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Incommensurability and the Discontinuity of Evidence

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Incommensurability between successive scientific theories—the impossibility of empirical evidence dictating the choice between them—was Thomas Kuhn's most controversial proposal. Toward defending it, he directed much effort over his last 30 years into formulating precise conditions under which two theories would be undeniably incommensurable with one another. His first step, in the late 1960s, was to argue that incommensurability must result when two theories involve incompatible taxonomies. The problem he then struggled with, never obtaining a solution that he found entirely satisfactory, was how to extend this initial line of thought to sciences like physics in which taxonomy is not so transparently dominant as it is, for example, in chemistry. This paper reconsiders incommensurability in the light of examples in which evidence historically did and did not carry over continuously from old laws and theories to new ones. The transition from ray to wave optics early in the nineteenth century, we argue, is especially informative in this regard. The evidence for the theory of polarization within ray optics did not carry over to wave optics, so that this transition can be regarded as a prototypical case of discontinuity of evidence, and hence of incommensurability in the way Kuhn wanted. Yet the evidence for classic geometric optics did carry over to wave optics, notwithstanding the fundamental conceptual readjustment that Fresnel's wave theory required.

In the late 1970s, Kuhn remarked in reference to his 1968 lecture "The Relations between the History and the Philosophy of Science,"

> In the almost nine years since its presentation many more philosophers of science have conceded the relevance of history to their concerns. But, though the interest in history that has resulted is welcome, it has so far largely missed the central philosophical point:

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> *the fundamental conceptual readjustment required of the historian to recapture the past or, conversely, of the past to develop toward the present.* (Kuhn 1977, p. xiv) [italics added]

The question that preoccupied him for 45 years, *what do such conceptual readjustments tell us about science as a kind of knowledge?,* forms a large part of Kuhn's legacy. The book he was writing when he died in 1996—"the grandchild of *Structure,* since the child was still-born"—was intended to answer this question, together with its correlate, *what do these readjustments involve and why do they occur?* Kuhn, of course, was scarcely the first to become preoccupied with the conceptual readjustments that have occurred in science. The revolutions marked first by the special theory of relativity and then by general relativity and quantum mechanics were as central to philosophy of science during the first half of the twentieth century as the extraordinary success of Newtonian physics was to philosophy of science during the nineteenth century. What separated Kuhn from those preceding him was his insistence that these revolutions marked radical discontinuities of the same sort as the transition from Aristotelian to modern physics.

1.0 Kuhn on Incommensurability

Starting with *Structure* and continuing to his last unfinished book, four propositions earmarked Kuhn's thought. First, scientific research is a highly specialized social practice modeled on a set of prior achievements that function as exemplars; learning how to engage in this practice is learning how to extrapolate, so to speak, from these exemplars. Second, scientific revolutions involve the replacement of one set of exemplars by another set, resulting in a distinctly new practice. Third, scientific revolutions in this sense have occurred far more often in the history of modern science than one might have thought from taking relativity and quantum mechanics as prototypical; indeed, revolutions in this sense lie behind the continual "speciation" of scientific disciplines into increasingly specialized sub-disciplines. Fourth, (now quoting) "the normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (Kuhn 1970, p. 103). Kuhn originally latched onto the term 'paradigm' in conjunction with the first of these four propositions, but he subsequently abandoned it because his expanded use of it ended up obscuring the root-idea that made it appropriate in the first place. Still, in Kuhn's mind this first proposition remained the cornerstone. The other three derive from it. The fourth proposition, needless to say, is the one that has provoked by far the most controversy. Kuhn never abandoned it. Indeed, the announced goal of his unfinished book was to clarify and defend it.

Kuhn's defense of incommensurability in *Structure* pivoted on his contention that Newton's laws of motion cannot properly be derived as limiting-case approximations of the laws of motion in relativistic mechanics. For "the physical referents of these Einsteinian concepts [of space, time, and mass] are by no means identical with those of the Newtonian concepts that bear the same name" (Kuhn 1970, p. 102). Kuhn subsequently concluded that the argument in *Structure* "was both uninformative and in a central respect misleading."[1] Its sweeping appeal to meaning-change failed to discriminate the specific, narrower kind of change in meaning that produces what he called in *Structure* "a displacement of the conceptual network through which scientists view the world." The defense of incommensurability in *Structure* seems to us unfortunate in still a further way. It focused the issue too much on the transition from Newton to Einstein, a transition that is far too contentious to serve as a prototype of incommensurability. A central claim of the present paper is that the transition from ray to wave optics at the beginning of the eighteenth century is a much better prototype for purposes of clarifying the intricacies of incommensurability.

Another shortcoming of Kuhn's "meaning-change" defense of incommensurability in *Structure* was the failure to tie it to his claim about the role of exemplars in scientific practice. The approach he took to incommensurability from the late 1960s on always had as its starting point the process of learning from examples—in particular, learning natural kind terms and the taxonomic distinctions they mark from instances.[2] Because natural kind terms are open-ended in their application, several complementary examples or illustrative situations are required to assimilate them. They are acquired not individually, but as an interrelated group, for they have the significance they do only in relation to one another. The fact that different individuals have acquired them along different trajectories involving different specific examples normally does not interfere with communication. Different trajectories of acquisition, however, do affect communication when anomalous examples are encountered, and such anomalies can throw the entire set of distinctions into question.

1. This quotation is taken from the beginning of the third chapter of Kuhn's unfinished book manuscript.

2. The development of Kuhn's approach to incommensurability after *Structure* is summarized in Buchwald and Smith (1997, p. 367–375). See also the various lectures and papers now published in (Kuhn 2000).

Factors like these gave Kuhn reason to think that taxonomy provides a way of explicating and defending incommensurability. Two scientific systems can be commensurate with one another—or, in the phrasing he often preferred, mutually translatable—only if they share the same *taxonomic structure.* Scientific categories can be thought of as forming a taxonomic tree. Distinct branches emerge from the trunk, marking principal kinds. Each branch may divide further, yielding sub-kinds. Every kind, however, emerges either directly from a single immediately preceding node on the tree or from the trunk. Because no kind descends from more than one immediate ancestor, there is no possibility of partial overlap among kinds. This requirement on kinds, which is equivalent to requiring each category to be immediately subordinate to at most one ancestor, Kuhn concluded is crucial to taxonomic distinctions in science. Any addition to a tree of kinds must accordingly be grafted onto its structure without violating its integrity: additions can be made, but multiple connections between existing kinds are not permitted, nor can new kinds be added unless they emerge directly from a preceding kind or from the trunk. Two taxonomies in science are then commensurable if either of two conditions is satisfied: (1) every kind in the one can be directly translated into a kind in the other, which means that the whole of one taxonomic structure is isomorphic to some portion of the other; or (2) one structure can be directly grafted onto the other without otherwise disturbing the latter's existing relations. When the first condition is fulfilled, one scheme can simply be subsumed under the other. When the second is fulfilled, a new scheme can be formed out of the previous two while preserving intact all the prior relations among kinds.

The conspicuous limitation of this approach to incommensurability is that the fundamental terms of the advanced sciences—terms like 'force' and 'mass'—do not signify components of what Kuhn called a *contrast set.* Terms like these do not signify sub-kinds of anything else; they rather signify distinct physical magnitudes, which Kuhn came to call *singletons,* or *artefactual kinds.* Though not marking taxonomic distinctions, these terms still have many of the features of taxonomic kind-terms. They are learned through examples, not through definitions, and several complementary examples are usually required to assimilate them. Different individuals acquire them along different routes, involving different examples, yet communication is unaffected until anomalies emerge. Terms like 'force' and 'mass' are not learned individually, but together, owing to the relations they have with one another as expressed in the law-like generalizations that are invoked in solving the exemplary problems through which students acquire mastery of them. The relations among theoretical terms expressed in these generalizations also provide multiple avenues for assigning specific values to the various physical magnitudes—that is, different ways of measuring them, ways that can often be embodied into instruments.

Given all these similarities, the obvious question is, what is the correlate for singletons to the no-overlap principle for commensurability in the case of taxonomic structures? This question remained Kuhn's central preoccupation through his last years, and it was lack of satisfaction with his answer to it that prevented him from finishing his book before he died. The quotation here, from his Nobel symposium lecture of 1986, displays the general approach he was attempting to take to it:

> Now suppose that neither the revisions that preserved [Newton's] second law nor those that preserved the law of gravity proved effective in eliminating anomaly. The next step would be an attempt at revisions which altered both laws together, and those revisions the lexicon will not, in its present form, permit. Such attempts are often successful nonetheless, but they require recourse to such devices as metaphorical extension, devices that alter the meanings of lexical items themselves. After such revision—say the transition to an Einsteinian vocabulary—one can write down strings that *look like* revised versions of the Second Law and the law of gravity. But the resemblance is deceptive because some symbols in the new strings attach to nature differently than do the corresponding symbols in the old, thus distinguishing between situations which, in the antecedently available vocabulary, were the same. They are the symbols for terms whose acquisition involved laws that have changed form with the change of theory: the differences between the old laws and the new are reflected by the terms acquired with them. Each of the resulting lexicons then gives access to its own set of possible worlds, and the two sets are disjoint. Translations involving terms introduced with the altered laws are impossible. (Kuhn 2000, p. 74)

An obvious challenge in carrying through this approach is to find a principled way to distinguish the revisions to laws that have lexical consequences which (in Kuhn's words) give rise to disjoint sets of possible worlds from ones that do not. Complicating this challenge was Kuhn's desire, as the quotation illustrates, to retain the claim of *Structure* that the transition from Newtonian to Einsteinian physics not only involves a shift from one lexicon to another, but further a shift resulting in incommensurability. From the time of *Structure* forward, the obstacle to this claim of incommensurability has been the well-known reasoning in which Newton's second law of motion is derived as a limit-case of the corresponding law of special relativity as the velocity approaches zero, and

Newton's law of gravity is derived as a limit-case of gravity in general relativity as the strength of the static field approaches zero. Kuhn needed but never found a decisive argument that these limit-case derivations are not by themselves enough to establish some sort of commensurability.

Limit-case reasoning represents one extreme of arguments put forward with the intent of showing that some newly advanced law or theory is a refinement of a previous law or theory, or conversely that the latter should be viewed as an approximation to the former. Claims of this sort are not restricted to laws and theories, however; they are even more commonplace when new methods or instruments for measuring given quantities are said to provide advances in accuracy and range over earlier ones. Indeed, at the other extreme from limit-case reasoning are the arguments put forward in the case of minor improvements in measurement and calibration. Between these two extremes is a whole range of arguments to the effect that the new is a refinement of the old and the old in some critical respect approximated the new. A primary thesis of this paper is that issues of incommensurability need to be considered in the light of the *substantive* role such arguments play in the development of science.

2.0 Continuity of Evidence

In *Structure* Kuhn in the end dismissed the limit-case reasoning from Einstein to Newton as, in effect, a re-writing of history that preserves the illusion of cumulative progress. But that reasoning cannot be dismissed so easily, for it appears to have played a substantive role in the transition. In particular, the limit-case derivations showed that all of the prior evidence for Newtonian mechanics and gravitation, respectively, was also evidence, to indicated levels of accuracy, for relativistic mechanics and gravitation. The process of marshalling evidence for the special and general theories of relativity clearly did not have to begin from the ground up. All the old evidence carried over immediately, with minor qualifications, so that the task of establishing relativity was one of adducing evidence supporting the points of contrast between it and Newtonian theory. Some of the evidence for relativity did countermand Newtonian theory, and the evidence for Newtonian theory appears in a different light as a consequence. Nevertheless, the limit-case derivations seem historically to have been taken within scientific practice as showing that Newtonian theory formally approximates Einsteinian in such a way that the evidence for the former carries over as evidence for the latter—this in spite of the huge conceptual readjustment separating the two.[3]

3. Invoking limit-case arguments in an effort to carry evidence from an old theory over to a new one has a long history. For example, Newton made this move, in a quite elaborate

In fact an even stronger claim can be made: the evidential reasoning supporting Newtonian theory must in some sense remain valid in spite of this conceptual readjustment. For the 43 arc-seconds-per-century in the precession of the perihelion of Mercury that was invoked as crucial evidence for general relativity is what remains after all of the various Newtonian effects are calculated and subtracted from the observed precession. To invalidate the evidential reasoning supporting Newtonian theory would consequently invalidate the legitimacy of invoking the specific value of 43 arc-seconds per century, for it expressly presupposes Newtonian theory.

Let the phrase *continuity of evidence* refer to transitions from an old theory or practice to a new one in which the old approximates the new in a way that (1) allows the evidence for the old to carry over to the new and (2) permits the evidential reasoning that supported the old to remain valid under a suitable reconstruction in the new. However great a conceptual readjustment may be, continuity of evidence necessarily poses a *prima facie* challenge to any claim of incommensurability—or any denial of no common measure. For, one must *de facto* be able both to argue that the new system accommodates the existing evidence for the old and to compare the two systems with respect to new evidence. This goes far toward explaining why Kuhn met so much resistance to his claim of incommensurabilty across the transition from Newtonian to Einsteinian physics, for dismissing the limit-case reasoning would alter the burden of proof that relativity theory was historically required to meet. Kuhn would have been in a considerably stronger position had he chosen as a prototype of incommensurability an example in which evidence is indisputably not continuous across the transition—an example that thus does not threaten to beg the question of comparability of evidence across the transition. We think that the transition from ray to wave optics early in the nineteenth century is such a prototype precisely because it brings out more clearly the subtleties involved in maintaining and losing continuity of evidence.

Before turning to this transition, we will consider an example in which the claim of continuity of evidence is not *prima facie* contentious. We can thereby clarify what such continuity involves. Some of the refinements of the gas law from Boyle to the twentieth century serve this purpose well. In its original form Boyle's law asserted that pressure is inversely proportional to volume—$p \propto 1/V$—under the (at the time irrelevant) *ceteris paribus* condition that temperature remains constant. Newton reformulated the law as pressure is proportional to density—$p \propto \varrho$. He did so because

fashion, in order to retain the integrity of Huygens's pendulum measurement of the strength of surface gravity under his law of universal inverse-square gravity. See Smith (2002, p. 53f).

he wanted to extend the domain over which the law was projected to hold beyond entrapped volumes of air so that he could apply it locally, without reference to containing boundaries, to atmospheric variations with altitude.[4] For cases in which a specific quantity of air is entrapped, $1/V$ can serve as a measure of ϱ just as the length of the air-containing tube served as a measure of V in Boyle's original experiments. The numerical relation between parameters in the two formulations is thus exactly the same for cases of entrapped gas. Differences in conceptualization aside, the disparity between the two lies only in the difference in the domains over which they are projected to hold (*viz.* confined volumes for Boyle, boundary-independent localities for Newton). Boyle's formulation can be viewed as just a special case of Newton's. One can equally speak of Boyle's formulation as approximating Newton's insofar as the domain over which Boyle's was projected to hold served historically as a first approximation to—a first stab at—a yet-to-be-identified more extended domain. Correlatively, one can speak of Newton's formulation as a *refinement* of Boyle's, one that eliminated a needless parochialism in Boyle's. The evidence for Boyle's formulation was clearly evidence for Newton's.

Temperature was incorporated, yielding the gas law in the form of $p \propto \varrho T$ early in the nineteenth century.[5] The subsequent shift from $p \propto \varrho T$ to $p \propto (n/V)T$, where n/V is the mole density, was both similar to and different from the Boyle-Newton shift. *Mole density* lies at a considerable conceptual distance from density, and its quantification involves new theoretical considerations. The immediate gain from shifting to *mole density,* or any of its counterparts, was to capture a broader generalization. The formulation, $p \propto (n/V)T$, telescoped a large number of gas-specific generalizations of the form $p = \varrho RT$ into a single, universal generalization—this by taking into account the ratios among the constants of proportionality of the gas-specific expressions and relating them to the ratios among the weights of equal volumes of gases at the same pressure and temperature.[6]

4. See Book 2, Section 5 of the *Principia,* where Newton uses his form of the gas law to calculate the variation of atmospheric density with altitude under different rules for the variation of gravity. Newton also uses his form of the gas law in calculating the speed of sound in air in Book 2, Section 8, producing a result in conflict with measurement as a consequence of, in effect, treating the expansion in sound waves as isothermal rather than adiabatic.

5. We here skip over a number of other, related historical developments—most importantly, Lavoisier's introduction of *gas* and the experimental efforts on the compressibility of different gases—that yielded gas laws of the indicated form for different gases.

6. The standard present-day way of relating the universal gas law to the gas-specific laws is by pointing out that the universal gas constant is equal to the product of the gas-specific constants of proportionality R and the molecular weights of the gases. See, for example, Keenan (1970, p. 95).

A simple numerical relationship holds between ϱ and (n/V) for each specific gas. Consider, however, an entrapped gas that changes chemical composition at certain values of density and temperature. In such cases $p \propto \varrho T$ and $p \propto (n/V)T$ can part ways, for $1/V$ and ϱ may no longer serve as an appropriate measure of (n/V). The two formulations thus make different empirical claims. All evidence for $p \propto \varrho T$ carries over to $p \propto (n/V)T$, but not conversely. As before, $p \propto \varrho T$ can be viewed as just a special case of $p \propto (n/V)T$, holding so long as chemical composition remains fixed. Considered from the point of view of the scientists who effected the generalization, it would be better to regard $p \propto \varrho T$ as an approximation to $p \propto (n/V)T$, and $p \propto (n/V)T$ as a refinement of $p \propto \varrho T$, in that as ϱ served as an initial approximation to—as a stab at—the yet-to-be-determined parameter (n/V) that allowed the broader generalization to be captured. Viewed retrospectively, ϱ had served as a proxy or surrogate for (n/V). This is not at all to say that the concept of density approximates the concept of mole density. Hardly—the term 'approximation' here refers to an initial quantitative relation that continues to be accommodated under the proviso that specific circumstances be taken into account that were not envisioned by those who propounded the earlier relation. Accordingly, ϱ continues to serve as a measure of (n/V) provided that chemical composition is considered.

The relationships among the three expressions—$p\mathrm{V} \propto T$, $p \propto \varrho T$, and $p \propto (n/V)T$—contrast with the relationship between $p \propto (n/V)T$ and the virial expansion,[7]

$$p \propto (n/V)T\ [\ 1 + (n/V)B(T) + (n/V)^2C(T) + \ldots\ ]$$

where the so-called virial coefficients, $B(T)$, $C(T)$, etc., "by means of statistical mechanics may be expressed in terms of the intermolecular potential functions. Consequently it is possible to obtain a quantitative interpretation of the deviations from the ideal gas law in terms of the forces between molecules" (Hirschfelder et al 1954, p. 131). For entrapped gases of fixed chemical composition, the three different formulations of the ideal gas law, with their different parameters, provide three different ways of stating essentially the same open-ended body of data, and the numerical relationships among p, V, and T remain the same. By contrast, while the principal parameters are the same in $p \propto (n/V)T$ and the virial expansion, the numerical relationships among p, V, and T do not remain the same. Here one says that the ideal gas law *numerically approximates* the virial ex-

7. This form of the virial expansion, which apparently dates from early in the twentieth century, is not the form initially put forward by Clausius and championed by Maxwell (see, for example, 1875). For historical background and technical details, see Mason and Spurling (1969) as well as Hirschfelder et al (1954), Maitland et al (1987), and Brush (1983).

pansion over a certain limited range of the parameters. This is the customary sense in which one numerical relationship approximates another. All the evidence for the ideal gas law can be evidence, to an indicated level of accuracy, for the virial expansion, but not all of the evidence for the virial expansion is evidence for the ideal gas law.

In the framework of statistical mechanics, within which the virial expansion has its home, the ideal gas law does more than just numerically approximate the expansion over a certain limited range. The ideal gas law is a limiting case of the virial expansion—the physically characterized limit under which all the terms in the expansion after the first vanish. Any number of mathematically disparate curves can be fitted to a body of data to any given level of accuracy and hence used to recapitulate those data. The limit-case reasoning shows that, from the standpoint of the virial expansion, the ideal gas law is more than just one of these curve-fits. The expansion first defines a physically-characterized range over which the ideal gas law approximates not merely the existing data, but any other data taken within this range; second, by giving a physical reason for why the ideal gas law approximates it over this range, namely that the range is one over which the intermolecular potentials are comparatively small, it implies that this is not a mere numerical coincidence; and third, it implies that all differences between it and the ideal gas law, whether within this range or beyond it, are again not mere numerical coincidences, but arise from specific physical factors. Because of these three considerations, especially the third, measured deviations from the ideal gas law were legitimately used to infer magnitudes of intermolecular potentials in different gases long before anything was known about the molecular structure giving rise to these potentials.[8] This was the crucial substantive role the limit-case argument played in the development of modern gas theory.

The remark at the beginning of the last paragraph that the virial expansion has its home in statistical mechanics calls attention to the major conceptual shift that it presupposed. Both $p\mathrm{V} \propto T$ and $p \propto \varrho T$ are neutral with respect to the molecular structure of matter. In fact, so too was $p \propto (n/V)T$ at least until the beginning of the twentieth century, for many chemists before then had employed *mole density* while dismissing the atomic structure of matter as metaphysics. The principal conceptual change from Boyle's and Newton's formulations to $p\mathrm{V} \propto T$ and $p \propto \varrho T$, besides the inclusion of (absolute) temperature, was the generalization from air to gases, a taxonomic shift in full conformity with Kuhn's no overlap principle. The further change to $p \propto (n/V)T$ presupposed the concept of *equivalent* or *molecular weight* from chemistry, the values for which vary

8. For example, in Maxwell (1875).

from gas to gas. In each of these cases, the conceptual change had the primary effect of allowing a broad generalization across gases to be captured. While these conceptual changes had important implications for laboratory practice, and they represented a fairly revolutionary conceptual shift within chemistry itself, they did not require anyone to learn a radically new way of thinking when considering the gas law in and of itself.

By contrast, divorced from the framework of statistical mechanics, the virial expansion amounts to nothing more than an open-ended scheme for curve-fitting, and the claim that for any chemically given gas the virial coefficients $B(T)$ etc. vary only with temperature has at best limited empirical justification. The systematic derivation of the virial expansion within statistical mechanics shows that the second term in the expansion represents the interactions of molecules in pairs, the third term, in triples, etc.[9] This derivation invokes both conceptual apparatus and mathematics far beyond anything needed to learn or to understand the ideal gas law in any of its forms. It is this derivation, however, that licenses the inference of intermolecular forces from measured deviations of real gases from the ideal gas law. Thus, the physics of both the virial expansion and its relationship to the ideal gas law could not have occurred without a revolutionary conceptual shift not merely to an ontology of molecules, but also to complicated forms of statistical aggregation via cluster integrals and partition functions. Still, as revolutionary as this conceptual change was, it did not produce a discontinuity of evidence from $p \propto (n/V)T$ to the virial expansion. Rather, the transition from $p \propto (n/V)T$ to the virial expansion opened new evidential pathways in a manner closely akin to the way in which the transition from $p \propto \varrho$ to $p \propto \varrho T$ opened an evidential pathway that yielded absolute temperature. This new evidential pathway as a matter of logic must presuppose the validity of the prior evidence for $p \propto (n/V)T$. Furthermore, the carry-over of evidence from $p \propto (n/V)T$ to the virial expansion made an important contribution to the evidence for this expansion: the good empirical agreement of $p \propto (n/V)T$ with experiment at low densities across all gases provided the principal evidence that the differences in the relationships among *pressure, temperature,* and *mole density* from one gas to another result solely from intermolecular forces that are peculiar to each chemically defined gas.

The transition from the ideal gas law to the virial expansion is thus an example of a Kuhnian *fundamental conceptual readjustment* across which, as a matter of historical fact, evidence was treated as continuous. We should

9. The now standard derivation, which includes quantum mechanical as well as classical considerations, can be found in Hirschfelder et al (1954). It dates from work by Joseph E. Mayer and his graduate student Sally F. Harrison in 1936–38 (Brush 1983, 247).

note that the taxonomic consequences of this readjustment did not violate Kuhn's requirements for commensurability. Just as the transition from individual cases of $p \propto \varrho T$ to the single law $p \propto (n/V)T$ provided strong grounds for treating *gas* as a taxonomic category, the transition from $p \propto (n/V)T$ to the virial expansion provided strong grounds for treating the chemically distinct gases as different taxonomic sub-categories of *gas.*[10]

The relationships among the four different formulations of the gas law are somewhat different from the relationship between Newtonian and Einsteinian theory, and hence what we have said in no way begs the question of incommensurability in the latter case. Kuhn would presumably not have said that the enriched lexicon obtained by the addition of, for example, *mole density* threatened incoherence if *pressure, temperature, density* and *volume* remained unchanged in meaning—any more than the historical addition of new ways of measuring these four quantities produced disparate lexicons. For, these four quantities, and *mole density* as well, were never intertwined with one another in the way Kuhn argues that the second law of motion made it necessary to gain mastery of *mass* and *force* together with one another in Newtonian mechanics. Thus, nothing in what we have said about the gas law is in conflict with the approach Kuhn was taking to 'singletons' in his later work on incommensurability.

The history of the gas law helps make clear that four distinct elements are involved when laws are said to approximate, in an evidence-preserving manner, subsequent laws, or when the subsequent laws are said to be a refinement of the earlier laws: (1) the *domains* over which the lawlike generalizations are projected to hold; (2) the *parameters* entering into the generalizations, with particular emphasis on the way they are quantified in practice; (3) the *numerical relationships* asserted to hold among these parameters and, by implication, among other closely related parameters; and (4) the *physical considerations* to which all differences in the numerical relationships are being attributed.

Correspondingly, four conditions apparently have to be met in order for the evidence for the earlier laws to carry over to the subsequent laws. First, from the subsequent point of view, the domain that the earlier laws covered must be contained within—and in this sense approximate—the domain over which the subsequent laws are projected to hold; and from the earlier point of view, the latter domain must be an extension with limita-

10. The transition to the virial expansion had the further taxonomic consequence of providing an explanation for the empirical need to exclude 'electrified'—that is, ionized—gases from the domain of the gas law. The conceptual changes from Boyle to Newton to *mole density* to the virial expansion amount to the sequence air, air having specific local density, air as one of many gases, gas made up of molecules, intermolecular forces in gases.

tion or a refinement of the former domain. Second, from the subsequent point of view, the ways in which the parameters of the earlier laws were quantified can still serve for quantifying the corresponding parameters of the subsequent laws, yielding at least approximate measures over the range of the evidence for the earlier laws; and from the earlier point of view, the parameters of the subsequent laws provide an alternative way of describing or stating the observations entering into the evidence for the earlier laws. Third, the numerical relationships asserted to hold among relevant parameters by the earlier laws must at least approximate those asserted to hold among their counterparts by the subsequent laws over a specified range—that is, the numerical values entailed for any one parameter by numerical values for the others must be approximately the same over this range. And fourth, from the subsequent point of view, the numerical discrepancies between the earlier and subsequent laws must result from physical considerations expressly represented in the new laws.

Clearly more needs to be said, and more examples need to be considered, in order to make the notion of continuity of evidence precise. This rough statement of four necessary conditions is intended only as an initial approximation to prepare the way for examining the historical transition from ray to wave optics. Moreover, we do not assert that these conditions work quite so independently of one another as the gas law example might suggest. They are probably far more often intertwined with one another than not. Finally, we do not offer these as the only necessary conditions. Something must also be said concerning the manner in which a shift to subsequent laws neither invalidates nor nullifies the various steps in the evidential reasoning that supported the earlier laws. It would therefore be premature to take the preceding explication as in any way settling the question of incommensurability between Newtonian and Einsteinian physics. But that is not the goal of this paper. The goal is to clarify what is involved in incommensurability by looking at a case where evidence did not carry across the divide.

3.0 From Ray to Wave Optics

The transition from ray to wave optics took place over a roughly 30 year period after polarized light, which had first been noted by Christiaan Huygens 120 years earlier and which constituted an anomaly for him, became the central preoccupation of optical research. Ray optics dates back centuries in the form of geometric optics, that is, mathematical accounts of phenomena of reflection and refraction that give geometrical rules for tracing light from a source (or from the eye, depending on physical assumptions) through the process of reflection or refraction to the formation of an image of the source. Ray optics underwent a rapid series of developments following Etienne Louis Malus's discovery in 1809 that polarized light can be produced not just by passing light through Iceland Spar, but also by reflecting it at a specific angle from glass, or indeed from any reflecting substance at angles that vary from material to material. In response to this discovery, Malus appropriated Newton's idea, advanced originally in response to Huygens's discovery, that rays of light are asymmetric and devised a quantitative theory of partial reflection in which the reflecting device selects which rays are reflected and which refracted according to the specific alignment of their asymmetries.

Dominique-François Arago in 1811 discovered the phenomenon of chromatic polarization, wherein polarized light passed through a birefringent crystal and then reflected at the polarizing angle exhibits colors that depend upon the various alignments of the planes of incidence and reflection and of the crystal. Jean Baptiste Biot then developed a comprehensive, quantitative theory of polarized light based on the principle that the relevant devices always select rays of particular symmetries in specific circumstances. Over the next 20 years—during which ray optics became embroiled in open conflict with Fresnel's wave optics—further novel effects emerged. Theoretical ray optics continued to be developed in response to these effects, most notably by David Brewster in response to the phenomenon of elliptically polarized light. Ray optics then gradually died off, not because decisive experiments had refuted it, but largely because wave optics was proving so fruitful in ongoing research.

The proponents of ray optics inherited from the Newtonian tradition the idea that a ray is simply the trajectory of a light corpuscle, and some of them fleshed this idea out further with conjectures about the asymmetry of these corpuscles and the action of forces on them. Still, however much of a heuristic crutch was provided by such pictures of what a ray is, the quantitative theories and the evidential reasoning for ray optics were predicated on a more limited, abstract characterization of rays. The two essential theses of ray optics, both in geometric optics and in the sequence of developments in response to the new phenomena of polarization discovered in the 19th century, is that a beam of light consists of individual rays, and that these rays remain intact during the action of devices that reflect, refract, and polarize the beam. That is, individual rays approach a device, and (ignoring absorption) those very same rays depart it. The long history of geometric optics provided strong grounds for tracing a geometrically defined ray up to a device that reflects or refracts it, and then continuing to trace this same ray during and after reflection and refraction. The continuing tradition of ray optics in the early 19th century can be thought of as taking polarization to have provided new information about these rays. In particular, polarization suggested that beams of light—that is, collections of rays—come in three distinct kinds depending on the distribution of the asymmetries of the individual rays composing them: natural or unpolarized light, polarized light, and partially-polarized light. These kinds could be distinguished among one another by their behavior in a device like Malus's polarimeter, which detected asymmetries.[11]

The tradition of wave optics dates back to Huygens and Robert Hooke in the late seventeenth century. According to it, in the form developed especially by Huygens, light does not consist of collections of individually distinguishable line-like objects (rays); instead, light is taken to be a surface (or front) that moves through space, with rays having the mathematical character of lines drawn from the point of emission to any point on the front. Although Leonhard Euler superadded periodicity to the scheme in order to accommodate phenomena of color (which were foreign to Huygens's concerns), only Thomas Young in England and, shortly thereafter, Augustin Jean Fresnel in France provided fundamental theoretical and experimental novelties. Wave optics came fully into its own only with Fresnel's work in the years following the discovery of polarizing reflection and chromatic polarization.

Two points are important for our purposes. First, wave optics is categorically incompatible with the idea that one and the same ray enters and departs from polarizing devices. Second, wave optics as developed mathematically by Fresnel and those following him involves both a substantially different mathematics and a radically different way of thinking from ray optics. In wave optics, we remarked, the fundamental entity is not a ray, but a front that does not consist of individuated constituents; fronts with complicated geometrical forms can be represented mathematically as the superposition of generally ovoidal wavelets (or, more properly considering that Huygens did not invoke periodicity, of what might be termed frontlets); finally a succession of fronts forms a continuous wave that is characterized by length, frequency, and phase. In short, the transition from ray to wave optics involved a Kuhnian conceptual readjustment if ever there was one. The demands imposed by this readjustment are still the major hurdle students face in learning optics.

The relation between ray and wave optics is complex, both historically and logically. Indeed, the standard contemporary treatise on optics (Born and Wolf 1980) devotes 150 pages just to the relation between wave and geometric optics. Here we are only going to highlight the relation be-

11. Specifically, an unpolarized beam exhibited the same intensity when reflected at the polarizing angle by a glass plate, whatever the orientation of the plane of reflection might be; a polarized beam varied markedly in reflected intensity with that plane and could be made to vanish altogether; a partially-polarized beam varied with the plane of reflection, but could never be made to vanish.

tween wave optics and the specific theoretical proposals put forward successively by Malus, Biot, and Brewster, the evidence adduced in support of them, and how this evidence was viewed from the perspective of wave theory; we will end the discussion by considering how, or even whether, evidence remained continuous from geometric to wave optics.

3.1 Malus on Partial Reflection

First noted by Huygens, until the early nineteenth century the phenomenon of polarization was thought to occur only when light has passed through the crystal Iceland Spar. Because the emergent light would refract in a second crystal in ways that depended on the respective orientation of the two crystals, the phenomenon was thought to indicate that a light ray which has passed through Iceland Spar must have some sort of asymmetry about its axis. In 1809 Malus discovered that the phenomenon is not peculiar to Spar, but can also be produced by reflecting light at a particular angle specific to each reflecting substance. This suggested that the asymmetry in question is a general property of light that is activated or modified under particular circumstances. Malus named the property 'polarization' and built a device to detect its presence, the polarimeter.

Within the tradition of ray optics deriving from Newton, rays were already thought to be distinguished from one another by an intrinsic, unalterable property that dictates color. Malus now associated two directions to each ray: the traditional one along the ray's length and a new one orthogonal to its length—the direction in which the asymmetry of the ray is present. For Malus, rays join in bundles, forming beams. Optical intensity involves just the number of rays in a beam, with each ray having, as it were, the same intrinsic intensity. Polarization is a property of beams—specifically, the degree to which the asymmetries of the rays forming the beam are aligned with one another. Bodies that affect a beam's polarization must somehow select a subset of rays according to the orientation of their asymmetry and then, depending on the body, alter these directions.

Malus divided beams of light into three kinds, each of which behaves in a distinct manner in his polarimeter. A *polarized* beam is one in which the asymmetries of the constituent rays are nearly enough parallel as to be indistinguishable by a polarimeter. The rays forming an *unpolarized* beam have asymmetries that are more or less randomly distributed, so that a polarimeter reveals no preferred direction. Finally, in a *partially-polarized* beam, the rays group into subsets, with each subset having virtually the same orientation of asymmetries, and different numbers of rays in each subset; a polarimeter then shows more light in some orientations than in others. These three categories exhaust the possible basic kinds of beams in ray optics.

The first novel application of this 'selectionist' approach was to the phenomenon of partial reflection, which poses the question of how much light is reflected, and how much is refracted, by a given material at a specific angle of incidence. Malus saw that the traditional way of thinking about this problem had to be altered because of his discovery that the amounts in question depend systematically, and quite markedly, on the state of polarization of the incident beam. What happens to an unpolarized beam is accordingly to be determined by considering what happens to polarized beams of various orientations. Malus was able to develop a quantitative theory that solves the problem by specifying the component subsets in the reflected and refracted beams with respect to both polarization and intensity.

As Figure 1 shows, the numerical values obtained from Malus's formulas differ from those that are now accepted (obtained from formulas developed by Fresnel on wave principles) by amounts less than could be detected before the development of photometric comparators. Yet the respective ray and wave formulas are in no other way connected with one another. Malus's ray formulas cannot be derived from Fresnel's wave formulas either algebraically or through any sort of limiting process. From the point of view of the wave formulas, the small numerical differences illustrated in Figure 1 are mere numerical coincidences, having no physical significance in their own right. Indeed, the ray and wave formulas differ remarkably in their detailed implications. For example, one consequence of Malus's theory is that a polarized beam on reflection other than at the polarizing angle is only partially polarized, though the added subsets must be small. By contrast, on wave principles the beam must remain completely polarized. Thus, the close numerical agreement notwithstanding, the fact that measured values agree well with Malus's formulas did not by itself provide evidence supporting Fresnel's formulas. Historically, the evidence for Malus's formulas did not simply carry over as evidence for Fresnel's.

The several differences between ray and wave optics with respect to polarization phenomena derive from the difference between the fundamental objects with which they work. In selectionism this is the ray. In wave optics the fundamental object is the wave front, and here the ray, as noted above, is altogether a mathematical construct—a line drawn from the point of emission of the front to a point on its surface. Such a line has no individuality in wave optics, and nothing corresponding to it persists intact during double refraction. The most that can be said is that such a line can be shown to represent approximately the direction along which energy flows by drawing a small region on the wave surface, erecting orthogonals, and considering the ray to represent approximately the path traced by the

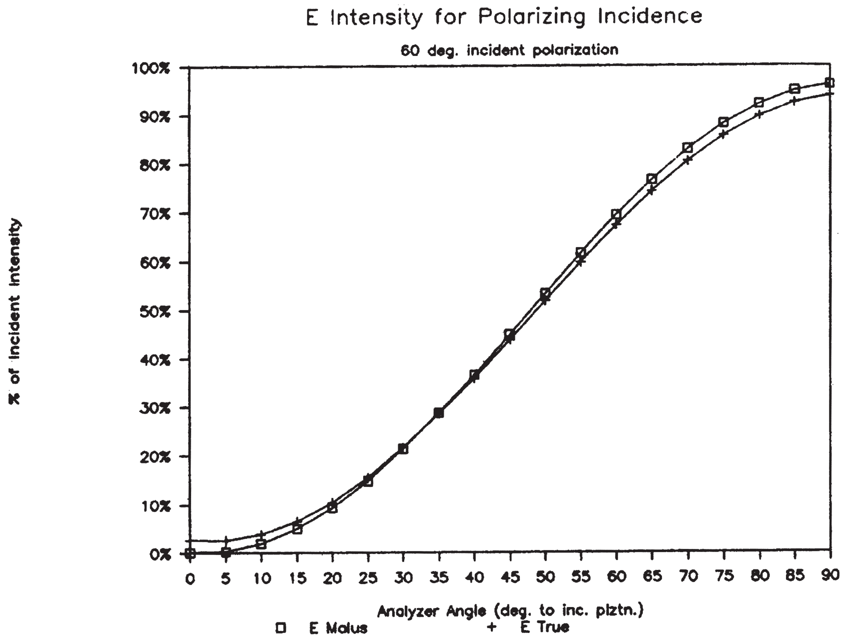

Figure 1. Values from Malus's Formulas versus Modern Values

orthogonal trajectory. As we shall see later, this construal of rays within wave optics allows it to incorporate geometric optics. But the loss of ray individuality precludes any comparable incorporation of Malus's ray optics.

Because rays have no individuality in wave optics, they cannot be grouped into sets, optical intensity cannot be thought of in terms of the number of rays in a beam, and polarization therefore has nothing to do with the production of subsets of rays with common asymmetries. In wave optics, partial reflection is determined by a process of decomposition of the oscillation at the interface, with components in and normal to the plane of reflection being affected in different ways. The recoverability of geometric optics within wave optics notwithstanding, there is no situation in which, by any approximation that would map a wave optics configuration into one in selectionist optics, Malus's selectionist formulas for partial reflection can be obtained from Fresnel's wave formulas. Consequently, as a matter of logic the evidence for Malus's formulas cannot carry forward to the wave theory of partial reflection; nor can the numerical differences between the two sets of formulas, or between measured observation and Malus's formulas, be taken to have any physical significance within wave theory. Historical documentation drives this point home: in a situation where Malus claimed on theoretical grounds that there had to be a little light, Fresnel, thoroughly puzzled by Malus's claim, insisted that there had to be none except what was due to instrumental imperfections. Hence, Malus construed as evidence data that Fresnel later dismissed as experimental artifact.

3.2 Biot on Chromatic Polarization

Chromatic polarization was discovered by Arago in 1811 during the course of his investigation of the polarization of Newton's rings. Having inserted a thin slice of the crystal gypsum within his polarimeter, he found to his surprise that the lower mirror reflected colored light, with the color depending upon the orientation of the gypsum slice in its own plane. Following selectionist reasoning, Arago proposed that gypsum, and as it turned out thin slices of other crystals as well, rotates the polarization through an angle that depends on the ray's color. He was not, however, able to develop, nor perhaps did he try to produce, a quantitative law. That remained for Biot, whom Arago felt intruded into this new area in an unfair manner. Biot was concerned, among other things, to show that Arago's new discovery did not cast any doubt on the continued viability of other claims concerning Newton's theory of color, including the far-from-universally accepted color circle. Biot immediately sought to generate a quantitative law, and he succeeded in developing an elaborate, fully quantitative account that (like Arago's qualitative proposal) depends upon the selectionist grouping of rays by their asymmetries and colors. In Biot's scheme, subsets of rays within a crystal lamina are grouped first in respect to their common asymmetries and then by common color. The full implications of Biot's formulas, however, just like those of Malus, could not at the time be tested except in extreme cases, because intensity could not be measured and because there was no widely accepted way of calculating the color that results from the combination of rays of different basic colors.

Biot obtained his formulas by creatively applying the selectionist principle of ray individuation to experiments in chromatic polarization. He, quite unlike Arago, specifically sought to isolate the rays whose asymmetries are affected by the thin crystal lamina, and thereby to quantify just what happens to them. After several attempts that did not stand up to experiment, Biot produced the following formulas, which worked very well in characterizing extreme situations:

$$O = U\cos^2 a + A\cos^2(2i\text{-}a)$$

$$E = U\sin^2 a + A\sin^2(2i\text{-}a)$$

where O and E are the respective intensities (or, in selectionist understanding, numbers of rays) of the "ordinary" and "extraordinary" light emerging from the double-refracting crystal; U and A are the ray numbers in the two subsets by asymmetry that (according to Biot's hypothesis) are formed in chromatic polarization; i and a are respectively angles of incident polarization and crystal lamina orientation; and subset U has the incident polarization (i), and subset A has polarization $2i$ with respect to the polarization of U.

The essential idea behind these formulas is simple. Polarized light passes through a thin crystal lamina and is then received by a double-refracting crystal, which will split the light into O and E parts, as crystals from the time of Huygens had been thought to do. The principal section of the receiving crystal forms the angle a with respect to the plane of incident polarization, which itself lies at angle i with respect to the optic axis of the thin lamina. According to Biot's account, within the lamina the light is divided into two subsets, each of which has a specific polarization. One subset, U, retains the polarization that the light had before it entered the lamina—it is not affected at all. The rays in the other subset, A, have their asymmetries flipped to the other side of the lamina's optic axis, which is to say that the polarization of subset a forms the angle $2i-a$ with respect to the receiving crystal (and of course $2i$ with respect to the common polarization of U and the incident beam). Since the incident beam is fully polarized, all rays of a given color must lie entirely within one of the two subsets—there cannot, for example, be red-making rays in both U and A since there was no physical distinction between red-making rays at their incidence on the lamina. Without elaborate experiments, however, which Biot never did perform, we cannot tell what subsets by color compose U and A.

Fresnel's formulas for chromatic polarization can be put into the same general form as Biot's:[12]

$$O = [\Sigma_\lambda \cos^2(\pi(e\text{-}o)/\lambda]\cos^2 a + [\Sigma_\lambda \sin^2(\pi(e\text{-}o)/\lambda]\cos^2(2i\text{-}a)$$

$$E = [\Sigma_\lambda \sin^2(\pi(e\text{-}o)/\lambda]\sin^2 a + [\Sigma_\lambda \sin^2(\pi(e\text{-}o)/\lambda]\sin^2(2i\text{-}a)$$

Here, however, we have explicit expressions, in fact infinite sums, for Biot's ray sets U and A. The sums are taken over all wavelengths in the incident light, and e-o is the path difference between the two waves with mutually orthogonal polarizations that are produced by refraction within the crystal lamina. In Fresnel's case there is no sense at all in speaking of

12. See Buchwald (1989, p. 247).

the original light having been divided by the lamina into two subsets, one of which has its polarization affected, while the other does not. Quite the contrary, on Fresnel's theory the thin lamina behaves precisely like a doubly-refracting crystal usually does, which means that the light within it can be thought to consist of two beams, one of which is polarized along the lamina's optics axis, whereas the other is polarized orthogonally to it.

If it were possible to map Fresnel's beams to Biot's subsets, then *both* of Fresnel's beams would have to be rotated in polarization with respect to the original beam, the one through an angle i and the other through an angle $i+90$ degrees. Neither angle corresponds to either of Biot's requirements, and this difference is a direct reflection of the conceptual incommensurability at the deepest level between Biot's account, which individuates rays, and Fresnel's, which does not. Fresnel's factors as a matter of fact result entirely from the particularities of interference and have no meaning at all in respect to ray individuality. This difference between Biot and Fresnel could show itself if the emergent light were operated on with other kinds of devices that manipulate the phase of the light.

Biot responded to Fresnel's apparent deduction of expressions for his U and A—which is what Biot insisted Fresnel had done—by arguing that Fresnel's expressions do not work empirically with respect to the kinds and saturations of colors they entail. Biot's argument at this point did not refer specifically to chromatic polarization, but rather to the tints produced in the phenomenon of Newton's rings, which involve reflection or transmission. Asserting that Fresnel's trigonometric factors apply just as well to the rings as to thin crystal laminae—a point on which Fresnel certainly did not disagree, since on wave principles they do—Biot attacked their implications here. In effect what he did was to take a given air gap and then to multiply it successively by factors corresponding to a group of wavelengths that match (in Biot's choice) the colors arranged around the rim of Newton's color circle. Calculating the resulting set of factors, Biot would then use each as a weight to be applied in the usual fashion along the circumference of the circle, placing each weight at the center of the corresponding color. The result would give both the color of the mix, and its degree of mixture with white light (in later parlance, its saturation). This is what one ought to see, according to Biot, in Newton's reflection rings at that thickness. But, he argued, this result is not correct for, in particular, the saturation of the compound light was in several cases considerably different in calculation from what it should be according to Newton's table of tints. Although there was no direct way to measure this, beyond saying that light appears weakly colored in situation x as compared to situation y, nevertheless Biot obtained considerable differences here (implying, e.g. in the case of a gap thickness of 11 and 1/6 millionths of an English inch, that the resultant light consisted of 16 parts violet mixed with 9 parts white, whereas in Newton's table the light here should be nearly pure violet).

Biot's argument had two major flaws, both of which Fresnel pointed out. First, it required use of Newton's color circle, and there was great controversy surrounding its employment, not least because there was no consensus concerning the circle's ability to capture many naturally occurring colors. This leads to the argument's second, and in fact central, flaw. Fresnel had observed his own uncle, Léonor Merimée—at one point head of the Ecole des Beaux Arts and author of a Treatise on oil painting—give very different names to colors than Biot would give, indicating wide variation in chromatic nomenclature—and accordingly in the appropriate kinds of colors naturally present. Conclusions about colors that had perforce to be drawn from use of the color circle in conjunction with Newton's table of tints were hardly compelling when agreement about the very colors involved was so labile. As Fresnel remarked in counter-argument to Biot, "It would not seem to me to be reliable to use this construction [the color circle] to judge in the final instance the correctness of a formula that gives the intensities of simple [homogeneous] light, in relying on a table whose perfect accuracy has not yet been demonstrated, and whose terms [*viz.* the named colors] can be differently interpreted by different observers, according to their manner of sensing and naming the colors" (Fresnel 1866, I:602)

Biot's challenge was aimed directly at Fresnel's claim to have provided formulas for calculating intensities (*viz. U* and *A*) that in Biot's scheme remained uncalculates. Of course, an infinite number of different possible expressions for these factors would all be compatible with the specific requirements of Biot's system for chromatic polarization, so that on this point nothing that Fresnel offered directly contradicted any of Biot's claims. Biot accordingly chose to counter Fresnel's claims by displacing the argument on uncontested grounds to another phenomenon (Newton's rings) and then claiming that a computational device (the color circle) leads to results concerning color saturations that are in conflict with the requirements of Newton's table of tints. Here we see that Biot was trying to draw on evidence from another regime—color calculations—to claim that Fresnel could not accommodate it. Fresnel took the obvious step—aided considerably by his uncle's painterly knowledge—to undercut the evidentiary stability of Biot's device.

We may put it this way. Biot had formulas that Fresnel could completely reproduce, with the addition of a specification of the forms of factors that Biot had left uninterpreted. This move apparently parallels the replacement in the case of Boyle's law of the reciprocal of volume by density: we may say that the original formulation was missing a factor representing the mass contained in the volume, though in the absence of specific prohibitions on Boyle's part nothing forbade that factor, or many others for that matter, from being present. Similarly, nothing in Biot's original analysis forbade his U and A from having the forms given to them by Fresnel, or many other forms as well (subject to the constraint that $U+A$ must add up to the original optical intensity). Biot nevertheless rejected Fresnel's suggested refinement because he knew perfectly well that the trigonometric dependencies obtained by Fresnel did not easily fit the conception of sets of discrete rays that underpinned his own U and A, which at least suggested a much more abrupt change than Fresnel's trigonometric forms.

Did the evidence for ray theory carry over to wave theory? Chromatic polarization presents us with a more complicated situation in this regard than did partial reflection. The sole *prima facie* grounds for suggesting that evidence supporting Malus's formulas for partial reflection might carry over to Fresnel's—which were entirely different in form—was their near numerical agreement; and this was then dismissed on the grounds that, because there is no way of deriving Malus's formulas from Fresnel's, whether as a limiting case or as reflecting some restricted physical situation, the agreement in question was a mere numerical coincidence, totally lacking physical significance. Because Biot's formulas for chromatic polarization are of the same general form as Fresnel's, however, and Biot took Fresnel to be proposing a way of quantifying his U and A, the question of carry-over of evidence requires a slightly more nuanced answer. By virtue of their common general form, both Biot's and Fresnel's formulas make the same claim about the systematic dependency of the intensities O and E on the angles of incident polarization and crystal orientation. Obviously any evidence bearing purely on Biot's statement of this dependency therefore had to carry over to Fresnel's statement of it. In particular, Biot's discussion of the extreme situations to which he was able to apply his formulas carries over perfectly well to Fresnel's expressions. Both, for example, imply that the ordinary and extraordinary beams in the analyzing crystal can be equally intense only if both are also white, and *vice versa.* Indeed, any observable implication of Biot's formulas is also one of Fresnel's, and *vice versa,* provided that we do not employ Fresnel's specific expressions for the factors in his formulas, but only require that they sum to unity.

The carry-over from Biot to Fresnel stops there, however. The quantities U and A in Biot's formulas represent the number of rays in two disjoint subsets demarcated by a contrast in asymmetry. In order for Biot to obtain appropriate expressions for the number of rays by color in each of these sets, he would have to have carried out photometric experiments with homogeneous colors corresponding to the ones on the periphery of Newton's color circle. Because photometric comparators were not available, he could not do so. But suppose they had been available, what then? Biot might have obtained expressions that could be used to compute resultants from the color circle, but—and this is the key point—he would have to have redone these experiments for every laminar thickness. There would not have been any way for Biot to obtain generally applicable expressions, whereas Fresnel's applied to thicknesses over which coherence holds. Consequently, there was no possible means for Biot to achieve Fresnel's level of generality.

Moreover, from the point of view of wave theory, expressions obtained in this fashion, had they worked at all, would have had to do so as a result of pure numerical coincidence, much as it was pure coincidence according to Fresnel that Malus's formulas for partial reflection worked as well as they did. Specifically, operating in Biot's fashion with homogeneous light presumes that this light is divided into two parts, one unaffected in polarization, and the other rotated through a certain angle. Fresnel's system denies this very possibility altogether, and hence it rejects as invalid the foundation of Biot's computation. There are accordingly no physical circumstances in which values assigned to U and A can serve to quantify, even approximately, the summations over wavelengths in Fresnel's formulas. The relationship between U and A and the two summations is thus not at all like the relationship between *volume, density,* and *mole density* in the different versions of the gas law. No measurement process for assigning values to Biot's quantities can be physically interpreted or recast as yielding a measure of Fresnel's summations.

To summarize, then, the only evidence for Biot's formulas that could have carried over to Fresnel's would involve experimental circumstances in which there was no basis for taking the values assigned to U and A to be measures of the numbers of rays in two differently-polarized subsets, only one of which (A) has been affected by the birefringence of the crystal lamina. This, however, would be evidence only for the general form of Biot's formulas, and not for the claim about the polarizations of the light incorporated in them. No evidence for this latter claim could have carried over to Fresnel's formulas because wave theory explicitly contradicted Biot's claim concerning the polarizations of his light within lamina. In Kuhn's terms, no translation could be carried out between the two lexicons.

3.3 Contrasting Taxonomies

The inability to translate between the lexicons used by Biot and Fresnel in characterizing chromatic reflection is not the only symptom that Kuhn could have invoked to argue that ray and wave optics were incommensurable. The differences between the two run deep, for they concern the character of their fundamental objects in relation to space, time, and individuation. Fresnel's wave fronts are not composed of the same material stuff as they move along, but are instead specified entirely by their location as curved surfaces in space and by the wavelengths, phases, and amplitudes of the oscillation at this surface. Fresnel was able to develop a mathematical system for working with these quantities that enabled him to determine the resultant amplitude (and hence intensity) when two (coherent) fronts with the same wave length meet. And, as we have seen in the last two sections, he could then extend that mathematical system to other phenomena.

Selectionist rays are quite different from wave fronts. To begin with, they retain their physical identity over time. They are, moreover, not curved surfaces, but lines in space. These lines have some sort of asymmetry about them as axes, and they can otherwise differ from one another with respect to their color-making property. Intensity does not pertain to individual rays, as it does to the amplitude at a point of a front in wave optics, but rather to the number of rays in a beam. So, for example, a ray's intensity-making power is fixed, constituting as it were the unit for intensity-counts over sets of rays, whereas the amplitude at a point on a front can have any magnitude.

These differences between rays and wave fronts have specific instrumental consequences. Selectionist polarization is completely determinable by means of a polarimeter, which is taken to detect beam asymmetries, for there is nothing else to measure. Wave polarization, on the other hand, cannot be determined solely by a polarimeter, which detects only a wave's amplitude in a given direction, but requires in addition a device that can detect the wave's corresponding phase. Direction-sensitive intensity detectors accordingly exhaust the instrumental requirements of ray optics, but not of wave optics, which requires phase detectors as well. These contrasting views of instruments cannot help but recur in the physical interpretation of what is being measured and how imperfections in that process of measurement can introduce artifacts and limitations in accuracy of measurement. These differences are sure to surface when some value for a parameter measured by means of one phenomenon is carried over into reasoning about other phenomena. As elsewhere in physics, the theory of measurement cannot remain neutral with respect to competing theories of a domain as the theories are extended to cover a wide range of phenomena within that domain.

Experiment indicated to ray scientists that their selectionist (or S) light could be sorted by a polarimeter into three distinct kinds: the polarized, the partly polarized, and the unpolarized. Wave (or W) light could also be sorted by a polarimeter into three kinds: the linearly polarized, the elliptically *or* partly polarized, and the circularly *or* unpolarized. These kinds were distinguished among one another conceptually, with a corresponding instrumental realization. Linearly polarized S-beams had the asymmetries of their component rays entirely (or nearly) aligned in a single direction; in partially-polarized S-beams the ray asymmetries divided into subsets with different alignments; and unpolarized S-beams had completely random asymmetries. In the polarimeter, these kinds of beams behaved differently: linear beams could be completely annulled at a particular orientation of the plane of reflection; partial beams had mutually orthogonal maxima and minima; and unpolarized beams exhibited no intensity asymmetries on reflection. Ray theory and practice required nothing more to characterize a beam's state of polarization.

The situation was considerably different on the wave account. Wave (or W) light could also be sorted by a polarimeter into three kinds: the linearly polarized, the elliptically *or* partly polarized, and the circularly *or* unpolarized. And each of these kinds behaved in the polarimeter just like ray theory's respective linear, partial, and unpolarized light. But wave optics in fact bracketed both elliptical and circular with fully *polarized* light. Elliptical and circular light accordingly have a close mutual relationship as sub-kinds of polarized. To detect these differences among kinds of polarized light, and to tell the difference between elliptical and partial, and between circular and unpolarized, requires a device that, in wave parlance, alters the phase difference between mutually-orthogonal projections of the wave vector: if the light has a phase (that is, if it is polarized), and the phase difference is zero, then the polarization is linear regardless of the ratio between the component amplitudes (which determines the angle of polarization); if the phase difference is non-zero and the amplitudes are different in magnitude, then the polarization is elliptical (with the angle of the ellipse's major or minor axis with respect to the plane of reflection determined by both the phase and the amplitude ratio); and if the phase difference is non-zero and the amplitudes are equal, then it is circular. Reflection of polarized light within a glass prism—or Fresnel rhomb—can effect the phase change, which would then be detected by a polarimeter when the light emerges from the prism: if, for example, light that tested as partial or unpolarized (in ray terms) by the polarimeter before entering the rhomb then tested as linear on exit, this meant that the entering light had been (respectively) elliptically or circularly polarized.[13] Reflection processes proved not to be the only ones that could effect phase changes, and so it is best to refer generically to instruments that can detect the sorts of differences that ray theory has no knowledge of as *phase detectors.*

Ray theory's light, in contrast, does not distinguish between elliptical and partly polarized, or between circular and unpolarized, and so a phase detector has no immediate place in S optics. As Figure 2 summarizes, W light must therefore violate Kuhn's no-overlap principle with respect to the polarized kinds of S light, for light that wave theory considers to be polarized (namely, elliptic) is considered by ray theory to be partly-polarized, and other light that wave theory considered also to be polarized (namely circular) is considered by S to be completely unpolarized. Suppose now that the two theories develop generalizations across their respective kinds of polarized light. The conflicts in the domains of these generalizations cannot help but prevent the evidence for any such generalization within ray theory to carry over to any generalization within wave theory.

3.4 Brewster on Elliptic-Polarization

Although Fresnel had discussed the possible existence and character of elliptically-polarized light, and though it had also been discussed by John Herschel, it ironically was a convinced selectionist, Brewster, who first produced it experimentally in light reflected from metals (announced in

13. It would be historically incorrect to characterize these numerical differences for phase and amplitude (zero or non-zero for phase, equal and unequal for amplitude) as implying that the types of polarization were among wave practitioners considered to be distinguished merely by the value of numerical parameters—even though in terms of mathematical representation that was indeed the only difference. The reason goes to the heart of wave optics as an e*xperimentally-based* science: each type of light had generic effects in specific kinds of empirical situations, and these situations were sufficiently different among one another that wave scientists distinguished among them as kinds. Indeed, it is likely the case that scientific kind-structure in general is closely bound to a particular universe of devices. Suppose, e.g., that the linear-elliptical-circular at some point in time becomes instrumentally unimportant because we are able to invent a device that can give a numerical read-out for phase, and suppose further that effects which had been very important in the early history of wave optics (such as Airy's investigation of birefringence in quartz, which involves a most complicated form of elliptical polarization) themselves become of purely historical interest. Then the textbooks of optics would probably not divide light into the old categories, but into new ones that better reflected the existing universe of significant apparatus and effects—though they might revert to the old terminology when discussing the old configuration. This does not entail that the old categories were in any sense arbitrary, though they are certainly *relative*—not to the principles and procedures of wave optics, but to the relevant universe of devices at a given time.

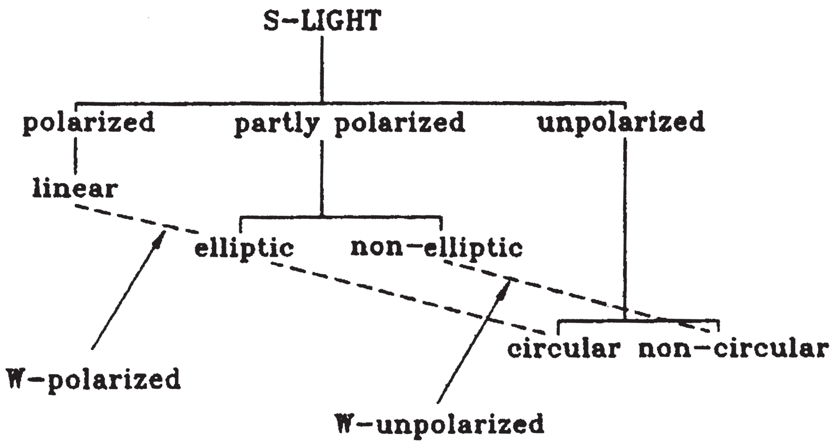

Figure 2. The Incompatibility of the Wave-Theoretic Taxonomy with the Selectionist Ray-Theoretic Taxonomy

1830, three years after Fresnel had died). Malus had long before discovered that light reflected from metals shows partial polarization. Brewster further found that linearly polarized light that is partially-polarized by a metallic reflection could, after further metallic reflections, be transformed back into linearly-polarized light. This was precisely analogous to the effect of internal reflection within glass rhombs on originally linear light, and Brewster knew perfectly well, if only by analogy (since he had not fully mastered the concepts and apparatus of wave optics), that it could be handled in a similar fashion. This, however, exacerbated the already existing quandary posed for ray practitioners like Brewster by circular light: namely, how could both the new light and circular be distinguished in a way that would not violate ray categories from, respectively, the partial and unpolarized?

Precisely because of elliptically—and circularly—polarized light's violation of the selectionist taxonomy, Brewster was forced to neutralize the threat by creating an entirely new distinction between normal light, which divides as always on ray principles into the polarized, partly-polarized, and unpolarized, and a parallel category that he referred to as "curious light." This he divided into two sub-kinds, complete and partial, with the complete sub-kind further subdivided into elliptic and circular. These are to be distinguished among one another on the basis of a specific kind of experimental process—they are indeed effectively *defined* by that process.

Brewster developed an associated mathematical apparatus by using formulas that he drew from Herschel's account of Fresnel on phase in order to predict how many reflections are necessary to effect these transformations. The basis of Brewster's theory lay squarely within the arena of ray optics, but one that deployed within it formulas and parameters that derive from wave optics and that have no conceptual seat within the ray system. His scheme was quite complicated, and tightly bound to reflection processes. It worked by dividing a beam into disjoint sets of rays, each such set having its own characteristic polarization; to each of these Brewster applied formulas that Fresnel had derived on wave principles for determining the angle of rotation of linearly-polarized light by reflection—formulas that had no meaning within wave optics for any putative components of such light. Brewster then developed a creative adaptation of Fresnel's mathematics for phase that enabled him actually to calculate values for the 'phase', in his words, "of the two inequal portions of oppositely polarized light, by the interference of which the elliptical polarization is produced." He could then produce tables that used this 'phase' to predict the number of reflections necessary at a given incidence to go from linear polarization back to linear polarization by metallic reflection.

On Brewster's view such a re-transformation was the sole phenomenon of interest in which "curious light" entered. The phase of Brewster's curious light was accordingly tied directly to processes of reflection. This meant that Brewster's phase could not easily be generalized to cover a family of phase devices. By contrast, Fresnel's phase could be so generalized, for it was not tied *ex definitione* to any particular instrumental process in the way Brewster's was tied to reflection.[14] We remarked earlier that ray

14. This is not to imply that Brewster's system was empirically adequate even when limited just to the reflection processes for which he had built it. Not at all—for there was an inherent problem with Brewster's creative adaptation of the Fresnel formula governing the rotation of the plane of polarization of light on reflection. Brewster had willy-nilly applied this formula (in complete violation of wave principles, but with reasonable rationale on ray grounds) to the major axis of elliptical light produced by several reflections in respect to the azimuth of the original linear polarization. This enabled him to calculate an effective 'index' for the metal, which he needed in his phase expressions. However, Brewster had actually used the presumed formula only under the degenerate circumstance that the resultant elliptical light is maximally polarized in the plane of reflection, and here the formula works well numerically in providing an empirically useful 'index'. It should also work in non-degenerate circumstances, on Brewster's account, but calculations from his own formulas show unequivocally that his scheme would fail, and fail markedly, under non- degenerate circumstances. Had he proceeded further, or had someone picked up his system—which no one did, because ray practitioners were few and far between by then—then the problem would rapidly have surfaced.

optics gradually died off, not because some decisive experiments refuted it, but largely because wave optics was proving so much more fruitful in ongoing research. The contrast between the ease with which Fresnel's phase could be generalized to cover a family of phase devices and the difficulty in correspondingly generalizing Brewster's phase is an example of how wave optics ended up proving so much more fruitful in ongoing research.

Although Brewster had used Herschel's version of Fresnel's formulas for elliptically polarized waves to develop his working mathematics for the polarization phenomena produced by metallic reflection, there is no way to derive Brewster's mathematics from Fresnel's formulas, either directly or by defining a special limited situation within wave optics in which Fresnel's formulas reduce to Brewster's. The relationship between Brewster's mathematics and Fresnel's is thus not like that between Biot's and Fresnel's, where commonality of form complicates the question of whether evidence for one carries over as evidence for the other. Moreover, while *phase* is a crucial quantity in both Brewster's and Fresnel's mathematics, *phase* for Brewster is tied strictly to metallic reflection, and even there cannot be mapped into a special case or limited use of *phase* in Fresnel's mathematics for elliptical polarization. Accordingly, while Brewster could use phenomena of metallic reflection to assign values to *phase,* the values in question do not coincide with or approximate the appropriate values of *phase* with respect to these phenomena in wave optics.

Still, the most important use to which Brewster put his mathematics was to derive the number of metallic reflections required to transform linear light back into linear light by a sequence of metallic reflections (any one of which would by itself have made the original polarization appear to be partial). And here the success of his formulas did provide evidence for his account (provided that the reflection used to calculate the necessary 'index' for the reflecting metal occurs under degenerate circumstances). Fresnel's formulas entail *precisely the same results* for this number of reflections.[15] Because Brewster's formulas are not structurally related to Fresnel's formulas, and *phase* in Brewster's formulas does not physically amount to a special case of *phase* in Fresnel's, this evidence supporting Brewster does not carry over to wave theory. From the point of view of wave theory, the evidence provided by success in determining the number of required reflections is a mere curiosity that was made possible by segregating this one phenomenon from all the others in optics.

15. To apply Fresnel's formulas here requires the assumption that the index of refraction in metals is complex. This was first accomplished by Augustin-Louis Cauchy in the 1830s. See Buchwald (1985) Appendices 8 and 9.

3.5 Geometric Optics

Selectionist ray optics emerged as an extension of geometric optics in response to newly discovered phenomena of polarization early in the nineteenth century. As such, it was virtually contemporaneous with the emergence of Fresnel's wave theory, and hence little time elapsed during which a substantial body of evidence could accumulate for selectionism independently of developments in wave optics. This is just another way of saying that there was not all that much evidence supporting the selectionist claims within ray optics that wave optics had to deal with in one way or another. By contrast, geometric optics had developed for centuries, with extensive evidence deriving from phenomena of reflection and refraction. Indeed, at the time of Fresnel the most compelling evidence that selectionists could offer for their ray optics was the huge body of evidence justifying ray-based mathematics in geometric optics. In this regard the burden of proof on wave optics to somehow accommodate the evidence for geometric optics was closely akin to the burden of proof on relativistic physics to accommodate the evidence for Newtonian mechanics and gravitation.

Geometric optics constituted a natural subdivision in selectionist optics because it is essentially the science of a ray's path in reflection and refraction. Geometric as a subdivision of selectionist optics is not limited instrumentally in any significant way. It applies to reflecting and refracting objects, however small they may be (or at least until they dissolve into their constituent particles).

Although the ray *per se* is not an object in wave optics, nevertheless the beam, considered as an orthogonal trajectory of an element of the wave front, is. The ray of geometric optics can be retrieved as a wave optics beam by considering a small enough element of the front that the ratio of the wavelength to the smallest part of the reflecting or refracting device is much less than can be measured by whatever device receives the light. As we remarked earlier, some 150 pages of Born and Wolf's *Principles of Optics* is devoted to showing how the various long-established theoretical results of geometric ray optics can be thus recovered within wave optics. Fresnel himself, as well as most nineteenth century practitioners of wave optics, spent little time on this point, because they thought that what pertained to rays in non-wave thinking actually applied empirically to beams (since only beams could be detected); and in wave optics beams are not collections of rays, but the spatial trajectories of delimited regions on the wave front—that is, *surface elements.* In effect, wave optics does not recognize that rays in the sense of linear trajectories have any physically meaningful existence at all; only *beams,* construed as surface-element trajectories, do. Whereas in ray optics the beam has physical meaning precisely as a collection of rays. And so long as the element of the front is small enough in relation to the dimensions of a device that intercepts, reflects, or refracts it, there will be no instrumentally detectable diffraction. In this case the evidence for geometric optics carries over directly into wave optics, for under these conditions the wave optics beam obeys the same laws as the selectionist optics ray with respect to path changes in reflection and refraction.[16]

Although the differences between classic geometric optics and its reconstruction within wave theory did not make so momentous a contribution to evidence as the differences between the ideal gas law and the virial expansion did, they did make a contribution. According to classic geometric optics, the smaller the diameter of the lens, the less the effects of spherical aberration. This has the practical implication that one should try to make the aperture of a microscope as small as possible, consistent with the need for sufficient light to yield a clear image (which in principle can be achieved by added illumination of the object under examination). But under the wave theory reconstruction of geometric optics, there is a minimum aperture size below which detail is lost regardless of the illumination of the object because of diffraction. In the 1870s Ernst Abbe used this fact to design improved objective lenses for microscopes (Feffer 1996).

The retrieval of geometric from wave optics is similar in one respect to the recovery of Newton's second-law from special relativity. The latter requires choosing a limiting condition, namely that of zero velocity, for those circumstances in which finite velocities have no instrumentally significant effects. Similarly, to retrieve geometric from wave optics requires the limit-case assumption that the ratio of wavelength to the dimensions of any optically active device must be nearly zero, or rather that the ratio must be sufficiently small in respect to the objects that act on the light that diffraction cannot be detected. In another sense, however, the two situations are quite different. With respect to the motion of material objects, special relativity just alters the laws of mechanics, providing specific replacements for them. It does not utterly transform the character of the underlying objects, or at least not in ways that are particularly relevant to problems of motion. *Mass* for example does not disappear from the equations of special relativity, though it acquires new properties that it did not previously have. The situation differs in optics, and perhaps the best way to understand the difference is to consider what optics might have been had diffraction operated in a different way.

16. Ray practitioners, including Poisson, resisted this transformation of a fundamental physical object, the ray, into an entity whose significance depended markedly on its physical context (the beam).

Suppose that in diffraction a ray of light is deflected from its linear path by passing near the edge of a material object. This is in fact just how Newton did think about the phenomenon. Imagine now a beam—in the ray optics sense—of purely homogeneous, linearly-polarized light, so that all of the rays in it are not in any physically important way distinguishable from one another. When passing near an edge, all of these rays would accordingly be deflected through very nearly the same angle. If we put a screen past the edge, we will find that the beam hits it at a point that does not lie directly along its path before passing the edge: the beam has been shifted, *but it has not otherwise been affected.* If diffraction operated in this way, then we might be able to build a system in which not only are the laws of geometric optics retrieved in a certain limit, but also there is no significant alteration required in the fundamental character of our underlying physical objects. We would still have rays in much the old sense, forming beams, only now they will not follow the direct paths they used to when sufficiently close to a material edge. This difference between ray optics with and without diffraction is certainly much smaller than the one between Newtonian mechanics and relativity, but it does seem closer to that difference than the distinction between ray and wave optics. Because in wave optics we must not just *deflect* an otherwise unaffected object (the beam of light); instead, if we wish to retain a meaning for the beam in circumstances where diffraction is efficacious, we must *multiply* it into a family of beams. Wave optics instead abandons the concept of *beam* altogether in such circumstances, relegating its significance entirely to situations in which it works well instrumentally.

4. Concluding Remarks

Our conclusion about the carry-over of evidence in the case of geometric optics brings out the principal lesson the transition from ray to wave optics offers on the subject of incommensurability. Without any question, the transition from ray to wave optics early in the nineteenth century involved what Kuhn would have originally called a revolutionary paradigm shift. Wave optics requires an entirely different way of thinking about light. Its conceptual framework could never have arisen through a sequence of small changes starting from the conceptual framework of ray optics. The conceptual discontinuity across this transition, however, does not automatically entail an across-the-board discontinuity of evidence. The situation is much more complicated than that. In the case of classic geometric optics, not only did the evidence carry over as evidence for wave optics, but the evidential reasoning developed in support of geometric optics carried over as well, under a new, more thoroughly spelled out interpretation of what a ray amounts to empirically in geometric optics. To put the point differently, careful logical analysis shows that the concept of a ray in the evidential reasoning within geometric optics can be stripped down to requiring nothing more than that some small portion of light remain fully individuated and geometrically identifiable through the relevant optical activity. Beyond this, the question of what a ray is was in effect left open in this reasoning. We say "in effect" here because we do not want to deny the heuristic value of further features attributed to rays. The point is that the full force of the evidential reasoning remains under this reconstruction of what a ray is, and hence this reconstruction is sufficient to recover the evidence. What wave optics did was first to add a non-geometric principle of individuation and second to indicate the conditions under which rays, so individuated and geometrically identified, remain intact in certain optical activity. By contrast, the evidential reasoning within early nineteenth century ray optics on polarization phenomena is not recoverable in wave optics because rays so individuated do not remain intact through the relevant processes. In other words, wave optics nullifies this evidential reasoning in ray optics on the grounds that the reasoning involves a premise that, even when stripped down to bare essentials, not only fails to hold, but fails to hold even approximately. The question of continuity of evidence comes down not to scale of conceptual change, but instead to often subtle details in the logic of the evidential reasoning.

This conclusion in no way denies Kuhn's claim about the importance of "the fundamental conceptual readjustment required of the historian to recapture the past or, conversely, of the past to develop toward the present." Nor does it in any way deny Kuhn's central claims about such things as the role of exemplars in determining scientific practice, the revolutionary effects of changes in exemplars, and the extent to which such revolutionary changes have occurred. All that we are arguing is that the question of commensurability of evidence across revolutionary changes in conceptual structure is far more intricate than the picture offered in *The Structure of Scientific Revolutions* suggests. In the long passage we quoted earlier, Kuhn spoke of the way symbols designating such physical magnitudes as *force* and *mass* "attach to nature." He always emphasized the role of the law-like relations among these physical magnitudes in determining how these symbols attach to nature. We agree with Kuhn about this as well. All that we are adding is that changes in the mathematical form of these law-like relations are not by themselves enough to make the evidence before discontinuous or incommensurable with the evidence after a revolutionary shift in conceptual structure. The question of the commensurability of evidence before and after turns on the specific ways in which the law-like relations in question entered into the evidential reasoning before the revolutionary shift, for it is through this evidential reasoning that the symbols became attached to nature. But then the relationship between the normal-scientific traditions before and after the revolution can be extremely complicated, with continuity of evidence in some places, discontinuity and hence incommensurability in others, and various gradations between full continuity and full discontinuity in still others. Claims about incommensurability will have to be adjudicated only through a detailed historical examination of the science before and after the revolution.

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