibbs 1997; Speed of Light 2006; Spring et al. 2004) Brown notes that: “Using modern estimates, the Earth’s orbital diameter is about 2.98×10^{11} meters, and the observed time delay in the eclipses … is about 16.55 min. = 993 secs., [giving] $2.98 \times 10^{11} / 993 \approx 3 \times 10^{8}$ meters/sec.” (Born 1965, p. 93; Brown 1999c, 1999f)

400 Using electrostatic laboratory values for permittivity and permeability provided by the work of Weber and Kohlrausch in 1857, in 1907 Rosa and Dorsey calculated *c* as 299,788 ± 30 km/s. (Gibbs 1997)

spectroscopic, and eventually microwaves and lasers - was brought to bear on improving both accuracy and then precision. Singular advances were made by Albert A. Michelson in a series of five experimental periods from 1878 until his death in 1931. Commencing with improvements to rotating mirrors, he published a result of 186,500 ± 300 miles/s (300,140 ± 480 km/s) at the U.S. Naval Academy at Annapolis. (Oldford 2000) After moving to Case Institute, in his third series of experiments Michelson recorded a light-speed in vacuum of 299,853 ± 60 km/s. In his fourth series, and incorporating principles of both a toothed wheel and rotating mirror approach, 8, 12, and 16 sided mirrors were employed, with a value of 299,820 ± 30 km/s. recorded. (Michelson 1924) Improvements in accuracy came with Michelson's flair for innovation which had called for incorporating his interferometer and rotating mirrors into configuration with the Mount Wilson Lick Observatory's refracting telescope allowing a 22 mile (35.4 km) baseline to Mount San Antonio. Some thirty years later, in 1926, he applied the same techniques to the 100-inch Mount Wilson Observatory telescope, then the world's largest. Michelson's fifth series initial determination of 299,796 ± 4 km/s (Michelson 1927) was finally re-evaluated at 299,798 ± 15 km/s. After his death in 1931 at age 79, Michelson's co-workers, F. G. Pease and F. Pearson continued work begun in 1929 on an evacuated one metre diameter, mile long pipe, located near Laguna Beach, California. They eventually claimed a low value of 299,744 ± 11 km/s., (Michelson, Pease & Pearson 1935) which may have suffered from systematic errors caused by tidal and seismic actions on the unstable alluvial soil the pipe was laid on.

In 1950, and using the improved precision offered by cavity resonance technology, Essen obtained a value of 299,792.5 ± 1 km/sec for the speed of light in vacuum, a value within two metres per second of the then most recent laser determinations.[401] (National Physical Laboratory 2005b) Essen championed the cesium spectrum as an international time standard and in 1967, the 13th General Conference on Weights and Measures (Conférence Général des Poids et Mesures) ratified a redefinition of the second in terms that had been precisely measured by Essen in collaboration with William Markowitz of the US Naval Observatory. By 1975, and acknowledging an improved precision claimable by laser techniques, the 15th General Conference recommended that the speed of light be adopted as 299,792,458 m/s.[402] Ultimately, in

[401] Replicating Essen's technique Froome reduced the experimental uncertainty during 1951-58 obtaining 299,792.5 ± 0.1 (Gibbs 1997)

[402] "The 15th Conférence Générale des Poids et Mesures, **considering** the excellent agreement among the results of wavelength measurements on the radiations of lasers locked on a molecular absorption line in the visible or infrared region, with an uncertainty estimated at ±4 x 10^{-9} which corresponds to the uncertainty of the realization of the metre … **recommends** the use of the resulting value for the speed of propagation of electromagnetic waves in vacuum c = 299 792 458 metres per second." (CGPM 1975) The

1983, the 17th CGPM meeting redefined the metre in terms of the now defined speed of light and resolved that: "The metre is the length of the path travelled by light in vacuum during a time interval of 1/299 792 458 of a second," abrogating "the definition of the metre in force since 1960, based upon the transition between the levels $2p_{10}$ and $5d_5$ of the atom of krypton 86." (CGPM 1983)

Nevertheless, controversial research results, published around 1999 and which examined measurements of distant quasar absorption spectra, raised the possibility that the 'fine-structure constant'(denoted *alpha*)[403] may have varied in the past history of the universe from its currently accurately determined value. (Webb et al. 1999) Further investigation appeared to confirm the initial troubling speculations as statistically significant. (Murphy et al. 2003; Webb 2003; Webb et al. 2001) Although only a variation in alpha of about 0.001% was being claimed, a debate on the implications for fundamental science became public after Australian theoretical physicist Paul Davies and his associates, noted that such a variation could only find explanation if the electron charge or the speed of light had changed. Claiming that studies on black holes suggested a more likely drift in light-speed, wherein its value was believed higher billions of years ago, Davies warned that: "If true, it would mean a rethink of Einstein's theory of relativity." (Davies, Davis & Lineweaver 2002; Thornhill 2002) A similar appraisal appearing in a later press release claimed:

> The findings suggest there is a more fundamental theory of the way that light and matter interact; and that special relativity, at its foundation, is actually wrong. (Adam 2005)

Additional impetus was given to the idea whereby a varying speed of light was proposed as a means of solving various contemporary cosmological difficulties such as the "horizon and flatness problems." (Albrecht & Magueijo 1999; Ellis & Uzan 2005, p. 240) On the other hand, however, continuing research by others has cast doubt on a changing *alpha* value. (Srianand et al. 2004; Wolfe & Hatsidimitris 2006a) Further, recent NASA studies of two galaxies (Markarian (Mkn) 421 and Mkn 501) each at about half a billion light-years distant, has claimed to discount the notion that light-speed has been found slowing during any recent cosmological time-span and that Lorentz invariance is consequently not vulnerable to that possibility. (Malik 2003; Seife 2005)

CGPM currently meets in Paris once every four years; the 23rd meeting of the CGPM was held in November 2007, with the 24th scheduled for 2011.

[403] "The fine-structure constant, α [*alpha*], is a measure of the strength of the electromagnetic interaction, and it quantifies the strength with which electrons bind within atoms and molecules. It is defined as $\alpha \equiv e^2 / \hbar c$ [and is a dimensionless constant] $\approx 1 / 137$, where e is the charge on the electron, $\hbar$ is Planck's constant divided by 2π, and c is the speed of light in vacuum." (Webb 2003, p. 33)

Apart from conjectures that would cast doubt on its constancy,[404] the "speed of light in vacuum … is not only the speed of light [and] not even always the speed of light." According to Ellis: "It is a complex quantity that turns out to have different origins that lead to coincident values." (Ellis & Uzan 2005, p. 241) Denoted simply c or c_0, it is a fundamental physical constant by *definition*, with an *exact* value (having no Standard uncertainty or Relative standard uncertainty) of 299,792,458 ms^{-1} (Mohr & Taylor 2004) On the other hand, the "electromagnetism constant," designated c_{EM}, is the velocity of any electromagnetic wave in vacuum[405] and finds its definition from the wave equation deriving from Maxwell's field equations such that:

$$c_{EM}^2 \equiv 1 / \varepsilon_0 \mu_0 \quad \text{or equivalently:} \quad c_{EM} \equiv (\varepsilon_0 \mu_0)^{-1/2}$$

where, ε is electrical permittivity[406] and ε_0 is is its quantification for vacuum, now designated the "electrical constant" or the "permittivity of free space."
μ is a measure of magnetic permeability[407] and μ_0 is designated the "magnetic constant" or the "permeability of free space."

As Ellis notes: "Calling c_{EM} the speed of light is too restrictive because it is characteristic of any electromagnetic phenomenon" (2005, p. 242) and enters into many of its relationships such as those of electrostatics, that do not indicate motion. In such a defining process however, a potential for confusion becomes apparent. A statement such as: "In a vacuum, light always travels at a speed of 299,792,458 metres per second, no matter how its speed is measured," (Guidry 1995) confounds c_0 with c_{EM} and is not strictly correct since $c_{EM,}$ measured to some desired degree of precision will most usually be expressed as a value different to c_0.

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404 A recent dissident literature presenting an interpretation of science deriving from a particular religious fundamentalism, has claimed academic credentials and has interpreted Michelson's data as suggesting a 1.74 km/s per year decay rate in the speed of light during the experimental period and further claiming "a 95.6% confidence interval that c was not constant at its present value during the years of these experiments." (Setterfield 2006) No other researchers have agreed and no independent confirmation has been claimed.

405 Although defining c as the velocity of light in empty space, Minkowski, added that: "To avoid speaking either of space or of emptiness, we may define this magnitude … as the ratio of the electrodynamic to the electrostatic unit of electricity." (Minkowski 1908, p. 79)

406 In simplest terms, electrical permittivity measures how much resistance is encountered when forming an electric field in a medium. Permittivity can also be described as that attribute of a material that permits it to store electrical charge. In SI units, $\varepsilon_0 = 1 / c_0^2 \mu_0 \approx 8.8542 \times 10^{-12}$ F/m

407 Magnetic permeability is the measure of magnetization that a material obtains when a magnetic field is applied to it. Heaviside coined the term in 1885. The magnetic constant is now defined at exactly: $\mu_0 \equiv 4\pi \times 10^{-7}\ H \cdot m^{-1}$. (NIST 2011)

A different basis for definition, however, is claimed for the "spacetime" constant: c_{ST}. It is this constant that, considering its theoretical origins, enters into the Lorentz transformations as defining the domain of applicability of local Lorentz invariance. This constant, ultimately deriving its definition from a Poincaré-Minkowski four-dimensional spacetime construct, can be defined quite independently of electromagnetism. As such, c_{ST} defines the relativistically claimed universal invariant limit speed. Additionally, Ellis argues that the:

> … conversion factor between energy and mass is given by c_{ST}^2 because it arises from the study of the dynamics of a point particle. Thus the correct equation unifying the concepts of energy and mass is $E = m\, c_{ST}^2$. (Ellis & Uzan 2005, p. 242)

Expanding on the definitions in detail, it is also to be noted that:

> … the speed of light, c_{EM}, agrees with the universal speed, c_{ST}, only to within the experimental precision of Michelson-Morley type experiments (or, put differently, the photon has zero mass only within some accuracy), and the causal cone need not coincide with the light cone. If we were to show experimentally that the photon has nonzero mass, then the standard derivation of relativity from electromagnetism would have to be abandoned. (Ellis & Uzan 2005, p. 242)

Additional discussion also proposes the constant c_{GW}, as the "speed of gravitational waves in vacuum;" a constant c_E, as an "(Einstein) spacetime-matter" constant; and c_m as the limiting velocity of massive particles. (Greene et al. 1991) Each claims independent historical and theoretical justification for distinct definition. The value of such multiplicity is seen in permitting unambiguous discussion in cases conjecturing phenomenological alternatives to special relativity. However, according to Ellis: "As long as we assume that general relativity is valid, [the] conclusion [that] the universal speed of [Minkowski] spacetime, $c_{GW} = c_E$ … cannot be avoided." Further, and with the assumption of a valid general relativity, it can be demonstrated that $c_E = c_{ST}$, although it remains relevant to distinguish these concepts when discussing varying speed of light models such as serving to solve some cosmological problems. Values may differ in such theories, depending on how the theory is formulated. (Ellis & Uzan 2005, p. 243)

Insofar as Ellis contends that a constant's definition may be model dependent, such as c_{ST} claimed as having dependency on a mechanical model of free-space radiation, it becomes necessary to re-evaluate the definition of a speed of light concept in the context of a predisposing electrodynamic continuum model. From the time of its inception at the hands of Christiaan Huygens in the late seventeenth century, and through extensive concretization and de-idealization that has witnessed wholesale remakes of such as the aether concept, the principal hypothesis of Huygens' original concept has remained basically unchanged: the expanding spherical wavefront of constant phase.

Contributions of a wavefront model to theory construction

For a mechanical model, massive phenomena can be successfully modeled by a geometrical point, - an idealization that selects an object's centre of mass as a minimalist representation of its location while holding its spatial extension as superfluous for the exploration at hand. However, historically, a similar mechanically-based scenario for the treatment of electromagnetic phenomena was claimed unsuccessful.

> Two different [mechanical] analogies were conceived, based on the behavior of ordinary material substances [portraying light as either] a stream of material corpuscles moving through empty space [or] waves in a pervasive material medium. Each of these analogies was consistent with some of the attributes of light, but neither could be reconciled fully with all the attributes … if light consists of material corpuscles, then according to Galilean relativity there should be an inertial reference frame with respect to which light is at rest in vacuum, whereas in fact we never observe light in vacuum to be at rest [Alternatively] if light is a wave propagating through a material medium, then the constituent parts of that medium should behave inertially, and in particular should have a definite rest frame, whereas we find that light propagates best through regions (vacuum) in which there is no detectable material with a definite rest frame, and again we cannot conceive of light at rest in any inertial frame. (Brown 1999e)

Therefore Brown, and in sympathy with the thrust of a pervasive twentieth-century philosophy of science, concludes that: "Thus the behavior of light defies realistic representation in terms of the behavior of material substances within the framework of Galilean space and time …" (Brown 1999e) It is in respect of such a difficulty that a wavefront modeling approach, seemingly denied a constructive role in the now preferred Einsteinian theoretical approach, arguably offers some potential for resolution.

In a Huygens' model, a wavefront is the sole detectable result of the superpositional addition of myriads of intermediate omni-directional 'wavelets' expanding from every place within the light complex, each with equal rate. Huygens denoted its physical import as "a kind of matter" – a "*quadam materia*" – a physically realized phenomenon, but one which experiences continuous change through renewal of its constituency from moment to moment. The wavefront further lacks an attribute of durability usually associated with material bodies in that its realization lasts only so long as the processes underwriting the light complex endure. Ultimately the physical reality of such an entity, at least in terms of endurance, lies in persistence of pattern rather than in persistence of constituency. However, such a pattern, existing in a dynamically expanding spherical-like region of space, frustrates any direct attempt to give accurate description to its 'location' in terms of simple co-ordinates. Nonetheless, a Galilean idealization that represents the wavefront region only by reference to a specific phase, can be said to successfully reduce the manifold complexities of its ephemeral regional distribution to that of a single spherical surface of constant phase.[408] (Breinig 2008b) Such an abstract geometrical representation then offers, as it would for any hollow but uniform material object, such as a beach ball or doughnut, the facility of having its geometrical centre act to represent the whole phenomenon for the purposes of denoting a purely representational but coordinate compatible location.

It is then from such an idealization, that an idealized representation of purely electrodynamic processes may, within its domain of applicability, be portrayed as conformable to the otherwise purely mechanical locality requirements of a Cartesian-styled reference frame system. Such a discussion could then claim to represent a behavior of light 'in terms of the behavior of material substances within the framework of Galilean space and time.' Although the claim stands in contention with edicts of received doctrine, it nonetheless suggests potentially fruitful avenues of investigation either inadequately or not directly addressed in earlier approaches to theory construction.

Expansion versus projection

Ensuing directly as a corollary of such a revised modeling approach, the "speed of light in vacuum," now viewed as a primary attribute of a Huygens-styled light complex, also finds need of a revised wavefront-consistent definition. For a particle model of light in vacuum, the "speed" of the phenomenon is determined as the rectilinear, and thereby vector quantifiable, travel of a photon at velocity *c*, (Breinig 2005a; Brown

[408] "The wave fronts of a spherical wave are spherical surfaces surrounding a point source. The wave vector $\mathbf{k}$ points away from the point source along the radius of the sphere at any point. The area of a wave front with radius r is $4\pi r^2$. Since energy is conserved, the intensity must decrease with r as $1 / r^2$. Since the intensity is proportional to the square of the amplitude, the amplitude must decrease with r as $1 / r$ …" (Breinig 2008b)

1999g) However, the equivalent "speed" attribute of an expanding wavefront, as the spreading away from a point of origin at a rate determined solely by values of the electric and magnetic constants, is more correctly determined in terms of an expansion coefficient or inflation rate. In such a model, light rays are arbitrary radial samples orthogonal to the idealized wavefront surface and no directionality of the expansion coefficient is possible in its definition. (Breinig 2005b) As such, the radial inflation rate is a scalar,[409] expressible in the same dimensions of 'metres per second' as for a speed or velocity. (Breinig 2008b) Thus, for a wavefront model, the "speed of light in vacuum" may be more succinctly and less ambiguously defined as the "electrodynamic radial inflation constant" in vacuum. Given the modern predilection for acronyms, "ERIC" may prove a useful nomination and serve to distinguish two quite different modes of measurement.

The "constancy" of such a propagation rate as ERIC is not, by virtue of its derivation, that of an absolute or axiomatizable feature of nature. Such a constancy is derived, being dependent in turn on electromagnetic characteristics measured for vacuum. In this respect, it is a constancy reflecting the empirically observed uniformity and isotropy of electric and magnetic properties of free space. Such a modeling scheme finds much to recommend it in the context of theory reformulation:

- Such a strategy holds ERIC as innately independent of the source by virtue of its sole dependence on the electric and magnetic properties of free space. In terms of an electromagnetic "speed of light" as portrayed by Ellis, quantification of ERIC is congruent with that of c_{EM}.
- Additionally, such a model leads to a constructive approach to theory formulation and is, in this respect, compatible with a Poincaré-like philosophy of science that would characterize ERIC as an empirical regularity rather than a phenomenon inviting award as an unexplained postulate of nature.
- Such a scheme views the spherical wavefront process as both a non-separable and non-local phenomenon, filling an ever increasing volume of space. In this respect a wavefront representation exhibits attributes that clearly distinguish it from a separable and local mechanical model. On this basis, such a model is pertinent to a now proven foundational theory disparity. The potential for a relativity theory reconstruction that avoids both foundational theory confrontation and supports non-local experimental predictions then also becomes apparent.
- Maintained within an idealized inertial environment of free space, a geometrically central coordinate representation of its 'location' should remain coincident with its

[409] For a spherical wave front of radius r: "If the point source is located at the origin, then **k** is always parallel to the position vector **r**, **k** | | **r**, and **k** · **r** = kr. Here k = 2π / λ is the wave number and r is just a scalar distance along any radial direction." (Breinig 2008b)

location of genesis, which is to say, that in free space, the centre of a symmetrically expanding light complex should remain coincident with, but independent of, the light-source.[410] (Spencer & Shama 1996; Torretti 1983, p. 55) It is also of interest to note that such an illustration is not confined to a Huygens' dynamics, having been cited by such as Ritz and Tolman as also applicable to ballistic models:

> … a source in uniform motion would always remain at the center of the spherical disturbances which it emits, these disturbances moving relative to the source itself with the same velocity *c* as from a stationary source. (Tolman 1912)

Extending Huygens' model – refraction and propagation in media.

In an inertial and isotropic setting, forecasts ensuing from a wavefront sponsored empiricism are to be seen consistent with investigations such as the Michelson-Morley class of experiments. In this respect however, empirical expectations remain undifferentiated from results expected of a Lorentz mechanics, an Einstein photon-emission model or even a Ritz ballistic scheme. Differentiation requires augmenting the model's repertoire of representational resources so as to extend its domain of applicability from its originally intended limitation to radiation in free space, to one that encompasses interface and interaction with matter and that then might include considerations of relative motion.

It is in pursuit of such an expanded domain of applicability that a gross difference becomes crucial between the descriptions and expectations of a particle dynamics and that of a wavefront. Importantly, it is such a distinction that may be found to necessarily sponsor different theoretical outcomes. A massive particle in force-free motion, as depicted in a standard inertial frame of reference, exhibits undeviating velocity in accordance with the classical Newton-Galilean edicts of inertial motion. Such a velocity remains constant independent of the electrical permittivity and magnetic permeability properties of its local environment. The velocity apparent at any one place or time is then necessarily a valid evaluation of its historical velocity whilst having been maintained within a consistent inertial scenario. On the other hand, the case for a wavefront is fundamentally different and it is this difference that would insinuate a quite different outcome for theory redevelopment. For a Huygens-styled representation of a

[410] As Spencer notes, for a wavefront interpretation, "in free space a light signal moves outward in a spherical wave" with radius $r_1 = c\ (t - \tau)$ where r_1 is the spherical radius, c is the velocity of light in vacuum, τ is the instant of generation, and t the instant of reception. Further, "the center of the spherical wavefront is always at the point where the source was when the light signal was emitted. It is assumed to be entirely independent of the motion of the light source." In an ideal inertial system this further requires that "the center of the spherical wavefront is always at the source …" (Spencer & Shama 1996)

light complex, it is the continual Markov process property of "memorylessness,"[411] that primarily proves idiosyncratic – a distinctive characteristic of such a propagation model wherein determination of its 'velocity' state, at any given moment, is independent of its past states or history. Irrespective of its environment being "force-free" in a mechanical Newton-Galilean sense, a wavefront propagating in an equivalent inertial setting, is, moment by moment, directly and solely an expression of the local electrical permittivity and magnetic permeability of its local environment. Instant by instant, the immediately local dielectric refractive index is the sole determinant of a wavefront's radial expansion rate, or 'velocity'. A wavefront's immediately prior velocity history is continually, and irretrievably, extinguished in the ongoing process of wavefront construction.

Huygens' principle as a Markov propagation process

Following extensive development by Fresnel resulting from his research into the problems of diffraction in 1818, a rigorous formulation of Huygens' principle was first given by Helmholtz for the stationary case in 1859, and then for the dynamic case by Kirchhoff in 1882. However, in a twentieth-century approach, Huygens' principle was claimed to follow formally from the fundamental postulate of quantum electrodynamics wherein wave functions of every quantum entity, from photons, electrons, and neutrons, to atoms and molecules, were held to propagate over all permitted paths from the source to a given location, and where it is the superposition or interference of all such path integrals that defines the amplitude and phase of the wavefunction of an entity at that location. In this treatment, Huygens' principle emerges by the mathematical fact that

> the solution of the wave equation at a point *M* of the three-dimensional space at the moment of time *t* is expressed in terms of the values of the solution and its derivatives on an arbitrary closed surface inside which *M* is contained, at preceding moments of time. (Sveshnikov 2001)

A number of additional approaches to derivation and proof of Huygens' principle have also proven successful including a recent use of Poisson's formula for the three-dimensional wave equation with initial conditions. (Zhenxiu 2003) In perhaps its best known exposition, in 1923 the French mathematician, Jacques Hadamard, formulated three different meanings attached to the term "Huygens' principle" as used in literature

[411] Named for the Russian mathematician, Andrey Andreyevich Markov, (1856–1922). Continuous-time Markov processes are used to describe physical processes where a system evolves in constant time and in which the Markov property holds. The Markov property refers to the memorylessness of the process whereby the state of the system at time $t+1$ depends only on the state of the system at time t, i.e. on the present state of the system, and from which both its future and past are independent.

of the time. (Chalub 2001) According to Hadamard's clarification and rigorous mathematical analysis, "Huygens' principle comprehends the principle of action-by-proximity and the superposition of secondary wavelets (Huygens' construction)" (Enders 1996 abs.) In this meticulous treatment of Huygens' principle as developed from Hadamard's investigations of the Cauchy problem for partial differential equations, a logical conclusion is derived from two premises, one of which, the *major premise,* states:

> "The action of phenomena produced at the instant $t = 0$ on the state of matter at the time $t = t_0$ takes place by the mediation of every intermediate instant $t = t'$ …"

The *minor premise*, which is a secondary attribute of propagation processes, states:

> "The propagation of light pulses proceeds without deformation (spreading, tail building) of the pulse."

Hadamard's syllogism or logical conclusion is then derived as:

> "In order to calculate the effect of our initial luminous phenomenon produced at ω_0 at time $t = 0$, we may replace it by a proper system of disturbances taking place at $t = t'$ and distributed over the surface of the sphere with centre ω_0 and radius $c(t' - t)$.

As noted by Enders, in respect of this conclusion, "the isotropy of re-irradiation can be replaced with re-irradiation according to the actual local propagation conditions." In its basic terms, this means that the secondary wavelets derive their spreading motion from determining properties of the material under consideration. For free space, it is the electric and magnetic constants that so determine the expansion rate of the secondary sources and thus the wavefront. For a material dielectric, the refractive index, deriving from the electric permittivity and magnetic permeability of the medium, is by definition, the regulatory factor. Appearances of velocity constancy then reflect only an isotropy of the medium. Thus, in anisotropic media, "the reaction of the secondary sources is anisotropic, while, in nonlinear media, their excitation and re-irradiation is not proportional to the amplitude of the exciting field." (Enders, p. 228)

Whereas the Hadamard conclusion is essentially Huygens' construction, as the manifestation of a spherical envelope or wavefront by superposition, the major premise presents as the principle of *action-by-proximity* and the minor premise postulates the optional propagation of a sharp wave front. In the Hadamard derivation, the minor premise presents as a specific case, and reflects the idealization of Huygens' construction modeled as a simple spherical surface of constant phase where:

> [the minor premise] is necessary for geometrically constructing the wavefront, but not for the basic principle of action-by-proximity and the cycle of excitation and re-irradiation of secondary wavelets. (Enders 1996, p. 228)

Well known practical examples approximating the more idealized case, where the minor premise is an essential ingredient, include the 'lossless' wave equation by d'Alemdert for odd space dimensions and "the 'distortion-free' special case of the 1D telegrapher's equation." (Enders 1996, p. 228) However, action-by-proximity is not restricted to such idealized sharp wave fronts. The action-by-proximity premise, pictured in Huygens' original model by "elastic waves in the aether," and combined only with Huygens' construction as presented in the conclusion of Hadamard's thesis, constitutes the more general statement of Huygens' principle. As Enders interprets it:

> The shape of the wave front may vary from case to case, without influence on these basic ingredients of propagation, but the essentials of Huygens' (and Faraday's) imagination of propagation are conserved. The advance of this notion of Huygens' principle consists in that its applicability becomes rather universal, in fact, in this form, Huygens' principle qualifies as a clue for unifying the physical and mathematical description of many different transport and propagation processes. (Enders 1996, p. 228)

On the other hand, following Feynman's development in the space-time domain and using appropriate field operators such as Green's functions in the state space, Huygens' principle also finds exact mathematical expression through the Chapman-Kolmogorov equation,[412] which is the equation of motion of Markov processes. According to Enders, such a derivation is as exact as Kirchhoff's formula but has the advantage of easier generalization to other propagation phenomena. In respect of this approach, "Huygens' principle is understood as a *universal* principle governing not only the propagation of light," but further, such a "formulation leads to a *unified* formal representation of not only classical and Schrödinger (matter) waves as well as diffusive transport, but of virtually *all* propagation phenomena, which can be described through *explicit* differential and difference equations, respectively." (Enders 1996, p. 226)

[412] The equation was arrived at independently by both the British mathematician Sydney Chapman and the Russian mathematician, Andrey Kolmogorov.

In its more frequent mathematical usage, particularly in probability theory and the theory of Markovian stochastic (or random) processes, the Chapman–Kolmogorov equation is an identity relating the joint probability distribution of different sets of coordinates on a stochastic process. Further, such a random process has the "memoryless" or *Markov property* if the conditional probability distribution of future states of the process, given the present state and the past states, depends only on the present state.[413] For such a process, the past is irrelevant since it has no influence on how the current state was obtained. (Gardiner 2004, pp. 42-44) A corollary of the Markov property then asserts that no examination of an event in such a stochastic sequence can reveal information concerning any prior state. In general, any process having a property in which its prior history is either erased or irrelevant, even to the somewhat simplistic results of the sequential random 'walk' of particles exhibiting Brownian motion, is to be recognized as a Markov process, (Gardiner 2004, p. 5) Here, the Chapman–Kolmogorov equation is deemed equivalent to an identity on transition densities. In simple terms this means that the probability of going from a space-time state (r_0, t_0) to a later space-time state (r, t) can be found from the probabilities of going from (r_0, t_0) to an intermediate state such as (r_1, t_1) and then from (r_1, t_1) to (r, t), by adding up over all the possible intermediate states (r_1, t_1). Compliant to such an approach, and following Enders, the general field "propagator $G(r, t) \mid (r_0, t_0)$ is assumed to describe the propagation of all necessary information from the space-time point (r_0, t_0) to the space-time point (r, t)." Huygens construction can then be expressed in terms of the Chapman-Kolmogorov equation as a Markov process by:

$$G(r, t) \mid (r_0, t_0) \;=\; \int\!\int\!\int G(r, t) \mid (r_1, t_1)\; G(r_1, t_1) \mid (r_0, t_0)$$

where $t_0 < t_1 < t$.

Since the time interval ($t - t_0$) can be made infinitesimally small for a continuous electrodynamic process, the Chapman-Kolmogorov equation becomes a mathematical formulation of Huygens' principle both in the sense of action-by-proximity and of superposition of secondary wavelets, (Enders 1996, p. 229) Huygens' principle is then recognized as a specific memoryless Markov process wherein, by definition, the history of its motion is lost, instant by instant. It is in this respect that, for a wavefront model of light, propagation in the electrodynamic sense is incommensurate with inertial motion in the mechanical sense and therefore a traditional approach to theory construction that

[413] Defined by Gardiner as the *Markov assumption* which is formulated in terms of the conditional probabilities. It is here required that if the sequence of event times are ordered as $t_1 \geq t_2 \geq t_3 \ldots$ then the conditional probability is determined entirely by the knowledge of the most recent condition. (Gardiner 2004, p. 43)

would attempt to proceed on a presumed basis of a mechanistic compatibility is thwarted.

Historically, Richard Tolman may have been the first to conjecture the potential for different theoretical outcomes based on such differences in propagation characteristics. Early speculations were however, demoted to a footnote to his 1912 examination of emission theories. Nonetheless, they were, in effect, a caution that also reflected on the validity of his own prior experiments in 1910 when utilizing the approaching and receding limbs of the sun as a presumed test of a light-speed constancy. (Kantor 1976, p. 109) Tolman's footnote was to add a disclaimer:

> Optical theories in which the velocity of light is assumed to change during the path are not considered in this article. It might be very difficult to test theories in which the velocity of light is assumed to change on passing through narrow slits or near large masses in motion, or to suffer permanent change in velocity on passing through a lense. (Tolman 1912, p. 136 fn. 1)

Such "theories" prompting Tolman's footnote had already found partial voice in his 1910 publications wherein he had variously conjectured that "a mirror acts as a new source of light" and that if the surface of a grating acted in the same way "the reflected light would have the same velocity whichever of our hypotheses were true." He further had to "consider the effect of the lens (and the earth's atmosphere) through which the light has to pass" with the speculation that "an original difference between the velocities of light from the two sources would be obliterated by passage through a stationary medium," along with the attendant "possibility that an original difference in velocity would be destroyed when the light reached the neighborhood of the earth …" Nevertheless, interpretations of evidence from relative motion experiments such as by Fizeau were construed to suggest that the "presence of air or other transmitting medium *would not* completely destroy such a difference."[414] (Tolman 1910a, pp. 33, 35-36) Finally, Tolman's 1910 conclusion was provisional in that:

> … until further evidence is presented, we may best accept that principle regarding the velocity of light which has led to the second postulate of relativity. (Tolman 1910a, p. 36)

[414] Such a speculation would be in sympathy with a Fresnel "partial aether dragging" or aether convection hypothesis as deemed confirmed by the 1851 Fizeau moving water experiment. (Fizeau 1860)

The Extinction theorem as a concretization of Huygens' principle.

Coincident with Toman's speculations, from 1907 until 1912, Paul P. Ewald studied physics at Munich University's Institute for Theoretical Physics under Arnold Sommerfeld, who was already notable as a pioneer of the study of wave propagation through dispersive media. During his final years Ewald completed a successful doctoral dissertation entitled "*Dispersion and Double Refraction of Electron Lattices (Crystals)*" under Sommerfeld's direction. Ewald's thesis question, "Does anisotropic arrangement of resonators produce double refraction?" and addressed a problem in birefringence and dispersion theory. Ewald's investigation extended application of the propagation of a wavefront beyond Huygens' original considerations of birefringence in Iceland spar, to include electrodynamic dispersion in crystalline structures now known to be built from arrangements of atoms each acting as an electromagnetic dipole resonator. [415] For Ewald "… the objective [was] to determine if the anisotropic arrangement of ordinary (isotropic dipoles) at the nodal points of an orthorhombic lattice would account for the existence of double refraction." In his conclusion Ewald claimed that the "value of the computed birefringence [was] found to be comparable[416] to the observed value." (Ewald 1970a, p. iii) Nonetheless, however valuable his result would be to crystallography, it was to be his investigations of the dynamics of wavefronts both impinging on material boundaries and propagating within crystalline dielectrics that was to mark the greater significance of his study.

In the then traditional "theory of dispersion," as formulated by such as Planck and Pauli, the incident optical wave, having entered a dielectric, continued to play a role.[417] Ewald noted however, that in an idealized medium which extends to infinity in all directions, no such wave could be assumed. In Ewald's model the "refractivity is shown to arise as an internal property of the medium." (Ewald 1970a, p. iii) Ewald published his revised dispersion conceptions in a series of papers commencing in 1916 with its

[415] "… crystallographers had long since developed elaborate theories of crystal structure but … these remained purely geometrical speculations as long as no quantitative consequences had been drawn.' (Ewald 1970b, p. A1) In neutral molecules the negative electron charges compensate the positive nuclear charges. "However, the centres of the positive (nuclear) charges and that of the negative (electronic) charges may not coincide; this system has then an electric dipole and is called *polar* [else] If a nonpolar molecule is subjected to an electric field, the electrons and nuclei are displaced and a dipole moment is generated." (Born & Wolf 1999, p. 96)

[416] "The calculation of double refraction became much more realistic once the determination of crystal structures had become possible after Laue's and Bragg's work in 1912." (Ewald 1970b, p. A6)

[417] For Ewald there seemed: "… to be some confusion in [Planck's] calculation of the field created by the resonators (dipoles) in the medium and the "incident field" which seemed to exist in addition. (Ewald 1970b, p. A2) It is also to be noted that several later studies of refraction attempted a photon explanation including relativistic treatment of a photon account of Snell's law but restricted to nondispersive media. "It is [however] true that attempts to extend the treatment to dispersive media have not yet been successful." (Michels 1947)

first two parts. (Ewald 1916) In Part II, Ewald considered the case where a crystalline medium filled a ‘half-space’ forming a boundary with free space upon which an ‘external’ wavefront might impinge. Theoretical development was assisted by a mechanical analogy:

> … the splitting of the entire problem into these two parts [internal and external] corresponds quite closely to the division into two steps of the mechanical problem of oscillations generated by an impact on a mechanical system. There a preliminary study is made of the free oscillations of the system, and the knowledge so gained is subsequently applied to the behavior of the system after an impact has set it in motion. The free oscillations are those requiring no outside forces and they can exist only at certain frequencies … to the determination of the proper frequency (or frequencies) of the mechanical system there corresponds the determination of the [electromagnetic ray or] wave vector … (or in the case of X-rays of the several wave vectors…) for which alone the system of field and dipoles is self-consistent. This knowledge is used to construct the state of the system filling a half-space when an impact is given to it by an external wave impinging on its surface. The development of the mechanical system in time is paralleled by the spatial development in the optical system. (Ewald 1970b, pp. A13-A14)

In summarizing the theoretical implications for an impinging wavefront,[418] in later review in 1970, Ewald explained:

> It is shown that this incident optical wave is actually prevented from entering the crystal because of the modification produced in the field of the crystal by the introduction of a boundary. Boundary waves are found to exist on both sides of the boundary. The higher their order, the more rapidly they attenuate as a function of distance away from the boundary but the zero order waves are ordinary undamped plane waves of vacuum velocity c. The fields outside and inside the medium are connected by the Fresnel formulae: These follow from the condition that the optical field inside the medium and the oscillations generated there form a self-consistent system.” (Ewald 1970a, pp. iii-iv)

Oseen and extinction in isotropic media

Ewald’s papers dealt with the resolution of optical dispersion problems strictly in the context of A. H. Lorentz’s Theory of Electrons,[419] but differed from his treatment by

[418] It is usual to employ the Galilean idealization of the “Fraunhofer approximation” whereby spherical wave fronts, very far from a point source, can be usefully treated as plane waves. (Breinig 2008b)
[419] Born and Wolf note that it continues to be valid to “give a simplified model of a dispersing medium” using the atomic theory of matter as advanced by H. A. Lorentz. (Born & Wolf 1999, p. 95)

the fact that no averaging over random positions of dipoles became necessary. By 1915, Carl Oseen[420] had published a complementary but more general examination of dispersion in isotropic media but deriving directly from Maxwell's electrodynamics. As Ewald recalled:

> … Shortly after my work, and independent of it, Professor C. W. Oseen in Oslo found the same screening action of a boundary that is so essential in my treatment. His work is based on Maxwell's equations for a homogeneous medium. He assumes a light wave to fall on a finite body bounded by a surface *S*, on which the usual boundary conditions hold … The condition of self-consistency in the body can be fulfilled only if those parts of the field vanish which progress with velocity *c*. That is, the external field into which the body is placed, must cancel that part of the field which originates in the volume polarization and progresses with velocity *c*. The latter part can be expressed as a surface integral of the polarization over the boundary surface *S*. Thus it is by the action of the surface that the body is shielded from the impinging optical field. (Ewald 1970b, pp. A12-A13)

Although their pathways to a reformulated dispersion theory were distinct, their conclusions were consistent. Ewald's acknowledgement is generously found in his summary:

> The conclusion that the incident optical field cannot penetrate the crystal boundary, together with a similar conclusion in a paper by Oseen, is the basis of the Ewald-Oseen Extinction Theorem. (Ewald 1970a, p. iv)

More recently, equivalent extinction theory has been advanced within the framework of Maxwell's electromagnetic theory by Lalor (1969) and Sein,[421] (1970); non-relativistic quantum mechanics, (Pattanayak & Wolf 1976a, 1976b); the scalar wave theory, (Visser, Carney & Wolf 1998; Visser & Wolf 1999) and extended to include conductors as well as dielectrics. (Ballenegger & Weber 1999) In modern optics, the formal statement of the extinction theorem is recognized as a special case of a "rigorous formulation of Huygens' principle," for the incident field inside a medium (Fearn, James & Milonni 1996; Sein 1970) and the "theory provides an alternative mathematical approach to the treatment of some problems of electromagnetic theory [as in] the derivation of the laws of refraction and reflection and the Fresnel formulae." (Born & Wolf 1999, pp. 76, 110-115)

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[420] Commencing in 1921, Carl Wilhelm Oseen was made a member of the Royal Swedish Academy of Sciences, and a member of the Academy's Nobel Prize committee for physics. As a full professor of a Swedish university, Oseen was also entitled to nominate Nobel Prize winners and nominated Albert Einstein for the 1921 prize in recognition of his exposition of the photoelectric effect.

[421] Sein shows that, in the absence of matter, the extinction theorem reduces of the Helmholtz-Kirchhoff integral theorem for the incident field. (Sein 1970)

Extinction and light-speed determinations

Implications of the Extinction theorem and their warning of potentially inconclusive, or even invalid, empirical procedures and conclusions, went apparently ignored for decades. Such implications warned that rays of light, thought to originate from a remote source, such as Tomascheck's or Miller's use of starlight, Tolman's light rays from the limbs of the sun, or de Sitter's light rays from double stars,[422] had never actually entered experimental equipment for empirical assessment. In terms of the Extinction Theorem, the passage of light was to be seen more akin to a relay race than a sprint. Even temporarily disregarding the dire implications of the memorylessness of the refracted propagation process in itself, an extinction event had been shown to impose a profound optical amnesia - any dielectric boundary encountered that abruptly changed the electromagnetic properties of the immediate optical environment, such as the refractive index of a lens of glass, would necessarily cause an impinging ray to be extinguished. Equally extinguished would be velocity information that a subsequent examination was intended to reveal. The refracted ray actually entering into empirical examination was a later generation differing fundamentally from its ancestor: not only was its propagation rate unique to new local dielectric properties, examination of this later generation could give no clue as to a 'relative velocity' its progenitor might or might not have had prior to engagement with experimental detection.

According to Kantor, Ernest Cullwick, in a detailed and comparatively dissident review of electromagnetism and relativity theory, may have been the first to rekindle interest in the distinctive features of wave-like propagation. Although not directly citing the Ewald-Oseen theorem, he noted that conventional claims then made for proof of a light speed constancy, such as advanced by de Sitter, might prove vulnerable to reassessment:

> Relativists … claim that the experiments, together with de Sitter's argument, prove the truth of the [absolutivity] principle … surely a reliable test of the principle requires a completely free path, devoid of matter, between source and receiver. This condition is not fulfilled even in the travel of light from a star to the earth, so it is evident … that the principle of constant

[422] The de Sitter argument has also been dismissed as circular "in that it tacitly supposes an unambiguous knowledge of the stellar motions. The only direct knowledge of the stellar motion is provided by the light received from the stars … the [conjectured] appearance of ghosts and distorted orbits could not be recognized as such, unless the speed of propagation of the light were known independently beforehand …Related arguments based on light or other radiative signals received from pulsating stars are also similarly circular unproductive speculations" (Kantor 1976)

> light velocity *in vacuo* is *proved* neither by these experiments nor by observations of double stars. (Cullwick cited in Kantor 1976, p. 110)

Following on closely, in 1962, John G. Fox was to publish a more detailed, yet cautious, criticism of “a particular class of direct experimental evidence” purporting to prove the light-speed postulate. Fox directly cited the extinction theorem of Ewald and Oseen as the determining factor.

> … the extinction theorem of dispersion theory … shows that an incident light wave is extinguished at the surface of a dielectric. This may mean that information about the velocity of light from a moving source would be lost if the light passed through intervening transparent, stationary material before it was measured. All past laboratory measurements … from moving light sources … were made only after the light had passed through stationary material Thus de Sitter’s proof … may not be conclusive. It is concluded that there may not exist any sure experimental evidence for the second postulate of special relativity. (Fox 1962, p. 297)

After checking all the experiments involving moving light sources and mirrors that claimed proof of the light postulate, as listed in Pauli’s 1958 *Theory of Relativity*, Fox concluded that, in every case, light generated by the source “was in fact extinguished in one way or another before its velocity was measured.” Further, although information was yet meager, accumulated evidence suggested that close binary star systems were usually surrounded by a gaseous envelope probably “sufficient for the extinction phenomenon to occur in them,” making de Sitter’s argument at least inconclusive. (Fox 1962, p. 298) That so much historical material should prove either irrelevant or unreliable was, for Fox, “a surprising situation in which to find ourselves half a century after the inception of special relativity.” (p. 299)

Nevertheless, unlike Cullwick, Fox was a committed proponent of a now conventional Minkowski-Einstein approach to relativity, and presented his arguments, not to discredit prevailing doctrine, but to ‘weed-out’ those aspects of claims to proof that, by his own assessment, appeared vulnerable to an ‘extinction’ criticism. However, in what appears as a concerted effort towards damage control given the repudiation of so much historical material, his early doubt that there “may not exist any sure experimental evidence” finds a later partial repeal, and with revision he suggests but a “small gap in the experimental foundations of special relativity.” Classes of experiments were to be exonerated, such as those investigating time dilation and a relativistic Doppler effect. Loop-holes were sought that might restrict the corrosive effect of extinction, such as for hard X-rays and gamma (γ) rays:

> The extinction theorem, of course, does not apply to electromagnetic radiation of a frequency so high that atomic electrons cannot follow it. (Fox 1962, p. 299)

Exclusion of extinction for gamma rays also appears implicit in more recent claims by Brecher. (Schewe & Stein 2000) From the study of gamma radiation arriving from distant 'gamma ray bursters,' Brecher has claimed an independence of the speed of light from its source "to within one part in 10^{20}. However, the possibility of optical extinction or source data adulteration as for the de Sitter claim, is nowhere considered in discussion. On the other hand, for Fox, decisive experiments avoiding an extinction trap were conjectured as possible "in principle." However, so certain was the truth of received relativity formalism that:

> The odds now that a decisive experiment will yield the expected [absolutist] result have become so overwhelming that the experiment may seem hardly worth doing … other fairly direct experimental evidence … fully confirm[s] special relativity. Far more important, the general principle of Lorentz invariance has long since so proved its worth in physics that it is all but incredible that some future experiment … could come to any but the expected conclusion. (Fox 1962, p. 300)

Despite his vigorous defense of the validity of the second postulate, response to his primary contention was muted. By 1967, Fox felt impelled to castigate recent experiments that continued to ignore a potentially extinction-based prorogue:

> … certain recent experiments … ignore an important aspect of the propagation of light through matter … which is the extinction of the primary radiation and its replacement by secondary radiation … Because of this omission, the conclusions of these authors are of little or no value.
>
> The whole history of the matter of proving the constancy of *c* has involved an unusually large number of errors … It is to be hoped that time will not be wasted in future on additional experiments or arguments which are nullified by extinction. (Fox 1967, p. 967)

Yet again, little attention was paid to the serious implications of extinction as it was held to constrain claims to proofs of the light-speed postulate. Jackson, acknowledging both Fox and Ewald and Oseen, briefly noted the problem in that : "It seems clear that most of the early evidence for the second postulate is invalid because of the interaction of the radiation with the matter through which it passes before detection." (Jackson 1975, p. 512) On the other hand, detailed discussions of the evidence available from test theories such as the kinematical RMS and dynamical SME models, (Mattingly 2005) have routinely failed to screen for any form of extinction adulteration or invalidation. Extensive surveys of the claimed experimental basis of Special Relativity such as by Holton, (1962) Newman *et. al*. (1978) or Schleif (1998) have equally failed to mention the topic. It was not until a 2007 revision that the subject was first briefly and tentatively included by Roberts and Schleif (2007) as a potential constraint on some experimental procedures or as possibly invalidating some unspecified light-speed constancy claims. This limited assessment also included a brief but unconscionably spurious account of the extinction theorem of Ewald and Oseen, quite falsely claiming that:

> This theorem states that the speed of light will approach the speed c/n relative to the medium (n is its index of refraction), and it also determines how long a path length is required for that approach. The distance required depends strongly on the index of refraction of the medium and the wavelength: for visible light and in optical glass it is less than a micron, for air it is about a millimeter, and for the intergalactic medium it is a few parsecs. (Roberts & Schleif 2007)

It would be tempting to imagine that such a misleading report with its violation of Ewald and Oseen's clear intent, was inspired to blunt the impact of extinction or possibly limit exposure of some empirical procedures to further destructive scrutiny. In the event, Robert's otherwise inexplicable betrayal of scientific responsibility is more likely to be found as an uncritical and careless confounding of additional ideas originating from Fox's earlier papers that dealt with high energy physics.

Extinction and high energy radiation

From at least 1964 onwards, Fox interpreted the notion of "extinction" as additionally including attenuation, normally observable in forward propagation within a medium as the expected exponential decrease of incident amplitude due to scattering and absorption.[423] Fox introduced an attenuation coefficient of $N\lambda f(0)$, where N is the

[423] With an obvious potential for ambiguity, in many branches of physics, attenuation, as the exponential, or even gradual, loss of intensity of a flux through any medium, might also be termed 'extinction.' In astronomy "extinction" describes the absorption and scattering of distant light by intervening dust and gas. For common exhibitions of exponential decay, such as the Beer-Lambert law in chemical spectroscopy, the distance over which a quantity decreases by $1/e$ of its previous value is often termed an "extinction length" or "e-folding length," (Koenig et al. 2007) and in engineering, exponential attenuation is usually measured in decibels per unit length of a medium. In climate science, an "extinction length" defines the height of a column of atmosphere containing sufficient greenhouse gases to fully absorb a certain wavelength of energy. (Raney 2008) In crystallography "extinction length corresponds to the thickness of a crystal required for the whole incident beam to be scattered into the Bragg reflection beam

number of scatterers per unit volume, $f(0)$ is the real part of the forward scattered amplitude and λ is the wavelength of the incident waves. As a generic description, but one that Fox suggested relates adequately to the standard optical theorem defining optical intensity decay, it is claimed valid for almost every wave motion and frequency. In such a case, intensity is expected to decrease exponentially to $1/e$, (or ≈37%) of its initial value for each distance of $1 / 2N\lambda f(0)$, termed an "extinction length." Although Fox, contrary to Ewald, discredited the Extinction theorem as applicable to high energy radiations, by 1964, he felt it necessary to denounce two new gamma ray-based claims to empirical proof of the light-speed postulate. Fox noted that it "has not been realized in connection with these new experiments that extinction [due to scattering and absorption] exists also for γ rays." (Filippas & Fox 1964, p. B1071) For Fox, extinction, as intensity decay resulting from interaction with intervening matter, potentially compromised an expected detectable velocity difference between ballistic and photon models, and was believed here extensive enough to have subverted safe conclusions. In one experiment (Sadeh 1963) the 1mm target "was about 3 extinction lengths in thickness" and the 60cm flight path "was also about 3 extinction lengths in thickness," incurring around 95% decays of initial intensities and in consequence, the possible substantial adulteration of original velocity information. In another experimental arrangement, (Alväger, Nilsson & Kjellman 1963) 4.4MeV gamma rays, with a suggested extinction length of 1.7m in air, were measured after a flight path of 5m. In summary, Fox claimed: "It is clear that the extinction [by attenuation] in both of these experiments was severe." (Filippas & Fox 1964, p. B1072)

Regrettably, the paper of 1964 ultimately contributed to some confusion between total incident boundary cancellations as intended by Ewald and Oseen, and an exponential intensity decay due to absorption and scattering as also employed by Fox. Some, such as Haseltine, clearly misunderstood the different meanings intended by the term "extinction," remonstrating with Fox, that in terms of the Ewald-Oseen theorem, there "is not a 'decay distance' … over which the change [from incident to refracted waves] occurs. (Haseltine 1964, p. 173) Such a confusion might also be thought to at least potentially explain the erroneous "extinction lengths" definition of the Ewald-Oseen Theorem by Roberts and Schleif. Unfortunately, such a confusion of terminology was not the only insult to be endured by the Ewald-Oseen theorem. In assessing its history, Juretschke remarks:

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and then to be scattered back into the direction of the incident beam." ('Production and Properties of Radiations' 2004)

> Any careful discussion or review of the Ewald-Oseen extinction theorem must always be welcome, especially when … even after more than 80 years the literature on this topic has remained full of contradictions and misunderstandings. (Juretschke 1999)

Others concur, claiming that "the subject has not received the attention it deserves in textbooks" (Fearn, James & Milonni 1996) and agreeing with the opinion that "the extinction theorem remains 'one of the most poorly understood theorems of classical electrodynamics'." (Wolf cited in Fearn, James & Milonni 1996, p. 987) In addressing some of these issues, Fearn and her associates note that, contrary to Ewald's and Oseen's equitable inclusion of all the dipoles constituting a medium:

> There are substantial discrepancies in the literature as to the *microscopic* basis for both the cancellation ("extinction") of the incident wave and the origin of the reflected and transmitted waves [which are typically only] attributed to the dipoles at or near the surface. (Fearn, James & Milonni 1996, p. 986)

On the other hand, Jackson, in his well-regarded text, and in concert with many others, has recalled pre-Ewald concepts in which the incident wave penetrates the boundary, claiming that cancellation inside a medium is "caused by dipoles on the *boundary* of the medium" and consequently it is plausible that:

> the extinction of the incident wave and its replacement … occurs over a finite distance *X*" [that] must be of the order of the distance required for the vacuum wave and the medium wave to get significantly out of phase … (Jackson 1975, p. 513)

As a counter, and vindicating Ewald and Oseen, it has been noted that, using a rigorous application of Huygens' wavefront model, the problem must be solved "as a boundary condition on the field rather than as an expression of an effect of surface dipoles," and thus:

> Provided there are enough dipoles per unit volume to approximate a continuum,[424] the incident wave is extinguished *everywhere* inside the dielectric [and] *there is no physical justification for concluding that the atoms or molecules at the surface play any special role*

[424] Berman holds that in a lattice of point dipoles, where the lattice is not considered a continuum, "there are evanescent waves between lattice planes." In the Berman model, "the field near the outer layers varies anomalously before settling into a smooth pattern" and claims this as "an extinction length over which the external wave is converted to the internal wave propagating at reduced speed." Perturbations are held to settle into bulk behavior within 4 to 5 lattice planes. (Berman 2003)

> [and thus any conjectured] "extinction theorem distance *X*" is irrelevant to the extinction of the incident wave. (Fearn, James & Milonni 1996, pp. 991, 993 orig. emph.)

On the other hand a quite different contention has surfaced that attempts a loophole such as was early conjectured by Fox, exempting hard X-rays and gamma rays of high energy physics from the effects of boundary extinction. In effect, such an exemption would admit certain experimental results for such frequencies, as valid verdicts on light-speed constancy claims. In such a conjectured absence of boundary extinction constraints, interpretation of an extinction length is sought to provide a partial reprieve for light-speed assessments. For Fox, it suggested that an experimental arrangement, incurring minimal interactions, might allow determination of high energy ray speeds as sufficiently unambiguous to confirm the second postulate. Fox and his associates measured what was believed to be "the relative speed of 68-MeV γ rays from the decay in flight of neutral pions," claiming that:

> While extinction [by attenuation] is not negligible in our experiment, it is much less than [elsewhere] and small enough that a definite conclusion [verifying the light speed postulate] can be drawn from the results." (Filippas & Fox 1964, p. B1072)

More recently, in their flawed assessment of the extinction theorem, Roberts and Schleif have also demand that we: "Note [that] this theorem is based purely on classical electrodynamics, and for gamma rays detected as individual particles it does not apply." (Roberts & Schleif 2007) Once again it must be noted, that in an approach to theory construction that recognizes the ancillary role of a *multiple models idealization* of electromagnetic phenomena, a particle approach to electrodynamic radiation, irrespective of frequency or mode of generation, is a representational model sharing citizenship with other equally valid, but incompatible depictions. On this view, a phenomenon that might find value in a particle analogy for some of its exhibitions does not preclude the employ of different or even contradictory analogies as appropriate for others. As models, the photon and wavefront representations are not held to be mutually exclusive.

Ever since Wilhelm Röntgen made his Nobel winning discovery of an unidentified high frequency radiation he termed "X" rays in 1895, collimating and focusing the phenomena proved daunting. Conventional lenses, as used extensively in visible-light optics, were found inappropriate for focusing X-ray optics, since refraction effects are extremely small and absorption is strong in materials such as glass. Additionally, although the physics of X-ray refraction, based on a wavefront model, appears superficially similar to refraction at lower frequencies such as for visible light, some major differences must be addressed. The real part of the index of refraction, *n*, of high frequency radiation[425] such as X-rays,[426] and gamma rays,[427] although close to unity, is slightly negative in contrast to its counterpart for lower frequencies of the electromagnetic spectrum. In the region of visible light, the real part of refractive indices are typically positive and refracted rays resulting from non-normal boundary incidence and entering a region of higher refractive index, bend towards the normal. However, for high frequency cases, where $n < 1$, the phase velocity, as the inflation rate of a Huygens' wavefront, is greater than the "speed of light" in vacuum. (Fitzpatrick 2009) Here, the otherwise converging effect of convex lenses changes to divergence. (Harbich, Hentschel & Schors 2001) Over time, a variety of solutions of varying practical efficiency have been applied to the idiosyncrasies of high frequency optics, including symmetrically cut silicon crystals, Kirkpatrick-Baez mirror systems, (Schreier 2002) grazing incidence mirrors, (Sveda et al. 2009) Fresnel and Bragg-Fresnel zone plates, and capillary (Kumakhov lens) optics. (Snigirev et al. 1996)

However in 1996, application of the wavefront model to high energy physics suggested a simple yet inexpensive approach as a solution for such long-standing technical difficulties. Since the index of refraction for such radiation is smaller than unity, a focusing lens must have concave form. A simple way to realize this form was pioneered by researchers including members of the European Synchrotron Radiation Facility, in which fine holes were drilled in a line with the intervening material between the holes used as the refracting medium. (Snigirev et al. 1996) A range of low Z (low atomic weight) materials, both metals and non-metals, were subsequently researched including lithium, (Dufresne et al. 2001) beryllium, boron nitride, pyrographite, plexiglass, polycarbonate, polyoxymethylene, Vespel (a DuPont plastic), magnesium, and aluminium. Since the refraction provided by a single lens is minuscule, large numbers of cylindrical holes drilled into blocks to form compound arrays were required for focusing high energy beams in one or two dimensions. By 1998, development of

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425 In general, the refractive index, *n,* of a medium is an expression of the ratio of the propagation speed of a wave phenomenon in a reference medium as against its phase velocity, v_p in the medium. For light referenced to vacuum $n = c_0 / v_p$ and in constructive terms of a medium's dielectric properties, $n = \sqrt{\varepsilon_r \mu_r}$ where ε_r is the medium's relative permittivity and μ_r is its relative permeability. However, the complex refractive index for high frequency radiation is often given by: $n = (1 - \delta) - (i\,\beta)$ where $(1 - \delta)$ is the real part and δ = the refractive index decrement (typically of the order 10^{-5} to 10^{-7}) and β = the absorption index.

426 In modern usage, X-rays are usually distinguished as radiation emitted by extra-nuclear electrons, and as having frequencies in the range 30 petahertz (3×10^{16} Hz) to ~ 30 exahertz (3×10^{19} Hz), corresponding to wavelengths in the range 10 to 0.01 nanometers. Ray energies from ~ 0.12 keV to ~12keV (10 to 0.1 nm wavelength) are commonly designated as "soft," whereas those from ~12 to 120 keV are often termed "hard" and exhibit greater penetration of media.

427 γ (gamma) rays are usually distinguished as radiation emitted by the atomic nucleus and typically have frequencies > 10^{19} Hz with wavelengths < 10 picometers (10^{-11} m) and energies > 100keV.

compound refractive lenses (CRLs) could claim a focal length of 1 m[428] using 856 holes of 0.25 mm radius drilled in PS (plexiglass) and focusing hard 40 keV X-radiation to a focal spot of 0.25mm. Smaller numbers of lenses drilled as linear arrays in other non-metals and in low Z metals such as Beryllium (587 lenses), Aluminium (371 lenses), and Magnesium (561 lenses), were found to give equivalent results (Lengeler et al. 1998, p. 5856) In its more recent incarnation, the CRL has evolved into the Transfocator,[429] an economical, versatile, high gain, and readily tunable high frequency focusing apparatus. The focal length and the energy focused, from soft X-rays to hard X-rays and gamma rays, can be varied continuously throughout a large range. The transfocator has proven effective alone or in combination with other monochromators or focusing elements, and now fills an increasingly valuable role in modern high-energy synchrotron beamlines. (Snigirev et al. 2009; Vaughan et al. 2009)

Extinction and the experimental record

Insofar as the geometry of the highly successful application of the wavefront model to the refraction of high energy radiation, as demonstrated by the CRL, requires that the refracted beam commences from the boundary of the CRL refracting medium, such devices stand as vindication of Ewald's original claim that the extinction theorem is equally valid across the entire known electromagnetic spectrum. The same practical success equally refutes claims such as made by Fox or Roberts and Schleif, as to its inapplicability to high energy physics, (Admans 2010) It notably further requires that claims to proof of the light-speed postulate such as by Filippas and Fox (1964) are misconceived. Thus the practical implications of boundary refraction, or extinction, are shown to deny direct experimental access to conjectural but pertinent relative velocity information for all known electromagnetic frequencies. Consequently the phenomenon invalidates claims to proof forming a significant historical apologetic of the constancy of light-speed edict. However, the special theory's claims to authentication embraces a much wider and more eclectic range of demonstration. The invalidation of albeit an entire class of experimental evidence cannot alone confirm rightful employment of a

[428] The focal length F, of a CRL with N holes of equal radius R is: $F = R / (2 N \delta)$ By varying the number of holes and their radius, it is possible to fine-tune the focal length and also the collimation effect of a lens array.
[429] Transfocators based on Aluminium parabolic refractive lenses and consisting of 7 cylindrical cartridges containing 2, 4, 8, 16, 32, 64, and 128 Al parabolic lenses respectively, can give a focal distance of 90m for an energy range between 25 and 60 keV. (Snigirev et al. 2009) A recent in-vacuum transfocator consists of nine pneumatically actuated, water cooled, cylindrical cartridges containing 1, 2, 4, 8, 16, and 32 beryllium lenses, and 32 and 64 aluminium lenses. A combination of such cartridges "allows complete tunability between 25 and 125 keV at 94 m, and 25 to 75 keV at 42 m. At 94 m from the source, gains on the order of 5 x 10^4 are achieved with respect to the unfocussed beam, and the microfocused beam at 42 m has gain of over 10^5 ." (Vaughan et al. 2009)

predisposing wavefront model or unilaterally act to disconfirm a conventional particle-based reliance.

It was, in part, for the arbitration of such contentions, that the kinematical RMS test theory regime, postulating a generalized parameterization of the Lorentz transforms, was originally adopted. Within this framework, modern high-precision re-enactments of three second-order historical experiments, are claimed, when combined in the test theory protocol, sufficient to validate an orthodox reading of special relativity. Possible demonstrations of theory invalidation are sought as a local Lorentz invariance violation: anisotropy of the vacuum speed of light by a Michelson-Morley class of experiments; dependency of the speed of light on the velocity of the source or the observer by a Kennedy-Thorndike class of experiments; or a modified time dilation factor by an Ives-Stilwell class of experiments. In principle, alternative formulations, such as informed by a change of sponsoring model, may seek a verdict based on their degree of conformity to the range of generalized RMS test theory parameters: recognizable departures would compel a judgment of hypothesis disconfirmation with possible impeachment of the sponsoring model. Nonetheless, for highly similar predictions, where experimental results might fall within the error bars of a currently achievable technological uncertainty, arbitration might remain ambiguous awaiting improvements in experimental precision.

An RMS test theory assessment of a wavefront sponsored hypothesis

In respect of conventional RMS tests, it is of note that three different explorations of electrodynamics are superficially similar: the ring laser, Michelson-Morley, and Kennedy-Thorndike apparatus configurations, all share a common component list that includes:

- A suitable source of electromagnetic radiation.
- A beam splitter.
- A collection of mirrors, or their functional equivalent, that effectively reflect outgoing beams back to their geometrical origin for recombination.
- A detector of interference or phase-shifted phenomena.

In principle, differences that give rise to distinctive use and individual purpose lie in the geometrical layout of the same functional elements. For a ring laser operational mode, split beams are sent opposed around a polygonal pathway before recombination, from which an expected "Sagnac" signal, proportional to the enclosed area, is amplified. For both Michelson-Morley, and Kennedy-Thorndike arrangements, the split beams are constrained to orthogonal return pathways. For the former they are of equal length and unequal for the latter, but ideally in both cases enclose no area, effectively diminishing a "Sagnac" induced artifact. In an idealized inertial scenario, all three layouts are expected to exhibit "null" results as detectors of light-speed anisotropy. However, in the slightly non-inertial practical environment, the Sagnac device is expected to amplify non-linear and terrestrially generated perturbations to a usefully measurable level. On the other hand, the Kennedy-Thorndike apparatus, although equally subject to such terrestrially imposed non-inertial motions, is expected to demonstrate their practical anullment in accord with special relativistic edict.

In modern re-enactments, orthogonal beams generated by a variety of sources and housed by a variety of different physical constituents, can be construed as high precision clocks, the periods of which are determined by their pathway return time. In this respect, the Michelson-Morley experimental class can be considered as comparing the frequencies of two identical clocks, demonstrating both that their relative frequency is not affected by either interferometer rotation or else by very different atomic constitutions. (Mansouri & Sexl 1977c) On the other hand, for Kennedy and Thorndike:

> … [the] experiment was devised to test directly whether time satisfies the requirements of relativity. [The experimental result] depends on the fact that if a pencil of homogeneous light is split into two components which are made to interfere after traversing paths of different length, their relative phases will depend on the translational velocity [i.e. non-inertial behavior] of the optical system … [such that] a point on the Earth should give rise to an interference pattern which varies periodically as the velocity of the point changes in consequence of the rotation and revolution of the earth … unless the frequency of the light depends upon the [universal constancy of light] velocity in the way required by relativity [and wherein] the Lorentz-Einstein transformation equations are valid. (Kennedy & Thorndike 1932, pp. 400-401)

It is generally accepted that these experiments are also consistent with some aether-based and ballistic models as well as the prevailing photon model. However, a wavefront model is equally well accommodated. To the extent that an enclosed medium is in practice homogeneous and isotropic or that an induced vacuum is sufficiently representative of free space, such systems would demonstrate the equivalence of orthogonal radial expansions and the phase constancy of the spherical wavefront model. As such, their equally predicted "null" results provide no more than commentary on the isotropy of local vacuum or dielectrics, but can neither address nor confirm a conjectured independent light-speed constancy.

The Relativistic Doppler effect and the wavefront model

By conventional acclaim, the original 1938 Ives-Stillwell experiment was the first historical occasion to measure time dilation directly by quantifying the wavelength emitted by a moving radiator. Here: "By measuring the radiation emitted in the forward and backward directions simultaneously one can eliminate the first-order Doppler effect. This leaves us with the second-order Doppler effect caused by time dilation." (Mansouri & Sexl 1977c) However, Ives and Stilwell also noted that: "The present experiment, giving a positive instead of a null result may hence be claimed to give more decisive evidence for the Larmor-Lorentz theory[430] than is given by the experiments which have yielded null results." (Ives & Stilwell 1938, p. 226) They further noted that:

> The Michelson-Morley experiment and Kennedy-Thorndike experiments, yielding null results , could, of themselves, be equally well explained – and more simply – by assuming an ether entrained by the earth, or a ballistic character of light emission, instead of assuming two concealed conspiring compensations – contractions of dimensions and of clock rates. [However the] discussion of the consequences of this change in clock rate … consists practically of the entire theory of the optics of moving bodies as developed by Larmor and Lorentz. (Ives & Stilwell 1938, p. 226)

In 1941, Ives and Stilwell again re-affirmed their 1938 experimental results after significantly increasing the range of canal ray velocities and technically improving their apparatus, and again repeated their conclusion:

> The net result of this whole series of experiments is to establish conclusively that the frequency of light emitted by [high speed hydrogen] canal rays is altered by the factor $(1 - v^2 / c^2)^{1/2}$ [where v is the velocity of the source, and c is the vacuum velocity of light] This agrees with the prediction of Larmor and Lorentz, and, unless such a frequency change should be shown to be the result of other factors than the speed of the particles, constitutes a verification of their prediction. (Ives & Stilwell 1941, p. 374)

As an integral component of current test theory doctrine, the Ives-Stilwell experiment makes a specific claim to a positive and faithful quantification of the relativistic time dilation factor in accord with the now traditionally interpreted predictions of Lorentz invariance. Quantification is held to accurately conform to calculations of the time dilation factor as determined by Lorentz transformation formulae. Taking advantage of

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[430] Both Ives and Stilwell, as with many other physicists, award priority for time dilation to Joseph Larmor. However, both interpreted their experimental results as confirming the earlier Lorentz mechanics in preference to the now conventional Minkowski-Einstein interpretation.

improvements in technologies, modern re-enactments now form a broader class and a more exacting empirical enquiry, that claims ever advancing levels of precision. (Kaivola et al. 1985; Mandelberg & Witten 1962; Reinhardt et al. 2007)

As an experimental procedure, the Ives-Stilwell apparatus is intended to detect and measure a difference in a second order frequency such as to confirm predictions of special relativity as against predictions presumed for alternative explanations such as from a more classical Newton-Galilean interpretation. When attempting explanation of the claim to an expected difference, it might prove valuable to recall the original motivation of such as Larmor, Lorentz, Poincaré and Einstein when invoking the Lorentz inter-frame transformations. It was through use of this formulation that the behavior of light, as it exhibited in the measurement frame of reference, was to be reconciled with the behavior that appeared to obtain in a relatively moving frame of reference. (Rybczyk 2002) Specifically, the velocity measurement, believed to characterize light rays in vacuum in a relatively moving frame, appeared to disobey the kinematical directive of a Galilean velocity addition law in that it exhibited the same velocity as that of light from a local source. Thus, as to its mathematical derivation, the Lorentz transformation formulae follow by logical necessity from a claimed constancy of the vacuum velocity of light in all inertial frames. (Jackson 1975, p. 515) On the other hand, put differently yet consistently, the same formulation offers explanation for the failure to detect a relative velocity component of light rays as would be predicted by a Galilean consistent theory.

In respect of a Huygens-styled predisposing wavefront model, with its Markov propagation and when extended to include the notion of boundary extinction, it is the prudent disclaimer by Ives and Stilwell that now warrants further consideration. Fundamental to the validity of their conclusion, is the belief that it is the “speed of the particles” that has in fact been measured. For such an assessment, however, the source phenomenon must necessarily be intercepted and conditioned for measurement. In practice, an eclectic collection of optical lensing, collimating, reflecting and frequency discriminating elements interdict between the creation of a canal ray and its practical quantification. (Ives & Stilwell 1938, p. 220, Fig.6, "Plan of apparatus") In terms of the extinction theorem, the ray that enters into assessment is cast as a later generation refracted ray. Source created rays never enter such a measuring system. Although it is recognizably related to the source radiation by an unbroken chain of cause and effect, the measured ray is held to contain no vestige of the original source velocity information that is integral to correctly evaluating the ‘clock’ frequency change the experiment is intended to demonstrate. Any optical element, or even dielectric medium, as part of, and thus at rest with, the measuring apparatus, is deemed to fatally adulterate the data. Specifically, such adulteration exhibits as the loss of relative velocity information from the source frame. Such a loss then provides an equally consistent, but far simpler, explanation for an apparent Lorentz invariance of the speed of light in vacuum along with the concomitant occurrence of Lorentz transformation formulae. Thus, a wavefront sponsored hypothesis of the Ives-Stillwell Doppler effect would equally predict a second order variation of source frequency by a factor of $(1 - v^2 / c^2)^{1/2}$ but one due to practical loss of source velocity information during transmission, rather than the constraint of a relative light-velocity to a universal constancy, wrought by conspiring spacetime compensations.

A similarly constructive analysis might also be thought to offer a simplicity of exegesis as against more complex explanations such as for the 1851 moving water experiment conducted by Armand Hippolyte Fizeau,[431] that was to later give inspiration to a young Albert Einstein. (Shankland 1963, p. 48) In attempting redress of "the absence of any definite idea of the properties of luminiferous aether, and of its relations to ponderable matter," Fizeau conceived an experimental approach that might determine "with certainty … the effect of the motion of a body upon the velocity with which light traverses it." (Fizeau 1860, pp. 245, 246) Utilizing a pair of parallel and connected glass tubes, 1.487 m in length and closed at each extremity by a glass plate, water could be made to flow through the apparatus in either direction at controlled rates.[432] An optical interferometer acted to quantify the frequency shift detected in collected light while being conveyed in either direction through the moving water system. Unaware that velocity amnesia by refraction and boundary extinctions, introduced into his apparatus by every one of an extensive range of optical elements, might have defrauded him of necessary source velocity information, Fizeau presumed an interpretation of the remote velocity through detected frequency changes. Using facts known for the water velocity,[433] Fizeau was convinced to adopt "the [partial aether-dragging or aether

[431] Michelson and Morley repeated the Fizeau experiment at Case Institute, Cleveland, in 1886, with improved apparatus and a greatly increased precision of the "Fresnel dragging coefficient." (Shankland 1963, p. 48) Following a classical interpretation, they regarded the retarded velocity of light as resulting from the number of collisions with atoms met in transit of the moving medium.
In 1920, Zeeman experimented with moving cylinders of flint glass ($n = 1.63$) and also quartz ($n = 1.42$) both of which claimed accord with the Fresnel drag coefficient. (Hoekstra 1995, p. 232)

[432] "For the most part the observations were made with a velocity of 7.059 metres per second; in a certain number the velocity was 5.515 metres, and in the others 3.7 metres." The results were then normalized to the maximum velocity of 7.059 m/s. Systematic errors were effectively addressed since "the two rays having travelled over exactly the same path, but in opposite directions, any effect due to difference of pressure or temperature must necessarily be eliminated by compensation." (Fizeau 1860, pp. 247, 250)

[433] The refractive index of fresh water was known to be 1.333 giving the velocity of light in water as 0.75018*c*. (Relativity Calculator 2011)

convection] hypothesis of Fresnel[434] … [in which] only a portion of the aether is free, the rest being fixed to the molecules of the body and, alone, sharing its movements." (ibid., pp. 246, 258) A derived light velocity obtaining in the environment of the detector was believed that of light originating from the relatively moving source, and through miss-assignment, the proposed mathematical solution exhibited the fundamental elements of a Lorentzian-styled transformation formulation. (Relativity Calculator 2011) On the other hand, for Einstein, the same result was interpreted as consistent with a (FitzGerald-Lorentz) length contraction, (Larmor) time dilation and the special-relativistic velocity-addition law. (Einstein 1916, Pt. I, Ch. 13) Such interpretations, then, appeared to confirm his non-classical conceptions of time and space. The episode was proudly recalled by Shankland in interview with an aging Einstein:

> [Einstein] seemed really delighted when I mentioned to him how elegant I had found …his method of obtaining the Fresnel dragging coefficient from his composition of velocities law of special relativity. (Shankland 1963, p. 48)

It must then be recognized that the discretionary powers, conventionally ascribed to a test theory, rests on at least two now debatable expectations. Firstly: that differing theoretical formulations should predict tangible and ultimately discernable empirical differences. For arbitration between alternative conceptions however, where the distinction is one of an interpretation of causality for essentially the same examinable phenomenology, predicted empirical results must exactly match those of the fiducial theory, and adjudication must fall outside of an empirical test theory's domain of competence. Secondly, it is now seriously in question that such differences, even if physically demonstrable, can be factually addressed by the experimental techniques as now employed. Every apparatus so far designed for implementation of test theory protocols and requiring the empirical measurement of electrodynamic attributes of radiation, inherently includes optical capturing and conditioning of source phenomena. According to the implications of refraction and extinction, this necessarily incurs the loss of source velocity information, consequently invalidating conventionally based test theory conclusions.

Nevertheless, insofar as two theories, advancing two incommensurate but otherwise self-consistent explanations of the same phenomena, cannot both be valid, and where

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[434] Fresnel's convection equation was: $u = c/n \pm v\,(1 - 1/n^2)$ where $(1 - 1/n^2)$ is the velocity "drag coefficient," u is speed of the light observed at the detector, c the speed of light in vacuum, n the refractive index for the transmitting medium, and v the velocity of the transmitting medium. (Relativity Calculator 2011)

adjudication by standardized empirical inquest must prove inconclusive, choice of a preferred explanatory rendition might yet seek an albeit provisional resolution in what epistemologist Jürgen Habermas, in his theory of rational discourse, conceived as the "force of the better argument." (Hesse 1995)

Information flow and relativistic effects

Our ancestors might well have imagined that the river gods defended their domain from privations by spear fishermen, by issuing false information as to the location of fish beneath the water surface. A direct line of sight, found by experience to give valid information as to the location of quarry on land, proved to be false when applied to the apparent direction of quarry beneath an interface with water.

Eventually, armed with a working understanding of refraction and its underlying processes, physicists such as Galileo and Huygens exploited the falsification of source direction information apparent after the passage of light through water or glass. Each harnessed the effect to manufacture lenses and so to turn a seeming disadvantage and natural infelicity into the useful process of magnification. In the hands of modern technology, the effective distortion of original information due to the refractive propagation of light has been turned to the greater good through a vast array of optical applications ranging from reading glasses to those of microscopes and space-borne telescopes. On this view, use of a lens can equally be interpreted as an intentional harvest of false or distorted source information. From the perspective of information flow, the "Lens Maker's formula," deriving from Snell's law of refraction and a knowledge of refractive indices, can be interpreted as a quantification of the extent of information distortion expected to be encountered in any particular optical arrangement. On this view, the intrinsic misrepresentation of source direction and location by the refractive redirection of optical paths, finds tacit approval under the disguise of 'magnification.'

Beyond the zero order of relative motion, the bunching and spreading of wavefronts inherent in a first order of optical relative motion, conspire to deliver patently false information of source frequency to an observer. Nonetheless, with an understanding of the processes involved, such "Doppler shifted" frequencies are routinely exploited to quantify the relative motion of the source and are pressed into service to determine such vastly disparate values as the recession rate of a distant galaxy and infringement of the traffic code by a speeding motorist. Analogous to the Lens Maker's formulae, the "Doppler formula" is engaged to quantify an inherent degree of source data deception.

Once again, false information is incurred through distortion in the information flow, when transmitted via a wavefront propagation. Once again, the insult to source data integrity is masked in practice by reinterpreting it in the guise of a useful result.

Consistent with such a view, relativistic effects might then be interpreted as distortions of information encountered in the second order of relative motion. Such distortions are deemed in accord with an understanding of the loss of relative source velocity information inherent in refractive propagation. Again analogous to the Lens Maker's formula and the Doppler formula, the Lorentz transformations might then be seen to quantify the degree of second order information distortion expected to be encountered when attempting electrodynamic quantification of spatial and temporal dimensions of phenomena in relative motion. On this view, a length dimension would not physically contract due to relative motion. Information about the dimension would be seen as distorted due to information losses incurred by refraction and boundary extinctions integral to the measurement process, to then falsely appear in quantification to be diminished in the direction of travel. Equally, an interval of time applicable to a system in relative motion, would also falsely appear in measurement as having taken longer to transpire or as being dilated. On such a view, the so-called 'proper length' of a physical object would be retained as its only physical length pertinent to an exact science. "Relativistic" variations could then be assessed as aberrations resulting from distortion in the information flow and with its Lorentz transformation value indicting the degree of information distortion incurred. Equivalently, an absolute time, as determined for time as measured in the local laboratory reference frame, would be the only definition of time necessary for the pursuit of an exact science. Also consistent with this view, any occurrence of Lorentz-like transforms in the mathematical interpretation of empirical data, might be taken as an indication of information perversion in the cause of data acquisition. Relativistic spatial and temporal measurements made of such systems in relative motion might then be understood as distortions of source data requiring correction.

However, perhaps the greater consequences of such a reinterpretation, are implications relevant to a possible relativity theory reconstruction. Of primary interest is the salient distinction between two models, each potentially employable as supporting relativity theory development. One, the now conventional mechanistic interpretation of free-space radiation as advanced by both Newton and Einstein. The other, the continuum and process-oriented wavefront as advanced by Huygens and concretized by Fresnel and Kirchhoff. Whereas the more atomist interpretation suggests both separability and simple location as entailed properties of the underlying model, a wavefront depiction suggests the possibility of theoretical development from a model both non-separable and non-local. It is then by such a distinction that the wavefront now presents as a model from whose insights a theoretical redevelopment might be achieved that removes the basis for a now patent foundational theory incompatibility.

On the other hand, other consequences of possibly equal importance also emerge. For a wavefront model, the speed of light in vacuum is found to be more appropriately defined as the scalar ERIC. Such a model depicts the "speed of light in vacuum" as a dependent local constant and incapable of being parlayed into a universal limiting velocity for the motion of massive particles. Equally it is not seen as necessitating a four-dimensional spacetime reconfiguration of the classical Galilean dimensional scheme. Deriving from such a modeling of light, it also becomes apparent that the electromagnetic radiation complex, viewed in vacuum as an expanding wavefront configuration with its geometrical centre remaining coincident with its source, is itself conformable to the classical velocity addition law. Consistent with such a model, it then becomes equally apparent that the relativity principle might be re-instated in its classical Galilean form and understood as a comprehensive claim incorporating both mechanics and a wavefront-based electrodynamics, without the need for further qualification.

However, beyond an immediate effect on interpretation of relative motion and Lorentz invariance, adoption of a wavefront model as the preferred picture underlying electrodynamic processes together with a Poincaré-Whitehead-like philosophy of science, must be admitted to support implications beyond those of an elementary conceptual renewal. Such an adoption would also appear to incur Bell's additional conjecture of being 'radical.' (Bell 1984, p. 172) In this respect, Einstein might appear to have been visionary when expressing his later doubts:

> I consider it quite possible that physics cannot be based on the field concept, i.e., on continuous structures. In that case, nothing remains of my entire castle in the air, gravitation theory included, [and of] the rest of modern physics. (Einstein to Besso in 1954, Pais 1982, p. 467)

Giving substance to Einstein's projection would appear to be his recognition of the acute dependence his particular theoretical development had on a mechanically compatible electrodynamic model. In what may now also appear as a pertinent insight bearing directly on the modern dilemma in theoretical physics, it was also apparent to him that adopting a continuum modeling of electrodynamics could support a radically different theoretical outcome, and one that might implicate the whole of modern physics.

Conclusion

An investigative reading of the history of science is brought to bear on the development of optical theories and particularly on the emergence of early relativity theories. When supported by a philosophy of science as embraced by such as Poincaré and Whitehead, such an investigation can be shown to provide an analytical approach to modern theoretical problems, and a method of interpretation that proves both enlightening and clarifying. In accord with such a philosophical orientation, the view of science in general and physics in particular, as modeling ventures, reveals that two divergent analogies have dominated optical modeling each with the capacity to have significantly influenced optical and relativity theory construction. The one, a representation of optical phenomena that holds valuable insights as deriving from the equal inclusion of relational continua and dynamic processes, found support by such as Huygens, Fresnel, Kirchhoff and Maxwell. The other, limited in scope to localized and separable depictions of light and its radiation, is found to have exemplified the approach adopted by such as Newton and Einstein, and accords with a now widespread prejudice in favour of a more mechanically based philosophy of science. Historically, eras of their respective dominance display as grossly unequal, with those periods when allegiance to a more atomist climate of opinion prevailed, extending to the scale of centuries. Such disparity is held as consistent with an interpretation advanced by such as Whitehead and Rescher, that identifies as both pervasive and pernicious, a bias towards an exclusive objectivity – a distorting preference for the overtly tangible and simply locatable - that is found capable of leading to the fallacious reification of abstractions and depreciation of wider fields of relations. Such a bias is equally shown to support the depreciation of modeling styles that depict phenomena in terms of non-separable and ephemeral processes.

Such an inquiry then makes apparent that the notions of separability and locality, now demonstrable as at least partly seeding current foundational theory incompatibility, entered early theoretical development of special relativity as entails of a neo-mechanistic modeling of electrodynamics. Such a depiction of optical phenomena was apparently embraced to provide a desired realism for the newly discovered energy quantum and thence to facilitate entry of electrodynamics into compatible description with mechanics.

It is then suggested that, in accord with the view that construes theory construction as developed from its predisposing models, failure of an otherwise self-consistent theoretical development to find congruity with other established theories or the greater body of established science, suggests an inadequacy or miss-application on the part of its facilitating modeling. To the extent that a *multiple models idealization,* as advanced by Weisberg, is conceded as underlying the historical development of electrodynamics, re-employment of the previously diminished Huygens-styled wavefront optical model, is then thought to potentially offer a more fruitful approach. If employed as the predisposing model for a reiteration of Lorentz invariance theory, such a model suggests the possibility of theory reconstruction without entailing the notions of separability and locality, thus avoiding both a known progenitor of foundational theory incompatibility and an apparent discrepancy with experimental predictions.

Although concretized by Fresnel and Kirchhoff to more usefully represent the dynamics of the electromagnetic field as later advanced by Maxwell, further extension of the Huygens' model to include the extinction theorem of Ewald and Oseen, is then shown necessary to extend its domain of applicability to include optical dispersion within media. However, benefits ensuing from appointment of a wavefront depiction of optical processes are found to come at a price. Such a model is held as also requiring the redefining of some common optical properties and parameters. Further, such a wavefront propagation is shown to be a Markov, or memoryless process, incommensurable with inertial particle dynamics. Adopting such a process could then sponsor a radical reinterpretation of the second postulate of special relativity such that a systematic loss of source velocity information could provide a constructive account of the apparent constancy of light-speed in all relatively moving inertial reference frames. By such an account, relativistic phenomena could be viewed as an inconvenient but systematic artifact of information distortion, occasioned by both memoryless propagation and boundary extinction events, to which contemporary attempts at physical measurement of wave-like phenomena in relative motion are inherently vulnerable.

Recommendation:

A "relativistic" Doppler effect has been theoretically conjectured as pertaining to both optics and acoustics according to a recent discussion of the effect by Huang and Lu. (2004)

> There exists the … relativistic Doppler effect for acoustic waves as well as for light waves …The effect depends only on the velocity of the source relative to the observer, irrespective of the velocities of the source and the observer relative to the body of medium. (Huang & Lu 2004, p. 963)

It is evident that acoustic propagation, as a Huygens-styled propagation, is to be recognized as an exhibition of energy transmission by wavefront spreading to which a Markov memoryless character is equally to be attributed. In this respect, it would appear reasonable to expect that, in an experimental arrangement functionally analogous to that employed by Fizeau for light passing through moving water, a similar loss of acoustic source velocity information should also become apparent. The recent development of the "saser" or sound 'laser' may further facilitate such analogous experimentation. (Johnston 2010) Successful experimental detection of an acoustic second order Doppler effect, and a concomitant mathematical occurrence of a Lorentz-styled formulation required for interpretation of collected data, could present a compelling case for explanation of optical "relativistic" phenomena as deriving from attributes common to such wave-like propagations *as a class*. Demonstration of such a result would support an explanation through information loss as preferable to seeking a unique explanation for optical phenomena by a not unproblematic special spacetime construct.

Endnote i
Priority and the Einstein papers on "Brownian Motion."

Early in the twentieth century, the prominent Australian physicist William Sutherland (1859-1911) was interested in explaining the diffusion by random motion of small particles suspended in liquids – a motion earlier noted by Scottish botanist Robert Brown in 1827 and thereafter denoted "Brownian Motion." Correspondence between Einstein and his engineer friend Michele Besso in 1903, reveals a keen interest in Sutherland's work. (Rothman 2006; Stachel 1989c, p. 213) In 1904, applying a dynamical approach, Sutherland derived an equation for the diffusion of a solute into a solvent, based on fundamental atomic and molecular models relating diffusion to viscosity, and reported in a paper given at the 1904 Dunedin ANZAAS conference and then published in the *Philosophical Magazine* in 1905[435]. Soon afterwards, also in 1905, Einstein published exactly "the same equation, having arrived at it by exactly the same line of reasoning," (Home, McKellar & Jamieson 2004)[436] but without acknowledgement of his extensive debt to Sutherland's prior work. For his part, Einstein claimed in 1906 that it was as a result of his 1905 paper that his attention was first drawn to the fact that experimental identification of Brownian motion had been published as early as 1888.[437] Holton also cites Einstein as writing later that his work of 1905 was undertaken "without knowing that observations concerning Brownian motion were already long familiar," (Einstein, Autobiographical Notes, cited in Holton 1960, pp. 629-630) – a claim which may be thought to sit awkwardly with his prior and extensive interest in Sutherland's work on the subject and further that a discussion on French physicist M. Gouy's 1888 reports on Brownian motion is contained in Poincaré's *Science et hypothèse*, (p. 179) which, as noted by Solovine, Einstein and his 'Academy' friends had read with relish. (Stachel 1989c, p. 211) Nonetheless although a general consensus allows the Einstein work as independent, Abraham Pais, in his classic biography of Einstein, suggests that in the interests of justice, the result should more correctly be known as the "Sutherland-Einstein" equation. (Pais 1982, p. 92)[438]

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[435] Listed as: Sutherland, W. "A dynamical theory of diffusion for non-electrolytes and the molecular mass of albumin." *Philosophical Magazine*, vol. S.6, No. 9 (1905), pp.781-785. ('Sutherland, William' 2007) The subsequent Einstein document "On the Movement of Small Particles Suspended in Stationary Liquids Required by the Molecular-Kinetic Theory of Heat," *Annalen der Physik*, vol. 17, 1905, is reproduced in "The Collected Papers of Albert Einstein, " vol 2, The Swiss Years: Writings, 1900-1909, trans. A. Beck, P. Havas, J. Stachel (ed), as Doc. 16.

[436] In his 1900 doctoral thesis, "The Theory of Speculation", which deals with random fluctuations of the stock market, French economist, Louis Bachelier advanced the same equation, anticipating the physicists. Bachelier also anticipated the "Black-Scholes" approach to options trading and earned the title "father of mathematical finance" (Rothman 2006) It is also of note that in 1906, and independently of Einstein, Polish scientist Marian von Smoluchowski, also published a theoretical description of Brownian motion: "*Zur kinetischen Theorie der Brownschen Molekularbewegung und der Suspensionen*," (*Annalen der Physik*, 1906, vol. 326, Issue 14, pp 756-780.)

[437] After his attention had been drawn to prior works, Einstein's second paper on Brownian motion, in 1906, cited the observations reported by M. Gouy in 1888 and as discussed by Poincaré, as being experimental confirmation of his own conclusions. (Stachel 1989c, p. 211)

[438] At a time when atoms were still thought to be a convenient fiction, Sutherland assumed their reality and that they exerted forces on each other. As some compensation, modern physics texts now refer to the "Sutherland model" and characterize inter-molecular forces as the "Sutherland potential" (Home, McKellar & Jamieson 2004)

Endnote ii
Questioning the Authorship of the Einstein papers.

In 1986, an already burgeoning Einstein archive of some 45,000 documents, was to receive an additional 54 letters exchanged between a young Albert Einstein and Mileva Marić, (1875- 1948) the only female student to be admitted to the physics and mathematics teacher training course in which Einstein was also enrolled,[439] and with whom he had a romantic affair resulting in the birth of a daughter and eventual marriage. The letters, which contained information previously unknown to Einstein scholars, were made available by Einstein's first son, Hans Albert. (Holton 1994a, p. 24)

Amongst early and endearing correspondence between the couple, Albert was to write to Mileva: "I'll be so happy and proud when we together shall have brought our work on relative motion victoriously to a conclusion." (Einstein to Maric, 27 March, 1901, cited in Holton 1994a, p. 29) A direct reading raised for some the possibility that Mileva was in collaboration with Albert on subject matter[440] which would eventually surface as the relativity paper of 1905. The possibility of co-authorship became the incentive both for speculative academic re-evaluations and various media events including television and film documentaries.[441] Evan H. Walker, a research physicist at the United States Army Ballistic Research Laboratory in Maryland, championed the collaborative conjecture noting that there are "more than 10 other instances in the approximately 40 letters from Einstein to Miss Maric before 1903, referring to 'our work' or to their collaboration." (Walker 1991; Walker & Stachel 1989) Walker also noted that Einstein, in accordance with a prior agreement, paid Marić the proceeds from his 1921 Nobel Prize, even although by then they were long since divorced and Einstein had remarried. (New York Times 1990) However, in other passages that must appear to sit uneasily with a co-authoring scenario, at least at this time, Einstein also wrote to his fiancé later in 1901:

> I am busily at work on an electrodynamics of moving bodies, which promises to be a capital piece of work. I wrote to you that I doubted the correctness of the idea about relative motion. But my reservations were based on a simple calculational error. Now I believe in it more than ever. (Einstein to Marić, 17 Dec. 1901, cited in Holton 1994b)

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[439] "During her high school years, she sought and received special permission to attend an all-male *Gymnasium* to take two years of physics – a course that otherwise was not generally available to girls. It is reported that she got top grades in physics and mathematics ... she did a summer semester in medical studies at the University of Zurich in 1896. That fall she enrolled in the city's Swiss Federal Polytechnic (Eidengenössische Technische Hochschule) [or ETH] ... in section VI A, which trained teachers of physics and mathematics ... Once again, she was the only female student in her class" (Holton 1994a, p. 23)

[440] It is, as Martinez points out, more likely to have been work on a version of an earlier aether hypothesis, and one which had led to Einstein speculating on an experiment to detect the aether. (Martínez 2004a) Such a view is consistent with his letter to Mileva in which he tells her: "In Aarau, I had a good idea for investigating what effect a body's relative motion with respect to the luminiferous ether has on the propagation velocity of light in transparent bodies." (Einstein to Marić, 10 Sept. 1899, cited in Holton 1994a, p. 26)

[441] "An Australian company, Melsa Films, created a documentary. Its producers interviewed various historians of Einstein's life, including Gerald Holton, Robert Schulmann and John Stachel. They also talked to proponents of Marić." (Martínez 2004a)

Then, only two days later, he was to tell her: "I explained to [Zurich University professor Alfred] Kleiner my ideas on the electrodynamics of moving bodies ... He advised me to publish my ideas." (Einstein to Marić, 19 Dec. 1901, cited in Holton 1994b) Undoubtedly, although Mileva would appear to have been aware of early theoretical musings and direction, and may have even offered various types of support, in view of such correspondence, it would also appear too difficult to suggest that level of participation upon which a co-authorship claim could be mounted, at least in the period prior to their marriage in 1903.[442] Nine days later, however, Einstein intimated his desire for future collaboration: "When you become my dear little wife, we'll work diligently on science together so we don't become old philistines, right?" (Einstein to Marić, 28 Dec. 1901, cited in Holton 1994b, p. 39) It was this specific collaborative effort that traditional Einstein scholars believe ultimately faltered. For Stachel, intense investigation "leads to the conclusion that [Mileva Marić] played a small but significant supporting role in his early work, but a role that later diminished to the point that she felt excluded from his career." (Stachel 1966, p. 207) Much however might be argued to depend on the timing designated by "early" and "later." During theory development in 1904, Einstein is reported as saying: "I need my wife, she solves all the mathematical problems for me." (Kruglinski 2008) Later, in 1905, the so-called *annus mirabilis* that produced the most seminal of the Einstein papers, Mileva is reported to have proudly told a Serbian friend, "we finished some important work that will make my husband world famous." (Oregon Public Broadcasting 2003a) However by 1909, Mileva confided that her part in the Einstein ascendancy had been compromised: "...with such fame, not much time remains for his wife ... but what can one do, the pearls are given to one, to the other the case ..." (Marić to Helene Savić, c. October, 1909, cited in Stachel 1966, p. 211) In terms of a possible collaboration, the meaning remains uncertain but taken with other correspondence of the time, has added fuel to extensive and sometimes bitter debate. On the one hand, undeniable proof exists that Einstein eventually treated his first wife disgracefully, raising suspicions that he may have deprecated Mileva's career and deliberately failed to acknowledge her contributions.[443] On the other hand, it is also acknowledged that, for her part, Mileva neither publicly claimed to have been Albert's collaborator nor sought any public credit.

Beyond the personal correspondence, however, an additional claim was to emerge. Support for a collaborative authorship scenario, at least for the 'relativity' paper, came from Senta Troemel-Ploetz, a research linguist at the German Research Society in Bonn. It was recalled that Russian physicist Abram Fedorovich Joffe (Ioffe) had alleged in his book, "*In Remembrance of Albert Einstein*," published some thirty years previously, that he had seen the original manuscript as submitted to *Annalen der Physik* in 1905,

[442] A daughter "Lieserl" had been born in January 1902. Albert Einstein and Mileva Marić married at Bern City Hall, 6 January, 1903. Mileva had turned 28 and Albert was 24. After marriage, and according to a Swiss custom, Mileva assumed the surname Einstein-Marić or its Hungarian variant Einstein-Marity. The couple subsequently had two sons – Hans Albert and Eduard.

[443] Writing in 1913 to his cousin Elsa Löwenthal, whom he married in 1919 after his divorce from Mileva, Albert says of Mileva: "I treat my wife as an employee whom I cannot fire." And eventually issued a long list of rules she must obey if she was to stay in his house including: "you must answer me at once when I speak to you," and to "renounce all personal relations with me, as far as maintaining them is not absolutely required for social reasons." In 1925, when Mileva thought of publishing her memoirs, Albert wrote to her: "One should ... keep one's mouth shut, that is my advice to you." (Kruglinski 2008; Oregon Public Broadcasting 2003a)

and that it carried the name *Einstein-Marity* as author.[444] Joffe was a respected member of the Soviet Academy of Sciences and worked as assistant to the 1901 Nobel laureate and then *Annalen* editor W. C. Röentgen in the crucial period between 1902 and 1906 in Munich. The claim was to be lashed by Stachel as a "fabrication" and unfortunately Joffe died in 1960 before his story became news-worthy. (New York Times 1990; Oregon Public Broadcasting 2003b) So tantalizing but unresolved was the evidence, yet important for the history of science, that Walker presented his version of events to the American Association for the Advancement of Science convening in February 1990. A possible Einstein-Marić collaboration, particularly in respect of the 1905 papers, was debated, but in the event the evidence was considered equivocal and no consensus was reached. (Oregon Public Broadcasting 2003b)

[444] Both Martínez and Bjerknes independently translate the pertinent Joffe text as: "The author of these [1905] articles - an unknown person at the time, was a bureaucrat at the Patent Office in Bern, Einstein-Marity. (Marity – the maiden name of his wife, which by Swiss custom is added to the husband's family name)." (Martínez 2004a) Bjerknes notes that "Swiss law permits the male, the female, or both, to use a double last name, [or Allianzname] but this must be declared before marriage, and it was Mileva, not Albert, who opted for the last name "Einstein-Marity." (Bjerknes 2002) Albert Einstein signed his marriage record as "Einstein" and never signed or in any other way used the name "Einstein-Marity." If this name ever appeared on the original document it had to refer to his wife.

Endnote iii
Are the concomitants of special relativity real or merely apparent effects?

For Born, reinterpretation of the world in the light of the radical Minkowski-Eintein review intends that: "A material rod is physically not a spatial thing but a space-time configuration." (Born 1965, p. 253) However, the implications of such a claim, both as a premise for physics and a philosophy of science, have not met with a consistent and unambiguous interpretation, even by the special theory's most ardent and acclaimed apologists. Forthright claims made in the name of an exact science, and claims heralded as verified by an equally exacting empiricism, find no uncontested unity of explanation by its own self-proclaimed orthodoxy even as to an intended meaning in terms of a physical reality. Inconsistencies and ambiguities evident in authoritative explanations continue to nurture confusion in the minds of a hapless public and provide grounds for perennial contentions, including such as the "pole and barn" paradox (Wolfe & Hatsidimitris 2006c) and the "twins" paradox. (Wolfe & Hatsidimitris 2006d) Such disunity of interpretation has, in part justly, enabled a now extensive dissident literature to pronounce implications of a space-time proposition as wanting and culpable on a raft of charges.

Is the FitzGerald-Lorentz contraction a real physical effect?

In his 1966 *Relativistic Kinematics*, Arzelies addressed the issue by commenting:

> Several authors have stated that the Lorentz contraction only seems to occur, and is not real. This idea is false. So far as relativistic theory is concerned, this contraction is just as real as any other phenomenon. Admittedly ... it is not absolute, but depends upon the system employed for the measurement; it seems that we might call it an apparent contraction which varies with the system. This is merely playing with the words, however. We must not confuse the reality of a phenomenon with the independence of this phenomenon of a change of system. ... The difficulty arises because we have become accustomed to the geometrical concept of a rigid body with a definite shape, whatever the measuring system. This idea must be abandoned. ... We must use the term "real" for every phenomenon which can be measured ... The Lorentz Contraction is an Objective Phenomenon. ...
> We often encounter the following remark: The length of a ruler depends upon its motion with respect to the observer. ... From this, it is concluded once again that the contraction is only apparent, a subjective phenomenon. ... such remarks ought to be forbidden. (Arzelies, 1966, cited in Walton 2011, p. 2)

Max Born, as a contemporary and colleague of Einstein, appeared to share a similarly objective interpretation when claiming: "Einstein's theory … rises above the standpoint of a mere convention and asserts definite properties of real bodies." (Born 1965, p. 252) However, Born's account might be deemed ambiguous or even inconsistent when considering other comments and assertions: "we are dealing with purely conceptual relationships … (Born 1965, p. 246): "… the length of the rod in the [relatively moving] system *S* **appears** shortened in the ratio $\sqrt{1-\beta^2} : 1$ " (Born 1965, p. 248): "… a time interval in the [moving] system *S'* **appears** lengthened if measured in *S*." (Born 1965, p. 249): "… a rod in Einstein's theory has various lengths according to the point of view of the observer. One of these lengths, the statical or proper length, is the greatest, but this does not make it more real than the others."(Born 1965, p. 255): "Thus the contraction is only a consequence of our way of regarding things and is not a change of a physical reality." (Born 1965, p. 254)

On the other hand, Eddington, whose enthusiastic promotion of claims to experimental verification of the Hilbert-Einstein theory of General Relativity in 1919 facilitated Albert Einstein's rise to fame, and who considered himself one of a handful of scientists to actually understand the new science, explained the reality of relativistic effects in his 1924, *The Mathematical Theory of Relativity*, such that the "connection between lengths and intervals are problems of pure mathematics." In his 1928 publication, *The nature of the physical world* Eddington claimed that:

> The shortening of a moving rod is *true*, but not *really true*. (Eddington, 1928, p. 33, cited in Walton 2011, p. 2)

Karl Popper, in addressing the issue of relativism and reality, noted that: "Einstein suggested that the contraction is a question of 'perspective' rather than a physical contraction in the moving matter, and that it is mutual." (Popper 1966, p. 332)
Many have encountered intellectual difficulty in attempting to assign the value, in the context of an exact science, to be correctly accorded to differing measurements of the same object based solely on different observer's "points of view." Rindler, in his *Introduction to Special Relativity* in 1991, offered some attempt at clarification: "Moving lengths are reduced, a kind of perspective effect. But of course nothing has happened to the rod itself. Nevertheless, contraction is no illusion, it is real." (Rindler, 1991, pp 25-29, cited in Walton 2011, p. 2) Pursuing a similar interpretation, but one that might be thought to add little to an arbitration between concepts of 'reality' and 'appearance,' Torretti has argued:

> Let *Q* be a solid cube at rest in an inertial frame F_0 … When considering this phenomenon, [of length contraction] one must bear in mind that *Q*, by hypothesis, is not subject to any action that might deform it. Hence we are not entitled to say that *Q* is contracted by motion. On the other hand, it is true that the place *Q* occupies in its own rest frame differs both in size and shape from the fleeting figure it cuts at each moment in a frame moving past it. But surely there is no more need for this figure to be cubic than for *Q*'s shadows to be all square." (Torretti 1983, pp. 67-68)

Commenting on the contention and the difficulties involved, Pauli, in his *Theory of Relativity*, was to expand on a reliance on the reciprocity requirement and suggested:

> We have seen that this contraction is connected with the relativity of simultaneity, and for this reason the argument has been put forward that it is only an "apparent" contraction, in other words, that it is only simulated by our space-time measurements. If a state is called real only if it can be determined in the same way in all Galilean reference systems, then the Lorentz contraction is indeed only apparent, since an observer at rest in K' will see the rod without contraction. But we do not consider such a point of view as appropriate, and in any case the Lorentz contraction is in principle observable. ... It therefore follows that the Lorentz contraction is not a property of a single rod taken by itself, but a reciprocal relation between two such rods moving relatively to each other, and this relation is in principle observable. (Pauli, 1921, pp 12-13, cited in Walton 2011, p. 2)

Møller, in his *The Theory of Relativity*, concurred that relativistic contraction is both real and observable but is nonetheless dependent as a reciprocal effect:

> Contraction is a real effect observable in principle by experiment. It expresses, however, not so much a quality of the moving stick itself as rather a reciprocal relation between

> measuring-sticks in motion relative to each other. (Møller, 1972, p. 44, cited in Walton 2011, p. 2)

On the contrary, however, in advancing their contribution to the RMS test theory for Lorentz invariance, Mansouri and Sexl require that direct measurement or even an observation of relativistic contraction is not possible:

> It is remarkable indeed that no direct measurements of the Lorentz contraction are possible, and it is very unlikely that any direct experiments can be found in view of the fact that a direct optical observation of the Lorentz contraction is impossible. (Mansouri & Sexl 1977c, p. 813)

For Weiss, in asking the question: "Can you see the Lorentz-Fitzgerald contraction?" (Weiss 1995) suggests that "the contraction can be measured, but the measurement is frame-dependent. Whether that makes it 'real' or not has more to do with your choice of words than the physics." Weiss further poses the question: "If you take a [idealized instantaneous] snapshot of a rapidly moving object, will it *look* flattened when you develop the film?" According to Penrose and independently, Terrell, (1959) the object will not appear flattened, but is conjectured to appear rotated according to the "Penrose-Terrell rotation."

> … an object will appear – optically – the same shape to all observers. A sphere will photograph with precisely the same circular outline whether stationary or in motion with respect to the camera. An object of less symmetry than a sphere, such as a meter stick, will appear, when in rapid motion with respect to an observer, to have undergone rotation, not contraction. (Terrell 1959, p. 1041)

Weinstein further explores the semantics of such relativistic discussions by claiming that:

> The existence of the Lorentz-Fitzgerald contraction has led educators to conclude that one *sees* a contraction of a rapidly moving body. However, the act of *seeing* involves a single observer, while the observation of the Lorentz-Fitzgerald contraction requires at least two observers[445] … the length seen by a single observer is not the usual contraction, and indeed, under certain circumstances, one sees a body considerably lengthened rather than contracted. (Weinstein 1960, p. 607)

Exploring a different approach to relativistic realism, Torretti, in his more recent pursuit of a resolution of such questions through an embrace of the history and philosophy of science, argues that whereas:

> Lorentz and FitzGerald regarded the contraction named after them as a real change in a rod… Relative motion will not make a solid body shorter in the way that say, heat makes it larger. Indeed, if the motion of inertial frame F' relative to inertial frame F effectively shortens the standard meter rod ρ' at rest in F, then we must also conclude that the motion of F relatively to F' also shortens ρ relatively to ρ' in exactly the same proportion. Some people relish such brain teasing descriptions, but others, more sensibly, perceive them as a misuse of language … Clarity of mind suffices to rid oneself of the spooks of rod

[445] Weinstein requires that "the definition of observed length requires both ends of a rod to be observed simultaneously … The Lorentz transformation gives the relationship between the position and time of occurrence of a *single* physical event, as observed by two observers moving relative to one another." (Weinstein 1960, p. 607)

> contraction and clock retardation. However, clarity of mind often is in short supply among educated people. (Torretti 2006, pp. 11-16)

Taking issue with Torretti's approach, Redžić perceives it as reflecting "a persistent terminological and conceptual muddle that accompanies relativistic length contraction since the advent of special relativity, " and alternatively suggests that:

> It seems, however, that uniform relative motion *by itself* cannot make a solid body shorter. I think it would be more appropriate to say that a rod initially at rest relative to *S* is contracted when in uniform motion relative to *S* because of being accelerated relative to *S*; also, the rod in uniform motion is elongated when brought to a state of rest due to the rod's deceleration. … Thus, forces producing acceleration or deceleration of a body relative to an inertial frame are the cause of the body's eventual contraction or elongation, respectively, relative to that frame. (Redžić 2008, p. 198)

Whereas Pauli claimed that relative motion "is the cause of the contraction," and Redžić accuses acceleration boosts, other causal accounts of relativistic phenomena are treated as indifferent to the usually accepted objective demands of an exact physical science. Although a certain poetic liberty might be extended to Minkowski when expressing his claimed great moment of mathematical insight, a physical explanation in terms of an exact science remains obscure:

> … the [FitzGerald-Lorentz] contraction is not to be looked upon as a consequence of resistances in the ether, or anything of that kind, but simply as a gift from above, - as an accompanying circumstance of the circumstances of motion. (Minkowski 1908, p. 81)

A similarly esoteric origin, and one consistent with Einstein's axiomatic and principle theory approach, is claimed by Born,

> But Einstein's theory gives no cause; rather it states that the contraction occurs by itself, that it is an accompanying circumstance of the fact of motion. (Born 1965, p. 253)

Pursuing his acausal theme, Born further attempted what he apparently believed would provide closure on the 'real' versus 'apparent' polemic,

> … we do not mean to say that a body which is moving uniformly in a straight line with respect to an inertial system *S* "undergoes a change," although it actually *changes* its *situation* with respect to the system *S*. Nor is it clear a priori what "changes" physics counts as effects for which causes are to be found …Thus the contraction is only a consequence of our way of regarding things and is *not a change of a physical reality*. Hence it does not come within the scope of the concepts of cause and effect … [this] view … does away with the notorious controversy as to whether the contraction is "real" or only "apparent." (Born 1965, pp. 253-254, emph. added)

Whereas Born repudiated the occurrence of real changes as physically involved in relativistic phenomena, Rogers, in *Physics for the Inquiring Mind*, has required their intrinsic existence to create the same phenomena: "Thus we have devised a new geometry, with our clocks and scales conspiring, by their changes, to present us with a univesally constant speed of light." (Rogers, 1960, p. 496, cited in Walton 2011, p. 2)

Endorsing a similar belief that a 'real' physical change accompanies relative motion, Dewan and Beran regret that:

> Writers in the field of special relativity have often given the impression that the Lorentz-Fitzgerald contraction is merely the result of certain abstract mathematical transformations and, as such, is merely an apparent effect without having, in a sense, "real existence." (Dewan & Beran 1959, p. 517)

Utilizing a thought-experiment, these authors consider two identical rocket ships situated one behind the other and connected by a taut but fragile thread. On a signal both fire simultaneously and maintain equal velocities relative to the same inertial frame, *S* – a scenario that also, by definition, requires that their separation remains constant:

> According to the special theory the thread must contract with respect to *S* because it has a velocity with respect to *S*. However, since the rockets maintain a constant distance apart … the thread … cannot contract; therefore a stress must form until for high enough velocities the thread finally reaches its elastic limit and breaks. This would appear differently to observers in various inertial frames along the track. (Dewan & Beran 1959, p. 517)

Presumably, on such an interpretation, for observers travelling at a lower velocity relative to the rockets, the thread would not be seen to break. In physical contradiction, however, other observers travelling at some greater relative velocity, would observe the same thread to break. Exploring essentially the same scenario, John Bell also took the view that the thread must finally break since, "at a sufficiently high velocity, the artificial prevention of the natural contraction imposes intolerable stress," and posed the problem to his colleagues at CERN.

> A distinguished experimental physicist refused to accept that the thread would break, and regarded my assertion, that indeed it would, as a personal misinterpretation of special relativity. We decided to appeal to the CERN Theory Division for arbitration, and make a (not very systematic) canvas of opinion in it. There emerged a clear concensus that the thread would not break. Of course many people who give this wrong answer at first get the right answer on further reflection. (Bell 1976, p. 68)

Is 'time dilation' a real physical effect?

On the other hand, a resolute denial of a real physical character underwriting relativistic phenomena is asserted by Taylor and Wheeler in their well-received text *Spacetime Physics*:

> Does something about a clock really change when it moves, resulting in the observed change in the tick rate? Absolutely not! Here is why: Whether a clock is at rest or in motion … is controlled by the observer. You want the clock to be at rest? Move along with it … How can your change of motion affect the inner mechanism of a distant clock? It cannot and it does not. (Taylor & Wheeler 1992, p. 76)

Whereas a rigid rod is claimed to 'contract' in some sense while in relative motion and to resume its 'proper' length when restored to rest with the measuring system, a clock in relative motion is portrayed as running slow during relative motion, and to retain the time lost as a measurable legacy of the period experiencing relative motion. The claim has given rise to both the 'clock paradox' and its possibly more familiar, or perhaps notorious, application to twins, one of whom enjoys space-flight at relativistic speed to a remote destination and returns to Earth only to discover that his twin is now appreciably older than himself. Whereas these 'paradoxes' have inspired ceaseless debates and unending confusion, and possibly also entertainment for the public, the conjecture of a measurable temporal legacy inspired an experiment intended to provide definitive empirical confirmation of a relativistic time dilation.

During October, 1971, J. C. Hafele from Washington University, Missouri, and Richard E. Keating from the U. S. Naval Observatory, Washington, D.C., escorted four caesium beam clocks over a period of 26.5 days, on regularly scheduled commercial airlines, and in firstly Eastward and then Westward flights around the world. Their claim to demonstration of a factual dilation of time was derived from the differences in time recorded by the clocks when finally compared with reference clocks stationed at the U.S. Naval Observatory MEAN (USNO).[446] Their published reports in *Science*, (Hafele & Keating 1972a, 1972b) are claimed to "provide an unambiguous empirical resolution of the famous clock 'paradox' with macroscopic clocks," (Hafele & Keating 1972a; Nave 2005) and have become the primary cited proof of the reality of a time dilation effect.[447] (Kelly 2000)

Given the crucial importance of Hafele and Keating's claim, and its endorsement in *Nature* proclaiming that "agreement between theory and experiment is most satisfactory," (W. B. 1972) a number of independent reviews of the experimental raw data were conducted, amongst others, by Essen, (1977; 1988) Spencer and Shama, (1996) and Kelly (2000). Essen, at the time both the inventor of the caesium atomic clock and an acknowledged world leader in its application, concluded "that the experimental results given in their paper do not support these [time dilation] predictions." His article, listing an unacceptably wide variation of raw clock values, (Essen 1977) was rejected for publication by a mainstream physics journal. In pursuing his critical review, he further noted with alarm that the "results obtained from the [four] individual clocks differed by as much as 300 nanoseconds," and concluded that: "This absurdly optimistic conclusion was accepted and given wide publicity in the scientific literature … All the experiment showed was that the clocks were not sufficiently accurate to detect the small effect predicted." (Essen 1988)

In a more recent review of the raw data, as supplied to them by Keating, Spencer and Shama examined the record for each clock and, contrary to the conclusions drawn from averaged results by its authors, claimed that the "smooth curves interpolated during flight appear to be <u>entirely unaffected by the [relative] motion of the airplane</u>." In elaborating this finding, they further claimed that:

> There appears to be no significant difference between the interpolated curves during the airplane flights and the measured portions when the clocks were on the ground. A smooth curve passes through all of the data points and <u>there is no indication of any significant difference in the behaviour of the clocks when in motion</u>." (Spencer & Shama 1996)

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[446] It was forecast that the clocks would lose 40ns on the Eastward journey and gain 275ns on the Westward leg relative to the Naval Observatory as standard. The Eastward flight incurred 13, and Westward flight 15, landings/takeoffs with several changes of aircraft. The results were expected to combine the more significant gravitational 'red-shift' effect with a smaller 'kinematical' effect due to special relativity. Atomic clocks are vulnerable to changes in gravitational and magnetic fields, and in contradiction to a pendulum, run faster in a weaker (higher altitude) gravitational field.
[447] Kelly notes that by 2000 their reports had achieved over 1000 references according to the Science Citation Index

Interpretation of such a finding allowed Spencer and Shama to claim to have

> found that an entirely different interpretation of the experimental data, which supports the [Galilean] universal time postulate on the velocity of light, is perfectly consistent with the experimental data obtained by Hafele and Keating. Thus one of the essential experimental supports of the relativistic theory of time dilation is shown to be invalid. (Spencer & Shama 1996)

In a more recent detailed and exacting review that flatly contradicts a *Nature* claim that only "delicate adjustments to the raw data were needed to correct for the vagaries of the clocks," (W. B. 1972) it is claimed that Hafele and Keating:

> … avoided giving the actual test results in their paper; they gave figures that were radically altered from those results. These altered results gave the impression that they were consistent with the theory [of relativity]. The original test results … do not confirm the theory. The corrections made by H & K to the raw data, are shown to be totally unjustified (Kelly 2000)

Kelly further concluded that "the clocks used were not of sufficient stability to prove anything. The magnitude of the random alterations in performance, during the air transportation, were such as to make any results useless."[448] In detailing support for his highly critical appraisal, he noted that, "there are two instances where a clock altered in the opposite direction to the theory …" and further notes that having abandoned an initial method of averaging what were obviously randomly variable drift rates with multiple abrupt changes in clock rates during flight, a second approach had applied multiple 'corrections' to each clock:

> … comparisons that were made between the four clocks during flights were used by H & K, to decide whether one clock had undergone what was deemed to have been a sudden change in pattern; in such a case it was assumed that the behaviour of the other three was correct. The rationale was that *"the chance that two or more clocks will change rate by the same amount in the same direction at the same time is extremely remote"*. Corrections were made for fourteen changes: clock 120 three changes Eastward and one Westward; clock 361 three Eastward and four Westward; clock 408 two Eastward; clock 447 one Eastward. These corrections were made after the 1971 report was produced. It might have been justifiable to ignore a single isolated sudden change on one clock during the complete 26.5 day period, but to have made corrections for fourteen such alterations in six days of flights and by amounts that exceed the forecast results by up to 5.5 times is breathtaking.

In listing examples of "how unreasonable were the corrections from the actual test results to the amended version," he included,

- clock 408 (Eastward) 'corrected' from +166ns to –55ns
- clock 361 (Westward) 'corrected' from –44ns to +284ns
- clock 447 (Westward) 'corrected' from +26ns to +266ns which is by a factor of 10! and despite the authors claim that "no significant changes in rate were found for clocks 408 and 447 during the westward trip."

[448] According to Kelly: "The Hafele 1971 report said *'Most people (myself included) would be reluctant to agree that the time gained by any one of these clocks is indicative of anything*' and '*the difference between theory and measurement is disturbing*.'" (Kelly 2000)

In Kelly's opinion: "This barefaced manipulation of the data was outrageous," as were corrections of +3.5 and –5.5 times the forecast theoretical –40ns result as applied to two clocks on the wastward test and corrections of 0.5 and 1.2 times the +273ns eastward forecast as was applied to three of the clocks. In his conclusion, Kelly notes that: "Only one clock had a fairly steady performance over the whole test period; taking its results gives no difference for the Eastward and Westward tests."

More recently, the UK's National Physical Laboratory commemorated the 25th anniversary of the Keating and Hafele experiment by flying "a single caesium atomic clock from London to Washington and then back again." (National Physical Laboratory 2005a) The clock was predicted to gain 39.8ns "including an additional geometric factor. This compared remarkably well with a measured gain of 39.0ns." Uncertainty due to clock instabilities and noise was estimated at around ± 2ns. Unfortunately only one clock was used and no independent appraisal of the raw data supporting the claim has yet been made available.

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