The Limits Of Reductionism

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ABSTRACT

The central claim of this thesis is that physicalism is not well supported by our scientific knowledge. The argument splits into four sections. In section I, I argue that given the current state of science a reductive version of physicalism cannot be maintained, if theoretical reductions are thought to be obtainable in practice. In section II, I argue that physicalists who are realists about higher-order properties are committed to reduction *in principle* of these properties to physics. In section III, I argue that one of the core elements of physicalist philosophy, the completeness of physics, is highly questionable in light of certain features of quantum theory. In the final section, I survey a range of possible alternatives ontologies to physicalism and argue for a pluralistic ontology.

CONTENTS

INTRODUCTION

Physicalism is an ontological thesis. In its broadest and most general sense, it claims that physical "stuff" and physical properties constitute the whole world.[1] Physicalism is the philosophical orthodoxy in all matters metaphysical. It is a view that dictates the nature of research which many philosophers pursue, whether it be in the philosophy of mind, the philosophy of science or some other discipline. However, I take it that physicalism is not a doctrine which is intuitively obvious. It is presumably a position that can be doubted and hence must be argued for. There are good ways and bad ways in which this might be done. We might consider a few of the bad ways first.[2]

Some physicalists might claim that their ontological position is motivated by simplicity or elegance – philosophical good taste decrees we should avoid the messy world of pluralistic ontologies. I do not find that argument remotely convincing. I do not see any *a priori* reason why God should have fashioned the world for Stoics rather than Epicureans. However, even if we do suppose it is a reasonable argument, it must have as much (or as little) force in the mouth of any other monistic philosopher. Why couldn't an idealist equally well claim his ontology was the simplest? A good argument for physicalism must have more bite.

Another argument physicalists might proffer is that only physicalism wards off philosophical disreputables – that is, "spooky" properties like Cartesian souls. But again I can not see why such a line of argument should persuade anyone. After all, what is a spooky property? Are the exotic properties of subatomic particles – spin, strangeness, charm – spooky? Certainly they are much less familiar than the sorts of properties non-physicalists might claim are not actually physical, viz. intentional properties. Indeed, it seems clear that what one does or does not decree spooky is

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[1] I take it that physicalists can remain neutral on certain questions, for example the existence of abstract objects such as numbers.

[2] The arguments I present here are anaemic versions of the summary of possible reasons John Haugeland, [1984] provides for phyicalism. Of course, I do not suggest that Haugeland himself finds such arguments, even in their full-blodded form, convincing.

usually decided after one has adopted a particular ontological position. Berkeley, for one, found incomprehensible the idea that inert matter could cause anything. For him materialism, the ancestor of physicalism, was a spooky theory. Spookiness, by physicalist lights, then does not appear to mean much more than non-physical; and while it is true that physicalism is the philosophical position which guards against the non-physical, that statement, by itself, is unlikely to have much persuasive force.

Any self-respecting physicalist should be embarrassed by these arguments. The real motivation for physicalism must come from somewhere else or one could not account for the widespread acceptance of the view within the philosophical community. I think what really drives the physicalist consensus is a deep respect for the natural sciences. In fact, I think physicalism has its roots in two other philosophical positions: scientific realism and naturalism. Scientific realism is the doctrine which asserts the way science describes the world is true or nearly true. Naturalism has a technical usage in philosophy associated with Quine: in essence it involves the denial of *a priori* knowledge. However, I use the term here in a much broader sense. I take naturalism to be the attitude that we should seek within the natural sciences for answers to our philosophical questions and we should judge our philosophically favoured theories against what the natural sciences say. One does not need to look hard to find physicalists proclaiming allegiance to both these views:

> Materialist metaphysicians wish to side with physics (Lewis [1983], p.364)

> Broadly empirical in character, they [physicalist claims] are supported inductively by scientific practice. (Hellman and Thompson [1977], p.311)

> According to contemporary physicalists, the principles of physicalism are to be treated as high-level empirical hypotheses or generalizations...If phenomena turn up which resist a naturalist

account even after years of trying, then the physicalists principles should be rejected or revised. (Post, J. [1991] p.75)

None of these quotes of course offer actual arguments, only intentions. But if these intentions could be realised, if physicalists could argue from the sciences to their favoured ontological position, that would be a powerful consideration in their favour. Such an argument might take a number of forms. The most complete form of justification from science would be to show that the physics does actually explain all the events of the universe. In other words, if we could show that all true theories reduced to physics, then we would know that physicalism is true. This has not been done. A close second as a way of motivating physicalism would be to show that given the state of science, we should expect reductions of other scientific disciplines to physics. This is an historically important position, associated with the so-called unity of science programme. If it were feasible it would provide strong grounds for being a physicalist. However, I shall show in section I that the current state of science cannot support the reductive programme.

A still less ambitious means of arguing for physicalism would eschew the possibility of actual theoretical reductions. Indeed, this view, what is called non-reductive physicalism, is probably the most popular version of physicalism touted by philosophers today. Such physicalists will have to provide some explanation of what the relation between physics and the other sciences is, if it is not reductive. They will also need to show, if they wish to claim their physicalism has naturalistic roots, that they have some argument for physicalism which emerges from physics itself.[3] I shall argue that neither of these aims can be achieved. In section II, I shall demonstrate that non-reductive physicalists, insofar as they are physicalists, are committed to reductions in principle. Section III will argue that if we take current physics itself at all

[3] This argument will of course apply equally to those who call themselves eliminativists

seriously it, if anything, undermines physicalism, rather than lending it support. These arguments will not refute physicalism but they should persuade the reader that physicalism has little support from the natural sciences.

By rejecting physicalism, though, we need not reject the realist or naturalist pillars on which it is built. In fact, it will be my contention in the final section that we can build an ontology that is not physicalistic but is still in keeping with the idea that our metaphysical views should emerge from our scientific theories. Hence, it will be this view rather than physicalism that has the honour of being scientifically respectable.

THE LIMITS OF REDUCTION

The issue we are concerned with here is theoretical reduction. Or to put it another way, when does one theory reduce to another? We are interested in theoretical reduction inasmuch as it is a motivation for physicalism. The reductionist hopes to be able to explain bigger things in terms of smaller things. He hopes to account for humans (and other animals) in terms of biology, biology in terms of chemistry and chemistry in terms of physics. The ideal end for someone who advocates reductionism as a means of formulating their physicalist views is that all non-physical theories should be explicable in terms of the one fundamental theory, physics (or microphysics). It will be my task to demonstrate that *actual* reductions between various sciences are unlikely, or impossible. Thus physicalism cannot be argued for as a corollary of the unity of science programme.

This section splits broadly into two sub-sections. The first sub-section is concerned mainly with what a reduction is. I shall argue that we have no good formal account of theoretical reduction. In the second sub-section, I shall address the question whether we have good reasons to believe that reductions are likely to be forthcoming in the sciences; I shall show we do not, and therefore one possible way of arguing for physicalism will have been undermined. Obviously one reason for thinking that reductions are in practice unfeasible is because we have no good formal model of reduction. However, I will argue even if we employ vaguer, more intuitive conceptions of reduction, then we have reason to believe that there will insurmountable obstacles for the reductionist programme to overcome.

Models of Reduction

A classical positivist gloss on reduction[4] suggests that all that is required for one theory, let us call it A, to be reduced to another, B, is that all the observational consequences of A should also be consequences of B. We conceive of B as the reducing theory since we take it to be superior to A in some appropriate way; it may have more verified observational consequences or posit fewer theoretical entities, for example. The positivist view makes no attempt to connect the ontologies of the two theories. In fact, it empties both theories of all their ontological content, making A and B merely instruments for generating predictions. But that cannot be the sort of model of reduction that suits physicalists ends for it seems to eschew entirely ontological questions. An argument for physicalism driven by reductionist considerations must take the theories in question to say something about how the world actually is.[5] Such an argument requires a notion of reduction that provides for stronger metaphysical commitments than is offered by positivism.

Perhaps the best known account of reduction that allows for a more realistic treatment of theories is Ernest Nagel's.[6] On his view the reduced theory must be *derivable* from the reducing theory. That is to say, the laws or an appropriate subset of the laws can be deduced from the laws of the reducing theory with the aid of some additional assumptions. These additional assumptions in some way connect or bridge the ontological and conceptual divide between the two theories. So for example, although it is true that neither physics nor chemistry makes mention of biological entities like testosterone, this does not mean biology is irreducible to physics and chemistry. One may introduce what Nagel called a bridge law that identifies or states

[4] For example Kemney and Oppenheim[1956]

[5] This is not to deny that the sorts of conditions set out by Kemney and Oppenheim need not be fulfilled, rather we demand to to understand more clearly what the relation between the two theories' ontologies is.

necessary or perhaps even only sufficient physio-chemical conditions for the presence of the biochemical. Indeed, as Nagel points out, logic demands that we introduce such bridge laws for without these principles there would be no way of deriving one theory from another, if the two theories contained different terms. Nevertheless, one might feel that if the reduction really were complete, we would expect the reducing theory to explain somehow the existence of the bridging law. That is to say, we do not want the bridge laws to be fundamental or on a par with the laws of the reducing theory. Rather they should be themselves explained, either by the reducing theory or in some other way which makes their appearance non-problematic. If no such explanation is offered, then we may become suspicious that the bridge laws, rather than the supposedly fundamental theory do the real work in the derivation.[7] In fact, without any obvious constraints placed on the bridging laws by the reducing theory, one can simply contrive to "reduce" almost any theory to any other. Consider, for example, theory A, that all Englishmen are bad losers and theory B, the inverse square law of gravitation. One can derive theory B from theory A (and hence on Nagel's model reduce B to A) simply by introducing the bridge law A→B. Nagel's account appears to make reduction trivial.

An obvious response to the above problem, put forward most forcibly by Robert Causey[8], is to insist that only identity statements[9] (as opposed to mere equivalences or even weaker conditions) which connect property attributes (or natural kind terms) will count as bridge laws. Such a view clearly satisfies our desire to account for the bridge principles, since an identity statement, unlike a law, does not beg explanation from the reducing theory. If water *is* actually H_2O, then one does not

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[6] Nagel, E. [1962.] See also Hempel, C. [1966] Ch.8
[7] Cartwright, N. [1981] expresses the same concerns about the role of bridge laws in connecting abstract fundamental physical laws to real concrete situations.
[8] Causey, R. ,[1977]
[9] Of course, there are some subtlies to be cleared up concerning the status of identity statements, whether they are necessary or contingent. And if necessary what sort of necessity: logical, metaphysical,

need to explain why wherever there is water there is H_2O. (Although, of course, one would expect there to be some justification for positing the identity statement in the first place.) Even identity statements, though, are not uncontroversial in the context of reductions. Often when the identity is posited there will be some features of the reduced property or entity that will be lost. For example, it is arguably a property of natural light that it appears to us to be a certain colour. But it does not appear to be a property of any frequency of electromagnetic radiation, as described by Maxwell's theory, that light appears any way to us at all. Maxwell's equations have nothing to say about the qualitative aspects of light. Yet many philosophers and scientists are still happy to assert that light *is* electromagnetic radiation.[10] Reductionists would hope, of course, that these missing properties, these secondary qualities, would eventually be explained away in the context of some other theory or some broader reduction. In this case, one would expect some explanation in terms of the physiology of the human sensory systems. Ultimately of course, for the advocate of the unity of science programme, all the theories should be explicable in terms of the ultimate physical theory.

Thus far I have given a fairly broad and abstract sketch of reductionist models and possible problems they might face. The only method of assessing the value of any model of reduction in detail is to consider putative examples of theory reduction from the history of science. First, I wish to examine the supposed reduction of Kepler's theory of planetary motion to Newtonian mechanics. This reduction involves two theories that clearly cover the same sort of phenomena; it is a reduction within physics (or astronomy). The purpose here is to demonstrate the problems of providing an adequate Nagel/Causey-style reduction for *any* proposed case of

physical? I do not have space to to do justice to these difficult questions. One should note though that Causey himself believes identity statements are at least *logically* contingent.

[10] This is a consequence of the asymmetry of the reduction. Since one description of the entity or property is deemed fundamental, that of the reducing theory, any aspect of the reduced theory description that does not fit is deemed "unreal" in some sense.

theoretical reduction. It will also allow us to consider possible extensions of the Nagel-model which have been suggested. I shall then turn to assess the reduction of chemistry to physics. Having shown the limitations of the Nagel-model, this case study should achieve two aims. Firstly, it will provide a structure to suggest weaker and vaguer accounts of theoretical reduction. Secondly, it will show that even these weak accounts do not apply to the relation between chemistry and physics and thus, chemistry must, for several reasons, be considered an autonomous non-reduced discipline.

The reduction of Kepler's laws to Newtonian mechanics

The claim that Newton's theory subsumes Kepler's goes almost unchallenged. Even a sceptic about reductionist programmes like Popper is prepared to admit, "Newton's theory was a completely successful reduction of Kepler's"[11]. It is this uncontroversial status as a successful reduction that makes it a particularly useful test case for models of reduction.

Prima facie, the reduction seems to fit well with the Nagel model. From the principles of Newtonian mechanics it is fairly elementary to deduce Kepler's three laws of planetary motion. For example, Kepler's first law, that the planets move in ellipses with the sun at one focus, can be deduced simply by considering the effect of two bodies interacting under a central force proportional to one over the distance squared. (In other words, interacting under the gravitational force.) In fact there does not appear to be any need to invoke bridge laws for the theoretical terms found in Kepler's laws, the orbits of the planets, etc., are also found in Newtonian mechanics. However, this relatively smooth mathematical reduction belies a conceptual revolution. According to Kepler's theory, some force emanating from the sun pushes the planets round their orbits, whereas, the orbital motion in Newtonian mechanics is explained in terms of the mutual attraction of two bodies. The ontologies of the two theories seem to contradict one another, there is no intuitively obvious sense in which we can say one of the theories *reduces* or approximates to the other. The two theories simply appear to contradict one another; if Newton's theory is correct, Kepler's is plain wrong.[12] Indeed the problem is more acute, if one believes, as many do, that the meaning of theoretical terms is wholly derived from their place within the total theory. If one accepts this form of semantic holism it becomes difficult to resist the conclusion that the entities named in two differing theories, even if they have the same name, refer to different objects. Therefore, one theory cannot intelligibly be compared to another. It follows then that the models of reduction we have briefly considered will be wholly inadequate. That is to say, a Nagel-style derivation can not properly speaking be a reduction of one theory to another, since the set of laws derived from the reducing theory (Newton's in this case) will have a different meaning from the laws of the supposedly reduced theory (Kepler's laws). [13] [14] Or even if we do not accept this more extreme view, we still must acknowledge that the two theories are mutually inconsistent. One cannot possibly derive a theory from another when the two are inconsistent (save in the trivial respect by adding inconsistent premises) and so no Nagel-style reduction is possible.

A model of reduction put forward by C.A. Hooker[15] and Paul and Patricia Churchland[16] attempts to address these problems in a way which does not deviate greatly from the original Nagel model. These philosophers maintain like Nagel that a reduction involves the deduction of one theory from another. However, they do not

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[11] Popper, [1982] p.172
[12] Even the mathematical derivation requires some qualification. Kepler's laws purport to refer to the actual motion of the planets but the deduction of the first law offered in Newtonian mechanics is only a mathematical idealisation. The actual orbits of the planets deviate from ellipses due to the perturbations of the other planets, most notably Jupiter.
[13] See Kuhn, T. [1970], for the classic account of incommensurability.
[14] Nagel distinguishes two types of reduction:hetergeneous and homogeneous. The former involve theories which have different vocabularies, and therefore require bridge laws, and the latter are reductions of theories that have the same vocabulary. To take the incommensurabilist seriously is to deny that there can ever be two different homogeneous theories.
[15] Hooker, C.A. [1981]
[16] Churchland, P.M. [1985], Churchland, P.S. [1986]

take the theory deduced from the reducing theory to be identical to the reduced theory. It will be rather, an equipotent image of the reduced theory. To put it more formally, if we have some theory, H, which is reduced to another T, then we can deduce some theory T(H) from T with some additional assumptions.[17] But T(H) does not equal H, although it is formally and metaphysically similar to the theory, H. Hence, we are apparently able to reconcile the fact that H and T are mutually inconsistent theories with the fact that we want to say H reduces to T. That said, it is not clear to me that the Hooker/Churchlands model solves the problems I have raised. In fact, it seems to me if anything to make them more perspicuous. If the deduced theory is *not* the supposedly reduced theory but something else on what basis do we say that one theory has reduced to the other at all? What we appear to have is a straightforward case of theory *elimination*. The real question which has to be addressed by this account is: what is the actual connection between the reduced theory and the image deduced from the reducing theory? What, in short, motivates calling it a reduction rather than an elimination?

When the dilemma between reduction and elimination is put as starkly as this, we can see that an account of inter-theoretic reduction must address the same problems philosophers of science tackle in discussions of revolutionary theory change.[18] Furthermore, we can interpret the eliminativist and reductionist responses to the problem of reduction, as parallel to realist and anti-realist[19] accounts of scientific revolutions. The reductionists and realists are conservatives together: both believe that there is some continuity during theory change. The eliminativist, on the other hand,

[17] Churchland, in fact thinks that given his account we do not need relations which are as strong as identity statements, we only need an ordered pair of expressions to function as correspondance rules between the theories, so long as the second element applies (the reducing element) wherever the first element (the reduced element) is thought to apply.

[18] Rorhlich, F. [1988] offers an account of reduction which simply ignores the problems of incompatible ontologies and provides an account of the formal deductions given by scientists when supposedly offering reductions. However, as I have said repeatedly, I am only interested in epistemological reductionism inasmuch as it can be shown to provide support for ontological reductionism.

[19] By anti-realism here I mean the view that scientific theories are false, and in particuar the view as motivated by considerations of the past falsity of successful theories.

finds a revolutionary comrade in the anti-realist: both assert that radical theory change marks an absolute discontinuity between one way of looking at the world and another.

The relevant question now, of course, is: what exactly is it, according to the realist/reductionist that remains constant during theory change? One possible solution is to fall back on the relatively smooth mathematical transition between theories. So one might argue it is not the ontological "content" of the theory that is real but the mathematical structure.[20] But this way of tackling the problem suffers from the same difficulties as the positivist account of reduction. It robs the theories of ontological content and that is too high a price to pay for the physicalist, for physicalism without firm ontological commitments is no doctrine at all. Physicalism requires a metaphysically more robust view to provide some substance (literally) to its ontology.

I think if we reconsider the Kepler and Newton case there are some particular points that can be made here which *might motivate* us to consider it an example of reduction rather elimination. First, it seems difficult to deny that the central terms of both theories pick out the same objects. Both Kepler and Newton are talking about for example Mars and the orbit of Mars. So the considerations which motivated the incommensurability claim and semantic holism have little intuitive appeal here. However, the observation that some of the terms in the theories refer to the same objects is not sufficient to ground the claim that one theory has reduced to the other. The same observation could be made about Ptolemaic and Copernican astronomy and I take it that intuitively at least we wish to consider these older theories to be eliminated rather than reduced. There are more significant links between Newton's and Kepler's theories which set them apart from their predecessors. Perhaps most importantly, Kepler and Newton were the first astronomer-scientists to provide *physical* explanations of the movement of the planets. That is to say, both offered dynamical explanations of the movement of the planets around their orbits that were in the spirit of the non-teleological approach of the then emerging sciences. It does not matter that they disagreed about the magnitude or the particular nature of the force. What is important is that both men were looking for the same type of explanation. This put them at odds with both Copernicus and his Ptolemaic predecessors. All of their explanations, in as much as they offered any reason for the planets following the particular paths they did, required new and extraneous metaphysical notions - turning crystal spheres and the like. Indeed, I think it is fair to speculate that the reason pre-Keplerian astronomy worked so comfortably with untidy epicycles is because it had sacrificed dynamical explanation for the greater metaphysical or theological virtues of explanation in terms of perfect circles. There is, it appears, some sense in which methodologically, and in their judgement of what counted as good explanations Newton and Kepler were as one.

Perhaps the foregoing considerations, in addition with the derivation from within Newtonian mechanics of something like Kepler's laws might persuade us that the Kepler-Newton case is an example of theoretical reduction as opposed to elimination. But we have no clear-cut way of making that judgement, only some vague motivations. I have no doubt one could make similar pleas for almost any theoretical change in the history of science. Perhaps we can consider Ptolemaic astronomy to reduce to Newtonian mechanics, if we sympathetically interpret the key terms. That is to say, we might take the turning of crystal spheres to refer obliquely to the gravitational force of attraction between the two bodies. Moreover, we can achieve something like a derivation of the Ptolemaic theory from Newtonian mechanics, since, the relative positions of the planets from the perspective of the Earth may be derived from Newtonian mechanics and these will approximate the predictions of Ptolemy.[21]

[20] See Worrall, J. "Structural Realism: The best of Both Worlds" in Papineau (ed.) [1996]

[21] Such a model of reduction would be dangerously close to the positivist model discussed first. But this seems an almost inevitable consequence of accepting that the reduced theory and the theory derived from the reducing theory are different. What is their ultimate mark of similarity? Surely, nothing more than they make broadly similar predictions for the outcome of events.

One's intuitions may rebel against such a course but there does not seem to be an adequate way to analyse what the difference is between the two cases.

However one feels ultimately about the Newton-Kepler case, a number of lessons should be drawn from this case study. First, a naive reductionism seems untenable, even in the best-case scenarios. A certain amount of conceptual change is inevitably involved in any theory shift. Once we accept this and move from a naive to a more sophisticated view, it becomes difficult to say why any case is a reduction rather than an elimination. One is forced to invoke, as I have done, the vague and the platitudinous: congruent ontological themes within the two theories and the derivation of mathematically similar structures. Churchland himself acknowledges as much – the reasons he offers for considering a theory to be reduced, rather than eliminated are largely pragmatic, involving considerations of "entrenchment, convenience and continuity"[22]. But these are judgements which will vary from individual to individual. Until such difficulties are sorted out no detailed analysis of reduction is likely to be forthcoming; so we are left with platitudes and personal judgements.

It may be felt that the above discussion is irrelevant to the real issue of reduction. What has been addressed by the accounts of Nagel and Churchland is best characterised not as a reduction of one theory to another but straightforwardly as theoretical replacement. Since I have drawn a parallel between scientific realists' concern for theory change and worries about differentiating reductions from eliminations, I am obviously not convinced there is any such distinction. Nevertheless, I am willing to entertain the thought for the present. What sorts of theoretical relations would be *real* reductions and how would they differ from the Kepler-Newton example? Several possibilities suggest themselves. First of all, one might simply believe that reductions involve explanations of larger things in terms of smaller things and the Kepler-Newton case does not appear to be an example of that sort. Such a position, though, can have no force unless it entails some categorical difference between the relation of microscopic to macroscopic theories and the example I have discussed. I can see no obvious reason to expect such a difference.

A superficially more persuasive reason for classifying the Kepler-Newton case as theory change might be because it involves a diachronic subsumption of one theory by another whereas reductions, so the thought might go, must be synchronic. Again, I do not think this is a helpful distinction. As the history of science testifies to very often the sorts of development that are considered reductions only occur after there has been some change in the discipline from which the reducing theory emerges.

Perhaps one way of understanding the relation between reduced and reducing theory which might introduce fewer problems for a reduction is to view the reduced theory as classificatory, rather than theoretically encumbered. By a classificatory science I simply mean one which picks out and groups together objects which are considered similar in some way. Such a view would appear to circumvent the problems of incommensurability since a classificatory scheme does not appear to have ontological or theoretical commitments beyond the entities that it classifies. In the next section I shall discuss the possibility of regarding chemistry as reduced to physics in this way.

The Reduction of Chemistry to Physics

Neither chemistry nor physics is a simple unified theory like Newtonian mechanics. Physics is made up of at least three sub-disciplines – quantum physics, statistical mechanics, and relativity theory, none of which reduce, as yet, to the others and, in fact, appear to contradict each other on various key points. For example, the determinism implied by relativity is at odds with the indeterminism implicit in both quantum and statistical mechanics; and one could make similar points about the various fields of chemistry. So first we must be clear about which theory is being

[22] Churland, P.S. [1986]

reduced to which. In keeping with the suggestion that we view chemistry as a classificatory science, I suggest we take the supposed reduction of chemistry to physics to be understood as the physical explanation of the periodic table. I do not think this is a fair representation of the discipline of chemistry but pursuing the matter in this way will be enlightening; and should demonstrate the limitations of a particular view of science espoused by certain philosophers, notably Quine[23] and Peirce[24]. That is, science proceeds by first grouping objects together into kinds and then providing explanations of these taxonomies in terms of some fundamental theory.[25]

Roughly speaking one can explain the structure of the periodic table as follows: the Pauli exclusion principle states that no two fermions, can have the same quantum numbers and same position at the same time. Electrons are fermions; it therefore follows that the electrons are constrained to group in certain characteristic shell formations. In broad terms, each new shell marks a new period or row of the table and those elements with the same number of electrons in their outer shells fall into the same columns. It is this latter fact which is taken to explain the similar chemical properties of elements in the same column.[26] We can also unproblematically assert identity statements between the physical constituents and the atoms. Each element is made of up of protons and neutrons, the number of protons determines which element is which. Obviously, any science that is classificatory is clearly more amenable to a "smooth" reduction; one simply provides a causal explanation for the phenomenologically contrived arrangement and identity statements to link the non-physical entities to their physical constituents. Indeed, there is little room here for

[23] Quine, W.V.O. [1969]
[24] Peirce, C.S. [1932]
[25] This may seem redolent of Rutherford's oft quoted remark that "there is physics and there is stamp collecting".
[26] Of course, it is not quite as straightforward as this; the way the electrons fill each shell is not linear process. It is often energetically favourable to move up to a larger shell, before a lower one is full. These nuances though can be demonstrated and in actual fact have little to do with the final placings of the elements.

disagreement between reductionists and eliminativists, since there seems nothing to eliminate.

This is all very well as far it goes but even classificatory schemes are committed to at least one piece of ontology; namely, the elements they classify. A complete reduction of the classificatory scheme must be able to account for the origins of those things that it classifies. We need to explain how it is these physical elements have come together to make up the chemical elements they do. Now, there is, of course, a cosmological account of the origin of the chemical domain but by accepting it we then have moved from a simple reduction of one theory to another to something quite different. The explanation offered at the cosmological level introduces concepts entirely foreign to quantum theory. That is, we are told that the elements are formed in the nuclear reactions of stars and disseminated throughout the universe in supernova explosions. And that explanation introduces a time-orientated evolution into the universe, something that is not found in the temporally symmetric equations of quantum mechanics. Moreover, in order to understand the nuclear reactions a new force must be introduced, the strong nuclear force, which only operates at very short distances. But this new force is strange: it only functions, in nature, if the gravitational force between protons (and neutrons) can overcome the stronger electrostatic repulsion. Hence, the explanation of chemical formations seems to depend upon a pre-established harmony between gravity, the strong force and the initial conditions of the universe.[27] The last of these conditions we are forced to admit because if the initial conditions of the universe were slightly different, so the story goes, then the universe would either have expanded too quickly for star clusters to form or collapsed back to a singularity almost immediately. If one had a theory of everything which linked the various forces of nature (and demonstrated they were in fact one, as superstring theory attempts to do) then the apparent collusion of the forces of nature (understood now not as separate forces but as just different aspects of the only force) would be less worrying. But that, and superstring theory with it, is mere speculation. As it stands there is no straightforward reduction of one theory to another here because there is no single account which explains both the properties of the elements and their emergence.

I may, at this juncture, be accused of missing the point or at least not thinking the case through properly. Just because there is no complete reduction of the periodic table to quantum mechanics, does not mean there is not a reduction to physics. After all, it was I who made the claim that the reduction was to quantum theory. But most people who talk of a reduction of chemistry only talk of physics in general. Any person defending such a view would have to be surprisingly sanguine about the apparent collusion between the forces of nature that our current account of chemical formation suggests. However, in another sense, they would be making my point for me. It is precisely because physics in general is not a unified discipline with logically consistent theories that makes the notion of reduction in principle to physics so problematic. In short, because physics itself is not *actually* reducible to any one of its elements there is little hope that any other discipline will be so reducible. The reductionist will no doubt hold out for some new complete theory of the universe, something like superstring theory appears to offer. But even assuming such a theory exists, and that is a major assumption[28], then the problems I outlined in the Kepler-Newton case will resurface. What relation will this new theory bear to the extant elements of physics? How much conceptual change will be involved in accommodating the differing ontologies of the current theories? If an argument for physicalism is to be based on such considerations of an ideal theory, then these problems need to be addressed. I shall return to this point in later sections.[29]

[27] These points are developed in greater detail in Popper, [1982].

[28] In fact granting this assumption would be to grant that physicalism is true and of course that is the point very much at issue.

[29] See the beginning of section III in particular for the suggestion that technical results in quantum theory may provide clues to the sorts of future theory we might expect.

The argument thus far has focused on demonstrating the conceptual problems of what appear like favourable reductions, either because of problems in defining what reduction is or because the reduction appears to involve overlapping and mutually inconsistent components from different aspects of some discipline. I now wish to argue in a more straightforward way that chemistry, understood more broadly now as a theoretical discipline, does not reduce to physics; and perhaps more, importantly, we should not hold out hope that chemistry will ever reduce.

Problems of Complexity

For certain systems, for example the n-body problem in classical mechanics, there is no closed algorithmic solution to their dynamical equations. So given the appropriate equations of motion and the initial conditions of the system one cannot, in one operation, leap from knowledge of the system now straight to knowledge of the state of the system at any arbitrary time in the future. In order to calculate the position of any one of the particles a series solution must be used. This fact in and of itself does not threaten the integrity of the prediction. After all, a calculation that takes many operations is as much a calculation as one that can be done in one step. However, for any real problem there will be some error in the measurement of the initial conditions. Human beings are, I take it, incapable of obtaining absolute accuracy, particularly since it is possible that accuracy of measurement may require irrational numbers. Given then some system which is complex enough (that is chaotic, in the technical sense of the word) the error in the initial conditions will increase exponentially with time (that is, with each step in the calculation). Hence, using such methods only very short-term predictions will be possible. Indeed, for certain very strange systems there may, in fact, be no appropriate series expansion of the solution; hence, not even short-term predictions can be made.[30]

[30] See Suppes, P. [1984] Ch.7 for a more detailed discussion of then-body problem. See also Hall, N. (ed.) [1993] for a discussion of some of the applications and implications of chaos theory.

Such considerations seriously undermine any strong reading of reductionism. For even if we grant there is some ideal-physics and we have knowledge of it, it will not follow that we will be able to predict or explain all phenomena in terms of that theory, as it will simply be far too complicated. But we can in fact argue the other way as well. Because we cannot predict or explain complicated phenomena with physics in practice, we do *know* that the phenomena which they describe fall under physical laws. Take chemistry as an example: given the complexity of certain calculations it is impossible to obtain answers from physics for certain chaotic systems. Chemistry then as a matter of necessity has to develop alternative ways of understanding the world. But how do we know these alternative ways of looking at the world are consistent with physics, how do we know they are consistent with physicalism? The simple truth is we do not and we can not.

The Limits of Reductionist Explanation

Apart from this technical worry about our ability to carry through reductions, there are also cases where we appear to have what looks like a reduction but may for other reasons be quite unsatisfactory. Consider again chemistry. It is possible by numerical methods to grind out results for certain reactions. Results that will accurately inform us of the quantitative measures of everything. For example, we can calculate the bonding energy of some molecule (provided it is not too complicated) by approximate, numerical methods. Now, that sort of achievement might be considered, under a liberal-interpretation, a reduction of some sort. It seems to display what Peter Smith[31] calls explanatory "inter-facing" between two theories; that is to say, a certain chemical fact is explained by physical and numerical methods. However, as an explanation of chemical bonding it will leave many qualitative questions unanswered, *not least what the chemical bond actually is.* To quote a practising theoretical chemist: "Brute force numerical quantum chemistry can hardly do justice to the qualitative features of chemistry. But without insight into the qualitative concepts we are losing chemistry. The allegedly basic methods often fail to illuminate the essential function of a molecule or a reaction which is evident to an experimentalist."[32] So even once physics has told us everything that it can in quantitative terms, there appears to be in the case of chemistry at least some qualitative residue still to be explained, and still to be understood if the discipline is to be practised at all. Reductionists might respond in two ways. Liberals like Smith might claim this "inter-facing" is enough to support a modest reductionist programme. But that is really to ignore the question of interest; namely that the explanation in terms of physics leave important questions unanswered and fails to elucidate what appears obvious to the chemist. Philosophers with more conservative reductionist leanings might wish to turn eliminative. Notably Quine[33], has been tempted to deny that there is any place in a finished science for qualitative considerations; all the meaningful questions can and will be answered by purely quantitative means. Surely, though, there is something wrong if a philosopher starts telling a scientist which questions are and which are not relevant, particularly if that philosopher considers himself a naturalist. So reductions as well as not being practically feasible in certain situations may also be unenlightening.

Such considerations also highlight the inadequacy of reductionism as a global methodological principle. The standard assumption, in both science and philosophy, is that by pursuing reductions light will be shed on the reduced elements, either by demonstrating that we may get by with fewer elements in our ontology or showing us of what the reduced elements really consist. But as the above example illustrates reductions do not always provide adequate explanations.[34]

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[31] Smith, P. "Modest Reductions and the Unity of Science" in Charles and Lennon (eds.) [1992]

[32] Primas, H. [1983], p.8

[33] Quine, W.V.O [1969]

[34] This is, I suppose, similar to John Dupré's [1993] remark that reductions tell us *how* things are but not *what* they are. (Perhaps this might be better put in the example I briefly discuss as follows: redeuctions tell us *how much* for certain interactions but not *what for*.)

Some qualification is required here. I do not wish to deny there have been many dramatic revolutions in the sciences brought about by reductionist programmes. Bohr's reductionist urge to explain hydrogen and various chemical phenomena precipitated the quantum revolution; but as we have seen that revolution did not result in a complete reduction of chemistry to quantum theory. Similarly the breakthroughs in molecular biology promised much in the way of reduction of biology to chemistry but again this has not been completed. Reductionism, as a methodological principle, has certainly led to great scientific successes but I would argue very few, if any, actual reductions. Nevertheless, the relative failure of most reductionist problems may not concern some. They might argue that the very fact reductionism is used as a methodological principle and is, in fact, partially successful demonstrates the validity of reductionist thinking. Or to put it differently, scientists who assume reductionism holds true are more successful; and this must give some support to the idea that reductionists are on the right lines. It is unclear what force such an argument has. Clearly, useful methodological assumptions may actually be false. It may, for example, be better in some methodologies to search for deterministic explanations, in the sense that a deterministic explanation seems more complete than an indeterministic explanation. Similarly one might think reductionist explanations render non-physical phenomena understandable, in a way which anti-reductionism obscures. But in either case that should not lead us to the conclusion that the world is either deterministic or that non-physical theories must reduce to physical theories. We may seek and we may not find as for example, 19th century physicists, did when they attempted to reduce electromagnetic phenomena to mechanics. The main problem with a methodological argument for reductionism, though, is that there does not appear to be a unified notion within reductionist programmes of the sort of theory one should be attempting to reduce others to. Perhaps some scientists try to reduce chemistry to quantum mechanics; biologists might attempt to explain the inheritance of phenotypic characteristics in terms of some genetic theory; psychologists may hope to reduce elements of their study to neuroscience. Reductionism as a philosophical position maintains that one (and only one) consistent theory of the world is required to account for everything. But reductions in science diverge, for sensible practical reasons, about which theory is considered fundamental. The tendency, one may think, is always towards physics but as before, it is not clear to what part of physics everything must be reduced; or any existing discipline comes anywhere near this ideal.

The partial success of reductions does I think support reductionism as a methodological principle, in some sense; not as a global principle, applicable in a uniform way to all cases, but rather as a local principle. The sorts of theories for which reductionist research bears fruit generally cover broadly the same phenomena, like Newton's and Kepler's theories, and even then it must be admitted that the reduction is unlikely to be completely successful. The grandiose pretensions of epistemological reductionism or the unity of science programme, which hopes to explain everything in terms of one theory, are just not supported by science or its methodological practices.

Physicalism without Reduction?

The arguments I have put forward in this section have been many and varied. The reductionist faces many problems. First, there is no satisfactory model of theory reduction that applies even to some of the more favourable cases of putative reduction from the history of science. Second we have seen that even when we consider reductions in a more charitable way as providing a rough and ready explanation of other non-physical phenomena, we have reason to think that it will not be possible to carry through the actual reductions because of problems of complexity. And even when something like a reduction is carried through many questions are left unanswered either of a qualitative nature or in terms of explaining how different and apparently contradictory theories conspire to account for the phenomena. We have even seen good reason to doubt the usefulness of reductionism as a global methodological principle.

Such considerations will not disturb some who are committed to physicalism. In fact, they may be able to use my examples to their own ends, reconciling the obvious lack of unity within contemporary science with their own favoured ontology. Chaos theory, so they could argue, shows the practical limitations of the unity of science programme; and the lack of informativeness of physical explanations demonstrates the need to introduce different levels and types of explanation. However, neither of these considerations threatens the *possibility* that there does in fact exist some theory, obviously unknown as yet, which *in principle* explains everything. Certainly I do not wish to deny this possibility but we should bear in mind that it is no *more* than a possibility. The actual state of science, even the discipline of physics itself is neatly summed up by Ian Hacking when he says: "Every single year since 1840 physics alone has used more (incompatible) models of phenomena in its day to day business, than it used in the preceding year."[35] Science is disunited and looks more and more disunited as it continues to develop. To hope that it might really, in principle, be otherwise, is I suggest to hope in vain.

Some physicalists might agree with this statement. They may be tempted to claim that non-reductive physicalism is consistent with the denial of reduction *in principle*. I shall argue in the following section that this cannot be the case. There is no coherent non-reductive physicalist position that is not committed to reduction in principle. The best a non-reductionist can hope for is to explain why we should not expect reductions in practice.

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[35] Hacking, I[1983] p.218

PHYSICALISM AND REDUCTION IN PRINCIPLE

The prevailing orthodoxy within the physicalist community is non-reductive physicalism: a position which attempts to reconcile the claim that there are autonomous special sciences with a metaphysical commitment to physicalism. Such philosophers appeal either to the idea of multiple-realisability or that of supervenience to justify this claim. I shall argue in this section that there is no coherent position that can be classed as both physicalist and *ontologically* non-reductive. And, moreover, that the standard appeals to supervenience and multiple-realisablility either lead to reductionism *in principle* or are not strong enough to guarantee a physicalist ontology. The discussion will move from the respectable earthly study of actual scientific theories undertaken in the last section to the oxygen-starved high planes of metaphysical speculation. In other words, we shall entertain theories which are entirely speculative concerning the relation between the physical and the non-physical. I shall concentrate mainly on the relation between physics and the special sciences of biology and psychology since these are the sciences most discussed in the literature. However, if we grant that these hypothesised relations hold, then the general points could equally well apply to any of the special sciences.

There are, I take it, three claims made by non-reductive physicalists that are constitutive of their position:

1) It is a form of physicalism. That is, it is committed to some view that places physics or some idealised notion of physics in a special position in the ontology.
2) It is non-reductive. That is, first it admits into its ontology properties, events, facts or something similar which are non-physical and second at least some parts of the ontology cannot be reduced to physics.

3) Finally, non-reductive materialists are not eliminativists. That is to say, the fact that events, properties or whatever of non-physical disciplines do not reduce, does not mean they are not *fully real* properties.

Some clarification of the first two points is required: what do the terms "physicalism" and "reduction" mean in this context? Thus far I have only shown that reductions to actual physics will not be forthcoming. Given non-reductivists accept this we need another way to characterise physicalism that does not presuppose reductionism. Since I am following Jaegwon Kim[1] in many of the arguments I present, and he is a self-professed physicalist I will utilise his conception of the position. A physicalist, according to Kim, is at least committed to the completeness of physics. Commitment to the completeness of physics (CP) is a *necessary* condition for physicalism. This condition might be thought to be far from illuminating. It appears we have simply shifted the problem of defining physicalism to one of defining what constitutes physics. I shall discuss this topic at greater in the length in the next section of the thesis but some general remarks need to be made here to clarify the ensuing discussion. It is clear, I think, that the completeness of physics implies that physics is a uniform discipline, a single idealised theory which, in principle, explains the occurrence of all physical events. As I argued in the previous section extant physics does not match up to this ideal. Therefore, the physicalist must have some extension and/or revision of current physics in mind. I shall call this "future-physics" to distinguish it from the actual subject physics that one may obtain degrees in and is written about in scientific journals. Various constraints though must be placed on the nature of the extension of physics to "future-physics" or (CP) will end

[1] Kim, J. [1989,1992,1996]

up trivially true. If we place no parameters on the sorts of properties or laws which can or cannot count as physical, then the physicalist is free to claim any property indispensably required to explain some event is part of "future-physics". For instance, if it turns out that psychological properties are essential to account for certain phenomena, then the physicalist might simply claim that those properties are really physical (part of the true, complete "future-physics"). So understood "future-physics" will be complete by definition.[2] The problem of prescribing the limits of "future-physics" shall not exercise me greatly here, since I will be concerned mainly with the relation between "future-physics" and mental or psychological properties as discussed in the literature on the mind-body problem. I will take it then for the time being that no psychological property, state, etc. can be part of our basic "future-physics".[3] A condition which amounts to the surely indisputable claim that whatever physicalism turns out to be, it should not be a version of Cartesian dualism. Hence, we shall assume at the very least that mental properties will have to be accounted for in some special way.

Reduction is somewhat easier to define since I have discussed it at length in the previous section. We may work here with the strong Causey-model of reduction. That is, reduction involves the deducing of one theory from another using identity statements to bridge gaps in the vocabulary of the two theories. This might be thought to be at odds with my assertion that we have no good model of reduction. However, it is not. I claimed in the previous section that the Causey-model did not appear to apply to any actual cases of reduction; if it did we would have clear cases of reduction. I shall argue that the sort of relations non-reductive physicalists maintain exist between

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[2] That physicalism is threatened by triviality in this sense is argued by Tim Crane and D.H. Mellor [1990].

[3] In essence, this is the understanding of the completeness of physics put forward by Papineau [1993] which he thinks gives the doctrine content.

the physical and the non-physical are in fact strong enough to support Causey reduction

Arguments from Multiple-realisation

Jerry Fodor[4] and Hilary Putnam[5] both noted that certain properties, in particular computational or psychological properties, could be realised or implemented, if you prefer, by a variety of different physical systems. For example, an electronic, mechanical, hydraulic, or (arguably) a biological computer may implement a simple arithmetical calculation. But since none of these computational devices has anything physical in common, it would be wrong to identify these computational states with any of their physical bases. It follows then, so advocates of multiple-realisability have argued, that since there is no one-to-one correlation between physical states and these higher states there can be no bridge laws (or identity statements) connecting the terms of the two theories; and thus there will be no reduction.

There are two levels of response to this argument both of which are found in Kim's writings. First one may question how deep multiple-realisability is supposed to go. Let us suppose that non-reductive materialists believe pain is a multiply realised phenomenon. Now, does that mean that pain has a different physical base for different species or does it mean even within the same species or same creature these characteristics would be multiply realised? If one prefers the first of these options, then it follows that although a pain-state in me would be physically different from, say a pain-state in an octopus, one would expect it to be physically similar to a pain-state in another human. If this is the case then there appears no bar to a more local sort of reduction. The appropriate bridge laws would then be of the form: pain-in-human =

[4] Fodor, J. [1974]

physical state X; and pain-in-octopus = physical state Y. Employing such bridge principles, one could then derive the psychological laws or states associated with human-pains from the physics of the human body. One might feel that these sorts of local reductions are in some way unsatisfactory and properties, laws or whatever should reduce across the board. However, some of the examples of reductions we find from the history of science are actually local reductions of this form. Take, for example, the supposed reduction of thermodynamics to statistical mechanics, the favoured example of a reduction for most philosophers. That reduction involves the following bridge law: temperature = mean molecular kinetic energy. Temperature itself, though, is a multiple-realised phenomenon. We may talk of the temperature of a gas, a liquid, a solid or even of the background radiation of the universe. But the bridge law only applies to gaseous states and hence only a local reduction of the property temperature is effected (which is not necessarily to say that thermodynamics is not reduced, only that it employs this specific notion of the term temperature). So, in principle, local reductions look likely given multiple-realisation relations.

Perhaps the advocate of multiple-realisation will prefer the more radical option of denying even local correlations between physical states and realised properties. Of course, this is a possibility; it may be the case that the realisation bases for pain properties are entirely heterogeneous. But the situation described becomes quite curious. Why do all these very different bases all realise the same property? The non-reductive physicalist owes us some account of how non-physical properties could have such utterly heterogeneous bases. Yet even if we do allow that such heterogeneity is possible, there is a fairly obvious response open to someone like Kim who opposes the idea of non-reductive materialism. He need simply maintain that the

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[5] Putnam, H. [1965]

disjunction of all physical states that realise the non-physical property can function as a bridge law. So we may deploy the following bridge law: M = P1 v P2 v P3 v… and use it in a Nagel-style reduction of say the mental to the physical.

Some non-reductive physicalists have tried to rule such a move out. Fodor, for one, has claimed that disjunctive properties like the above are not *natural kinds* and only bridge laws which cite connections between such kinds are to count in reductions. Presumably then Fodor would deny that the identity statement as I have written it above holds true. But this all seems very mysterious, since after all the disjunction is coextensive with mental property. In fact it is coextensive with M in all worlds where the laws of physics hold. Why is that not sufficient to allow one to assert the identity statement? It simply begs the question to say that M is a kind and the disjunction is not and therefore the identity statement does not hold; for we want to know why M is a kind, what is it that it has that the disjunction has not. I cannot conceive of any way to make sense of this claim which would not imply that M is something over and above the realisation relation; that for example all the P states *caused* M, where M is now considered something different from the realisation bases. But no self-respecting physicalist is likely to endorse such a view; it would be inimical to their physicalism. It would imply there was more to the world (metaphysically speaking) than that described by physics, which is to reject to physicalism.

Asserting that disjunctions are not kinds cannot threaten the validity of the identity statement; it can only lead to the conclusion that if the disjunction is not a kind, then neither is the non-physical property, M. If the special science properties are not genuine kinds then that would imply that the laws cited by these sciences will not be genuine laws. So any special science which has multiply realised properties will not be a proper science. In short, to accept the disjunction as a real property, and therefore a genuine natural kind is to embrace reductionism about the non-physical properties and the theories in which they feature. To go the other way and reject that the disjunction is a real property and a natural kind would appear to point towards eliminativism concerning the theories in which it features (or possibly towards local reductions of the kind described above). It does not matter to me at the moment which particular option any physicalist might favour, it might vary from case to case. What is important to realise here is that there is no non-reductive, non-eliminative position in between. One cannot assert the realisation relations between the physical and the non-physical and then claim there is some sort of asymmetry between the properties cited on either side of the relation, that one is a kind and the other is not, for example. If one is a kind, both are; if one is not, neither is.[6]

The general point holds no matter how pathological one imagines the realisation bases to be. It has been argued for example that we could imagine that the disjunction of physical properties to be infinitely long and non-recursively specifiable.[7] If that were the case it would be impossible for any being with finite cognitive capacities, which I take it is the category of being most of us fall into, to derive one theory from the other. This is undoubtedly correct but such pathological bases only place a pragmatic or epistemic constraint on the reduction. If such realisation relations do actually exist then, in principle reductions should take place, even if no human could *actually* demonstrate the reduction held. Some philosophers, of course, wish only to defend non-reductive materialism as an epistemic doctrine and

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[6] Kim elsewhere seems to favour the idea that disjunctive properties are not kinds because they, like that other disjunctive property grue, are not projectible. That is to say, they will not be suitable for the use in reliable inductions. One may summarise the position like this: given appropriate data on non-physical property, M, realised by base P, that will give us no right to expect reliable inductions concerning property M when it is realised by property Q.

[7] Hellman and Thompson [1975]

multiple-realisation may provide some support for that idea.[8] Or at least it provides a way of understanding how you might be both a non-reductionist in practice but have reductionist beliefs in principle. Why anyone should be so philosophically two-faced with respect to his or her ontological and epistemological perspectives is of course a different matter. Let us now turn to the supervenience doctrines to see if they are better equipped to support non-reductive physicalism.

Supervenience

Supervenience came to prominence in the philosophy of mind with Donald Davidson's paper "Anomalous Monism". However, Davidson's writings did not develop the idea of supervenient properties much beyond the slogan that there can be "no change in the mental, without a change in the physical". Other philosophers, notably Kim took the task of providing a precise definition of supervenience much more seriously. That work has led to many permutations of the basic idea, but each of these can be grouped into three main categories. If we take M and P to be non-empty families of properties to which F and G belong respectively, then we may define the three relations as follows:

(SS) **Strong Supervenience** M strongly supervenes on P just in case necessarily for any object *x* and any property F in M, if *x* has F, then there exists a property G in P such that *x* has G, and necessarily if any *y* has G, it has F.

(WS) **Weak supervenience** M weakly supervenes on P just in case necessarily for any object *x* and any property F in M, if *x* has F, then there exits a property G in P such that *x* has G, and if any *y* has G, it has F.

[8] For example Bonevac, D. "Reduction in the Mind of God" in Savellos and Yalçin (ed.) [1995]

(GS) **Global Supervenience** M globally supervenes on P just in case for any two worlds, w1 and w2, if they are P-property indistinguishable, then they are M-property indistinguishable.[9]

To put that in English: if the mental strongly supervenes on the physical then wherever there are certain physical properties there will be certain mental properties and that relation holds counterfactually. (WS) is identical, except the relation does not hold counterfactually. (GS) simply implies that given the complete physical state of the universe, the complete mental state of the universe is fixed. I think it should be clear from these definitions that (SS) implies both (WS) and (GS).

The definitions I have provided are not usually thought to be sufficient to guarantee the sort of dependency relation between the physical and non-physical most physicalists seek. That is because a dependency relation is presumably asymmetric, if A depends upon B, it cannot be the case that B depends on A. However, none of the supervenience relations defined above are, as they stand, asymmetric. (For this reason supervenience relations are sometimes referred to as co-variance, as opposed to dependency, relations.) Consequently, philosophers who advocate supervenience as a way of understanding the relation between the physical and the non-physical normally wish to add extra conditions to ensure asymmetry. Supervenience plus these additional conditions then is thought to give support to non-reductive physicalism because it is a relation at once strong enough to support physicalism, yet weak enough to block reduction; that is the idea, at least.

[9] Different philosophers will defend subtly different variations of these relations depending upon on how they construe the modal terms in these definitions and depending on whether they think the supervenience relation holds for objects or events.

It will be my contention in the rest of this section that none of the supervenience relations outlined above will be adequate to support non-reductive physicalism. I shall look at each briefly in turn and then examine a more complicated supervenience argument based on considerations from biology. I think, this biological variation on the supervenience argument goes further than other positions in making non-reductive physicalism a credible *epistemological* position but it too falls short of an ontological position worthy of that name.

Let us start with global supervenience for it is the most obviously flawed of the three. Assume that mental properties globally supervene on physical properties. Now, imagine two worlds which are physically identical and hence, by (GS), mentally identical. Let us suppose there is some minor change in physical properties in one of the worlds: that for example, a few hydrogen atoms are shifted in a galaxy far, far away. The two worlds now differ physically. Hence, it is perfectly consistent with (GS) that the two worlds may differ radically now in the distribution of their mental properties (or that there may be no mental properties at all in one of the worlds). It seems utterly mysterious then what the relation between physical and mental properties is, or why one should believe that the mental is actually dependent upon the physical.[10]

Advocates of (GS) will not be so easily persuaded. For example, John Post[11] argues that although global supervenience is *consistent* with the sort of story told above, it does not *imply* its truth. It may well be that moving a few hydrogen atoms has no affect at all on the mental properties of the universe. Post suggests then that (GS) be understood as programmatic. It defines the minimum structure which physicalism has to satisfy, the detail of which will be filled in later. But what will this

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[10] This is the standard argument given against global supervenience, first made by Kim[1989]

additional detail consist in? Presumably it will involve citing particular physical bases upon which particular mental states supervene. In other words, it will involve setting out the sorts of relations that characterise the (SS) relation. I do not see anyway of defending (GS) as providing an explanation for the link between the mental and the physical that does not make it into a version of (SS).

Weak and strong supervenience look more promising alternatives. They are clearly explanatorily more robust forms of the supervenience relation because they tie the supervenient property more closely to the physical manifestation of the thing we take to have that property. That is, if the mental supervenes on the physical it supervenes on the sort of physical thing that has mental properties, i.e. human bodies.[12] Yet both (WS) and (SS) imply the existence of psychophysical laws. From the above formulations (WS) implies $Gx \rightarrow Fx$ and (SS) implies $Gx \Box\!\!\rightarrow Fx$. (WS) though is consistent with reading the correlation between the subvenient and the supervenient as merely accidental; and most philosophers I suppose would think that accidental covariance of properties is not a strong enough relation to ground a dependency relation. Certainly, I take this to be Kim's view. Hence, the only relation robust enough to ground dependency is (SS) which implies the existence of nomologically necessary connections between Fx and Gx.

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[11] Post, J. "Global Supervenience: Too Permissive" in Savellos and Yalçin [1995]

[12] One of course might not believe that all mental properties supervene simply on what's in the head. Arguments for so-called "broad content" may lead a philosopher to believe that parts of a thinker's environment form some of the supervenience base. Such a view if accepted will not undermine any of the following arguments.
Also one should note I think that Post is scared off by (SS) because he takes it to imply individualism, that is narrow content. This is obviously not the case, even if historically advocates of (SS) have also been believers in narrow content. The relevant physical base may be a relation between the thinker and some section of his environment. What Horgan [1993] calls regional supervenience would be a version of this doctrine.

Kim's Argument

Kim in his original paper suggests[13] that the existence of such laws should lead us to expect reductions. It is, though, quite interesting to follow through the steps of Kim's argument to see how his own analysis leads to a rather surprising result.

Presumably the idea is that given (SS) we may use laws of the form cited above as our bridge laws in Nagel-style reductions, either singly to yield local reductions or jointly (and disjunctively) to provide us with a global reduction of some special science discipline to physics. However, I think it pays to look more closely at the sort of relation between the mental and the physical implied by the (SS) relation. One can best appreciate this with the following diagram:

Here we take the horizontal arrow to indicate causation and the vertical arrows the dependency relation that is in part given by the supervenience relation (SS). It seems to be perfectly consistent with this picture that we imagine the M's to be of an entirely different type of property. In other words, we may imagine the diagram represents the relations for a form of dualistic epiphenomenalism, rather than a form of physicalism. Indeed, it seems that is exactly what it does represent if we suppose that the mental property is not identical to a physical property. Kim, though, places an extra condition

[13] Kim [1989]

on a property being real which would seem to rule out construing the supervenience relation as at once dualistic and also epiphenomenalist. He claims that in order for any natural property to be construed realistically it should do some causal work. Since, by construction, one would think epiphenomenal mental properties do no causal work, they must be ruled out as real by this strengthening condition. But Kim's own analysis of what makes higher-level states causally efficacious is revealing. He maintains that one can claim that some mental event caused some physical event if the mental event in question strongly supervenes on the physical base which would feature in a complete *physical* explanation of the cause of the physical event. In terms of the diagram above, we could say then that M caused P' for example on the Kim model. A direct diagonal causal arrow from M to P' is ruled out because if M causes P' directly that would violate the completeness of physics; or if it does not then it would at least mean P' was causally overdetermined which Kim thinks is unacceptable. So the above diagram is an acceptable form of supervenient causation. But we have yet to see any reason not to interpret the diagram as describing a form of dualistic epiphenomenalism. In fact, this seems the most natural reading. *All that Kim's analysis has added is the counterintuitive idea that if one is a dualistic epiphenomenalist then one can claim Kim-style that mental properties are causally efficacious.*[14] Kim still maintains, though, that the relation between the mental properties and the physical properties is strong enough to support reduction. So, according to Kim although it would appear non-reductive physicalism is not a tenable position, given the various constraints he has placed on the mind-body relation, what we might call *reductive dualism* is perfectly acceptable. I think this must show there is

[14] The argument can probably be run with a version of psychophysical parallelism as well. However, it might be objected since that theory of the mind-body relation severs any connection between the two domains it would only be strong enough to support (WS), not (SS). (Although it is not obvious that must be the case.)

something deeply wrong at the heart of Kim's argument. It will be no surprise to find that I think it lies in Kim's reliance on the Nagel-model of reduction. A stronger model of reduction is clearly required, one in which bridge principles are identity statements, to prevent such pseudo-reductions and absurd consequences.

All that said, none of the criticisms I have offered of Kim should provide the non-reductive physicalist with any comfort. The supervenience relation as it stands is not strong enough to guarantee physicalism. Moreover, the fact that epiphenomenalism can be construed to make the mental causally efficacious, if we understand it as aversion of the (SS) relation shows not that epiphenomenalist of the past had misunderstood their own doctrine. Rather it shows that there is a general problem about construing any supervenient property as causally efficacious. If the supervenience relation is strengthened in a way that rules out dualism, by say the use of property identities, it will simply be a variation of the multiple realisability thesis and will encounter the same problems described above.

The Aetiological Account of Functions

I wish to turn to one last variation of the supervenience and multiple-realisability relations, one which does not simply rest on the fact (if it is such) that the relations between lower and higher order properties are heterogeneous in order to block reduction. It is an account put forward in various forms by David Papineau,[15] Ruth Millikan, Cynthia and Graham Macdonald[16][17] and Ned Block[18] that employs the theory of evolution to make sense of the multiple realisability and supervenience claims.

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[15] Papineau, D. [1993]
[16] Macdonald, G., "Reduction and Evolutionary Biology" in Charles and Lennon [1992]
[17] Macdonald, C., "Psychological Supervenience, Dependency and Reduction" in Savellos and Yalçin [1995]
[18] Block, N. [1995]

The basic idea is as follows: non-physical, in particular biological and psychological properties, are not reducible to physical states because although they are realised by these properties they are underpinned in special ways that are not captured by the physical bases and their relations. In the case of biology this is understood in terms of some functional or teleological property. A non-physical functional property then is explained in terms of the selective pressures which brought about its existence. So we may say biological property, B, has the function of doing A, if, and only if, B is present (that is, selected) as a result of causing A. This is known as the aetiological account of functions. Advocates of the theory can cash out the explanatory debt I claimed any physicalist owed us of why one should believe such properties could be so radically multiple-realised. For example, certain creatures may all have the functional property of rearing up in order to frighten off predators and that, as a matter of fact may be physically realised in a variety of ways, according to the advocate of the theory at least. The aetiological account provides us with a way of understanding how that could be; all the various biological systems (or their ancestors) performed that same function and as a result survived.

The thesis can be put in terms of supervenience as well. However, so construed it is a non-standard variant of the supervenience view. Since the functional property can only be understood by reference to the causal history of the biological entity, functional properties are taken to supervene on both the physical states of the creature and its ancestors. That is to say, we must understand the supervenience doctrines *diachronically*.

But it is neither the fact that such accounts imply multiple-realisability nor their construal as supervenience relations which makes them non-reductive. If that is all there was to the claim that they do not reduce at the ontological level, then there would be no additional interest in these forms of non-reductive physicalism; they would be no different from standard functionalist views. It is the additional assertion that a full understanding of bio-functional properties brings in teleological notions, goals, ends, etc.; and such teleological notions are in principle irreducible to the causal-nomological framework of the physical. Similarly with psychological properties, they will contain teleological, rational or normative elements which, so it is argued, prevent them being reduced to physics. As Cynthia Macdonald puts it "[non-reductive physicalists] will argue that intentional properties have a nature, due to their possession of intentional content, in virtue of which they enter into broadly logical relations...These general relations exhibit a pattern which is not causal-nomological but rationalistic."[19] This account attempts once again to introduce an asymmetry between the physical bases, which physicalists understand to realise the higher-order properties, and those properties that are so realised. Yet at the same time, it appears, at least as formulated by Macdonald, to acknowledge that all the causal work is done at the physical level; the properties that are characteristically mental or biological fall into patterns or theories which explain different sorts of relations, normative or teleological relations. That may seem counter-intuitive. It is often thought to be one of the attractions of physicalism that it accounts for mental causation in way which does not treat psychological causes as special but as simply part of the normal causal order. Moreover, it is surely a strong intuition of ours that mental states do in fact cause physical states but the aetiological account of functional properties seems to deny that we understand that intuition in any straightforward sense. It is beyond the scope of this work to discuss the specific problems any such account may have with mental causation. However, I think it is important to note that

[19] Macdonald, C. "Psychological Supervenience, Dependency and Reduction", p. 147 in Savellos and

understanding the mind-body relation in a way which characterises the mental as non-causal will leave serious questions about the value of such account.

Whatever the short-comings of the aetiological account as an explanation of mental causation, I am at the moment interested in the idea as a way of supporting non-reductive physicalism; and that depends on construing the mental and biological as different from the normal causal order of things.

How successful is this strategy? I think it pays to be obdurately simple-minded when analysing the force of the aetiological account's claim to be at once physicalist and non-reductionist. Let us first consider the doctrine as a variation on multiple-realisation. We can argue in a parallel fashion to the first section. Biological states are not type identical with their physical bases but are realised by them (and this is a relation which presumably holds across physically possible worlds). It would be natural to think then that one can assert that the biological property is equal to a disjunction of its physical bases: Bf = P1 v P2 v ...Pn, (where Bf is the bio-functional property and P1, etc. are the physical bases). Now, if such a relation holds we can demand, as above, that features ascribed to one-side of the relation must be ascribed to the other side too. So either both are kinds or neither is. Similarly either both properties have a teleological component or neither does. The advocate of the aetiological account must show us why the identity does not hold.

The answer an advocate of the aetiological account is likely to give is: because the biological properties supervene diachronically on their bases. Therefore, the identity statement as I have it cannot hold because it only links *actual* physical states with biological states; and the biological state is more than simply the physical states that actually underpin it; it is those physical states plus the physical states of its

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Yalçin [1995]

ancestors. One might counter that such considerations might lead us simply to construe the realisation relation and the identity statement historically. The states P1, etc. will therefore include reference to physical states of ancestors. Possibly that seems a little awkward. However, I doubt one need make such response, for the diachronic supervenience thesis is I think highly questionable anyway, if we consider the following example.

It is presumably possible, although unlikely (I hope) that I, as I write this thesis, am destroyed by a bolt of lightening. It is also possible, at least by physicalist lights, that an exact physical replica of my body be formed from, say, the surrounding material in the room (assuming, of course there was the right sort of material present). So we can imagine a scenario where the biologically evolved me is destroyed and a physical me-duplicate is spontaneously created. Now given the diachronic supervenience thesis holds true it would have been correct to ascribe biological functions or properties (and I assume by extension psychological and normative states) to the now, unfortunately, deceased me. The same privilege, though, cannot be extended to the me-duplicate since it did not evolve and has no ancestors. Now this seems very curious. Presumably, if physicalism is assumed to be true (i.e. that there is a complete physical explanation of all physical events), two physically identical individuals in the same environment will behave in exactly the same way (or will have the same chance of behaving in the same way). If created-me behaves in exactly the same way as evolved-me *would* have behaved had he not been destroyed, then on what grounds could we deny he had the same functional or intentional properties? If for example, he immediately sat down at a computer and began in a similar manner railing against physicalism, why would it be wrong to say that he believed physicalism was ill supported by science or philosophical argument? I think there can be no real reason. It is not clear at all how differing causal histories can legitimate the ascription, the *real ascription,* of one property to physically and environmentally indistinguishable creatures.[20]

If we are suspicious then of the claim that causal history is relevant to the ascription of such functional properties, then we will return to our simple-minded view that actual bases are sufficient for realisation and hence reduction. Moreover, we are led back to our subsidiary conclusion that either both sets of properties have teleological and normative components or neither does. But why would anyone favour the former option? It would seem very strange to think of normative properties as part of the basic make-up of the world. Given that it seems natural for a physicalist (or indeed anyone) to deny normativity to the physical bases, he must deny *real* normativity to the higher-order properties. In fact, this appears to be what a number of physicalists are prepared to contemplate. As Terence Horgan puts it in his admirably clear and candid survey of the current state of physicalism in the philosophy of mind: "The task of explaining supervenience facts, including perhaps psychological supervenience facts therefore...includes the task of explaining how certain objective, in-the-world, is-ought gaps get bridged...and given that challenge...materialistically-minded philosophers ought to be exploring *irrealist* ways of accommodating higher-order discourse."[21]

It is well beyond the scope of the present work to address the question whether or not physicalism *commits* you to some sort of irrealism about normativity. But I feel obliged to say, if the irrealism is understood in eliminative terms (that is we can just do without the normative) then I am inclined to agree with Putnam when he says in

[20] My example is of course just a variation of Donald Davidson's [1986 swamp man, although I reach the opposite conclusion from him. See Sosa, E. "Abilities, Concepts and Externalism" in Heil and Mele (eds.) [1995] for similar thinking to mine on the issue.
[21] Horgan, T. [1993] p.581

grand rhetorical mode: "[If the eliminativist is right] why should we expend our mental energy in convincing ourselves that we aren't thinkers, that our thoughts aren't really *about* anything, noumenal *or* phenomenal, that there is *no* sense in which any thing is *right* or *wrong* (including the thought that no thought is right or wrong)."[22] Indeed, an eliminativist approach would appear to threaten the whole of science itself since science is presumably to some extent a rule-governed activity. That being the case it would seem, as Putnam suggests, that eliminativism concerning the normative would undermine physicalism itself, for it would undermine the assertion that it was a *better* world-view than any other, since that is obviously normative notion. Of course, one can be irrealist with respect to a certain property without thereby being an eliminativist; one can be what Horgan calls a *preservative irrealist*. The most obvious form of preservative irrealism is instrumentalism, espoused most famously by Daniel Dennett[23], and there are other irrealist possibilities countenanced by Horgan. I think though one should be suspicious that any non-realist understanding of the normative can save the physicalist from the seeming paradox of asserting that we ought to believe physicalism and at the same time confessing that no worldly account of the "ought" in that assertion can be given. To convert that intuition into an argument would take us too far from the main thesis and it is not clear that in the end it would be conclusive. It may be possible to render an *ersatz* normativity respectable and it may be possible, though I have my doubts, to be a normative realist and a physicalist.

Physicalism or Non-reductionism

It is worth reviewing the dialectic of the argument up to this point. I have *shown* that the sort of relations non-reductive physicalists posit, supervenience or multiple-realisation relations, are either not sufficient to guarantee physicalism or

[22] Putnam, H. [1983]p.246

imply reduction in principle. On the way, I have *suggested* that certain forms of physicalism may either fail to address pertinent questions, for example, issues of mind-body causation, or teeter on self-refutation by denying the normative; but these have been asides to the main argument. The basic structure of that main argument has always been the same: given the realisation relations, no asymmetry between the base and higher-order properties can be maintained; hence, reductions should be forthcoming. Now, this might lead some to the conclusion that we should be *reductive physicalists*, certainly this is Kim's view. However, I do not accept this. First of all, one should remember that all the realisation relations discussed so far are merely speculative. Secondly, I take it that one of the motivations for non-reductive physicalism was the failure of *actual* reductionist programmes both within philosophy and science. I would support that view; the arguments of the first section were meant to lend further support to it. Moreover, antireductionists have provided their own examples and case studies to lend further weight to the suggestion that *actual* reductions are unlikely to be forthcoming. I also share with the anti-reductionist a rejection of eliminativism for non-physical disciplines. Laws and theories of the non-physical sciences appear to work well within their domains and hence, it would seem to me, we have as much (or as little) reason to treat them realistically as we do physics itself. Given then the foregoing argument to show that one cannot hold to anti-reductionism (ontologically speaking), physicalism and anti-eliminativism I, not surprisingly, think we should reject the belief in physicalism. The only other option available (rejecting eliminativism outright) is to retreat to a denial of epistemological reductionism, that is the possibility of actual reductions, while simultaneously affirming physicalism, and hence some view that commits you to reduction (or

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[23] Dennett, D. [1987]

elimination) in principle of higher-order disciplines. But why should anyone do this? Richard Boyd[24] has argued that the sort of relations conjectured to exist by antireductionists demonstrate that it is possible to reconcile physicalism with the apparent disunity of science. But since the realisation relations are only speculative, Boyd only shows that epistemological anti-reductionism and ontological reductionism, that is physicalism, are consistent. The question is given that the epistemological doctrine is untenable why *should* anyone remain committed to physicalism; what is the remaining attraction of physicalism?

In the next section I shall address one possible very strong motivation for remaining a physicalist in the absence of actual reductions in science, a belief in the completeness of physics. I shall show argue that this doctrine is highly questionable for several reasons.

[24] Boyd [1980]

QUANTUM QUANDRIES

The purpose of this section is to find the beginnings of an argument that will undermine physicalism. I cannot refute physicalism. One cannot refute any internally consistent ontological position. However, I believe I can demonstrate that physicalism faces serious conceptual and explanatory problems. In particular, I will argue that if one takes contemporary physics seriously, then one of the core principles of physicalism, the completeness of physics is highly questionable.

The Completeness of Physics

Before we begin to analyse the completeness of physics, we need a definition of the doctrine. Papineau provides as good an account as any:

> I take it that physics, unlike other special sciences is complete in the sense that all physical events are determined, or have their chances determined, by prior *physical* events according to *physical laws*. In other words, we never need to look beyond the realm of the physical in order to identify a set of antecedents which fixes the chance of any subsequent occurrence. A purely physical specification plus physical laws, will always suffice to tell us what happened, in so far as that can be foretold at all.[1]

The problem the above definition encounters, which was briefly raised in the last section, is explaining exactly what is meant by physics. If we identify physics with the discipline currently practised by scientists, it is almost certainly false. The very fact that physicists are currently engaged in research to account for phenomena which they are currently unable to explain should suggest that it is likely physics, as it stands now, is not complete.[2] However, if we allow the physicalist to expand his definition of physics so that it includes whatever one needs to explain all events, it may encompass properties that would normally be considered non-physical. For example, if it turns out certain mental events or properties are required to explain

[1] Papineau, D. [1993]

[2] This was of course argued in the first section.

certain physical events then those mental events would be deemed physical by such physicalists, which seems absurd. The completeness of physics on such a reading would be an analytic truth.[3] In the previous section I tended towards the latter option, simply defining physics as the non-mental. However, I would now like to make a more substantial suggestion: one which will allow the physicalist to steer some course between the Scylla of obvious falsehood and the Charbydis of vapidity.

I suggest physicalists adopt a view with regard to current science similar to that of contemporary scientific realists. A physicalist should, in other words, acknowledge that existing scientific theories are most probably *strictly* false. Yet, he should ameliorate that pessimism with the claim that current, mature science is *approximately* true.[4] The ideal physical theory that forms the basis of the completeness of physics will be, therefore, a conservative extension of some current theory. Given the nature of reductionist philosophy, that big things should be explained in terms of little things, the only obvious candidate theory is quantum mechanics.

It will be the burden of the rest of this section to argue that certain features of quantum theory bring into doubt either the truth of the completeness doctrine or in more general terms the feasibility of physicalism. There are three consequences of quantum theory which, I will argue, cause problems for physicalism - indeterminism; indeterminacy of states; and non-locality.

Before expounding the arguments in detail, I should explain why I think these features must be part of some future theory. After all, while the physicalist may agree with me that he should view scientific theories as approximately true, he might maintain this commits him to nothing concrete, since he does not know which parts of the current theory are true (or nearly true). It would be wrong then to attempt, as I

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[3] Crane, T. and Mellor, D. H. [1990] argue the case that the completeness doctrine cannot avoid either

have done, to select certain elements of the existing theory that must be retained by future theories. Certainly I have some sympathy with this sentiment. It is not generally the business of philosophers to second-guess the development of science. Nevertheless, in the particular case of quantum theory, certain technical results, the Bell theorem and the Kochen-Specker theorem, place strong restrictions on the sorts of successor theories that could reproduce the results of quantum mechanics. To go into the technical detail here would be inappropriate.[5] Suffice to say that the upshot of both theorems is that it is impossible to maintain value definiteness (hidden variables) and determinism unless one violates certain principles of relativity.

So much the worse for relativity theory some might think.[6] Of course, equally one might think so much the worse for quantum mechanics. The true, complete physical theory *may* well be a radical departure from contemporary physics. So much so that it may make no sense to talk of recovering the results of quantum mechanics as the Bell theorem assumes. One must certainly accept that is a possibility. However, a physicalist who adopts an eliminativist line with regard to contemporary physics will I think be dangerously close to making his doctrine vapid. Indeed, I think such a dismissive attitude to present day theories does not sit well with the general character of physicalist philosophy. If physicalists are forced to conclude that contemporary physics is false, radically false, I hardly think their doctrine is worthy of the name *physical*ism. Such considerations I think should be decisive for physicalists should be

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obvious falsehood or vapidity.

4 This is essentially the suggestion made by Melnyk, A. [1998]

5 One can find an accessible explanation of the technical details of Bell's theorem and the Kochen and Specker argument in Hughes, R.I.G. [1989].

6 The Bohm theory is an example of such a theory. It retains a classical notion of position but implies the existence of privileged frame of reference (which contradicts special relativity). I do not think, though, we should entertain the possibility that the Bohm theory offers a quasi-classical alternative to quantum mechanics and thus an alternative version of physics on which to construct one's physicalism. First, because no working scientists are Bohm theorists. Secondly there is no feasible extension of the Bohm theory to provide an analogue of quantum field theories.

decisive for physicalists; and so the problems generated by quantum theory must be taken seriously.

However, if there remain doubts within physicalists ranks, we may shift the burden of proof. Surely it is a *possibility* the world is as quantum mechanics describes. In fact, it is a better bet, one would think that the world is that way than any other that we have thought of up till now. It is equally certain, I think, that no physicalist wants to be in the business of telling scientists that their theory is wrong because it clashes with their favoured ontological doctrine. So if physicalists wish to retain the high scientific ground, they had better be able to accommodate the *possibility* that quantum theory is true. Let us turn then with a serious mind to the three problems I claim quantum theory creates.

Indeterminism: A Problem of Definition[7]

The problem that indeterminism creates for physicalism and the completeness of physics is, as I see it, one of definition. I think it is the most tractable of the problems I shall raise for the physicalist. It is nevertheless worth raising, if only to focus physicalists' attention upon possible solutions.

Perhaps the point I wish to make can be best illustrated by a contrast with the deterministic case. I think it is fairly clear what the completeness doctrine entails if the world is deterministic. We may imagine that the world is in some physical state, P, at time t. The evolution of P is entirely fixed by the laws of physics such that they will map P onto some unique state, let us call it P(T), for any arbitrary time, T, in the future. Therefore, no non-physical event can have a physical effect on pain of causal overdetermination, which is presumably ruled out for reasons of philosophical good

[7] There are obviously reasons beyond those provided by quantum theory for thinking the world is indeterministic. Theories of statistical mechanics and evolutionary biology seem to posit fundamental randomness. So the need for physicalism to be consistent with indeterminism is more pressing than perhaps any of the other features thrown up by quantum theory.

taste. Allowing then that the definition of physics is true, and that overdetermination is forbidden, we appear to have an argument which shows that any event with physical effects must be a physical event and hence the beginnings of a justification of physicalism.

In the indeterministic world things are less neat. First of all we can see that any argument for physicalism that is based on considerations of overdetermination is bound to fail. One cannot rule out mysterious non-physical causes because they would overdetermine their effects, simply because given indeterminism, no event is determined in the first place. The overdetermination argument is, I think, a red herring. It is best understood not as an argument for physicalism but as a vivid, rhetorical way of restating the physicalist position. Given indeterminism, and thus no determination at all, let alone overdetermination, one needs another way of asserting that non-physical events have no impact on the physical world; and this is precisely what I take to be the definitional problem for the physicalist. How does one explain the idea that physics supposedly "determines the chances" of some occurrence?

Perhaps the most natural interpretation of the phrase "determined chance" is fixed probability. But that definition simply gives rise to the equally thorny problem of how to interpret probability statements. Since we are assuming these are genuinely chance events, obviously what is required is some sort of objectivist interpretation of the probability calculus, i.e. an interpretation that makes probability a property of the world rather than a property of our beliefs about the world. Let us suppose we opt for a frequentist account. How will that affect the definition of the completeness doctrine? The frequency interpretation equates the meaning of a probability statement with the ratio of positive occurrences of the event in question to the total number of trials, as the number of trials tends to infinity. Put less abstractly, if the probability a coin landing heads is 0.5 then that means, in the limit, half the coin tosses (the trials) will end up as heads. Given a frequentist definition of the probability calculus course it is fairly elementary to construct an example which satisfies the frequentist notion of fixed probability but which contradicts physicalism.

Imagine a situation where a physical body (let's say it is a human body) is in some physical state Q and there is 0.5 probability that it will develop into state R and 0.5 probability that it will develop into state S. Now, on the frequentists understanding of the probability calculus that means given an infinite number of Q events half will develop into R events and half will develop into S events; and that is all it means. Let us add to our imagined situation the fact that some mental property, which by construction is non-physical, occasionally affects the outcome of Q events. That is, it has a positive causal impact on whether or not R or S occurs. We might say for example that the intervention of M guaranteed an R-occurrence. Intuitively, one would think, such a situation violates physicalism since mental events have a positive effect on certain physical outcomes. However, if we imagine that M intervenes a finite number of times, so the overall effect of M events is negligible within the infinite series of Q events, then the ratio of R-occurrences and S-occurrences to the total number of events will be unaffected. Hence the probability will remain unchanged; and thus, it will in fact be correct to say that purely physical antecedents will fix the probability. But something has gone badly wrong, for at least sometimes it is just not true to say that all non-physical causal factors are excluded; clearly, a frequentist account of probability statements just will not do for a definition of physicalism.[8]

A more promising objective formulation for a physicalist is provided by the propensity account. For a propensity theorist a probability is a disposition that some

[8] This is a variation of Popper's[1972] argument against the frequency interpretation.

single object or experimental arrangement possesses. So to return to the above example, to say that event R has a probability of 0.5 of occurring, means Q has a disposition to develop into R states half the time. The putative dispositional property is taken to explain the emergence of the statistical frequencies which probabilities are tested against. This definition clearly overcomes the problem I outlined for the frequency view. Given an intervention by some mental property M, in a series of Q-events, if M has a positive effect on either an R or S occurrence, that will change the propensity, and hence the probability, of that particular trial producing that particular result. Consequently, one would seem to save the intuition that in a physicalist world, mental properties or events should not have an effect on physical outcomes. However, if we make the example more complicated it does not straightforwardly follow that given a fixed propensity one rules out mental intervention.

Consider a situation in which the physical factors X,Y and Z determine the propensity of some event, W, occurring. We might imagine that sometimes when the events X, Y, Z occur they are preceded by another physical event, D. D in turn causes two events, a mental event M and a physical event P. Event M has a positive causal impact on the outcome, that is by itself it would make the occurrence of W more likely. P has a negative impact on the overall outcome of W. It just so happens that M and P never occur in this world without being preceded by D. It further happens to be the case that the amount which M increases the probability is exactly cancelled out by the amount which P decreases the probability. So the propensity of X, Y, and Z to cause W is unaffected by the additional physical factor D. Should we conclude, though, that these three factors are the only relevant causes? I think not, since the way I constructed the situation ensured M had a positive effect on the outcome of event W.

The argument here focuses not on the interpretation of the probability calculus but how the concepts of probability and causation are connected.[9]

Certainly the example is highly abstract and highly unlikely but that should not affect its significance. I am trying to provide scenarios were the physicalist definition does not seem to guarantee their doctrine. Clearly, the example shows that the notion of probabilities being fixed, in some sense, by physical antecedents is no guarantee that non-physical causes are doing no work. Nevertheless, I think the problem could be resolved by physicalists. A closer analysis of what we might mean by fixed probability or determined chance, combined with an analysis of probabilistic causation, and how non-sufficient probabilistic causes may contribute to determining the same effect would be a start. It is beyond the scope of the present work to attempt to discuss any of these issues here in the depth they demand. But before these issues are resolved I do no think one can give a clear explanation of what is meant by the completeness of physics for an indeterministic universe.

Quantum Mechanics and the Indeterminacy of States

There are more substantial objections to physicalism to be found in the details of quantum theory. However, this will require some explication (but not too technical) of the formalism of quantum mechanics.

Consider an electron, according to physics electrons has certain properties: energy, mass, spin, momentum, etc. Let us focus one of those properties, spin. I choose spin not because there is anything in particular special about this property in terms of its quantum description but because it will make the explanation easier. Spin's appeal is that for an electron at least, it can only assume one of two values (eigenvalues): it is either spin up or spin down. The obvious disadvantage of selecting

[9] This is of course just a variation of the argument of Hesslow's [1976] that causation does not

this property is that it has no classical analogue, so it is perhaps difficult to obtain an intuitive grasp of what is being discussed. If the reader finds that he is uncomfortable with the strange property spin, then he may replace talk of spin with one of the other more familiar properties. Nevertheless, he should appreciate that although this will not change the nature my analysis, it would make the examples more complicated since, in the case of the other properties, the electron has an infinite number of possible values it might take on.

In order to discern the value of spin for a particular electron, we must measure the spin along some particular axis. By convention we name one such axis of measurement the *x*-axis. Let us imagine we have just measured the spin of the particle along the *x*-axis and we have discovered the electron is in a spin up state. We label the quantum state of the electron which corresponds to that result |UP*x*⟩ (and we call this an "eigenvector"). Quantum measurements are such that if I were to immediately remeasure the spin of the electron along the *x*-axis we would always obtain the result, spin UPx[10], since the particle remains in the state |UP*x*⟩. No great surprise. However, if we now take the same electron and try to measure the spin of that particle along some different axis, say the *y*-axis, things start to become stranger. Sometimes when we measure the electron it will be spin up, that is in state |UP*y*⟩, and sometimes it will be spin down, that is in state |DOWN*y*⟩. A natural conclusion to draw from these results would be that some electrons that have the property UP*x* also have the property UPy and some have the property DOWNy. One would then expect that, if we measured the electron along the x-axis of spin immediately after it had been measured along the y-axis, we would always obtain the result spin UPx. This is not the case. The measurement of the system along the y-axis in some way disturbs the quantum

necessarily imply probabilistic variation.

system so that the outcome of measurement along the x-axis becomes totally randomised, sometimes one obtains the result UPx and sometimes the result DOWNx. This strange effect is a consequence of what is known in quantum theory as the *incompatibility* of the two variables, spin along the *x*-axis, and spin along the *y*-axis. Other pairs of properties are also incompatible, for example position and momentum; energy and time; the various components of angular momentum with each other. The consequence of this incompatibility is that it is impossible to have definite values for incompatible pairs simultaneously. So given that we have obtained a definite value for spin along the y-axis, we cannot provide a definite value for the spin along the x-axis, it is neither spin UPx, nor spin DOWNx. However, one can represent the state of the system for the spin x-component after such a measurement, in the following way: $A(a|UPx\rangle + b|DOWNx\rangle)$, where, a and b are constants and A is some normalisation factor. This is called a superposition of states. Although the superposition itself does not represent a definite value, it does allow one to predict precisely the probability that either an $|UPx\rangle$ or $|DOWNx\rangle$ state will occur after measurement. It is these superposed states that I think are likely to cause problems for physicalism.

A first naïve argument against the physicalist might be to say that superposed quantum states do not represent definite properties and hence cannot form the bases for a physical description of the world. As Jeffrey Poland puts it in defining his physicalism: "[I]f there is no fact of the matter regarding as to what objects are included in the bases, then there is no fact of the matter regarding what the theses [physicalism] are expressing, and hence there is no fact of the matter regarding whether the theses are true or false. Vacuous or indeterminate content, therefore,

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[10] I hope the nomenclature here is clear: the unbracketed state represents the value after measurement, the bracketed state is the wave equation.

undermines the significance of physicalist doctrine and obstructs the attainment of the goals of the physicalist programme."[11]

The anti-physicalist might argue then that because superposed states are value indefinite, there is no fact of the matter that the electron is in one state or the other. Hence, a physicalist could not assert, say P = m, where P is a physical property and m a non-physical property, because P is not well defined for any superposed states (and all quantum states are superpositions under some description). But Poland has an obvious reply: "the wave function gives very definite descriptions of the system in terms of the probability distributions; such descriptions give a complete characterisation of the system. Thus for any such description or associated probability distribution there is a fact of the matter whether it is in the physical bases. What more could anyone ask for in characterising such bases?"[12]

In other words, Poland is prepared to characterise the physical bases as *completely and determinately* described by the superposed states.[13] Now, this may answer the naïve objection that the bases are ill defined. But it simply gives rise to a more profound problem. Namely, if superposed quantum systems are the base physical states, then how do they give rise to macroscopic phenomena which never appear to be in superpositions but always in some definite property state? We seem to have simply reversed the direction of the mystery. Looking down from the macroscopic domain to the quantum domain, one cannot understand how quantum states can be well defined. Looking up from the quantum domain, we cannot see how to recover the macrophysical image.

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[11] Poland [1994], p.148
[12] ibid. P.174
[13] Poland also suggests that the definite value states acquired after measurement could also form base properties. However, one should remember they are only definite states from one perspective. For an incompatible variable such terms will be superpositions of states.

This question is in fact closely related to what is known as the "measurement problem" in quantum theory. It is perhaps easier to appreciate the force of the objection in terms of that problem. To explain the details of the measurement problem, we need a richer account of quantum theory than has been given up till now.

Quantum mechanical states are governed by the Schrödinger equation, which tells us how some state, say, |UPx⟩, develops through time. After some time |UPx⟩ will develop into some superposition of |UPx ⟩and |DOWNx⟩ states. However, on measurement, as was implicit in my earlier discussion, the wave equation "collapses" from a superposed state to an eigenvector either |UPx⟩ or |DOWNx⟩. The problem here is that the measurement interaction is not part of the Schrödinger evolution of the system; it is a new type of interaction. If one treats the measuring apparatus like a quantum system that interacts with the electron, then one does not obtain a determinate property state but an entangled system of the form:

|M(UPx)⟩ |UPx⟩ + |M(DOWNx)⟩ |DOWNx⟩, where |M(UPx)⟩ and |M(DOWNx)⟩, represent the states of the measuring apparatus for measurements of |UPx⟩ and |DOWNx⟩ respectively. But of course we never do observe situations like this. In fact, we probably cannot even begin to comprehend how we could observe superpositions of states. Measurement then appears to induce a different sort of interaction not captured in quantum theory. The conceptual problem is how to understand what is special about measurement interactions that takes particles from superposed states to definite property states. And this is just the problem of how quantum systems relate to macroscopic systems, since the measuring device is a macrosystem.

Some of the solutions mooted both by philosophers and by physicists to account for wave function collapse should also be disturbing for a physicalist. For example, Neils Bohr thought that collapse was induced when the system interacted with an irreducibly classical macroscopic object. He thus denied the possibility that one could apply quantum theory to the measuring apparatus. Irreducibly classical features that are always external to quantum theory, according to Bohr, govern measurement interactions. Hence, the completeness of quantum theory is undermined.[14] A similar but more extreme view, held by Eugene Wigner[15], attributes collapse to the interaction of the human mind with the system. Again, non-physical, in this case mental, causes are taken to affect quantum events; again completeness is undermined.

The above views both attribute collapse to some non-quantum element of the measuring situation. Thus both seem inevitably to lead to viewing quantum theory as incomplete. However, there are interpretations that deny this duality between quantum system and measuring system. Such interpretations, inspired by the seminal work of Hugh Everett III, all deny there is any such thing as wave collapse. The great advantage of these interpretations is that they allow one to treat the measuring system as a quantum system. On the other hand the major problem for such accounts is to explain why we think we see collapses and never superpositions. Explanations have split into two competing accounts: the many worlds view and the many minds interpretation. The former interpretation states that during measurement interactions the universe splits into many universes each one corresponding to each possible outcome of measurement.[16] The latter formulation makes the comparatively speaking more modest suggestion that the human mind splits when measurement takes place.[17] So, although on both accounts the universe is actually in a superposed state because

[14] I mean completeness here in the sense formulated by Papineau in his description of the completeness of physics, *not* the sort of completeness of quantum theory that is discussed in EPR-type phenomenon.
[15] See Hughes [1989] for a discussion of Wigner's and Bohr's position.
[16] DeWitt, B. [1970]

my mind or body splits during measurement, I am confined to represent the world in a nonsuperposed way. There are technical problems with both approaches, in particular when it comes to understanding what the probability assignments in quantum theory mean. But such technical problems are not, I think, the cause of the resistance amongst philosophers of physics to either view. I take it that what most find objectionable, and no doubt what any lay physicist will think too, is that such views are fantastical. Who could believe in splitting worlds or minds? But what that strong intuitive objection expresses, I think, is a dissatisfaction with these theories' attempts to explain the emergence of the macroscopic image. In fact, both interpretations make our perception of the macroscopic world false (or at best seriously incomplete). They are, in other words, eliminative about the macroscopic image of the world.[18] I doubt it is the sort of world-view most physicalists will find satisfactory. Furthermore, it is doubtful whether such theories have really explained measurement interaction and the connection between the quantum and the macroscopic level. What for example is so special about measurement interactions that they cause splits in the universe or minds. Why do splits not equally occur at lower levels of reality? In short, the proposed solutions to the measurement problem seem either to lead to the imposition of non-quantum causes or to understanding the world as completely different from the way we think it is. But neither type of view elucidates the relation between the macroscopic and the quantum. One asserts simply there is a strong dichotomy and the other fails to give an adequate answer to why we think there is a strong dichotomy.

Now, it may be the case that some as yet undeveloped account or extension of quantum theory will make the connection between the macroscopic and quantum

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[17] See Albert, D [1992]. Ch. 6 and 8

[18] In fact, the many minds view (or rather its extension to quantum field theory) is consistent with the universe being in a vacuum state, that is empty although because of the superposed states we think otherwise. See Albert [1992] Ch. 6 for a brief explanation.

domains clear and uncontroversial. Moreover, it may emerge in a way that will allow one to treat measuring devices as governed by the same principles of interaction as electrons. But no such account is immediately forthcoming. The two types of "solution" to the measurement problem I have discussed are the only major alternatives on offer. That, in itself should be sufficient to cast doubt upon the idea that the physicalist can assert the sort of dependency relations which form the backbone of their position.

Non-locality: A Problem for Parochial Physicalisms

The problem of understanding how the quantum and the macroscopic image relate, I take to be the most serious difficulty posed by quantum theory for physicalism. However, superposed states have another property that will cause certain construals of physicalism profound problems of adifferent kind. A few more elements need to be added to the account of quantum theory so far given to bring this point out.

The measurement problem arises from attempting to treat the measuring apparatus as a quantum system, the result is not a collapse but what I called an entangled system. Quantum particles can also be come involved in such entangled systems. Let us imagine then that two electrons have interacted in a way that leaves them in an entangled state. We can describe the spin-x states of the system as follows:

$$1/\sqrt{2}(|UPx\rangle_1|DOWNx\rangle_2 - |DOWNx\rangle_1|UPx\rangle_2),$$

where the subscripts distinguish the states corresponding to the two electrons. This complicated expression tells us a number of things. First, that if we measure the spin state of electron 1, there is 0.5 probability it will turn out spin up. If such a result is obtained on measurement, then the above state vector collapses to the form:

$|UPx\rangle_1|DOWNx\rangle_2$. This expression tells us, without need to measure the second electron, that it must be in a spin down state. Conversely if we obtain a measurement of spin down for electron 1, electron 2 must be in a spin up state. Why should one think this is curious? Well, the two electrons that interact could be miles and miles, galaxies and galaxies apart before one is measured. We must also remember that quantum theory as I have described it does not allow us to think of the particles having definite values before measurement.[19] Hence, the properties exhibited by electron 2, that is whether it is in a superposed state or eigenstate, will depend on something which happens to electron 1 and that could be an event on the other side of the universe.

This result should be disturbing to physicalists who understand the relation between parts and wholes as one in which the properties of the whole depend upon the properties of the parts. Here we seem to have the exact opposite. The properties of the whole, that is the entangled quantum state as described above, appear to determine the properties of the constituent individuals, the two electrons. Moreover, it does so in a way that undercuts many intuitions concerning part-whole relations. That is, a natural physicalist assumption is that the properties of macrosystems will be entirely explained in terms of the intrinsic properties of microparticles and their extrinsic relations with each other. But if any the microparticles are entangled with particles not thought to be part of the system, one is able to affect the properties of the macrosytem by interfering with a quantum entangled object which may be spatially far removed from it.

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[19] This is in fact what the Bell theorem, that I somewhat enigmatically referred to in the opening section, tells us. If we assume that the electrons have definite, hidden values we will get different results from those predicted by quantum theory. Experiments have been conducted by Aspect to test these alternative views: all have confirmed quantum mechanics and disconfirmed the local, hidden variants. See Hughes, R.I.G.[1989] for details.

Paul Teller[20] has used such considerations to argue against what he calls local physicalism and in favour of global (or holistic) physicalism, a view which is not dissimilar to the global supervenience doctrine, briefly discussed in the last section. Certainly I think it is possible to retain some sort of physicalist position, given these specific considerations. Perhaps, in light of non-locality, the idea that moving a few hydrogen atoms could radically alter the distribution of mental properties is not so bizarre after all, if we imagine these atoms are entangled with distant, macroscopic physical states.[21] Perhaps, or perhaps these strange results are just further evidence that we do not have a natural way of reconciling the quantum domain, where non-locality is rife, and the macroscopic image were our intuitive understanding of causal interactions allow only local correlations between states.

Where to now?

Of the three problems I have presented for physicalism: indeterminism, non-locality and indeterminacy, the greatest of these is undoubtedly indeterminacy. Each though has a certain potency. The weakest is undoubtedly, the argument from indeterminism. I have shown that at least for certain situations and certain interpretations of the probability calculus, that the simple notion of physical antecedents fixing the probability is not enough to guarantee physicalism. This should focus physicalist minds on the question of how probabilities and causes should be related in a physicalist world. I imagine this can be done in a way that is satisfactory to physicalists. The argument from non-locality is decisive against certain forms of physicalism. It completely alters our understanding of compositional claims, and the general reductionist claim that big things are made of little things; however, it will have no affect on others who take a more holistic view. The problems, though, of

[20] Teller, P. [1986]. See also "A Contemporary Look at Emergence" in de Beckermann, Flohr and

indeterminacy are both profound and wide-ranging. The fact that we cannot understand how properties of the quantum level relate, or could relate, to higher-order properties should encourage us to look beyond physicalism and explore other options. This will be the objective of the final section. It is to be hoped that by laying bare other ontological options one will be better able to assess the value of the various alternatives to physicalism.

Kim(eds.) [1992]

[21] See pages 37-38 for a discussion of global supervenience.

EMERGENT ALTERNATIVES

It is not enough simply to show the limitations of a physicalist ontology. After all, it is never the case that a philosophical research programme is dead and buried. Perhaps a new form of physicalism will be able to overcome the various problems I have described. Perhaps some may feel that whatever the problems with physicalism it is still the most promising point of view from a naturalist perspective and that perspective is the only reasonable one. The objective of this section of the paper is to explore and recommend alternative avenues of enquiry, which will not be physicalist but will still be scientifically respectable.

I shall lead this grand tour under the banner title of emergent properties, although perhaps some of the views I shall discuss will not be thought to fit naturally under this heading. The discussion will be divided into three broad categories of emergent positions. By far the largest group of ontologies I shall consider will be bottom-up emergentist strategies; the second shall be what I deem top-down emergent; and the last and I shall argue the most promising will be discussed under the epithet of cross-ways emergent. The significance of each of the names will become apparent during the discussion.

Some Preliminary Considerations

Before I develop any account in detail, some general remarks need to be made about the concept of emergence itself. Emergence most broadly defined simply means the coming into being of something new: a property, law or relation or whatever. Defined in this vague manner, many, if not all, physicalists will accept some type of emergence. It is practically a truism to say that there are some properties that exist now which did not exist at one time or other. For example, the property of being a human is something which *emerged* through the process of evolution. But for a physicalist this should not be worrying since presumably he understands the process of evolution to be an entirely physical process, with an entirely physical explanation. Similarly, employing again the criterion that an emergent is a new property, one might claim that certain relational properties are in some sense emergent. If I place an object, A next to an object, B, it becomes an emergent property of A that it is next to B; again this is trivial and of no threat to physicalism. Since we are interested in emergence as providing an alternative to a physicalist ontology we shall need to differentiate these physicalistically speaking respectable notions of emergence from more robust forms emergence. That is to say, we shall be interested in defining emergence in a way that excludes physicalism.

Bottom-up Strategies

Bottom-up emergentists share many assumptions with non-reductive physicalists. They believe for example, that reality can be divided into various layers. Like physicalists, they place physics at the bottom, next chemistry, then biology, etc. all the way up to the human mind. In fact, the only area of profound disagreement concerns what the relations between the layers of reality actually are. I shall attempt to articulate this emergentist position by contrasting it with physicalism. I shall argue that given it shares so many assumptions with physicalism, it will not provide a compelling alternative ontology.

I have argued in section II that if a physicalist wishes to be a realist about apparently non-physical properties then those properties must be identical to some physical property or set of physical properties. Bottom-up emergentists, on other hand, can allow for more permissive relations between the layers of reality. The relevant contrast is neatly illustrated by considering the labours of God.

According to the physicalist all God has to do is write the laws of physics, set up the initial conditions of the universe, wind up the clock and let everything run. Everything follows as a consequence of the physical state of the universe. For a bottom-up emergentist God must be more industrious. In addition to creating all the physical facts and laws, he must also create non-physical facts, properties and laws which connect these non-physical states to each other and the base level physical states. So, God creates autonomous laws for higher-order disciplines and laws which link the various layers with each other.[1] Such a view then embraces what in Nagel-style accounts of theoretical reduction were thought to be bridge laws. However, the bottom-up emergentist makes them *sui generis*, as fundamental and irreducible as physics itself.

Some elaboration here is needed to explicate the kinds of laws and connections between properties and their parts that are emergent, in a metaphysically interesting sense, and those which remain physicalistically "kosher". James Van Cleve[2] has suggested that the relevant contrast between emergent and non-emergent strategies is that the connections between part-whole relations that are emergent are nomological, whereas the connnections between part-whole relations that are non-emergent are logical. That would seem congruent with my characterisation of physicalism. That is, the only way to bridge the layers of reality is by the use of identity statements. However, Van Cleve thinks this distinction applies across the board so any connection between parts and wholes which is only nomological is in some sense emergent. But this cannot be right for many part-whole property relations. To take a trivial example, the mass of a higher level systems is not *logically* dependent upon the mass of the subsystems. In the case of hydrogen, for example, the following relation is true:

[1] These creation stories are due to Tim Crane [1991]

$M_{hydrogen} < M_{electron} + M_{proton}$. The actual mass of hydrogen depends on the binding energy between the electron and proton; that is upon some law of nature governing the interaction of the two particles. But this is not emergence of the metaphysically interesting kind that a physicalistic is likely to deny. In fact, leaving Spinozistic rationalism to one side, it is questionable whether there really are any compositional principles that are logically necessary. Van Cleve suggests position but even this I think can be disputed. For any system in which the parts are held together by some force, if the forces were different i.e. the laws of nature were different, then the positions of the parts would be different too. So any philosophical position that denied such logically contingent compositional principles would be obviously false; and whatever the short comings of physicalism, this is not something which I think it can fairly be accused of. Granting such nomological compositional principles are acceptable for a physicalist, a second way of bridging the connection between the layers of reality becomes possible. The physicalist need simply claim that the property which is thought to be novel at some level of reality is actually a latent property of physical reality. Its apparent emergence, that is its existence at one level of reality and not another, is accounted for by some law of nature linking its latent state to an actual state. Such an ontology might be thought unattractive for a variety of reasons. The notion of a latent property may strike one as *ad hoc* and in need of explanation itself. Nevertheless, it is certainly a possibility open to a physicalist philosopher.

We may summarise this discussion up to this point by considering four principles, various combinations of which will yield different versions of physicalism, emergentism and more esoteric ontologies:

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[2] Van Cleve, J. [1990]

(1) The completeness of microphysics or, if you prefer, reductionism. The view that physics is, in principle, able to account for all physical events.

(2) Logical connection of properties. Higher-order non-physical properties are identical to or logically derivable from, the base level physical properties.

(3) Nomological relation of properties. Higher-order properties are nomologically dependent (either deterministically or indeterministically) on physical properties i.e. there exist some "bridging laws" between the domains of reality.

(4) The various layers of reality are entirely independent of one another.

All physicalists must maintain (1). They may do so with (2) alone or with (3) also, if the higher-order properties are construed as latent properties of the physical base. Combining (1) and (4) we obtain a version of parallelism. If we combine (1) with (3) then we can derive an emergent ontology, although not a very interesting one. If the law relating higher-order and base properties is deterministic, then a form of the epiphenomenalist/supervenience view discussed in section II emerges. If the connections are understood as indeterministic, then one still obtains a version of epiphenomenalism but the supervenience thesis no longer holds, since it is presumably possible to have changes in the mental (chance changes) without changes in the physical. Epiphenomenalism is I take it not a position that we should take seriously. Surely we want our higher-order properties to be causally efficacious. The interesting emergentist alternatives involve an affirmation of (3) and a denial of (1). That is to say, the doctrines we will be concerned with will suggest some sort of interactionist model which will allow us to understand how higher-order properties can affect lower-order properties. This is in broad outline the view I would attribute to such philosophers as Lloyd-Morgan[3], Alexander[4] and Broad[5]. (Hereafter I shall refer to the three philosophers as the classical emergentists.) It is this version of bottom-up ontology that I will now turn to investigate. I shall argue that although the classical emergentist position is conceptually sound, it does not provide an attractive alternative ontology to physicalism.

The Classical Emergentists: A Model of Emergent Causation

The classical emergentist divided the laws of nature into two types: "intra-ordinal", laws between entities or properties of the same layer of reality, and "trans-ordinal"[6], laws that cross layers, such as psychophysical laws which cite emergent properties. Physicalism as we said admits the existence of the former but denies the existence of the latter. Two questions might be asked about these trans-ordinal laws. Where do these non-physical properties or parts come from which feature in these laws and how are they causally efficacious?

As to the question of how to account for the emergence of these genuinely novel properties, the classical emergentists had no answer. It was something that simply had to be accepted with, to use Alexander's phrase, "natural piety". I think this should be no surprise. Any call for a further explanation of higher level properties can only be a plea for a reductionist[7] explanation. However, the classical emergentists did attempt to explain how emergent properties might be causally efficacious. I shall concentrate in the following paragraphs on Broad's account for it is the most fully developed.

Broad thought that notion of causal efficacy was intimately tied to that of force; for there to be causes there must be forces produced somewhere. He also noted that

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[3] Lloyd-Morgan [1923]
[4] Alexander, S. [1920]
[5] Broad, C.D. [1926]. I am particularly indebted to the works of Blitz [1990] and the Beckermann, A., Flohr, H. and Kim, J. [1992] volume for my understanding of the classical emergentists position
[6] Some emergentists share Crane's belief that trans-ordinal relations may be indeterministic.
[7] I mean reductionist in the broadest sense here.

Newton's second law of motion, F=ma, said nothing specifically about where those forces arose. Certainly it was true that in the case of gravity any body with a mass would generate an attractive force between it and other massive bodies, but Broad thought that this was the exception rather than the rule. Most forces, he thought, will be active only in particular circumstances, under special conditions. Electromagnetic forces, for instance, only operate for charged bodies, and not all bodies are charged. Broad thus thought it was reasonable to postulate what might be called "configurational forces", forces which like electromagnetic forces act when certain conditions are fulfilled. A configurational force though is very different from the notion of force used standardly in physics. All the supposedly *fundamental* forces that we know of in physics involve interactions between pairs of particles. A configurational force, though, is a force exerted when, as the name would suggest, certain configurations of particles are present. It is also important to realise that for the classical emergentists these forces are as fundamental as any physical force, since the properties requisite for their functioning will be emergent properties and emergent properties are, of course, not reducible to other properties. The idea is of course speculative; we do not know of any configurational forces. Nevertheless they will explain how the emergentist can account for what Kim calls "downward causation". That is, how higher-order properties can have a causal impact on lower-order properties. Certain particles, involving emergent properties, take on certain configurations and these configurations produce forces which are exerted on physical particles (that is some body with a mass). One should note though that although such an ontology breeches the completeness of physics, since certain physical events have partial non-physical causes, it does not mean that the forces of physics disappear or become false in certain situations. Rather, Broad believed that the overall influence, that is to say the *total* force, acting on some body would be the vector sum of all the physical, gravitational, electromagnetic, etc., forces plus the configurational forces. So we can make physical and non-physical causes peacefully cohabit, without overdetermination, on this view.

One possible line of objection to this view might be that if configurational forces exist at all they must be part of physics like (apparently) all other forces. So if such forces exist they simply show that higher-order disciplines, inasmuch as they involve forces, are dependent upon on or part of physics. Certainly this is true in a very broad sense, the fundamental laws of *mechanics*, i.e. Newton's laws, are necessary to make sense of configurational forces (or any force for that matter). Nevertheless, I take it the classical emergentists were making a claim about the *nature* of forces that might feature in a true theory of mechanics. The claim being that certain forces were not physical forces, that is interactions between pairs of fundamental particles but were induced by emergent properties.

I do not doubt that the classical emergentist notion of configurational forces outlined above is internally consistent. However, I think it is unlikely to provide the foundation for a strong alternative to physicalism. Contra, Brian McLaughlin,[8] I do not believe that this is because there has been a successful reduction of chemistry to quantum mechanics. The empirical issue is unresolved. We do not know, as I have argued at length, or even have good evidence for thinking that everything (or even chemistry for that matter) can be explained in terms of physics (whatever that turns out to be). *A fortiori*, we do not know as McLaughlin claims that: "it is a fact about the world that the fundamental forces which influence acceleration are all exerted at the

[8] McLaughlin, B. "The Rise and Fall of British Emergentism" in Beckermann, A., Flohr, H., Kim, J.(ed.) [1992] Of course, we actually have no adequate account of how the gravitational force might be exerted at the sub-atomic level, so McLaughlin claim is ore than a little presumptuous.

subatomic level"[9]. Equally we have no evidence, as McLaughlin rightly asserts, for the existence of anything like configurational forces. So why should we prefer one ontology over the other? If we understand the relation between chemistry and physics to involve a partial explanation of one theory in terms of another, rather than a reduction, then a related but weaker criticism can be levelled at emergentism. The emergentist needs to draw a clear boundary between the emergent and the non-emergent. He needs to know when configurational forces will apply and when they will not. But partial reductions are likely to force emergentists to redraw their boundaries. The emergentist is likely to have to make *ad hoc* adjustments in the light of scientific advances. The physicalist on the other hand has no such problems, for he expects one type of explanation to cover, in principle, everything. The fact that it does not at the moment he can always dismiss as merely a limitation of our present knowledge.

In fact, given that both views accept the primacy of physics, that is they place the science at the heart of their ontologies, physicalism is always going to look a better bet. Physicalism at least holds out the *possibility* of explaining the existence of higher-order properties which the emergentist just takes to be brute facts. In contrast, emergentism of this bottom-up sort seems to trade in mystery. Once you have accepted the primacy of physics, physicalism, it seems provides a richer research programme for science. Because of this appeal to the inexplicable, and the emergentists inevitable defensive retreat in light of certain scientific advances, I think one can hardly deny that physicalism is more appealing.

Contemporary discussions of emergentism have been content to stop at the classical emergentist position and conclude as I have done that although consistent, it

[9] ibid. p.91

seems a little strange and runs against the grain of scientific progress. I suppose this sort of thinking is meant to lend oblique support to physicalism as it removes a possible competitor ontology. But the real fault of bottom-up physicalism I think is that it shares too many principles in common with physicalism. A genuine alternative to physicalism I suggest will have to question the primacy of physics in its ontology. It will therefore require a more radical conceptual shift than is offered by bottom-up emergence. It is to these models I now turn.

Top-Down Emergence

What I have dubbed top-down emergence does provide such a radical alternative to reductionist thinking. Unfortunately, I think the position as it articulated and defended by its main (possibly only) advocate Joseph Margolis is also flawed and for similar reasons to bottom-up emergent strategies. I cannot possibly do justice to the Homeric qualities of Margolis's extensive and scholarly work here so I shall confine myself to recounting the basic plot line.

Margolis, like many who call themselves non-reductive physicalists, believes that there are certain functional properties, in particular mental states, which are not reducible to physical states. However, unlike the non-reductive physicalists discussed in section two he does not base his philosophy on supervenience or multiple-realisation arguments. Margolis thinks the positions put forward by philosophers such as Putnam, Davidson and Fodor render functional properties epiphenomenal or as he puts it "practicably eliminable". The only way to guarantee that functional properties are real, causally efficacious and non-reducible to physical entities is by making them "indissolubly complex, incarnate and emergent with regard to some physical or biological sector". This notion of functional properties as incarnate is what ground

Margolis's emergentism. Incarnate properties are supposed to have parts which are inseparably physical or biological but which do not reduce to these parts.

None of this marks out Margolis's position as particularly special. His notion of incarnate property does not sound that different from the emergent properties discussed by philosophers like Broad. What is distinctive about his philosophy then is not so much the ontological structure of the emergent properties but the properties he deems emergent and how, according to Margolis we come to know about them in the first place. Margolis claims that all artifactual objects, that is entities which have cultural or linguistical components, are emergent or incarnate in his sense. Michaelangelo's Pieta is a favourite example of his. The statue, Margolis argues, is more than the mere sum of its proper physical parts and that more comes from understanding the object as a work of art or culture.

Why is this top-down emergence? Margolis combines this notion of emergence with what he calls the "bifurcation of the sciences". That is to say, the methodology for discovering truths in the social science will have to be different from the natural sciences because we can only come to know these culturally incarnate properties self-reflexively, not empirically. The implications go deeper than just this bifurcation of methodology since the natural sciences themselves are products of human culture. As Margolis puts it: "Our understanding the natural world already *presupposes and entails* the reality of human culture; and that mode of *understanding presupposes and entails* our own understanding of our cultural world."[10] Hence, our understanding of incarnate properties must precede our understanding of any supposedly physical, property. In other words, our knowledge of emergent properties must proceed top-

[10] Margolis, J. [1986] p.374

down. That is, from our understanding of ourselves to what we take to be the natural world.[11]

I have no doubt that Margolis's work raises important issues. In a much more direct way, for example, it tackles some of the concerns I raised in section two. Namely, can we accommodate the normative or the culturally rule-governed in a physicalist ontology? If not, then how can we accommodate the discipline of physics itself on which physicalists base or hope to base their ontology? It is posing questions like this that makes Margolis doubt that traditional functionalist accounts are adequate to ensure the causal efficacy of just those functional properties. But just as I was ambivalent before about the possibility of a physicalist making sense of the normative, so I am now ambivalent about Margolis's programme. On the negative side, Margolis's position seems to teeter dangerously close to idealism. One in which we are trapped by our own language and culture into seeing the world in a particular way. Margolis denies this is what his position entails. Yet it is difficult to make sense of his claims about the cultural status of science and that emergent properties can only be studied top-down, if he is not offering something like idealism.

Equally though when he sounds less idealist and more explicit about his explanation of emergence, his position tends to come across as a version of classical emergentism. Consider the following passage: "...within some critical stage of development of the biologically grounded capacities of human beings – in a way that, quite frankly no-one understands – structures begin to emerge, that depend upon the incipient reflexive capacities of the species."[12] Again, it seems that like Alexander, Margolis thinks emergence is something you must accept with "natural piety". But if Margolis's emergentism steers towards more traditional accounts, I think it will come

[11] Note this an epistemological point about our grasp of the properties, not the properties themselves.

unstuck for similar reasons to the classical emergentists. The emergence of these reflexive properties is mysterious. Moreover, at the ontological level, Margolis retains it appears some sort of heirarchical notion of the levels of science; that is biological structures emerge from physical structures and social and cultural structures emerge from biological structures. But that is simply to acknowledge the general ontological structure of physicalism; except that a physicalist is able to fashion the structure into a more economical, less mysterious philosophy.

There are good intentions in Margolis's philosophy. His desire, for example, to make functional properties causally efficacious is certainly something I share. However, I am not convinced that Margolis's idea of an incarnate property and his consequent appeal to the bifurcation of the sciences is the right way to proceed. Even if we could make sense of his philosophy in a way that avoided both idealism and classical emergentism, I think we should reject his position. Firstly, because I think we have reason to be non-reductionist before we even encounter functional properties; and secondly because I do not find any reasonable argument for the bifurcation of the sciences in the text which does not already presuppose Margolis's notion of an incarnate property.

Cross-ways Emergence

Much of what I have said thus far has had little bearing on the arguments I offered against reductionism and physicalism in the preceding sections sections. I wish now to describe an ontology much more consonant with those arguments. In section one, I argued for the disunity of the sciences. Even sciences which were supposedly close to another, like physics and chemistry, or the disciplines which make up physics itself do not support strong reductive positions. In section three I argued that there was

[12] Ibid. p.374

an enormous conceptual gap between the quantum and the classical worlds. The conclusion to draw from these arguments it seems to me is that the sciences are not a hierarchically ordered system in which physics is king. Rather, the structure of the sciences is much more democratic. Different disciplines investigate different phenomena, some of these partially overlap and some do not. In short, I advocate a healthy pluralism. A pluralism which denies that any one science is more fundamental than any other. In effect, I am asserting that we may take the laws of the sciences whether they be quantum mechanics, quantum chemistry or neurobiology to hold true *within their specific domains*. We should not be surprised then that quantum theory holds for microphysics but modifications of the theory are required to understand larger scale events described by quantum chemistry and similarly that different theories are required to account for yet different phenomena not accounted for by these theories.

The general kind of view I am articulating has been most neatly expressed by Nancy Cartwright (John Dupré[13]for one subscribes to a similar metaphysical perspective).[14] It is once again fruitful to consider the labours of God:

"Metaphysical pluralism supposes that God...is very concerned about laws, so he writes each and every regularity that his universe will display. In this case St. Peter is left with the gargantuan task of arranging the initial properties which will allow all God's laws to be true together." [15]

One might wonder whether St. Peter is in fact redundant in Cartwright's creation story. Logic alone one might think would prevent the laws, if they were true laws, from contradicting each other. (The point will depend of course on what ontological

[13] Dupré, J. [1993]
[14] One might also interpret Crane[1991] as a version of this view, although I think he may be better classified as bottom-up emergentist.
[15] Cartwright, N. "Fundamentalism versus the Patchwork of Laws" in Papineau (ed.) [1996], p.324

status you think laws or regularities have. That is to say, if you think a particular law obtaining is a *fact* about the world, the claim that all laws are consistent is no more than the claim that all the facts of the world are consistent, which I take it is a truism.) Obviously, though, in some sense the laws of say physics cannot straightforwardly be taken to hold in all circumstances for all events. Parameters of acceptability must be defined for their operation; the laws must be hedged. Cartwright's own view seems to allow for this hedging by combining top-down idealist or social constructionist elements with her metaphysical pluralism. She, for example, converts Ian Hacking's[16] slogan for ontological commitment, "if you can spray them, they exist" into the much more idealist sounding "*when* you spray them, they exist". Laws of physics on this view only hold in the phenomenologically contrived settings of the laboratory; that is, they hold where they were designed to hold. So if one feels compelled, like Margolis, to eke out a special role for the human sciences this metaphysical view would be perfectly compatible with that stance. That said, I find the idealist overtones of Cartwright's philosophy less compelling than its overall pluralistic motivations. The pluralist, I suggest, should attempt to make sense of hedged laws without recourse to idealist or social constructionist arguments.

However, before I begin to deal with possible problems for the pluralist in detail, I should explain why I think cross-ways emergentism is superior to the other forms of emergence I have considered. Essentially there were two criticisms levelled at other emergentist strategies. First that the emergent properties seemed miraculous and second because physicalists and bottom-up emergentists alike had adopted a hierarchical structure, physicalism seemed to be able to account for partially successful reductions that the emergentists could only accommodate in an *ad hoc*

[16] Hacking, I. [1983]

manner. The latter objection falls away once we have rejected the primacy of physics. Some physicalists might think is insufficient by itself; the explanation of one theory in terms of another seems equally mysterious on a cross-ways emergent model as on other emergentist accounts. But the metaphysical pluralist may simply reiterate the point made in section I: reductions where successful are *partial and domain specific*. It is perfectly consonant with the idea of cross-ways emergence that there are some reductions in particular in the areas where theories overlap. What the pluralist does resist is the idea that there will be across the board reduction of the sciences to one theory; he has nothing to fear from successful local reductions.

Similarly, once you have removed the hierarchical structure the first objection, that emergent properties are mysterious, disappears too. A property only appears mysterious, if we suppose it arises from a more fundamental level of reality. But since all levels of reality have the same status, no property is any more mysterious than any other.

Of course, I have glossed the situation somewhat. There are obviously difficulties that must be overcome. We have already encountered one: how can we make sense of the idea that laws do not apply everywhere? Another important objection that might be raised is what one might call the compositional motivation for all bottom-up ontologies (whether physicalist or emergent). That is big things are made of little things (of course exactly how they are so made is up for debate; hence, the contrast between bottom-up emergence and physicalism) and little things are physical things.

Let us first consider the compositional argument, what will the metaphysical pluralist make of that? First we have to decide what is meant by "physical things". If all that is meant by thingness is spatio-temporal location then I think the pluralist can readily accept the argument. All concrete objects exist in space and time (this is surely what makes them concrete in the first place). But this should hardly lead us to the conclusion that all objects are fundamentally physical. The argument could be made more interesting if it were phrased in terms of properties rather than things. That is, if we removed all the physical properties of some entity (excluding the spatio-temporal properties), then that entity would cease to exist. The real question is in what sense are properties like spin, charm, charge, etc. parts of larger wholes and what sense does it make to talk of their removal.

I think the first thing to note, is that a lot of these properties do not even feature at the non-physical levels of reality so it is not clear what removing properties like, say spin might mean in this context. But that cannot be an adequate general explanation for the pluralist, since there are some clear cut cases of properties which do feature at differing levels of reality, in particular dynamical and kinematical properties. For example, as I move my hands erratically across the keyboard in order to type this thesis then presumably that is concurrent with the movement of physical parts of my hand (say the atoms of my index finger). The deep metaphysical question is what is the relation between these sets of movements.

I think the pluralist can admit several different types of answer. Perhaps most radically he can deny there is a connection between the two; there is a coincidental movement of hands and hand parts. Given what was said about the relation of quantum phenomena to macroscopic phenomena, I think this is less unreasonable than it might at first sound. The sub-atomic particles that make up my hand can *qua* sub-atomic only be described accurately by quantum theory but how that theory can be related to hands *qua* hands is a mystery. Scott Sturgeon[17] has some interesting

[17] Sturgeon, S. [forthcoming]

suggestions about how we might consider hand and hand-parts to support a different range of counterfactuals possibilities, although in our world they are coincident. So we can for example, say that as a matter of fact hand movements are constituted by hand-part movements but atomic-hand parts do not *cause* hand movements (because of the partial failure of counter-factual dependence). The idea is certainly interesting but it is not clear why we should think the world is like this. I would prefer to develop an account we might have more reason to think is true.

We can I think achieve a better understanding of these issues if we turn to quantum theory. Given that quantum theory seems to presuppose the existence of non-quantum phenomena to account for measurement type interactions, it seems to provide a neat case study of the general perspective a pluralist should take. (That is it seems to address the second problem, how we understand the fact that the laws of physics do not apply everywhere, as well.) Small things are accurately represented by quantum theory. But if we attempt to apply that description to large objects, notably measuring devices, our theory fails to recover our intuitive conception of the macroscopic image. Quantum measurement is best described as interaction between a quantum and a classical system. In other words, it requires us to hedge the laws of quantum mechanics so that they apply to the quantum system but not to the measuring apparatus. How this affects the general material composition of things is unclear. For Cartwright it would appear that we should not think of the hand as composed of sub-atomic constituents. But I do not think we need make this claim, we simply say that given the way the systems are, the constituents of different things are parts of objects which have different properties and therefore fall under different laws. That is to say, quite simply there is more to hands than atomic hand parts. One might think this would again imply the sort of mysterious physical to non-physical connections which force us to accept "with natural piety" the emergence of the non-physical. But that of course would be contrary to the pluralist's outlook; he denies there is anything special about the kinematical properties in the first place. So as I said earlier worries about the "spookiness" of property emergence do not arise, or if they do, they do so for all properties equally.

I have no general theory about how exactly it is that laws are hedged but I simply make the observation that this how the world looks as science describes it; different laws applying to lots of different situations. In particular, it is the way that quantum mechanics, our most fundamental theory, represents the world; that is, as requiring the intervention of something other than elements solely described by quantum theory in order to make sense of its results. In light of the relation of the quantum domain to the macroscopic domain, the hedging of laws is something which I find I am forced to accept with "natural piety".

The account of metaphysical pluralism given above is obviously incomplete. There are questions that remain unanswered and room for disagreement between the advocates of the general position. This I just take to be the sign of a decent metaphysical perspective. What is certain though is it is a general view which is more consonant with the way the sciences operate now and what the various sciences tell us about the world. In that sense, I take pluralism to be the truly naturalistic perspective. The metaphysical pluralist directly observes the disunity of contemporary science (and indeed common sense views of the world) and he claims reality should be described in broadly those terms. Different theories, laws and entities should apply in different cases; and indeed, this is of course precisely what scientists and ordinary individuals do.

CONCLUSION

I have shown that physicalism is not well supported by our current science. Some physicalist may be unperturbed by that conclusion. They may hope to justify their favoured ontological position by *a priori* methods. If so, I can only wish them luck. No doubt they will find good company amongst similarly high-minded dualist, idealists and a great many other "-ists" who seek to justify their conclusions by the same method. However, for those who prefer to muddy their hands in the empirical side of things, I suggest my conclusions should have serious repercussions.

We have seen in section I that science is neither unified nor shows signs of being unified; that even in what look like cases of reduction, there always appears to be some explanatory residue, some slack that has to be taken up by some other theory. We have also seen that physicalists are committed to explaining this lack of unity as *merely* an epistemological fact. That is to say, given a commitment to physicalism, one should expect reductions *in principle.* Such a position is of course entirely consistent. It is perfectly possible that the apparent disunity of the sciences and scientific explanations is just due to our lack of knowledge. However, I do not think this is very satisfactory. As someone approaching metaphysical questions from a broadly naturalistic basis, I do not think we should be so schizophrenic about our epistemological and ontological attitudes. If the world looks disordered, I say it is disordered.

I have, though, gone beyond even these considerations and shown that even where a physicalist would expect to find friends, within the discipline of physics itself, there lurk ghosts of non-reductivism. Indeed, there is something quite ironic about modern day *physical*ists lack of engagement or indeed even acknowledgement of the conceptual problems *contemporary physics* causes for their doctrine. Of course, these difficulties will never refute physicalism. Metaphysical doctrines are always vague and plastic enough to accommodate practically any scientific or empirical result. Die-hard physicalists will no doubt interpret my arguments as presenting problems that the programme of physicalism should, and will, overcome in time. But in light of my arguments, I do not see why we should be continued to be interested in physicalism. I do not see why we should attempt to patch it up to make it compatible with contemporary physics. Rather, I think the arguments of the first three sections should encourage us to turn away from physicalism.

Eschewing physicalism entails for a number of philosophers a wholesale rejection of the very general types of metaphysical question physicalists address. Tim Crane and D. H. Mellor[1] argued there is no question of physicalism; that is to say, the concerns of physicalism cannot answer the interesting problems in the philosophy of mind: in particular the problem of intentionality. Lynne Rudder-Baker[2] in a similar vein has counselled us to drop metaphysical questions and concentrate on our explanatory practices instead. I have some sympathy for such metaphysical deflationism. Certainly if it is meant to be taken as methodological advice, I whole-heartedly embrace it. That is we should proceed in a naturalist vein, starting with our scientific knowledge and proceeding to our metaphysical view. However, if it is meant in a more wide-ranging sense, implying that the sorts of question physicalists are concerned with should not interest us, I cannot concur. It might be case that other questions are more tractable and of more immediate interest but that should not mean general metaphysical questions should not be pursued. A rejection of physicalism should simply encourage us to look for a metaphysics which is better justified by our scientific knowledge. I think, as I argued in section IV, that some sort of pluralism

[1] Crane, T. and Mellor, D.H. [1990]

provides the best alternative. But that presents us with many problems both in terms of compositional relations and understanding how the various laws of science can all peacefully co-habit without implying reduction. Pluralists need to provide a deeper more general ontological perspective than I have been able to sketch here. They need to address the problems of laws, causation and theoretical relations; and they must address these questions by detailed analysis of actual scientific theories and practice. Only when this done will we have a vigorous and viable alternative to physicalism. The road ahead for the pluralist is difficult. But as long as he sticks to the well-beaten track marked out by the empirical sciences, at least he will know he is travelling in the right direction.

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[2] Rudder-Baker, L. "Metaphysics and Mental Causation" in Heil and Mele [1996]

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CONCLUSION

I have argued at length that physicalism is not well supported by science. Some physicalists may remain sanguine about this conclusion. They may wish to seek ultimate justification for their position in *a priori* arguments. That is to say, some physicalist may believe that the real strenghths and motivations for physicalism