Molecular Models and the Question of Physicalism

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Abstract: By their own account, physicalists are committed to the claim that physics is causally complete, or closed. The claim is presented as an empirical one. However, detailed and explicit empirical arguments for the claim are rare. I argue that molecular models are a key source of evidence but that, on closer inspection, they do not support the completeness claim.

Keywords: *chemistry, physicalism, reductionism, supervenience, disunity of science.*

1. Introduction

There has been much investigation of whether chemical theories and entities are reducible to those of physics.[1] Given the consensus that *classical* reductionism is false, attention understandably turns to the question of whether the dependence of the chemical on the physical is of some looser kind, as might for instance be elucidated by the relation of supervenience. The aim of this paper is to connect the specific issue of the relation between the subject matters of chemistry and physics with the broader question of physicalism as it is addressed within contemporary philosophy of mind. There is an emerging consensus that the central question of physicalism is whether – and in what sense – the physical is causally closed, an issue that famously goes back to Descartes. I will argue that if that question is to be addressed empirically, the right way to do so is to attend to the detailed structure of, and the direction of explanation within, quantum-mechanical molecular models.

Most of what follows will be directly relevant only to the purported dependence of the chemical on the physical, although that dependence claim, if established, *would* perhaps also be relevant to physicalist dependence claims concerning the biological, the mental and so on. Classical reductionism as characterized by Putnam and Oppenheim (1958, p. 407) was *explicitly* hierarchical and cumulative, and so must be *non*-reductive physicalist positions, in

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so far as the dependence relations on which they turn are also transitive. Appeal to chemical theories is widespread in biochemistry, and part of the scientific case for the physical dependence of the biological (and the mental, and the social) must be an argument for the dependence of the chemical on the physical. That being so, chemistry might seem to be the discipline for which physicalist dependence claims are at their most plausible, but I will argue that this appearance is deceptive: reasons for thinking that the chemical depends on the physical are *at best* only *as good as* the reasons for thinking that the biological, the mental and so on are so dependent. This is because 'physical' can be construed as a contrastive term: when distinguishing the physical from (say) the biological or the mental, it is often taken broadly, to mean the 'physico-chemical'. Dependence claims targeted on these other disciplines can enjoy extra support from such principled claims as are embodied in functionalism, about concepts that characterize discourse within these disciplines, concepts that also help to mark their domains off from the (broadly) physical in principled ways. However, the question of the dependence of the chemical on the physical must involve a *narrow* conception of the physical, one that is more closely connected with the science of physics. Hence arguments for this thesis must depend solely on the plausibility of (broadly) empirical claims, such as are cited within physics itself for various physical laws. The burden of my argument is that evidential support for that last thesis is weak.

2. What is Physicalism?

Physicalism is the ontological position according to which the physical facts determine all the facts. Physicalists are therefore committed to one of two positions: either chemical facts are determined by physical facts, or there *are* no chemical facts, strictly speaking. The second kind of position, involving an instrumentalist stance towards chemical laws and entities, is implied by at least one anti-reductionist commentator on chemistry (Primas 1983, for further discussion see also Hendry 1998b, pp.127-30), while analogous positions have been taken with respect to biology (Rosenberg 1994).[2] It is the purpose of this paper to explore the content and standing of the first kind of position, however: to do that requires some account of what it is for one set of facts to determine another (and conversely, for one set of facts to *depend* on another), and also some account of what it is for a set of facts to count as 'physical'.

An initial and well-known attempt to explain how the facts, entities, or laws associated with one domain could determine those of another proceeded in terms of *reducibility*. To borrow a formulation from Field (1992, pp. 272-

3), the classical reducibility of chemistry to physics would require that for every true sentence in the language of chemistry there would be a physical transcription: a sentence in the language of physics that (as Field puts it) "expresses the same facts" (p. 272). A second (and arguably subordinate) requirement concerns explanation: that every good chemical explanation could be recast as a physical explanation, although the physical explanations may well be less illuminating than their higher-level counterparts. So the reductionist picture required chemical and physical laws to be bound together by *bridge laws* from which, together with a precise physical description[3] of the physical entities, the chemical laws (or approximations to them) could be deduced. The difficulties associated with the reductionist picture are well-known, and center on the bridge laws. Even if everything is, at bottom, physical, higher-level properties might admit of multiple physical realizations that would make the disjunctive lists of properties on the physical side of bridge laws open-ended. Furthermore the length and complexity of those disjunctive lists might debar them from counting as genuine physical properties, and hence appearing in genuine physical laws and explanations, all of which presents reductionism with a major difficulty in meeting its explanatory commitment.

However, an argument for physicalist reduction of chemistry should not require that a classical reduction be displayed for *every* chemical law. Field (1992) and Smith (1992), by way of refining and defending reductionism, have presented plausible relaxations of the physical transcription requirement, and along with it, a response to the explanatory problem. Field and Smith require only what Field calls 'sketches' (1992, p. 274) and Smith calls 'quasi-reductions' (1992, pp. 29-30). Sketchy, approximate quasi-reductions fall short of offering the tight bridge laws that would allow *replacement* of the dependent ontology within science (in this case, that of chemistry): the dependent ontology might remain indispensable in explaining higher-level regularities. But the reductionist dependence claim surely *would* require successful reductions (or quasi-reductions) of at least a representative sample of dependent systems, plus reasons for thinking that this good fortune will continue, with further reductions and quasi-reductions forthcoming.

According to classical reductionism, the dependence of chemical facts on physical facts would be reflected in tight *logical* relationships between chemical and physical theories. Given that physicalism is driven by an ontological intuition, it would make sense, if the reductionist picture is to be rejected, for the physicalist to detach the logical and ontological claims. Perhaps the logical relationships only became primary in the heyday of that descendant of logical empiricism, the received view of theories, with its inherited mistrust of ontological relations that fail to be expressed in logical relations between theories. At the heart of *non-reductive* physicalism would be a relation that could hold between the entities, properties, events or processes associated with two sciences that would capture the idea that one science's facts determine the other's, without making too many hostages to fortune as to logical relationships between the *theories* that describe those entities, properties, events or processes. One move, famously, is to require only token identity of higher-level with lower-level entities or events, leaving type-identity open (another is to relativize the type-identities to particular higher-level species, but this is a form of reductionism). Another alternative is, of course, supervenience, which allows for the distinctness of the higher-level entities, properties, events or processes, but attempts to capture the sense in which they are nevertheless dependent on lower-level entities, properties, events, or processes.

However, supervenience is a family of relations, and an extended family at that. Supervenience is usually characterized as a relation between groups of properties (see Kim 1984). Roughly, a group of properties, A (the supervenient group), supervenes on another, B (the subvenient, or base group) when there cannot be variation in respect of A without variation in respect of B. Supervenience does seem a plausible determination relation in that it shows how the A-properties are fixed along with the B-properties. Not only that: coupled with a claim about the causal completeness of the physical and a denial of causal overdetermination, we could add that causal (and possibly explanatory) claims associated with the 'higher level' science in which the supervenient properties are discussed fail to be autonomous. Causal powers conferred by the possession of supervenient properties are really conferred 'in virtue of' the subvenient properties, since we can always look to the lower-level change as the real cause of any effects apparently brought about by changes among the supervenient properties. Hence supervenience offers something parallel to the explanatory commitment in reductionism: although we may not have physical explanations of higher-level regularities, we do have an assurance that the causal processes that constitute the basis of higher level explanations are, in fact, physical processes.

However, the situation is a bit more complicated: for A to supervene on B requires some modal force to the covariation of A and B (hence 'cannot' rather than 'does not'),[4] and how closely the A-properties are determined by the B-properties depends famously on the strength of the modal force. Weak supervenience requires that B-identical systems within the same possible world must be A-identical, but allows A-discernibility between B-indiscernibles in different possible worlds. Strong supervenience requires in addition that B-indiscernibility entails A-indiscernibility *across* different worlds. If we think of supervenience as a consistency requirement on the attribution of supervenient properties given the fixation of the base properties, weak supervenience requires consistency only within possible worlds, strong supervenience requires consistency also between them. There are, of course, significant differences between physicalist claims formulated in terms of the various kinds of determination. It is sometimes held that too much variation is allowed among weakly supervenient properties for it to be said that they are determined by the assignment of subvenient properties. However, there are contexts in which weak supervenience is a very plausible way of capturing determination. On a projective or response-dependent account of the supervenient properties (think for example of evaluative properties supervening on descriptive ones), there might well be differences among supervenient-property attributions between counterpart-communities of supervenient-property attributers in different possible worlds. On the other hand, strong supervenience has sometimes been argued to require such *close* determination of supervenient by subvenient properties that it threatens to collapse the new, supposedly non-reductive physicalism into reductionism after all, although the seriousness of that threat depends on the extent to which one thinks that properties are closed under logical operations (see Kim 1984). One further objection to the kind of physicalism that is limited to a strong supervenience claim is that, to accompany correlations among higher-level properties, it posits correlations among base properties. Such correlations are unexplained except by the reductionist (see Field 1992, Papineau 1992, Smith 1992 and Kim 1997). Hence in a world where strong supervenience holds, epistemic values will force us to seek reductions, to the extent that explanation is a telling epistemic requirement. Reductionism is superior, but *only if* the relevant explanations can be found: reductionists sometimes seem to argue as if the injunction to seek them implies that they will be found.

So to the second dimension in the elucidation of physicalism: the boundary of the physical. In arguments concerning the dependence of the mental on the physical, a broad sense of 'physical' is typically at work: roughly, one according to which physical objects are those that are spatially located, and physical properties are those that can be borne only by physical objects. However, this construal is obviously far too broad for our present purposes, since it fails to exclude the chemical. If the physical includes the chemical in this way, then dependence claims of all kinds are established very easily, and are correspondingly uninformative. If, for instance, chemical properties are a subset of physical properties, then because supervenience is a reflexive relation among sets of properties (trivially, there cannot be change in respect of A without variation in respect of B where $A \subseteq B$), the chemical supervenes on the physical, but only because it supervenes on the chemical. This would be a *terminological* answer to the question of chemistry's dependence on physics, because it would leave open what dependence relations hold between different subsets of 'physical' properties. A narrower, and therefore more informative, conception of the physical proceeds in terms of the *discipline* of physics. However, this is not yet adequate, for physics itself studies a heterogeneous array of entities and processes. It is hard to see why theories constructed within such areas of physics as fluid dynamics and astrophysics should be thought more 'fundamental' than chemistry.[5] Nor do physicalists give much serious thought to which particular domains of phenomena have come to be studied in physics departments, rather than (say) departments of engineering or chemistry, or why historical accidents of this sort should give physics all the ontological authority. Rather, a well-motivated conception of the physical will presumably proceed in terms of the laws and categories associated with a few 'fundamental' – for which read, general or abstract – theories in physics, namely quantum mechanics and particle physics. This, to be fair, is how physicalists have tended to identify the physical: in a sense that allows it to contrast with the chemical, and be correspondingly informative (see for instance Quine 1981, Papineau 1990 and Field 1992). In what follows I will use the term 'physical' in this narrow sense.

Thus we have physicalist positions of a variety of strengths, each of which seems to capture in its own way the one-sentence sketch of physicalism with which I began this section. Applied to the relation between chemistry and physics, they yield the following possibilities: that chemical facts are determined by (micro-)physical facts in the sense that the chemical entities, properties, events or processes to which they relate just *are*, or are reducible to, or supervene on, micro-physical entities, properties, events or processes. Having done all this work to review and distinguish the various kinds of physicalism, I propose to lump them all together again, at least for the purposes of appraising their support. For physicalists seem to agree that physicalism, properly so-called, involves a claim that has been called the *completeness* of physics: physics alone is self-contained; its explanations need make no appeal to the laws of other sciences, and its laws cover the entities, properties, events or processes that are studied by the special sciences. Hence all versions of physicalism, properly so-called, stand and fall together, along with that claim. This is not to say that they are all on a par, epistemically: were the universality of physics to be established, important work would have to be done in finding out, for each higher-level science, which version of physicalism best captures the detailed structure of relations between physical and higher-level properties. However, for those who, as I do, think that there is little reason to accept the completeness of physics, this is not a pressing task.

3. Arguments for Physicalism and the Completeness of Physics

Why should every physicalist be committed to the completeness of physics? The thought is as follows: from the causal point of view, higher-level entities and properties can work only *through* the physical entities and properties on which they depend for their existence, whether or not we count the former as distinct from the latter (in whatever sense of 'distinct'). Whenever I intentionally move my arm, my intentions can act only through the medium of my central nervous system. Whenever a chemical reaction occurs, this can occur only through the rearrangement of charge densities and the like. However, the laws that cover interactions among charge densities in the simpler situations studied by physics cannot magically be violated in the more complex situations studied by chemistry. Hence the physicalist cannot countenance that higher-level entities or processes enjoy causal powers over and above those conferred on them by the physical entities and properties on which they depend. As stated, the non-physicalist will rightly see this argument as begging the question: why *should* the laws governing the microphysical be uniquely inviolable, unless we already think that the chemical whole is 'no more than' the sum of its physical parts?

In response, physicalists can offer two interconnected, broadly empirical arguments. Firstly, unlike the laws of microphysics, the laws of the higher-level sciences are (implicitly or explicitly) hedged by *ceteris paribus* clauses: laws expressing the chemical behavior of molecular species, for instance, apply only in the lower-energy environments in which those molecules are stable. After that, the microphysical laws take over. Unlike higher-level laws, which need not be generally applicable to be true, to doubt the full generality of a microphysical law is to doubt its truth. The second argument concerns the direction of scientific explanation: in general, explanations of the behavior of entities at level n proceed in terms of the laws governing their parts (*i.e.* entities at level n-1). In particular, the twentieth century has been witness to the onward march of quantum-mechanical explanation within chemistry – of chemical bonding and spectroscopy, for instance – and these explanations appeal to the fundamental microphysical laws governing the behavior of electrons and nuclei, but no special (or mysterious) non-physical chemical causal powers. The thought must be that the direction of explanation mirrors the direction of determination. I will return to these arguments in Sect. 4.

None of this yet shows that the physicalist must *first* establish the universality of physics. Perhaps physicalism of one sort or another can be argued for on independent grounds, with the completeness claim dropping out as a corollary. However, the completeness of physics typically appears as a lemma. In 1966, Lewis, arguing for (a restricted) type-identity of mental and physical, was explicit about his appeal to the completeness of physics, and the empirical nature of that claim:

> A confidence in the explanatory adequacy of physics is a vital part [...] of any full-blooded materialism. It is the empirical foundation on which materialism builds its superstructure of ontological and cosmological doctrines [Lewis 1966, p. 105].

Papineau's argument that all facts (strongly) supervene on physical facts (Papineau 1990, 1993, 1995) is similarly explicit in this commitment, as is Loewer (1995), Smith (1992), Smith & Jones (1986, pp. 57-9). Field (1992), Papineau (1992), and Smith (1992) begin their arguments for reductionist theses by criticizing supervenience versions of physicalism on the grounds that supervenience fails to explain the physical correlations that underlie mental (or other) correlations. Supervenience physicalism and its basis in the completeness of physics are taken to be unproblematic, and the argument is about whether or not to go further. On these arguments, (modern, reformed) reductionism too inherits the evidential route through the completeness of physics, although Field (1992, p. 283) also commits himself independently to the completeness claim. But how is the claim supported?

Detailed arguments for the completeness of physics have been fairly thin on the ground, beyond claims that it is somehow built in to the methods of both physics and other sciences. I will examine those arguments in the next section. Papineau's exchange with Crane, however, is an exception (see Papineau 1990, 1991; Crane 1991), for Papineau offers what I will call a *contrastive* argument for the completeness of physics. The argument is 'of general significance' (Papineau 1990, p. 66), in that it is intended to establish the supervenience on the physical of such diverse categories as 'the psychological, the biological, the meteorological, the chemical', although his examples concentrate on the psychological.[6] His argument is premised explicitly on the completeness of physics:

> all physical events are determined (or have their chances determined) entirely by prior physical events according to physical laws [Papineau 1990, p. 67].

Papineau (1990, p. 70) acknowledges one obvious objection to the completeness of physics, which arises from the pessimistically inductive thought that it is highly likely that there are kinds of event that current physics *does not yet* cover. Hence current physics is not complete. Given that one would not want the provisional nature of physical knowledge to allow so easily for the independence of the mental, Papineau makes the completeness of physics *trivial* by identifying 'physics' with whatever body of scientific theory turns out to give a complete account of such *straightforwardly* physical events as stone-fallings.

Crane (1991) complains that Papineau's argument begs the question: if it supports a non-trivial physicalism (*i.e.* one that can reasonably exclude the mental from the physical), then it must appeal to just the sort of mereological supervenience intuitions that a non-physicalist will dispute. Otherwise it is only an *equivocation* between what he calls PHYSICS (the by-definition complete science) and *physics* (the discipline that is practiced in present-day physics laboratories) that allows us to claim that mental states are unlikely to be cited in explanations of physical events offered by PHYSICS. We do not know anything about what will turn out to be required by PHYSICS. However, perhaps this is unfair to the kind of argument that Papineau is offering. As Papineau himself points out, the physicalism so defined is uninformative only if it turns out that (complete) PHYSICS must appeal to mental states. The intention is clearly that there is an *evidential* connection between (current) *physics* and (complete) PHYSICS, in the sense that what is appealed to in explanations offered by current physics gives us a guide, albeit a fallible one, to what kinds of states will be appealed to in explanations offered by (complete) PHYSICS:

> I take it that current physics is committed to developing a complete theory formulated in terms of the categories of energy, field and spacetime structure. Now, it seems reasonably plausible to me that no such theory is possible, because of as yet unknown physical effects that cannot be accounted for in terms of these current categories, and that therefore a genuinely complete theory (PHYSICS) will need to appeal to further explanatory categories. What seems unlikely to me is that these further categories should include mental ones [Papineau 1991, p. 38].

Now some dependence claims are *transparently* generated, or at least supported, by reflection on the concepts whose use characterizes the supervenient domain. Take, for example the view associated with Moore and Hare that evaluative properties supervene on natural properties: part of what makes this view plausible is that it would be *evaluatively* inconsistent to make differing evaluative judgements on two things that did not differ descriptively. Other supervenience claims do not so obviously turn on claims about the supervenient domain, but on further examination turn out to do so. To take a relevant example, functionalist approaches to the mental make plausible the supervenience of the mental on the physical, because functionalism has it that mental states are identified by their (physical) causes and effects (Lewis 1966, Papineau 1990, Loewer 1995). Hence a view of the supervenient domain (in this case the mental) motivates a view of its connection with its putative base domain. Mental states necessarily have physical causes and behavioral (*i.e.* physical) effects, which are then the subject of requests for physical explanation. The causal completeness of the physical and the implausibility of causal overdetermination are then raised to establish that if mental states are causally efficacious, they must be so in virtue of their dependence on physical states of certain sorts, even if, as in the case of mental properties, multiple realizability means that characterizations of these physical states in physical terms might be very complicated.

I think that Papineau's argument implicitly turns on intuitions about one supervenient domain – the mental – in another way, and in a way that means his argument cannot establish the physicalist claim with respect to the chemical. Let us accept for the sake of argument Papineau's (quite plausible) contention that reflection on the practice of present-day physics supports the view that mental categories will not be cited in the laws of a completed physics. Mental categories are rarely, if ever, cited in present-day physics,[7] and if final physics is anything like current physics – and we surely have no better model – then mental categories really are unlikely to appear in the laws of a completed physics. To establish the general claim that *all* facts – and not just mental facts – supervene on the physical, we need to know that categories appearing in theories of the biological, the meteorological and the chemical will *also* fail to appear in completed physics. What reasons have we been given for thinking that they will not appear? Papineau left them implicit, but whatever the science, the plausibility of the claim would presumably need to be established by inspection of current physics. Now one method is to think in a general way about how the relevant higher-level domain is marked off from the physical: biology, for instance, is the science of living things, and discourse in biology is characterized by functional concepts, concepts that (reflection quickly assures us) do not appear in the explanations offered by current physics. It is difficult to imagine how a similar argument would run in the case of (standard) chemical categories, most of which are hard to differentiate from physical categories in any principled way. So even supposing that versions of Papineau's contrastive completeness claim are true for some supervenient domains like the psychological and the biological, it is difficult to see how one could be made out in respect of chemical categories. Therefore, we are thrown back on physics itself – and more particularly its successes, methods and guiding assumptions – for support for the completeness claim.

4. Chemistry and The Completeness of Physics

There are two interlocking elements to the claim that physics is complete: (i) the *autonomy* and (ii) the *universality* of physics. A science is *autonomous* if its laws and explanations make no appeal to the laws or categories of other sciences. On the physicalist view, only microphysics is autonomous in this way, since only its laws are free of *ceteris paribus* conditions that must be expressed in the language of other sciences (that is, to the extent that *ceteris paribus* conditions can be expressed in any but the sketchiest terms). A science's laws are *universal* if they cover the behavior of *every* real entity or process: note that the universality of physics need not beg the question of physicalism if 'coverage' is suitably understood to allow that a physical law may 'cover' (*i.e.* fix some truths about) an entity or process without determining *all* the truths about that entity or process. The universality claim for physics is made plausible by the ubiquity of microphysical entities: according to chemical theory itself, the parts of chemical entities are studied by physics (the same mereological intuitions work, *mutatis mutandis*, for the dependence of yet higher-level entities on chemical entities). The autonomy and universality claims work in partnership: the universality claim establishes that microphysical laws act everywhere (since everything has physical parts), while the autonomy claim establishes that microphysical laws *determine* the behavior of whatever they cover, to the extent that that behavior is determined by law, since there are no situations that must be covered by non-physical *ceteris paribus* conditions. Whatever physics' laws cover, they cover alone, and they cover everything.

What evidence is there for completeness, so understood? What physicalists have in mind, I take it, is straightforwardly scientific evidence of the kind that is considered by Nobel Prize Committees, and may subsequently appear in textbooks of physics and chemistry. The evidence, presumably, is of two sorts.[8] On the one hand are the well-known experiments which, according to the textbooks, could be accounted for by only quantum mechanics, but not classical mechanics: take the behavior of the Stern-Gerlach apparatus as a canonical example. On the other hand are the explanatory achievements of quantum mechanics within chemistry, starting with the opposing treatments of the hydrogen molecule due to Condon, and Heitler and London, and expanding outwards to provide all the well-known explanations of molecular structure and spectroscopic behavior. Now the physicalist will concede that for practical reasons, it will never be the case that there is a detailed quantum-mechanical treatment for every situation: the physicalist argument necessarily involves generalization from a few cases. The physicalist and the non-physicalist will differ over whether this matters. Physicalists present their generalization as an ordinary scientific inference, and view any resistance as unmotivated skepticism. Hence, the onus appears to be on the non-physicalist to provide *specific* reasons to resist the generalization.

Turning to the experimental evidence first, Cartwright (1994) does indeed oppose the inference to the generality of quantum mechanics from its successful treatment of carefully controlled situations like (say) the Stern-Gerlach experiment, on the specific grounds that such situations *are* carefully controlled. Firstly, to perform the experiments that constitute the main evidence for fundamental physical theories like quantum mechanics requires a great deal of skill, involving detailed knowledge of the kinds of perturbation that can defeat the fleeting effects that such experiments seek to display. Even if the physical states whose causal powers are displayed in these experiments persist in the wild, some assurance is required that in persisting, they make more than a negligible contribution to the behaviour of systems of which they form part. No argument to that effect is likely to be forthcoming from physics, because theoretical descriptions of the wild behaviour are intractable even when they can be written down. Secondly, it is far from clear that quantum-mechanical accounts of the Stern-Gerlach experiment are autonomous, in the sense outlined earlier. Hasok Chang (1995) has argued that many experiments of central relevance to quantum mechanics are understood in terms of calculations that are grounded in classical electrodynamics and mechanics. Among Chang's examples are: the use of magnetic and electric deflection to measure the kinetic energy and momentum of microscopic particles, the charge-mass ratio for electrons (Thomson) and alpha particles (Rutherford); and Millikan's study of the photoelectric effect and calculation of Planck's constant (see Chang 1995, pp. 122-5). On the face of it, the classical theories are incompatible with quantum mechanics, but the use of pre-quantum-mechanical theories might be sanctioned by the well-known convergences – Ehrenfest's theorem and Bohr's principle of correspondence – between quantum and classical predictions. But these convergences are limited, and in any case Chang (1995, p. 127) points out that *predictive* convergences mask *conceptual* inconsistencies: in deflection experiments, for instance, classical equations are used to deduce the *path* of a particle with a given energy, but under standard interpretations, quantum mechanics denies that microscopic entities *have* paths. The universality of quantum mechanics is not, in practice, assumed in theoretical accounts of the very experiments that are adduced in its support. Surely, an experiment supports a theory *as a universal theory* only if theoretical accounts of that experiment are understood in ways that are consistent with its universality. It is one thing to have a quantum-mechanical account of the spin states of a silver atom, quite another to have a quantum-mechanical account of the whole Stern-Gerlach apparatus: the universality of quantum mechanics demands the latter.

Of course, the physicalist will reply that classical physics is still physics. True, but classical physics certainly is not microphysics. Part of the initial plausibility of the completeness of physics comes from what I called the ubiquity of physical entities. However, the price of the ubiquity argument is that it motivates only the universality of *microphysical* laws. And surely the disunity of physics supports the incompleteness of microphysics.

The second kind of evidence concerned the explanatory applications of quantum mechanics to molecules: I will argue that the models on which those explanations turn typically fail to display the autonomy that physicalism requires. Cartwright (1983) long ago argued that the central equations of physical theories are expressed in such abstract terms that they say very little about the specific situations covered by higher-level sciences. Newton's second law, for instance, tells us about the acceleration on a body only if we know the total force that acts on it. Analogously, Schrödinger's equation can tell us something about a system's possible quantum states only with the benefit of a Hamiltonian for that system. Of course, these great theories are not purely kinematical. Associated with Newton's second law there is the law of gravitation, and Coulomb's law. To go with Schrödinger's equation we have a choice: on the one hand are the familiar gravitational and Coulombic force laws, and on the other hand, a (relatively short) list of what Cartwright (1983) called 'model' Hamiltonians (the hydrogen atom, the harmonic oscillator and the rigid rotator among others), usually to be found in the chapter of the textbook on quantum mechanics *after* the chapter in which the Schrödinger equation was introduced. Cartwright argued that it is the *model* Hamiltonians that get applied, rather than the general principles. Now it may not be immediately obvious, but this bears directly on the argument for the completeness of physics: if the general principles were widely used in explanation, then the direction of physical explanation would be as the physicalist expects, that is, from the part to the system as a whole. However, where a 'model' Hamiltonian is used, the direction of explanation is far less clear. In the case of quantum chemistry, I will argue that it is from whole to part.

As any undergraduate quantum chemist knows, one can write down a Hamiltonian for any molecule, given the enumeration of particles present, and their interactions (which are usually taken to be Coulombic only). But the Hamiltonians so constructed conspicuously fail to appear in interesting explanations.[9] Here, for instance, is how one textbook of spectroscopy describes carbon dioxide:

> The CO_2 molecule is linear and contains three atoms; therefore it has four fundamental vibrations […] The symmetrical stretching vibration is inactive in the infrared since it produces no change in the dipole moment of the molecule. The bending vibrations […] are equivalent, and are the resolved components of bending motion oriented at any angle to the internuclear axis; they have the same frequency and are said to be doubly degenerate [Silverstein *et al.* 1981, p. 96].

The next step is to apply quantum mechanics, via the model Hamiltonians. With some adjustments, the quantum-mechanical rigid rotator and harmonic oscillator allow us to quantize the rotational and vibrational motions that background chemical theory tells us the carbon dioxide molecule must exhibit. This provides the energy levels: differences between these energy levels correspond to spectral lines (in the infrared region in the case of CO_2's vibrational modes). For a more accurate account, vibrational and rotational modes are coupled. To a first approximation, vibrational and rotational modes are taken to be additive, but finer structure can be explained in terms of anharmonicity and other effects of the distortion of the molecule away from its equilibrium geometry (see for instance Steinfeld 1985, chapt. 8). The model hinges on the prior specification of the molecular backbone, whose quantized motions give rise to the spectroscopic behavior.

The widespread use of models like these is relevant to the physicalist argument in two ways. Firstly, the molecular backbone is a creature of a body of chemical theory going back to the nineteenth century. *Prima facie*, the attribution of such structure without further quantum-mechanical explanation is inconsistent with the autonomy of the quantum-mechanical explanation as an item of physical theorizing. Secondly, it is natural to read the attribution of such structures as the direct attribution of a state to the molecule as a whole, a state that is not further explained in terms of the more fundamental force laws governing pairwise interactions between the constituent electrons and nuclei. Given that this state constrains the quantized motions of the functional groups appearing in the spectroscopic explanation, the direction of explanation appears to be downwards – *from* the molecular structure *to* the motions of the parts – *contra* the intuitions about mereological determination that drive the physicalist argument.

Of course, the physicalist can respond with what I have elsewhere called the 'proxy defence' (Hendry 1998b). The model Hamiltonians are not free-standing, unexplained chemical explainers. Rather they are proxies for rigorous treatments (which do meet the physicalist's requirements of autonomy and upward-explanation). In principle, given enough computing power and the right Hamiltonians, a rigorous quantum mechanical treatment would go through. However, in their absence we use proxies, which we can justify and to that extent explain, as approximations to the exact treatments. What we do explain with the model Hamiltonians, we could explain with the exact treatments. That argument, however, needs to be made out in detail, case by case, and that argument must establish that explanatorily relevant features of the model Hamiltonian are shared by the exact version. For instance, one way of justifying the classical treatment of the molecular backbone is via the Born-Oppenheimer approximation: the clamped nuclear geometry corresponds to the first term of an expansion series in terms of nuclear-electronic coupling that in the limit tends to the exact treatment. However, it is far from clear that the justification works, or what it would show if it did. Thus, for instance, the symmetry properties of the model Hamiltonian – which certainly are explanatorily relevant – may not survive the expansion (see Woolley 1976, and Hendry 1998b, sect. 2). Now this argument is far from closed: my point is just that physicalism makes commitments whose truth is far from clear.

According to one last argument, the requirement of universal coverage is built into the very practice of physics. Thus Quine has it that

> If the physicist suspected there was any event that did not consist in a redistribution of the elementary states allowed for by his physical theory, he would seek a way of supplementing his theory. Full coverage in this sense is the very business of physics, and only of physics. [Quine 1981, p. 98]

Thus does Quine establish a form of supervenience on the back of a methodological claim. How does a physicist go about showing that some event *does* consist in a redistribution of the elementary states allowed for by his physical theory? By giving a suitable application of the theory in question that *does* imply that the event occurred. So Quine is arguing that physicists perceive a duty to provide applications of their theories to every kind of physical situation. This seems to me to be about as false as claims in philosophy about scientific practice ever are. What serious effort has *any* physicist ever put into checking whether the motions of banknotes like Neurath's consist in redistributions of elementary states allowed for by current physical theory? My point is not just the Lakatosian one that there are 'recalcitrant instances' for every theory, that every theory 'wallows in a sea of anomalies'. Nor is it the 'pessimistic induction' that we can expect *any* particular theory, in the fullness of time, to be overthrown. If true, both of these claims apply equally to sciences other than physics within *their* domains, and could apply to physics even if physical theories were subject to a duty of universal coverage, that is, even if the domain of physics is all-encompassing. No: the point is just that there are large classes of events for which there is no tendency for physicists even to *begin* to construct detailed applications, which are therefore not part of the 'business of physics', but which *are* the business of other sciences.

Field (1992, p. 283) and Smith (1992, p. 40) make much weaker methodological claims than does Quine, arguing that physicists feel explanatory, rather than predictive duties. Field argues that physics is in the business of ensuring that its theories 'mesh' with those of higher level sciences, and that 'successful meshing' between physical and higher-level theories requires explanation: the explanations are provided reductively. Thus, the explanatory duties of physics push it towards reduction of other sciences. Smith has it that

> any physicalist worth his salt will insist that, where a low-level theory interfaces with a higher-level theory, we should be able to use the lower-level theory to explain why assumptions of the higher-level theory actually obtain [Smith 1992, pp. 39-40].

These claims, like Quine's, are false. For a 'mesh', like an 'interface' consists of a sketched and approximate microreduction. When we examined the detailed applications of quantum mechanics (to carbon dioxide), there was no mesh, or interface *between* the quantum mechanics and chemistry, at least none that required explanation. Rather than an explanation *of* chemical structure *by* physical theory there was a joint venture: the explanation of various facts by appeal to a molecular structure put in by hand.

Perhaps this is too easy. Perhaps universal applicability is a *duty* that physics accepts for its theories: attempts to unify disparate domains have motivated some of the most ambitious and successful episodes in the history of science. Newtonian mechanics, we are often told, was the synthesis of terrestrial and astronomical physics. More poignantly for the present discussion, in the early 1920s – the last years of the old quantum theory – attempts to fit atomic models to spectroscopic data required a diverse battery of inexplicable and mutually incompatible quantum conditions. Pauli and Born, among others, saw in this chaos the need for a radical departure. Hindsight tells us that it was quantum mechanics that they foresaw, a theory whose appeal, initially at least, lay in its unifying power. A *methodological* objection to the acceptance of disunity in science follows: so much the worse for subsequent progress had Heisenberg been content with the disunited scene that prompted his efforts. One might, of course, quibble with the details: firstly, quantum mechanics merely ushered in a new set of disunities, as we have seen; secondly, the historical claim that important advances in physics always arise from the unifying impulse is surely false. However, there is some justification in the complaint, for whether or not quantum mechanics *really* unified physics, it was an important advance. With hindsight, would we really have counseled Pauli, Heisenberg, and Born to be content with the old quantum theory? This is *only* a methodological complaint, however. If expectations of unity are sometimes fruitful, this does not imply the truth of the underlying reductionist metaphysics. The fruitfulness of an aim does not imply its achievement.[10] Nor should it blind us to disunities in science.[11]

Notes

1 For a guide to some of the relevant literature, see Scerri 1997.

2 Dupré 1993 offers an opposing, realist – but still anti-reductionist – approach to biology.

3 'Physical description' is here used in the broad sense, encompassing the physical laws that are taken to govern the target systems.

4 Although supervenience now customarily involves a modal element, Quine has formulated a 'nonreductive, nontranslational' physicalism that is recognizably a member of the supervenience clan despite (unsurprisingly) being non-modal: 'nothing happens in the world, not the flutter of an eyelid, not the flicker of a thought, without some redistribution of microphysical states' (1981, p. 98).

Quine's thought is that physics alone is in the business of full coverage. For criticism of non-modal supervenience as a determination relation see Field 1992, pp. 280-1.

5 For discussion of these issues see Crane & Mellor 1991, sect. 1 & 2; Papineau 1990, 1991; and Crane 1991. See also Sect. 3, below.

6 This may seem like an irrelevant aside, but it is a pertinent point to which I will return.

7 The Copenhagen interpretation of quantum mechanics looks like an exception, but part of a claim like Papineau's may be the plausible bet that nothing like the Copenhagen interpretation of quantum mechanics will appear in a completed physics.

8 This way of thinking about the evidence, and the following critical discussion are a more developed version of arguments presented in Hendry 1998a.

9 Apart, that is, from a few very simple cases like the hydrogen atom, whose Coulombic Schrödinger equation can be solved exactly, and whose solution therefore occupies much of the time in the average course in molecular quantum mechanics.

10 I have argued elsewhere for a similar separation of methodological claims and philosophical conclusion in the case of scientific realism: see Hendry, 1995.

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