To What Physics Corresponds*

In what follows I wish to reconsider certain ideas in Post's (1971) defense of the 'retentionist' or 'accumulativist' view of science. In particular I shall focus on heuristics and methodology and will confine discussion to physics, especially theories of *dynamics* (this, I hazard, is to be counted a constraint in principle: it seems unlikely that similar considerations will apply to any other branch of empirical science). Post's thesis (what he calls the "generalized principle of correspondence") is both historical and methodological; it may be simply put as the claim that what is taken over from preceding theories is not only those laws and experimental facts which are well-confirmed, but also 'patterns' and 'internal connections', that in this way the successor theory accounts for whatever success its precursor enjoyed, for it "… will in fact embody a good deal of the (lower) theoretical structure of the [precursor] theory" (1971, p. 229).

By 'dynamics' I mean to include statics and kinematics, as well as mechanics and field theory. The 'constraint in principle', as I understand it, is that in no other field does one see so powerful and interplay between mathematics and phenomenology, and only in mathematics has one the resources to elaborate a notion of 'patterns' and 'internal connections' that is something more than the generic concept of metaphor. For these reasons I shall further consider only those dynamical theories that achieved and internally consistent, systematic and highly mathematical formulation, with a substantive and well-confirmed body of quantitative applications (with the exception of astronomy and statics, we are therefore limited to the Modern period). My principal target is the 'anti-accumulativist (or 'anti-retentionist') consensus that has, by and large, replaced the traditional reductive account of inter-theory relationships that we owe to the positivists. This consensus appears a haphazard and perhaps temporary convergence of a number of themes in contemporary metaphysics and epistemology, ranging from social constructivism and historicist epistemology to linguistic holism and anti-realism. Correspondingly, those who have most vigorously championed the 'anti-retentionist' view of scientific prog ress come from widely different traditions (consider Kuhn, Feyerabend and Laudan). For convenience, however, I shall refer to this view as 'relativist'. It is my contention that relativists and realists alike have overlooked important and, in comparison to other disciplines, quite atypical features of dynamical physics.[1] I has

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* (Note added June 2025. First published in *Correspondence, Invariance, and Heuristics; Essays in Honour of Heinz Post*, S. French and H. Kaminga, (eds.), Springer, p.295-326 (1993). It was written in 1992. I changed my views in certain respects since (most notably, regarding the Everett interpretation, that same year), and it is clearly somewhat dated, but as an early foray into what came to be called structural realism – even, into its present 'maths first' formulation -- it may remain of interest, at least to those with an eye for the history of the subject. I dedicated it to Heinz Post, in his 75th year, and I rededicate it to him now: without Post and the department he created at Chelsea College, London, in the 1970s, philosophy of physics would scarcely have existed in the UK, in the last century.)

[1] The so-called 'scientific realism' of recent decades has proved largely ineffectual in the face of the challenge form relativist and pragmatist views as diverse as Fine, Laudan, Cartwright, Hacking, Rorty and Putnam. Laudan's (1981) onslaught against 'convergent realism' appeared particularly damaging to the arguments, based on a retentionist view of theory change, to be found in (Boyd

long been apparent (to physicists if not to philosophers) that we do not really understand the role of mathematics in dynamical theory, that there is something more herein than a 'mere' instrumental utility or economy of expression.[2]

II

Kuhn and Laudan acknowledge that mechanics is exceptional in the continuity of its development, but go on to isolate this 'branch' of science (but it is not any old branch) from quantum theory, spacetime theory, and gravity, as though it may be accounted some sort of recherché preoccupation of the mathematicians in the late eighteenth and nineteenth centuries, outside of the mainstream of scientific advance. Newtonian theory, on the other hand, is treated on a par with the crude and figurative mechanical schemes of Galileo and Descartes. Thus Laudan (1981) cites "the theory of circular inertia" as a counter-example to the correspondence thesis (but this was not and internally consistent and systematic mathematical theory, still less one that achieved detailed empirical support), and considers the later history of mechanics a "rare occasion" on which the thesis of correspondence might prove justified.

Laudan's sights were set on more sweeping claims of 'convergence' of scientific theories, in the service of a still more resounding argument to successful reference, realism and the 'approximate truth' of contemporary theory. It is in connection with the more restricted claim that "in mature science" there is evidence that "mechanisms, models and laws" are preserved in theory-change, particularly within the developed theories of physics, that one is less than convinced. His examples (from physics) where (he claims) correspondence *fails* are these: Copernican astronomy vs. Ptolemaic astronomy, Newton's physics vs. Cartesian mechanics, astronomy and optics, Franklin's electrical theory vs. that of Nollet, relativistic physics vs. the ether and "the mechanisms associated with it", statistical mechanics vs. "the mechanisms of thermodynamics", the wave theory of light vs. "the mechanisms of corpuscular optics".

One wants rather more detail (a curious failing, here and elsewhere, given that this is a historical critique), but surely Laudan is right to insist that one can always find some theorem, deduction, conjecture, or explanation that has no precise correlate in the successor theory (what Post calls 'Kuhn-losses'). But what are we to conclude from this? Clearly one needs desiderata as to which, and in what respect, such losses are to count as significant (one must make evaluative judgments). Alternatively, if one is not always to be compiling lists of examples, one wants a principled argument bearing one way of the other on the thesis at issue. Post's theory of heuristics is exemplary: given its emphasis on the heuristic importance of

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1973), (Putnam 1978), (Watkins 1978) and (Newton-Smith 1981). McMullin's (1984) otherwise admirable riposte to Laudan explicitly *excluded* dynamical physics from his critique (and indeed, his methods are scarcely applicable outside of the macroscopic realm).

[2] Amongst physicists, the writings of Dirac, Wigner and Penrose are exemplary; see Mickens (1990) for a recent collection of papers devoted to the issue. Among philosophers, Steiner (1989) and Zahar (1980) are exceptions. The examples thee summarized illustrate the thesis of correspondence, especially its heuristic component (but Steiner may not agree). These issues are important to contemporary methodology; see Cao and Schweber (1993) for current debates concerning renormalization theory, effective field theory and superstring theory.

theoretical unity, and on the conservation of those features of past theories "which have been confirmed without exception", it is clear enough that what 'Kuhn-losses' are to count as significant. Against this it may be said that significant theoretical innovation is only possible because scientists have been willing to tolerate Kuhn-losses, a points stressed by Feyerabend and Laudan.

Like Post, my concern is with heuristics; the point of view I shall sketch does, however, do justice to the latter objection. A part of the response is evaluative: that what are frequently called 'Kuhn-losses' are rather to do with high-level interpretations of dynamical theory (or with figurative schemata – usually precursors to the Newtonian synthesis – such as those of Galileo and Descartes). But this is only to say that some of the examples need not concern us; it cannot be denied that there is also radical innovation at a more substantive level, what is to be reconciled with the conservatism implicit in the thesis of correspondence.

To this end I want to focus on a certain kind of abstraction, distinctively mathematical: a level of 'pattern' or 'form' to a dynamical framework or application, the recognition of which (in existing theory) makes for the first phase of innovation (what Kuhn would include in 'normal' science).[3] In what follows the term 'heuristic' will always be taken in this sense, i.e. that of an abstract principle, or a mathematical abstraction. As examples I would cite the action principle, the theory of Euler and Lagrange, Hamiltonian mechanics and the Hamilton-Jacobi theory, differential geometry, the theory of topological groups, Klein's 'Erlangen' program, the relativity principle of Poincaré and Einstein, the equivalence principle, the relationship between difference equations and the derivative, and the gauge principle (there are many more). Revolutionary developments, I suggest, occur when such a heuristic (embedded in extant theory) is subject to radical, and more or less autonomous development, with little or no regard to the 'high-level' interpretation of that theory.

What will emerge as central to the thesis of correspondence are questions to do with the 'plasticity' of such heuristics (and of abstract structures in general), and with criteria for their identity over time. It seems to me that there are many examples where this identity is not in doubt, despite dramatic changes, usually in the direction of greater abstraction, over the course of their development (we shall encounter some presently). There are also phases in which heuristics become more or less stable sub-structures of dynamical physics. Given this sort of 'entrenchment' (the parallel's with Goodman's account of 'projection' are obvious) one does better to characterize these abstractions differently (I shall use the term *canonical*). It is, then, a part of the thesis of correspondence that such structures (heuristics and *a fortiori* canonical structures) a preserved or evolved in theory change.

I shall illustrate this account of heuristics in some detail in the context of the development of quantum mechanics (Sections V-VII); but first, and in a more philosophical context, let us consider Laudan's examples. Our criteria, as to what is to count as a dynamical theory, are exacting. Of those cited by Laudan, only Ptolemaic and Copernican astronomy, statistical mechanics and thermodynamics, and relativity and the ether theories (presumably Laudan here means pre- and post-relativistic electromagnetic theory) come into consideration.

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[3] There are parallels here to Post's conception of the 'footprint' of a successor theory, present in a precursor, a further heuristic in theory innovation. Much of what follows may be considered and elaboration of this device, although I shall not adopt his terminology.

Let me make some brief (and therefore sketchy) remarks on the first and third, the only *prima facie* cases of the wholesale *abandonment* of entrenched dynamical theory.[4]

The 'Copernican Revolution' (a phrase due to Kant) is a much-cited example of the elimination of previous theory, but also illustrates the cumulative methodology just summarized. To begin with let us view the introduction of epicycles (by Appolonius of Perga) into the geocentric astronomy as an example of a "first phase of innovation" (the autonomous development of the basic concept of Eudoxus and Plato, that celestial bodies move in circles with constant angular velocity). It was, therefore, a heuristic (and became – and remains – canonical). In particular, it was abundantly clear that one could represent the relative motion of the planets (including the Earth) and the Sun with respect of a system of epicycles centered on the Sun rather than the Earth. The obstacle to such an innovation was, of course, the conflict with Aristotelian *physics*. The contribution of Copernicus was precisely that he valued the parsimony and simplicity of the conceptual scheme of Plato, Eudoxus and Appolonius above that of Aristotelian physics and was prepared to modify the latter for these reasons. Considerations of simplicity then favored a heliocentric scheme. Of course something of the Aristotelian *Gestalt* (statics and astronomy to one side, here one has *only* a high-level interpretative scheme), had to be given up in this process.

In its details, the Copernican system made frequent use of epicycles and eccentrics, and could easily demonstrate the equivalence of heliocentric and geocentric motion.[5] What was altogether missing from the new system was the equant, introduced by Ptolemy, by means of which the deferent was assigned *non-uniform* angular velocity.

The controversies which surround this development are remarkable. Kuhn and Feyerabend focus on the abandonment of the Aristotelian world-view; what was involved was a wholesale change of paradigm. Laudan points out that since the Copernican theory did not preserve a 'mechanism' of the Ptolemaic theory, namely the equant, the latter cannot be understood as a limiting case of the former. Glymour argues on independent grounds (i.e. according to his 'bootstrap' methodology) that the Copernican theory to that of Ptolemy, and thereby defends a thesis of theory progression independent of correspondence. But I take my cue from Dijksterhuis (1961, p. 288):

> When at the end of his years [Copernicus] reviewed his life's work once more, he considered the greatest gain it had brought astronomy was not the changed position of the sun in the universe and the resulting simplification of the world-picture, but the abolition of the *punctum aequans*, the atonement for the sin against the spirit of Platonic philosophy which Ptolemy had committed in an evil hour. Really to understand the workings of Copernicus' mind, one can no

[4]It should not need to be stated, but apparently does after all need to be stated, that the investigation of quantum and classical mechanics and field theory, in relation to equilibrium and non-equilibrium thermodynamics, constitutes the major part of condensed matter physics and hence one of the most active areas of contemporary research (for a truly exotic example, consider Hawking radiation and the Hawking-Bekenstein equations).

[5] See e.g. Dijksterhuis (1961, p. 291) for an immediate demonstration.

more overlook this statement than in studying Goethe one can ignore the fact that in his old age he appeared to attach more value to the theory of colours than to his literary achievements.[6]

Einstein once remarked that each of us has our own Kant; it seems we each have our own Copernicus too. This does not seem the stuff of revolutions in the philosophy of science. But one point needs clarification. I remarked that the central heuristic – that of Appolonius – became and remains canonical. By this I mean that it was and still is and essential conceptual structure of dynamical theory. The technique can be simply illustrated for periodic motion in two dimensions. With the usual isomorphism onto the complex plane, motion using a single epicycle is and expression of the form $c_1 \exp i\omega_1 t + c_2 \exp i\omega_2 t$ (where $|c_1|$ is the radius of the deferent, $|c_2|$ that of the epicycle, and ω_1, ω_2 the angular velocities of the deferent and epicycle respectively). An astronomy based only on epicycles (in particular which does *not* make use of equants) corresponds to and expansion of the form $\sum_j \exp i\omega_j t$ (with the Earth chosen as origin). That the innovation of Appolonius is recognizably a primitive version of the Fourier analysis is an example of the 'plasticity' of a heuristic.[7]

The relationships between pre- and post-relativistic theories of electromagnetism, and between Galilean and Einsteinian relativity, appear, on the face of it, much more problematic. The elimination of ether is certainly the most significant case of referential failure in dynamics. But reference is not is here at issue, and the distinction between the theory proper, and an interpretative superstructure, must be handled with care. For example, in Helmholtz's (influential) synthesis of Continental potential theories and Maxwellian field theory, the ether was treated as a dielectric substance in exactly the same way as any (ordinary) material medium; evidently there is still a sense in which we think of the vacuum state in this way (we still talk of the 'electric permittivity and magnetic permeability of free space'). Following Helmholtz, there was scarcely and application of ether theory by Continental theorists that cannot be taken over to the electrodynamics of material media. What appears to be at issue is the distinction between the abstract framework of continuum mechanics, and its application to electromagnetic phenomena *in vacuo*. The former is eminently canonical (as such it survives in contemporary dynamics); the latter application has been abandoned. It is not, of course, that the generic notion of *some* substructure to radiation has been given up – nor, for that matter, that this substructure has a *dynamical* description (it has a *quantum mechanical*

6 For and evaluation of shifting interpretations of Osiander's preface to *De Revolutionibus*, see Dijksterhuis (*ibid.* pp. 296-7). The reader should also bear in mind Ptolemy's fictionalism, in contrast to the serious commitment of peripatetic (and of course Platonic) philosophy to perfectly uniform motion.

7 There is some evidence that the full generality of the theory of epicycles (that any periodic motion may be so represented) was already conjectured b Appolonius and Hipparchus (the latter proved that any eccentric motion could be replaced by the simple epicyclic motion, the basis of the equivalence between geocentric and heliocentric systems). I do not know who was the first to recognize the relationship between epicycles and Fourier analysis, but it may be found in Torretti (1978, p. 19).

description). But there is an important difference between these dynamical frameworks: the former, but not the latter, implies the existence of a privileged frame of reference.[8]

The continuum mechanics proved remarkably successful in the formulation of Maxwell theory, but implied something more – that motion with respect to a privileged frame should be observable. The repeated failure of attempts to detect such effects then indicated that Maxwell theory could not, after all, be viewed as a species of this theory. But quite independent of these developments, Maxwell theory had been largely isolated from and detailed model of the underlying medium (and therefore also from mechanics), just because no viable and consistent model had been developed. Its relationship to mechanics was left hanging.[9] The subsequent development of relativity concerned a quite different canon, the concept of inertial frames (grounded in Newton's laws) and their covariance group, isolated and subjected to autonomous development as a heuristic (that is, independent of the specific form that it took in Newtonian theory), yielding the relativity principle. Once again what is at issue is heuristic plasticity. In the context of Maxwell theory (more specifically, to a certain consequence of this theory, the independence of light-speed from the speed of the source), Einstein was led to a wholesale reappraisal of the concept of simultaneity; from this the Lorentz group followed.

This history is therefore complex: two distinct heuristics were isolated within mechanics, wave equations (partial differential equations), and the relativity principle, both of which were subject to autonomous development. Their reconciliation (but also the reconciliation of the relativity principle with electromagnetic *phenomenology*) then led to a new theory of space and time, and thereby a new mechanics. But this did not bring about a *rapprochement* of continuum mechanics and Maxwell theory (i.e. through a relativistic mechanics of media), for the very concept of a medium (as opposed to more wide-ranging notions of some sort of geometric or dynamical substructure) defines a privileged frame of reference.

One could say that we have the abandonment of a canonical structure (Galilean relativity), the abandonment of an application of another (the continuum mechanics), and correspondingly, the elimination of ether *qua* absolute resting frame, and ether *qua* mechanical explicandum of Fresnel and Maxwell theory *in vacuo*. Should we not count these as instances of Kuhn-losses, of mechanical concepts, eminently successful in their time, subsequently consigned to oblivion? But this evaluation is too quick. Quite apart from the self-evident importance of Galilean theory to contemporary physics (and every other branch of empirical science), and the countless applications of continuum mechanics to the

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[8] I make this remark with some reservations, in view of the difficulties over the definition of local measurable quantities in relativistic quantum theory. See e.g. Bacry 1988, Fleming 1988. For relationships between the ether and quantum field theory, see Saunders and Brown 1991.

[9] As a result, certain features of Maxwell theory – e.g. radiation pressure – remain contentious. The price to be paid for the relative autonomy of Maxwell theory from detailed models of the ether was, roughly speaking, widespread uncertainty with regard to the proper formulation of energy and momentum conservation in radiative interactions, for it was just here that the ether was thought to have and important substantive role (as the bearer of properties). For a detailed evaluation of these functions of the ether I refer to Stein (1981). My claim is that with relativity these matters were resolved, and that a strategy – but no previous resolution – were abandoned.

electrodynamical properties of material media, no ether which provided a mechanical reduction of Fresnel and Maxwell theory *in vacuo* proved satisfactory. Had the attempt to reduce Maxwell theory to mechanics been successful, one would, on the abandonment of ether (and with it, one supposes, the reduction(, have a *prima facie* case of Kuhn-loss: but there was no such reduction.

III

How is it that Galilean theory co-exists with Einsteinian theory? One sort of relativist response (favored, for example, by Feyerabend) is that there is every reason to simultaneously pursue quite different theories, that in any case there is no real sense to the *incompatibility* of these theories (since they are 'incommensurable'). Of course this is not quite what happens (as though applications of Galilean relativity are somehow made in a spirit of rivalry, or mutual incomprehension, with their Einsteinian counterparts). But let us concede that the onus is on those who would maintain that dynamics is progressive and cumulative to provide an account of the curious 'peaceful coexistence' of the two theories. Evidently they must be brought into relation with one another, but here, it is claimed, one has neither invariance of meaning nor of reference, with respect to concepts such as space, time, energy and mass. In Kuhn's original formulation of the objection, meaning and reference are lumped together: because the concepts have different meanings, "… we have had to alter the fundamental structural elements of which the universe to which they apply is composed" (Kuhn 1962, p. 102).[10] It is not enough, in Kuhn's view, to recover the laws of Newtonian theory in a suitable approximation (relative velocities small in comparison to that of light); the positivist thesis that that, in any case, is the only validity that should have ever been claimed for Newton's laws (for the only data available concerned this regime) is rebutted on the grounds that proceeding in this way, one could equally insist that the claims of phlogiston theory, restricted to the concrete data then available, were not and cannot be challenged by successor theories.

That the *mere* recovery, in a suitable regime, of formal equations of a subordinated theory (what I shall call *analytic reduction*), is an inadequate grounding for the progressive and accumulative view of scientific progress, is a point well-taken. The argument runs: (1) if the precursor theory is taken to make claims restricted to the domain of experiments actually performed, it collapses into a mere catalogue of observations; (2) if the equations are taken to express *conceptual* truths (of hypotheses), then these concepts will depend on the successor theory and in any case differ from those of the precursor; (3) for the latter reason, such formal equations cannot even be taken to have the same referents.

As it happens, on quite independent grounds, the notion that the *meanings* of 'theoretical terms' (as they were then called) were somehow fixed once and for all, had already come under attack from a quite separate quarter (as part of the fall-out of the Quinean critique of the analytic-synthetic distinction). Putnam's (1962) notion of 'law-cluster concepts' (and the

[10] As will become clear, I am sympathetic to the blurring of the distinction between sense and reference for such abstractions.

issues there explored) largely anticipated the problematics of 'meaning invariance' subsequently posed by Feyerabend (e.g. that because 'energy' means something different in relativity and Newtonian theory, the one theory cannot contradict the other). Evidently this sort of line (that kinetic energy E does not *mean* $p^2/2m$) appears necessary to rebut the charge (2), but on the other hand (of course Putnam was *not* responding to the relativist challenge), just because the meaning of a law-cluster concept is something more nebulous, it would seem that no response is possible to (3).

To be more precise, it seemed that if such concepts have no precise meaning, they have no precise denotation either. Israel Scheffler (who *was* responding to Kuhn and Feyerabend) made tentative steps (Scheffler 1966) towards freeing the reference (what he also called 'descriptive meanings') of such concepts from their theoretical meaning (the former, in contrast to the latter, he supposed stable under theory-change; note that even if the theoretical meaning determined reference, *at most* one could conclude that the reference *may* differ in (3)). A similar approach is evident in Dudley Shapere's notion of 'trans-theoretic terms' (Shapere 1969). Supposing the theoretical meaning of a term is what is 'grasped' in the use of a term (so that it is, roughly speaking, its Fregean sense), these strategies amount to making the sense/reference distinction carry much the same burden as the positivist distinction between theoretical and observational terms. But if they do not in fact *presuppose* that distinction (in which case reference *per se* is no longer what is at issue) it was scarcely clear how much a notion of reference could be made out.

The denouement to this story, what is probably the most popular contemporary response to the relativist challenge, we owe to Putnam and – with a somewhat different motivation – to Kripke. It is, essentially, that one allows 'natural kind' terms to provide a limited Fregean sense, a pointer to reference, and leave the rest to the *world*. The electron of J.J. Thomson, of Bohr and of Dirac, was one and the same, not because they conceived of electrons in the same way, but because the same entity (whatever in fact it is – for of course the story does not stop with Dirac) was causally involved in all of the experiments to which their theorizing was directed (Putnam 1973, p. 197). Putnam's later elaboration of this doctrine (Putnam 1975) shared with Kripke a focus on (observable) natural kinds – but it was clear enough that failing a Fregean *pointer* to reference such causal theories cannot possibly be of much use in the case of unobservable objects or properties, for how else are we to know there is any one entity (or kind) causally responsible for a class of experimental phenomenology?[11]

In short, there had better be enough meaning invariance across theory change to ensure that we are talking about the same thing, not in the sense that 'that thing' is conceived of in exactly the same way (for of course it is not), in in the sense that whatever it is, we can be confident (because this much is invariant across theory change) that there is one sort of thing (or property or magnitude or whatever) causally involved in the relevant class of experiments. Or so it is claimed.

One sort of problem with this account (but I think it is one of many) is that it does not, after all, work very well in the context of relativity theory. The difficulty was thrown into

[11] Kripke makes a similar concession in the context of homonymous names, where one has, so to speak, the same 'phenomenology', but distinct causal chains to quite different objects (Kripke 1980, pp. 8-10). Compare also Putnam (1990).

sharp relief by Hartry Field in 1973 (Field 1973; although it was more or less implicit in Feyerabend's (1968) arguments for meaning-variance): accepting that relativistic concepts of mass (total and rest mass) do, in fact, refer, he asked: to which of these did the *Newtonian* concept of mass refer? The answer, according to Field, is that there is no very good answer, but neither is it the case that Newton referred to a thing sharing some of the properties of total relativistic and rest mass ('Newtonian mass'), because there *is* no such thing (and what would it be to say there is an *approximate* thing of this sort?). Field did not specifically address the 'causal' theory of reference, but it should be evident that it is not going to resolve this sort of conundrum: in this case the best 'expert' stereotype in the pre-relativistic corpus did not single out one referent rather than another, and supposing that the *real* referent is somehow singled as s whatever *in fact* enters into causal relationships with the experiments with which classical theorists were concerned is not much help, for on that account (supposing our present theories in some sense 'true') we will arrive at different referents depending on the particular experiment involved, and even worse, depending on how we reconstrue that experiment. More fatal still, to say even *this* (if we can say even this) we must make use of something more than the Fregean sense of a 'law-cluster' concept – we must make use of the *detailed* concepts of contemporary theory.

To suppose that on each tokening of the term 'mass', relative to a specific phenomenology and laboratory procedure, there is some determinate reference, some 'fact of the matter', as to the entity or magnitude causally responsible for the phenomenology at issue, on some notion of 'cause' neither so strong that it hinges on contemporary theory, nor so weak that one has referential indeterminism, is to whistle in the dark. For the moment I suggest the more pertinent response is to recognize that if the thesis of general correspondence can be sustained, *we need nothing more*. We can do no other than suppose that contemporary theory (our 'best theory') is referentially successful (that is, construing reference in terms of Fregean sense; no easy task, needless to say). And by such standards, we may attempt to find referents for applications of precursor theories. But if we can explain the success of precursors on the ground of their correspondence with successors (and have good warrant to suppose our present theory superior), that will suffice. The notion of reference is a red-herring; we must meet the relativist challenge head-on.

A better perspective, I suggest, goes something like this: Putnam was much closer to the mark with the notion of 'law-cluster concepts', but there need be nothing vague in this notion; on the contrary, the 'clustering' of concepts may be as complex nd precise as we care to make it. Putnam, contrasting the status of truisms of the form 'all bachelors are unmarried' with the principle '$E = p^2/2m$', remarks that the former "… cannot be rejected unless we change the meaning of the word 'bachelor' and not even then unless we change it so radically as to change the *extension* of the term 'bachelor'". He continues:

> In the case of the terms 'energy' and 'kinetic energy', we want to say… that the meaning has not changed enough to affect "what we are talking about"; yet a principle superficially very much like "All bachelors are unmarried" has been abandoned. What makes the resemblance only superficial is the fact that if we are asked about what the meaning of the term 'bachelor' is, we can only say that 'bachelor' means 'unmarried man', whereas if we are asked for the meaning of the term 'energy', we can do much more than give a definition. We can in fact show the way in which the use of the term 'energy' facilitates an enormous number of scientific

explanations, and how it enters into an enormous number of scientific explanations, and how it enters into an enormous bundle of laws (Putnam 1962, p. 53).

It might seem overly ambitious to ask: How, in detail, does the term 'energy' enter into and enormous bundle of laws? It might seem that to answer such a question, one would have to more or less recapitulate the basic structure of the dynamical physics. But that is *exactly* what we must do. Only then can we address the question of how, precisely, the Newtonian concept of 'energy' (or 'mass', or 'momentum') compare with their relativistic analogs.

Of course it is not a matter of listing the equations of dynamics. It is their structure which is at issue, in particular the heuristics and canonical forms that we have already encountered. To begin with, we have the concept of spacetime as a topological group (Lie group), here the semi-direct product of a non-abelian group (containing the rotations and boosts) with the abelian group of translations on $\mathbb{R}^4$. The energy and momentum appear as the generators of transformations on the latter group. In the relativistic case the non-abelian group is the Lorentz group (and the semidirect product is the inhomogeneous Galilei group or IHLG); in the non-relativistic case we have the inhomogeneous Galilei group or IHGG. The energy, which in both cases is the generator of time translations, is in the relativistic case what is also called the total relativistic mass. The non-relativistic mass, in contrast, has a quite different interpretation (as does also the relativistic rest mass), bound up with more detailed properties of the respective Lie algebras; in the case of the IHGG, to the 'neutral elements' of the algebra (it therefore defines the momentum and the energy in conjunction with the velocity); in the relativistic case to the Casimir invariants (a function of elements of the Lie algebra, not a separate element). In both cases these quantities have vanishing Lie bracket with every element of the Lie algebra; they are therefore conserved. One has a quite reasonable understanding of their inter-relationships as provided by the theory of group contractions.[12]

Of (perhaps) even greater importance is the structural characterization of the dynamical theory at the level of the spacetime manifold. The essential basis for comparison – the 4-dimensional manifold structure of Galilean spacetime – was laid down by Elie Cartan; this development, like those in pure group theory just discussed, followed the elaboration of the analogous ideas in special and general relativity by Minkowski and Einstein, ultimately going back to the theory of Gauss and Riemann. The differential geometry was itself and extension of the analytic 3-geometry of Descartes, and essential component of the Newtonian synthesis. That is, the 4-dimensional Galilean theory was in many ways the *last* theory to be properly investigated (this remark is even more pertinent when one comes to the Hilbert-space representation theory). We see here how canonical forms associated with contemporary theory are extended back to reformulate its precursors (or are recognized as implicit in precursor theory; cf. Post's notion of 'footprint'). In this way we understand that Newtonian theory makes use of a timelike congruence of spatial frames with associated degenerate (signature 3 and 1) space and time metrics. In comparison to the pseudo-Riemannian spacetime of the special theory, what is involved is the deformation of the light-cone structure into a set of

[12] For and excellent introduction see Sudarshan and Mukunda 1974; for more background on the quantum mechanical case see Varadarajan 1970. For the theory of group contractions see Inonu and Wigner 1953.

spacelike hyperplanes (one is then left with only the time metric; in terms of the theory of group contractions, this is the zero momentum limit).

All of this is to give an abstract framework; we are a long way from writing down any *formal equations* common to the two theories (i.e. solving the analytic problem of reduction; the formal agreement between the two theories in a certain limiting regime, which relativists seem to regard as a foregone conclusion). In fact here it is the latter which is problematic, whereas the *conceptual* relations between the two theories are readily surveyable. The reason why formal agreement, at the level of specific equations of motion, is so difficult to establish is that the *mere* existence of a formal limit (taking c as infinite) tells us nothing about how fast, and with respect to what class of interactions, limiting agreement between the solutions of the equations of motion may be found; the equations themselves do not even always become identical in the limit (at *best* they coincide at the limit). What is needed is 'hard' analysis, the establishment of concrete bounds and rates of convergence in the parameter *vlc*, in comparison to the 'soft' analysis given above (compare Berry 1991). There are fortunately some results of this kind (for smooth and slowly-varying external fields), in both the quantum and classical case.[13]

One moral that we can draw is that Field's dilemma – to what does the Newtonian mass correspond? – is problematic just because this quantity is *not* a generator of any 1-parameter subgroup of the IHGG. It shares this distinction, embarrassingly enough, with the quantity *position* in the case of the IHLG; the position is the generator of boosts in the Galilean case (this is why it is a self-adjoint operator, which satisfies canonical commutation relations, as also a Lie bracket, with the momentum in quantum mechanics), but since the boosts are *not* an abelian subgroup of the IHLG (because of the Wigner rotation; thus the Lorentz group is semi-simple, unlike the Galilean), their generators do not have vanishing Lie brackets with one another (hence they do not commute in quantum mechanics). As a result there are no covariant position operators in relativistic quantum mechanics, and no covariant Born interpretation either.

On the other hand, to strengthen the point I have already made, the IHLG and IHGG have substantial similarities. Most importantly, they both contain the Euclidean group as a subgroup, hence the generators of this subgroup have precisely the same group-theoretic meaning: the angular-momentum, energy and momentum, are the generators of angle, time and space translations, respectively, and are therefore conserved in any inertial frame. Correspondingly, that in Minkowski space one can pick out differently oriented spacelike hypersurfaces (with different associated energy and momentum, generating translations along the associated orthogonal vector fields), is directly responsible for the four-vector character of the combined energy and momentum (the 'combination' of energy and momentum conservation into a single conservation principle).

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[13] There are very difficult, though perhaps not insuperable, problems when one comes to covariant (relativistic) theories of inter-particle forces. The use of retarded potentials is certainly inadequate (there are, indeed, 'no-go' theorems which indicate that no non-trivial theory of this sort may be formulated consistent with 'manifest' covariance – see Sudarshan and Mukunda 1974). On the other hand, the inverse-square law and its application to gravity are both understood in the context of field theory (and in particular General Relativity).

But it should be clear that all of this is not quite to specify the 'law-cluster concepts' of mass, energy, momentum, space and time. We have, to begin with, altogether left out of consideration the role of gravity, and more important still, there is no account here of concrete phenomenological applications. I do not suggest that these things can be completely codified (especially the latter). My point is that the *attempt* at codification (or integration) is precisely what is, and what has always been, the essence of the enterprise of dynamics. And in this process, the existence of heuristics and canonical forms by means of which precursor theories may be understood in terms of successor theories (here, most especially, the concept of dynamical variables as generators of symmetry transformations, thereby the connection between symmetry and conservation laws, the concept of Lie group an its use in the characterization of space and time, the concepts of spacetime manifold and differential geometry) have an importance that it would be hard to over-estimate.[14] The process is of course two-way: the precursor theory is used to interpret the successor equally as the successor interprets anew the precursor.

Here I have concentrated on the recognition that heuristics, developed in the pursuit of relativity theory, were present (and are now seen as canonical) in non-relativistic theory, that in this way the two theories can be reconciled. I have said little of the context of discovery of the Lorentz transformations. Here one finds a remarkable circumstance: with only small modifications of Einstein's procedure, one can in fact deduce the form of the coordinate transformations as functions of a single parameter, and invariant velocity; if it is infinite one obtains the Galilean transformations, otherwise those of Lorentz. This fact is quite exceptional; not only is one fragment of the 'soft' reduction of these theories guaranteed, but in a certain sense the two theories are exhibited as the only possibilities consistent with the existence of global inertial frames.[15] As we shall see, something similar can be said of quantum and classical representations of probability, but in the case of relativity the procedure of perspective by which it and its precursor may be derived is close to that actually employed in the context of discovery.

IV

The tenor of my argument is somewhat unorthodox; I have spoken of the *mathematical* structure of dynamics as the conceptualization of the world, leaving reference (if and when it can be made out) to Fregean sense. Let me now make some concessions.

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[14] These canonical forms make up the nearest thing we have to inviolable laws; but we see that they depend on the existence of spacetime isometries, and that we must dispense with them in General Relativity for spacetimes of low or vanishing symmetry. How this is to be done, if, indeed, there is any sense to a global dynamics within such spacetimes, is an unresolved problem particularly when addressed in the context of quantum theory (where symmetries play and even more important role in defining the basic dynamical variables).

[15] This insight is due to Ignatowsky. A number of qualifications should be made; see Torretti 1983, pp. 76-82 and Brown (1993). I trust it is clear that there are deep connections between the relativity principle, Klein's Erlangen program, and the symmetry properties of dynamical equations.

Clearly there is a somewhat different practice of reference at the level of everyday (and not so everyday) experience. In particular, we refer to laboratory apparatus, and to systematic and detailed phenomenological properties to do with the manipulation of laboratory instruments and their construction, with a certain independence from theory. Just how 'deep' this sort of reference goes, the extent to which, indeed, one has a genuine autonomous basis for the interpretation of physical theory, has been well illustrated in Hacking's accounts of microscopy and his subsequent elaboration (Hacking 1983) of 'entity realism' (as Allan Franklin has emphasized , "There are no anti-realists in the laboratory."). It is, I hazard, just such and appeal to 'common-sense' reference that underlay Scheffler's assumption of some sort of autonomy of reference from Fregean sense (if so, that it is parasitic on a theory/observation distinction is evident).

This sort of reference is surely what underlies talk of 'discovery' of elemental particles, of the 'real existence' of e.g. neutrinos (in some weak sense – cf. Grover Maxwell's arguments – these are indeed observable). But there is always a gap between what is observed (in Hacking's sense) and the putative referents of theoretical concepts; what is at issue appears more bound up with some sort of process of 'triangulation' by different methods (in each one of which there is *always* a gap). At the last, it seems we must make sense of some sort of coherence theory of truth, however unfashionable the strategy.

The appeal to any one method of observation, no matter how flexible a line one takes, is not going to be of much use when it comes to more general concepts and more structural features of dynamical theory; and the difficulty now (as was exhibited in connection with Newtonian 'mass', by most accounts and *observational* term), is that we do not know how changes in this level will force a re-evaluation of what we have been taking as 'observation' (as Putnam once remarked, we have "… the almost untouched problem, in thirty years of writing about 'theoretical terms' [as to] what is *really* distinctive about such terms." – and *that* comment was made thirty years ago). In other words, however plausibly we may suppose we make reference to objects and properties in laboratory manipulations, there can be no guarantee as to the stability of this sort of referential success (no guarantee that we will not learn new ways of thinking according to which we are *not* successful).

The most striking example is at the level of *everyday* experience. It *seems* that we make reference to *colors,* for example, that colors are *out there*, in the world, but 'in fact' (according to current theories of perception), color words are ordinarily used (in particular as used to characterize objects) refer, if they refer to anything, to equivalence classes of relative spectral reflectancies of surfaces and boundaries, and only then subject to normative constraints on the range of permissible lighting. And what defines the equivalence class, is the structure of the *particular* retina and visual cortex which sees the color (in particular, there is only a loose correlation with the wavelength of light incident on the retina; see Hardin (1988) for a comprehensive review).

Evidently we are in deep waters. It seem to me one has no real option but to approach the concept of reference in the context of cognitive science and the philosophy of mind (I take it that this is what a naturalized epistemology demands of us). For our present purposes I suggest that we may forgo talk of reference and 'entity realism' at any level deeper than that countenanced by Hacking. Such a strategy might best be termed 'methodological antirealism': by this I do not mean that we foreclose on issues of truth and reference. (The position is motivated by a *naturalized* epistemology; in a sense we *are* committed to realism. There is and evident analogy with the notion of 'methodological solipsism'). My claim is only that we may understand significant components in methodology, heuristics, and the notion of 'progress' more or less independently of questions of reference over and above the everyday (this is not Hacking's position, who – and something similar may be said of Laudan's more recent writings – countenances talk of 'progress' and 'success' in much the same sense that the terms might be applied to engineering and technological advance).

With his said, let me turn to quantum mechanics. There are three elements of the previous discussion that I wish to consider by turn. These are:

(1) *Heuristics*: Innovation proceeds by isolation and independent development of structural features of extant theory. Once entrenched, such heuristics (or canonical forms) are preserved in subsequent developments, and pervious theory reformulated in their terms.
(2) *Unity*: Among these, one may provide over-arching abstract frameworks (Lie group, spacetime manifold) within which one dynamical structure may be embedded in another.
(3) *Analytic Reduction*: experimentally well-confirmed formal equations of the subordinate theory may be recovered in a well-defined approximation to the dominant theory.

I take it (3) is necessary to the thesis of 'zero Kuhn-loss'. Evidence that the problem of analytic reduction may be solved is provided by the existence of singular limits ('soft analysis') according to which formal equations of the one theory reduce to that of the other. Progress will in general depend on (2) being satisfied. (2) is, in any case, essential if the conceptual structure of the precursor theory is to be 'surveyable' with its successor. In turn, (2) (hence also (3)) depend on satisfaction of (1) (I take this to be self-evident). The conjunction of all three is what I take as the thesis of correspondence. The following three sections are somewhat more technical in nature and illustrate each component in turn in the context of quantum mechanics. I begin with (1) and the historical development of the theory.

V

By 1925 the conflict between the Bohr theory and classical electrodynamics had reached and impasse. According to the first, the frequencies of emitted radiation (the Bohr transition frequencies) could be related to the energy difference between the stationary states involved in the atomic transition; according to the second, the intensities were related to the amplitudes occurring in the Fourier expansion of dynamical variables (specifically the electric polarization). A partial, but obviously unsatisfactory, compromise was reached by modeling the atom as a multiply periodic system – despite the fact that one was concerned with alkaline metals, with only one optically active electron – for thereby one had two numbers available to parameterize each amplitude: the number of the harmonic, and the number of degrees of freedom involved. But there was a second problem: in this expansion each term represents a harmonic of the oscillatory motion of the electrical moment; according to classical electrodynamics, this frequency is also that of the radiation emitted and absorbed. Therefore it was necessary to identify these harmonics with the Bohr frequencies, *not* the orbital frequencies of the stationary states (those frequencies at which the electrons actually vibrated, according to the Bohr theory). Correspondingly, these harmonics could not be simple multiples of a fundamental frequency (for the Bohr frequencies, given by differences in energy levels, displayed a quite different structure, except in the special case when these energy levels were equally spaced, as in simple harmonic motion). Therefore this series expansion could not properly be understood as a Fourier expansion at all, and even if it could, it could not describe the optically active electron alone.

What were these objects, the oscillatory motions of which were described by the 'Fourier series'? It seemed that they were transition processes themselves, but how was it that more than one such transition process could be brought into play by the coupling of the atom with the radiation field? These objects were first introduced by an experimentalist, driven entirely by a pragmatic concern for a serviceable mathematical algorithm.[16] Ladenburg's 'dispersion electrons' – the objects vibrating at the transition frequencies – provided a key input to the Bohr-Kramers-Slater theory (where they were known as 'virtual oscillators'). Their 'number' could be directly measured via Ladenburg's algorithm, which was derived by straightforward application of classical dispersion theory; but it had no relationship to the electron number in the sense of the Bohr theory and quantum chemistry.

Heisenberg thus contemplated a mathematical formalism with no reasonable physical or mathematical foundation: it seemed necessary to associate a large (actually infinite) number of dynamical variables with a single dynamical variable – describing the motion of a single electron – and for each of these develop a second infinity of labels of state through the Fourier expansion. But here the various frequencies associated with the Fourier coefficients are all multiples of the fundamental; one also had to break the strict proportionality of harmonics.

His response, an act of unqualified genius, was to consider the series expansion for each degree of freedom as simply a collection of terms, but to take over from the mathematics of Fourier series the rule of combination of terms, *modified* so that frequencies combined as given by the Bohr frequency condition. He ended up with a two-fold infinity of terms, a multiplication rule, and an association of each term with a Bohr transition: the basic elements of matrix mechanics. Let us look at the steps.

First, one has the classical Fourier expansion for the position coordinate (proportional to the electric dipole moment) for the kth degree of freedom of a multiply periodic system:

$$x_k(t) = \sum\nolimits_n a_n(k) \exp i\omega(k)nt$$

The $\omega(k)$'s are the fundamental frequencies of the multiply periodic motion (one fore each degree of freedom); the expansion is over the harmonics, frequencies which are integral multiples of ω, as determined by the integer n. Second, a similar expansion for the square of

[16] Ladenburg was fortunately undeterred by the implication of the Bohr theory that there must be infinitely many such electrons, because infinitely many possible transition frequencies, for he could measure but few of them. In this way he cut the Gordian knot that had held up development of the Bohr-Einstein electrodynamics for several years (see Van Der Waerden 1973, for a detailed commentary and original papers).

the dipole moment (introducing a new set of fundamental frequencies $n(k)$ and Fourier coefficients $b(k)$) yields:

$$x_k^2(t) = \sum_r b_n(k) \exp i\nu(k)rt$$

By direct calculation from the original expansion the quantity should equal

$$\sum_{m,n} a_n(t)a_m(k) \exp i(\omega(k)n + \omega(k)mt)$$

On writing $n + m = r$, it follows that $\nu = \omega$, since:

$$\omega(k)n + \omega(k)m = \omega(k)(n + m). \quad (1)$$

Evidently the fundamental frequency of any polynomial in the dynamical variables is unchanged, and only integral multiples of this fundamental occur in the oscillatory motion. As a consequence of Eq. (1), it follows that:

$$b_r(k) = \sum_n a_n(k)a_{r-n}(k) \quad (2)$$

To see how these formulas must be modified in order to interpret the frequencies as Bohr transition frequencies, Heisenberg introduced the new notation (I depart slightly from Heisenberg's notation):

$$\omega(k)n \leftrightarrow \omega(k,n)$$
$$a_n(k) \leftrightarrow a(k,n)$$

making clear that both degree of freedom and harmonic were to be treated on a par, both interpreted in terms of Bohr energy levels. Eq. (1) now takes the form (writing $m = r - n$):

$$\omega(k,n) + \omega(k,r-n) = \omega(k,r).$$

to be compared with the additional rule which follows from the Bohr frequency condition, namely $\omega(k,n) = (E_r - E_n)/h$, i.e.

$$\omega(k,n) + \omega(n,r) = \omega(k,r).$$

Imposing the latter, in place of Eq. (2) we obtain $b(k,r) = \sum_n a(k,n)a(n,r)$, i.e. the matric multiplication law. In the original notation this gives:

$$b_r(k) = \sum_n a_n(k)a_r(n) \quad (3)$$

This is, of course, just the beginning. Heisenberg then proceeded to apply this new calculus to a concrete dynamical problem (the harmonic oscillator), essentially using *classical* dynamical theory amended in accordance with Eq. (3). The justification for this was starkly stated:

> If one seeks to construct a quantum-mechanical formalism corresponding as closely as possible to that of classical mechanics, it is very natural to take over the equation[s] of motion directly into quantum theory. (Heisenberg 1925, p. 267)

This is what one does if one wants to maintain the thesis of correspondence. At every step, Heisenberg first wrote down the classical equations (for a perturbative solution to the equation of motion $x + \omega_0 x + \lambda x^3 = 0$, expanding x in a Fourier series), and then the analogous quantum mechanical quantities, employing Eq. (3) in place of Eq. (2).

A final point: I have suggested that the heuristic component of the correspondence thesis is bound up with the isolation and autonomous development of a mathematical abstraction embedded in previous theory. Here it is clear enough that the Fourier analysis was used as a heuristic in this sense; it is not so clear that the Bohr frequency condition, which evidently played a crucial role in the 'autonomous development' of the Fourier analysis, can be understood similarly. Its origins are in fact multi-faceted. The condition was hit upon by appeal to the more or less intuitive pictures of energy quantization, the concept of stationary states, and energy conservation, but it was Bohr's great genius to see in this condition a deeper connection with classical theory, in particular the concept of the derivative and the action principle. The former is defined as the limit of a difference equation:

$$\lim_{\delta J \to 0} \frac{1}{\delta J}[W(J + \delta J) - W(J)] = \frac{dW}{dJ}.$$

If, in particular, W is the total energy and J the action variable for a periodic motion one has, from Hamilton's equations $dW/dJ = d\theta/dt$, where θ is the canonical conjugate to the action. When $\theta = \nu t$, with ν the frequency, one obtains $\delta W = \nu \delta J$, or $W(J + \delta J) - W(J) = \nu \delta J$. For $\delta J = h$, the 'minimum' change in action, the correspondence-limit of the Bohr frequency condition follows. This condition emerges as the product of the autonomous development of one of the canonical forms of classical dynamics.

The frequency condition was not, therefore, first formulated as a heuristic, but it was subsequently understood in this sense. Indeed, its autonomous development as the Bohr correspondence principle was, together with the quantization condition, the basis of the 'old' quantum theory. Meanwhile the quantization condition and the Hamiltonian mechanics emerged as so deeply interwoven – here I have in mind Ehrenfest's adiabatic theorem and the relations between separability, adiabatic invariance, action-angle variables and the Hamilton-Jacobi equations, as formulated by Schwarzschild, Levi-Civita, Epstein, Ehrenfest and Burgers – that one has a system of real structural integrity; the fundamental lacuna concerned the change of quantum number (quantum mechanics proper), which was related via the correspondence principle to the classical derivative and to classical dynamical continuity. A relationship between these heuristics – for that is what they clearly are – and probability theory was meanwhile established by Einstein (the theory of 'A and B coefficients'), and was further developed by Bohr (what became known as the Einstein-Bohr electrodynamics): symbols and coefficients classically given by derivatives in the action were taken to represent the probabilities of transitions between stationary states, and were written as difference equations in the Heisenberg-Kramers dispersion theory. These probabilities were in turn related to the radiation intensities (which returns us to Heisenberg's problem situation).

Schrödinger's route to the wave mechanics reveals a similar strategy, although here he was also guided by a new 'high-level' interpretation of previous modifications of dynamical theory (the wave-particle theory of Einstein and de Broglie). This in turn (specifically in de Broglie's contributions) hinged on the formal analogy between principles of Hamilton and Fermat, thereby linking mechanics to optics (and hence wave theory). The crucial step was to seek modifications to the Hamilton-Jacobi theory. This procedure is rather better known than that of Heisenberg, since it is reviewed in a number of introductory texts, and I shall speak no more of it here.

I take it that the heuristic component to (1) finds overwhelming confirmation in the genesis of quantum theory. As for structural similarities with classical theory revealed by subsequent investigation, let us note: (i) Von Neumann's systemization, by means of which states in both classical and quantum theory are measures over the states space (phase space and Hilbert space respectively), in the one case given by a phase space integral of real functions on phase space, and in the other by the trace of self-adjoint operatators on Hilbert space (the distinction between pure and impure states being formulated in exactly the same way in the two theories, with the same intended interpretation); (ii) subsequent abstraction of this framework in terms of lattice theory; (iii) the Dirac correspondence, whereby the Poisson bracket algebra of classical observables is (formally) related to the commutator-bracket algebra (Heisenberg algebra) of quantum observables; (iv) the Weyl form of the canonical commutation relations, whereby the latter express (in infinitesimal form) the existence of additive groups of transformations on phase space (translations in space and boosts) as in classical theory; (v) the various Hamiltonian and Lagrangian fuction(al)s, identical in quantum and classical theory for electromagnetic couplings.

Once again these results are well-known; it is now clear that many of these structural inter-relationships of NRQM and classical mechanics hinge on properties of the IHGG (see e.g. Varadarajan 1970, Mackey 1962). Others, such as the Dirac correspondence, have been extensively investigated in the context of 'geometric quantization' and the synthesis of differential geometry and Hamiltonian and Lagrangian dynamics (see e.g. Woodhouse 1980). The lattice-theoretic structure is the object of the logico-algebraic approach (and also more abstract algebraic approaches to quantum mechanics and field theory); here one has the hope of a more generic, over-arching framework in the sense of (2) (see e.g, Hooker 1975-9, Beltrametti and Cassinelli 1981, Saunders 1988). Indeed it is surprising, with such extensive interplay between quantum and classical concepts, that this remains only a hope. As we shall see, paradoxical though it sounds, there is actually *too much* interplay.

VI

The difficulty is, of course, the problem of measurement. Here it is not so much that one cannot accommodated both quantum mechanics and classical theory in one and the same scheme, but rather, that we do not know how to formulate quantum mechanics *as a universal theory* in the first place. In a certain sense, the situation is actually made *worse* when viewed in a unitary context which includes classical theory as a special case (for then we see that what should function as the fundamental level of description in quantum theory *makes no sense* when interpreted probabilistically, if the quantum mechanical description is to be 'complete').

As illustration, consider the Mackey theory (1963). In this framework, the notions of 'state', 'observable', and (most importantly) 'probability' occur as primitives. We suppose we are given a prescription, for any state *f*, for assigning probability to the proposition that the value of an observable *A* lies in some subset of the reals, (for convenience we suppose these to be Borel, a very mild constraint). Since the values assigned are probabilities, we require $f(A,\emptyset)=0, f(A,\mathbb{R})=1, f(A,\cup_i E_i)=\sum_i f(A,E_i)$for E_i pairwise disjoint Borel sets. Next, we impose some natural requirements of parsimony: if $f(A,E)) = g(A,E)$ for all observables A and Borel sets E then $f = g$. It is necessary to consider functions of observables: if g is such a function, then the probability that $g(A)$ has value E should equal the probability that A has value $g^{-1}(E)$. To make this well-defined we require that g is a Borel function. Armed with this notion, we can define 'properties' as special sorts of observables, namely those for which $f(a,\{0,1\}) = 1$ for every state f (I use lower case letters to denote properties). $f(a,\{1\})$ (which may be written $f(a)$ is then naturally interpreted as the probability that the system 'has' property a. It follows that for any observable B and Borel set $E, \chi_E(B)$ is a property (the property that the observable *B* has the value *E*; here χ is the characteristic function of the set *E*). There is a further natural requirement: call properties a, b *disjoint* when $f(a,E) + f(b,E) \leq 1$ for every Borel set *E* and state *f* (if a physical system has property *a* with certainty, it is certain that it does *not* have property *b*, and *vice versa*). Require that for any countable set $\mathcal{D}$ of pairwise disjoint properties, there is some property c such that $f(c) = \sum_{a\in\mathcal{D}} f(a)$ for every state f (this is the appropriate modification of the σ-additivity of probability measures for disjoint outcomes, familiar from standard probability theory). In exact parallel to the familiar theory, require that for any finite set of real λ_i in the interval (0,1) which sum to unity, and set of states f_i , then $\sum_i \lambda_i f_i$ is also a state. Armed with this structure (the set of states is now a convex set) we may define the pure states as extremals, i.e. those states which *cannot* be written as the convex sum of other states. Such states provide the most detailed or maximal description possible (possible, that is, in accordance with the present framework).

With this we have the basis of a generalized theory of probability. In fact, from these requirements, it follows that the set of properties is a σ-orthocomplete, orthomodular, orthocomplemented poset. The partial ordering is defined as follows: if $f(a,E) \geq f(b,E)$ for every state *f* and Borel set *E* then we write $a \geq b$. If in particular the system has property *b* with certainty then it also has property *a* with certainty. If we also require that this poset is a lattice we (almost[17]) have the conditions of the Piron theorem: such a system *must* have a representation as the projection lattice of a Hilbert space (over one of the reals, complex numbers or quaternions). If, however, we insist that the pure states assign probability 0 or 1 to *all* properties (a probabilistic version of bivalence), then there also exists a representation of the lattice as a classical phase space.[18]

The Mackey theory is commonly regarded as one of the 'cleanest' and most elegant abstract characterizations of classical and quantum theory, but, in a sense I now want to make

[17] That additional (more technical) requirements are needed to secure the Piron representation theorem (see e.g. Varadarajan (*op cit*)) suggests that there may be some more general structure than Hilbert space which satisfies Mackey's axioms in the absence of bivalence, on which a dynamics can be formulated. In this way one would obtain a generalization of quantum mechanics. This is, indeed, a principal motivation of the quantum logic program. However, no such structure has been found, for the more general lattices or posets (that do not satisfy the Jauch-Piron axioms) do not appear powerful enough to support dynamical applications.

[18] The essential step uses a generalization (due to Loomis) of the Stone representation theorem for complete Boolean algebras. One ends up with phase space as it should be, a C^∞- manifold in a metric topology, endowed with a Borel $\sigma-$ algebra of sets generated by this topology. The quantum mechanical case (where one gives up bivalence) is in certain respects more simple.

plain, it also highlights the sense in which quantum mechanics is deeply problematic. The point is this: the abandonment of bivalence is, in a certain sense, incompatible with the notion that the quantum mechanical state is or can be regarded as a *complete* description of a system at a given time. And just because the abandonment of bivalence (virtually) *forces* a quantum mechanical representation of state (and is clearly a necessary condition given the standard probabilistic interpretation of NRQM), one has precious little room for maneuver. (One might suppose that the Hilbert space structure could be traced to some other generic viewpoint on the nature of microscopic phenomena; that it appears to follow from elementary notions of probability together with the abandonment of bivalence tells against such a view).

The conflict can be formulated very simply. One wants to assert *both* that a pure state description is possible for every system at all times, *and* that such a description is *complete* (i.e. that it cannot be supplemented by a more detailed description). But if that is so, there can be no sense to the notion that, given that that a property p of the system is assigned probability $f(p) = \lambda \notin \{0,1\}$, the system 'in fact' has or does not have that property with probability λ and $1 - \lambda$ respectively; for if it did, 'in fact', have property p, then it would surely have that property with probability 1, and therefore he system should be described by some other state g such that $g(p) = 1$ (i.e. f would *not* then be a complete description of the system).

The appeal to the notion that there is some *fact* to the matter as to whether or not a system has property p is evidently crucial; I for one am happy to renounce any *a priori* appeal to intuition in this regard, *with the exception of those properties that are directly observed* (macroscopic pointer-positions and the like). It is this that makes this difficulty specifically a problem of *measurement*, the one case where we *must* interpret quantum mechanical probabilities at the level of what is directly observed.

At this point one might feel that what is at issue is some sort of 'emergence' of value-definiteness through measurement. It may be that it is only when one performs an experiment to determine whether or not the system has a given property, that such probabilistic statements come into play, and that one must then deal with a modified state (i.e. that describing the system coupled to the measuring device) – that in this modification resides the transition from the state f to the state g (cf. the terminology 'transition probability', for the probability that a system in the state f 'will be found' in the state g). But so far we have made no mention of *dynamics*; the statement of the difficulty applies equally to the state describing the apparatus (and object system) *after* the measurement is completed.

Alternatively one might suppose (in view of the fact that a pure state description of a composite system does *not* imply that each subsystem can be described by a pure state) that the difficulty is rather that *no* system (considered in isolation from the environment) *has* a pure state description. This point is particularly telling in consideration of the hypothetical (pure) descriptions of macroscopic objects, since every such object is certainly coupled to the environment (in contrast to microscopic systems). But it is hard to see how the same argument can fail to apply to the environment *together* with the macroscopic measuring system (and microscopic system). One may, perhaps, make progress with the latter line, including 'the observer' as part of this environment (which appears to lead to some variant of the Everett 'relative-state' interpretation), but only insofar as the properties of systems, *as perceived by the observer*, are in fact always dispersion-free when described by the state attributed to the system by the observer. The generic solution to the problem of measurement appears to require that at least some of the properties of a system (those described at the classical level) are in fact bivalently described by some pure state (in the hidden-variable approach, of some *other* theory) at all times. In particular the supposition that 'wave-packet reduction' is an *objective* physical process, common to a variety of proposals, is committed to this strategy, as is also the supposition that one must look to dynamical evolutions which transform pure states into impure states (whether through non-linear modifications to the Schrödinger equation, or by appeal to super-selection rules or the existence of unitarily inequivalent representations for macroscopically distinguishable properties).

The present state of this problem may be briefly summarized: despite intensive debate over more than six decades, no satisfactory resolution of this dilemma has emerged. In particular, in any application of quantum mechanics in finite systems in finite times, pure states are preserved under unitary evolutions, and yield non-bivalent probability assignments to macroscopic properties.

VII

There is another viewpoint. One can suppose that quantum theory is not, properly speaking, an independent theory of dynamics at all (that it is only quantum theory *together with* classical theory that can provide a consistent description of phenomena). So much is implicit in the usual text-book formulations of the 'measurement postulate' (the pragmatic 'operational' interpretation of quantum mechanics by means of which it is actually applied), where the distinction between classical and quantum descriptions is *sui generis*. In a certain sense, this appears the orthodoxy within the physics community (it is the viewpoint of Bohr), although it is not clear that more than lip-service is paid to it. The implications of this point of view are so revolutionary (both to philosophy and to physics) that one scarcely knows how to proceed; the most *conservative* approach would be to embrace a thorough-going instrumentalism (one recalls Bohr's statement: "there is no quantum reality").

But if this is the right sort of response to the measurement problem, it would seem that the relativist has been completely outflanked. One cannot say that the classical theory has been superseded at all, for on this scenario *there is no such thing as quantum mechanics*, as an autonomous theory (distinct from and independent of classical mechanics). It would then seem that there could be no such thing as Kuhn-losses. If classical mechanics is only 'supplemented' by the quantum algorithm (let us now call it that), it is far from clear how any instance of successful application of classical mechanics could be jeopardized (in this sense it would seem that the analytic problem of reduction does not exist).

If, on the other hand, we suppose that the measurement problem must eventually submit to an autonomous resolution within or beyond quantum mechanics, then it would also seem that an analytic reduction will depend on the sort of resolution on offer. In particular, if we suppose the measurement problem points to the existence of a more detailed level of description of microscopic phenomena (that quantum mechanics is incomplete), it remains unclear whether the classical and quantum descriptions are to be considered as both 'approximating' this more detailed level of description, or whether the chain proceeds from this *via* quantum mechanics to classical theory.

These points notwithstanding, even though we do not know the circumstances under which the quantum description of phenomena (supplemented with the 'minimal' operational interpretation) *should* go over to a purely classical analysis, there is nonetheless the clear-cut question of whether, where classical analysis has been successful, consideration of quantum-mechanical effects modifies or otherwise calls into question those very applications (and her one must use the operational interpretation, disregarding awkward questions of whether or not the latter depends on the validity of classical mechanics).

The foregoing comments are brought into focus when we consider the strategy employed. In brief, the idea is to equate the equations of motion, and/or the solutions to the equations of motion, for classical pure states, to the expectation values for the corresponding quantum mechanical equations (or solutions), in some suitable pure state. At the crudest level we have Ehrenfest's theorem. This shows that for the time-dependent Hamiltonian functions on $\mathbb{R}^3$ of the form

$$H = \frac{1}{2m}(p - A)^2 + V$$

the classical equations of motion are formally similar to the expectation values of the Heisenberg-picture quantum mechanical equations of motion for the Hamiltonian

$$\hat{H} = \frac{1}{2m}(\hat{p} - \hat{A})^2 + \hat{V}$$

for any state $\psi \in L^2(\mathbb{R}^3)$. Post (1971, p. 233) is, of course, quite right to point out that at best one has a correspondence with respect to 'mean-values' of dynamical quantities, and not with respect to 'higher-moments' of these quantities, but the situation is in fact worse than that. First, there remains a discrepancy

$$\frac{i\hbar}{2}\langle \nabla \cdot \hat{A} \rangle_\psi$$

(evidently one must take the limit $\hbar \to 0$); second, one has not thereby proved (not even in the formal sense) that the expectation values of the quantum operators satisfy the classical equations of motion, for there occur quantities of the form:

$$\langle \nabla \hat{V} \rangle_\psi$$
$$\langle \nabla (\hat{A} \cdot p) \rangle_\psi$$

etc., rather than:

$$\nabla \langle \hat{V} \rangle_\psi$$
$$\nabla \langle (\hat{A} \cdot p) \rangle_\psi$$

(in general we cannot make sense of the latter expressions). But one point should be stressed: not even this qualified and formal result would be attainable were there not a sense in which dynamical variables *have the same meaning* in the two theories. And this, as we have seen, is a consequence of the fact that both theories define representations of the IHGG (it is for this reason the we have been able to employ the same symbols in the foregoing).

Coming back to Post's point, on reflection it is far from clear whether one can make sense of the notion that there be a correspondence between higher moments of quantum-mechanical dispersion and some sort of classical dispersion; the latter would require that we formulate a correspondence between quantum mechanical *pure* states and classical *impure* states (a sort of approximate 'hidden-variable theory'?), or else that we formulate a correspondence between unitary quantum evolution and a stochastic classical evolution. On reflection, given the 'curious logic' of the pragmatic problem of reduction posed above, such maneuvers do not even come into consideration. For at the pragmatic level, all that matters are successful applications of classical theory, and if we consider classical *mechanics* (and not classical statistical thermodynamics or stochastic theories of e.g. Brownian motion), the classical variances are all exactly *zero*. In effect, we suppose we are given some phenomenon, and analyze that phenomenon (i) within classical theory, by whatever means prove most effective (ii) within quantum theory (presumably in a more integrated and controlled sense), and then seek an analytic basis for comparison of the two models. So much would be a principled approach if one supposed quantum theory incomplete, and both quantum and classical theory approximations to some unknown to some unknown and deeper level of description.[19]

This applies in particular to the detailed and more general approach to the analytic problem of reduction as development by e.g. Hepp, Hagadorm, Heller, Simon, Taylor, Voros and Yajima. These authors are concerned with the establishment of precise error bounds and controlled approximations to expectation values of the momentum and position coordinates parameterized by the time t for some finite interval $t \in [0, T]$ (what may be called 'finite propositions'). Following Taylor (1984), one may state the strategy in the following terms: given classical parameters $\langle \Omega, H, T \rangle$ (where Ω is a set of classical pure states) and acceptable error ε, for each $\alpha(0) \in \Omega$ prove that there exists $\psi \in \Psi$, where Ψ is a set of quantum pure states, such that for all $t \in [0, T]$, one has

$$\left|\alpha(t) - \langle \hat{\alpha}(t) \rangle_{\psi}\right| < \varepsilon.$$

Progress with the strategy has been considerable, but it should be clear that it does not provide a basis to a resolution of the problem of measurement (but rather seems to presuppose the incompleteness of quantum mechanics). A similar formulation in the case of higher moments of dynamical variables will beg all the interesting questions at issue; since the classical pure-state variances are all zero, the quantum mechanical variances will all have to be bounded by MATHS. The gap between the 'objectivity' of outcome in the classical case (variance zero) and the 'outcome-indeterminacy' in the quantum-mechanical case is now assimilated to the notion of 'error' (and what does that mean in this context?).

VIII

What I contend should be concluded with respect to this decidedly unsatisfactory state of play is that there is a great deal more to be learnt about the relationship of quantum and classical

[19] There is an equivocation in the case of radiation theory. In semi-classical theory one has a rather good idea of how one can formulate a correspondence between classical impure states and quantum pure states (using the zero-point fluctuation and coherent states, respectively). One also knows more or less precisely when the semi-classical description will fail (through the Sudarshan correspondence, Sudarshan 1963; see also Glauber 1963). The equivocation arises because the phenomenology associated with coherent states can be understood in terms of the notion of 'intensity' (in a sense one does not need bivalence in the 'wave limit' of such and ensemble).

mechanics, but that what makes this relationship so problematic is precisely the fact that we do not, as yet, have distinct and autonomous theories.

Give further, the thesis (1) – that in dynamical physics one has no option but to build upon the structures laid down by pre-existing theory – and given further that the development of quantum theory unequivocally bears out this contention, it is by no means surprising that we should find ourselves in this position. Indeed, *regardless* of whether or not there are cases of 'Kuhn-losses', Kuhn *et al,* appear (to put the matter bluntly) to have *missed the point*. What emerges as the *central* characteristic of dynamical physics, from its inception with Newton to the present day, is the slow evolution of the mathematical and conceptual structure, in which changes introduced at one level are subject to complex, and only partially understood, inter-relationships at virtually all other levels. It is, in other words, exactly the conceptual and mathematical integrity of the entire monolithic framework which is the subject of sustained investigation, and here the problem of measurement – the most significant case of *failure* – appears to be a consequence of the *lack* of a sharp separation between quantum and classical descriptions of observable phenomena.

Of course the relativistic case is not (or not primarily) made out in the context of the history of dynamical physics. Further, my defense of Post's position – given the powerful constraint that we attend only to mathematically highly developed and empirically well-supported theories – has perhaps only limited philosophical interest, in view of its studied indifference to the question of reference. It might seem that one can then find no purchase on the task of *explaining* the success of dynamical theory, in whatever sense we are to understand the term.

As it happens, I believe that the latter conclusion would be pessimistic. Just as (or so I have argued) variance in meaning is not incompatible with a cumulative progressive view of dynamical theory, so too (as I have hinted), if one is prepared to adopt a more 'structuralist' view of physical ontology, one could say that in parallel, variance in reference is not incompatible with a cumulative and progressive view of the constitution of dynamical phenomena. It seems that what is required is that we take the mathematical conception of the world rather more *at face value*; that is, I suggest we allow that the 'furniture of the world' be rather more structural, more *mathematical*, than is ordinarily permitted within the nominalist tradition. In any case, it has long been apparent that no workable account of nomological necessity can be made out at the level of unstructured particulars (except in the context of an unfathomable and antiquated notion of the 'rule of law'). There may indeed be a further implication for the general account of inter-theoretic relations – that one has to do not only with different regimes of scale and energy, but with different *types* of description, the difference between describing what a system *does*, as opposed to what it *is* (this seems to me to apply most particularly to the relationship between the thermodynamical description of state versus that of dynamics, but also to the wider context of the application of mathematics to other disciplines).

These remarks are tentative; I wish only to indicate the lines of future development. But within the narrower confines of the thesis of correspondence, I suggest that the relativist has only two options: *either* one must acknowledge that dynamical physics is somehow immune to the well-documented vagaries of other disciplines, *or* one must argue that the constraint to highly developed and empirically well-supported dynamical theory is too strong – that, in some sense, the thesis becomes vacuously true.

I have already remarked that one finds some acknowledgement, among historians and philosophers of widely differing persuasions, of the fact (I think one must call it a fact) that dynamics is somewhat anomalous. This (which would seem to deny the charge of circularity) may be considered a tolerable exception to those of the persuasion of Laudan and Kuhn, but only if one fails to consider that as a matter of record dynamical physics *preceded* the development of virtually every other branch of science (certainly as 'mature' disciplines). This in turn leads to the obvious question: what are the implication for the *general* account of scientific epistemology, methodology and heuristics, (that is, an account directed to other disciplines), if every advance was made by scientists who had at their disposal the (essentially stable and unique) concepts of dynamics? And here it would seem that the epistemological situation is altered across the board in virtually every other discipline. This option (that dynamics is an 'exceptional case') leads, willy-nilly, to a wholesale re-evaluation of the conventional historiography and philosophy of science.

Is it, then, that the 'triviality' charge may be well-founded? Laudan (*op.cit.*) appears to adopt this line in his attack on the much broader claim of 'convergent realism'. Adherents of this claim (the early Putnam, Harmann, Boyd, Watkins) typically restrict to 'mature' branches of science, and it is with this that Laudan takes issue. In consideration of Krajewsi, for example, who insists that the thesis is nevertheless not rendered vacuous (and is subject to historical evaluation) because "every branch of science crosses at some period the threshold of maturity", Laudan retorts that since the threshold is not specified, it can always be shifted to counter any conceivable unfavorable historical observation. As it stands, this response is not particularly relevant to the thesis at issue here. The nearest analog would run along the lines that what is to count as *mathematically highly developed* and *well-confirmed* are shifting goalposts.

But this is no argument at all, except (perhaps) in connection with the Newtonian theory of light. For what is at issue are *accepted* theories of dynamics, beginning (astronomy and statics apart) with Newton. There have only ever been the theories of mechanics, electromagnetism, relativity and quantum theory, and these we have just reviewed. And if the relativist insists that one consider the rich and extensive pre-history of dynamics, then we may simply adopt the normative stance: let modern science be judged to begin with Newton (and what is *ad hoc* about this judgment?).[20] Might the relativist instead object that we have simply separated off that part of physics (indeed, that part of empirical science) that resists the relativist critique? The objection might have some force if 'that part' were some sort of heterogenous collection of results, those that happened never to be subsequently challenged; but the whole point is that that which has been separated off displays and internal and structural integrity of quite fabulous proportions. Taken in a different spirit the observation is exactly correct: we have isolated some part of science that resists the relativist critique. It is

[20] Evidently there is a great deal to be said on this score; so too, concerning the pre-history of mechanics, and the general relationship between mathematical innovation and the systematization of experience (the experimental method). These topics are so large that I can do no more than gesture to them.

that part which is distinctively mathematical, which employs an ill-understood but characteristically progressive heuristics and methodology, the emergence of which constituted the beginning of the Modern period and which has provided its most central and unifying framework. That this part exists – could there be a more damaging critique of the relativist thesis?

Post, I suggest, was right, and Kuhn was wrong. Let relativists defend their thesis, for if it fails in so central a field as this then it would seem wholly misconceived. There are passing fashions in philosophy, of which this has surely been the most pernicious: the important tasks of understanding better the role of mathematics in science, of developing a theory of truth and reference appropriate to the thesis of correspondence in dynamics, which do justice to its vast and exponentially-increasing range of phenomenological applications, lie still before us.

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