The Rise of Physicalism

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"No one could seriously, rationally suppose that the existence of antibiotics or electric lights or rockets to the moon disproves . . . mind-body dualism." Stephen R.L. Clark, 1996.

1. Introduction

In this paper I want to discuss the way in which physical science has come to claim a particular kind of hegemony over other subjects in the second half of this century. This claim to hegemony is generally known by the name of "physicalism". In this paper I shall try to understand why this doctrine has come to prominence in recent decades. By placing this doctrine in a historical context, we will be better able to appreciate its strengths and weaknesses.

As a preliminary, note that contemporary physicalism is an ontological rather than a methodological doctrine. It claims that everything (1) is physically constituted, not that everything should be studied by the methods used in physical science. This emphasis on ontology rather than methodology marks a striking contrast with the "unity of science" doctrines prevalent among logical positivists in the first half of the century {, and discussed by Thomas Uebel in the previous chapter of this book}. The logical positivists were much exercised by the question of whether the different branches of science, from physics to psychology, should all use the same method of controlled observation and systematic generalization. They paid little or no attention to the question of whether everything is made of the same physical stuff.

By contrast, physicalism, as it is understood today, has no direct methodological implications. Some physicalists uphold the view that all sciences should use the "positivist" methods of observation and generalization. But as many would deny this. You can be a physicalist about biology, say, and yet deny that biology is concerned with laws, or a physicalist about sociology, and yet insist that sociology should use the method of empathetic verstehen rather than third-person observation.

This methodological liberalism goes with the fact that the ontological claims of fin-de-siecle physicalism are often carefully nuanced. If physicalism simply meant type-type physical reduction, of the kind classically characterized in Ernst Nagel's The Structure of Science (1961), then methodological unity of science would arguably follow, in principle at least, from physicalism. But physicalism today clothes itself in various subtler shades. We have physical supervenience, physical realization, token-token physical identity, and so on. These more sophisticated doctrines leave plenty of room for different sciences to be studied in different ways.

But I am already drifting away from the main subject of this paper. My concern here is not to distinguish the different species of physicalism, though I shall touch on this in passing later, but to try to understand the reasons for physicalism of any kind. Why have so many analytic philosophers in the second half of the twentieth century suddenly become persuaded that everything is physical?

2. Fashions and Arguments

It certainly wasn't always so. Perhaps the easiest way to highlight the recent shift in thinking about physicalism is to recall a once-heated mid-century debate about the status of psychological explanation. In contemporary terms, this debate was about the scientificity of "folk-psychology". On the one side were those, like Carl Hempel and A.J. Ayer, who argued that "reasons are causes". By this they meant that psychological explanations are underpinned by empirical generalizations, implicit in everyday thought, which link psychological states like belief and desire to subsequent behaviour. Opposed to Hempel and Ayer were thinkers like William Dray, and Peter Winch, who argued that the links between reason and action are "logical" or "meaningful", not empirical. (Hempel, 1942; Ayer, 1969; Dray, 1957; Winch, 1958.)

In one respect this old debate is still up-to-date. It concerned the question of whether everyday psychological thinking is suitable for incorporation in a scientific psychology -- whether folk psychology is a "proto-science", as it is sometimes put -- and this question is still very much a live issue. But at another level the old debate is now quite outmoded. This is because the participants in the old debate showed little or no interest in the question of how the mind relates to the brain. They wanted to know whether there are testable, empirical laws linking mental states to behaviour. But they seemed to see no connection between this issue and the question of the relation of mental states to brain states. {In one perfectly good sense, they were addressing the issue of whether psychology is part of "the proper ambition of science". But for them this meant the question of whether categories like belief and desire conform to regular patterns at the psychological level, not the further question of how the categories of belief and desire relate to occurrences at the physical level.}

Nowadays, by contrast, everybody has a view on this latter question. Indeed nearly all analytic philosophers in this area, including those who side with Dray and Winch against the scientificity of common-sense psychology, now accept that the mind is in some way constitutively connected with the brain. (Thus consider Donald Davidson. He is the modern champion of the Dray-Winch view that the explanatory links between reason and action are a sui generis matter of rational understanding, not scientific law. Yet he made his name by arguing that, even so, "reasons <u>are</u> causes". In effect, his contribution was to show how the Dray-Winch methodological denial of psychological laws could be combined with a physicalist commitment to mind-brain <u>constitution</u>. Davidson, 1963.)

This transformation of the old "reasons and causes" debate happened very quickly. Until the 1950s the issue was purely about lawlike patterns. The issue of mind-brain identity was not on the agenda. Then suddenly, in the 1950s and 1960s, a whole stream of philosophers came out in favour of physicalism. First there were Herbert Feigl and the Australian central state materialists, and they were followed in short order by Donald Davidson, David Lewis, and functional state theorists like Hilary Putnam. While the old "reasons and causes" issue continued to be debated, from now on this debate took place within the larger context of physicalist assumptions about the mind-brain relation. (Feigl, 1958; Place, 1956; Smart, 1959; Armstrong, 1968; Davidson, 1963, 1970; Lewis, 1966; Putnam, 1960.)

Why exactly did physicalism come to prominence in this way in the 1950s and 1960s? Those antipathetic to physicalism sometimes like to suggest that the emergence of physicalism is essentially a matter of fashion. On this view, the rise of physicalism testifies to nothing except the increasing prestige of physical science in the modern

Weltaunschang. We have become dazzled by the gleaming status of the physical sciences, so the thought goes, and so foolishly try to make our philosophy in its image. (Thus Stephen Clark, in the sentence immediately following the quote at the begining of this paper: "But such achievements [antibiotics, lights, rockets] lend authority to 'science', and science . . . is linked in the public mind with atheistic materialism.")

I think this attitude quite underestimates the significance of contemporary physicalism. What is more, it doesn't really answer the question about physicalism's sudden emergence. It is not as if the prestige of physics suddenly had a big boost in the middle of the twentieth century. I would say that physics has been pretty prestigious for about 300 years, with occasional ups and downs. Yet the philosophical physicalism we are concerned with is a distinctively late twentieth-century phenomenon.

In this paper want to offer a different suggestion. My explanation for the rise of physicalism will be that it follows from an argument, or rather a family of arguments, the crucial premise of which was not available, at least to philosophers, until relatively recently. This is because this crucial premise is an empirical claim, and the evidence for it has only become clear-cut over the last century. Prior to that, this premise was not upheld by scientific theory, and so was unavailable as a basis for philosophical argument.

If this explanation is right, it casts a different light on physicalist views. Physicalism has been pressed on philosophers, not by fad or fashion, but by a newly available line of argument. In saying this, I do not want to suggest that the argument for physicalism is uncontroversial, or that the crucial premise I shall focus on is incontrovertible. But I do want to urge that physicalism deserves to be taken seriously, and that those who want to oppose it have an obligation to show where the argument in its favour goes wrong.

Of course, there are those, like Stephen Clark, who think that "no one could seriously, rationally suppose" that empirical considerations could possibly yield a disproof of mind-body dualism. I shall not explicitly engage with this attitude in what follows, but shall merely invite those who find it plausible to consider the matter again at the end of this paper. Of course, to repeat a point just made, the empirically-based arguments in favour of physicalism are not incontestable. But, even so, it scarcely follows that you have to be unserious or irrational to suppose that they in fact succeed in establishing physicalism. Indeed it is my contention in this paper that a number of the most influential of late twentieth-century analytic philosophers have supposed just that.

3. Phenomenalism and Physicalism

Before I give my own explanation for the rise of physicalism, in terms of the new availability of an empirical argument, let me quickly consider an alternative possible explanation, namely, that the rise of physicalism is simply the other side of the demise of phenomenalism.

No doubt there is something to this thought. Phenomenalism was the dominant metaphysical view among logical positivists and other scientifically-minded analytic philosophers in the first half of this century. And there certainly isn't much room within phenomenalism to be a physicalist . If you think that everything, including

physical stuff, is logically constituted out of mental items like sense data, then you would seem already to have ruled out the thought that mental items are in turn constituted by physical items.

Even so, I don't think this is a sufficient explanation for the rise of physicalism. For one thing, the rejection of phenomenalism doesn't yet explain the acceptance of physicalism. After all, you can deny phenomenalism without embracing physicalism. Indeed a significant number of contemporary philosophers do exactly that. These philosophers reject phenomenalism, but see no reason to privilege the physical among the different categories of things that exist, and so do not agree that everything is physically constituted.

Apart from this, there is the question of why phenomenalism died in the first place. This is of course a big subject, and any full answer would have to mention Wittgenstein's private language argument and Sellars' attack on givens. But I suspect that just as influential as these was the empirical argument for physicalism I am about to discuss. It is a simple argument, from uncomplicated empirical premises, and phenomenalists would have been as well-placed to appreciate its force as anybody else. If there is anything to this suggestion, then it wasn't so much that physicalism happened to fill the space created when phenomenalism left the stage. Rather the argument for physicalism was itself partially responsible for the overthrow of phenomenalism.

It is high time I described this empirically-based argument for physicalism. It is simple enough in outline. The crucial empirical premise is the completeness of physics, by which I mean that all physical effects are due to physical causes. And the argument is then simply that, if all physical effects are due to physical causes, then anything that has a physical effect must itself be physical.

The important point, for our purposes, is that the premise here, the completeness of physics, is a doctrine with a history. It was not always widely accepted. In particular, it was only after some decades of the present century that it became part of scientifically educated common sense. This in turn was because evidence favouring this thesis did not start to emerge until the mid-nineteenth century, and did not become generally persuasive until much later. Once the thesis was widely accepted, however, its implications were obvious, and nearly all philosophers with some acquaintance with modern physical science became physicalists.

In the rest of this paper I shall proceed as follows. First, in the next two sections, I shall get a bit clearer about what the completeness of physics says, and how different philosophers have used it to argue for physicalism. In the following sections I shall then examine the history of this thesis, and in particular the reasons why it has come to be widely accepted nowadays, even though it wasn't always.

4. The Completeness of Physics and the Argument for Physicalism

Let me start by formulating a more precise version of the thesis of the completeness of physics:

All physical effects are fully determined (2) by law by a purely physical prior history.

Note first that this thesis does not yet assert physicalism. Physicalism is the doctrine that everything, including prima facie non-physical stuff, is physical. But the

completeness of physics doesn't itself say anything about non-physical things. It is purely a doctrine about the structure of the physical realm. It says that, if you start with some physical effect, then you will never have to leave the realm of the physical to find a fully sufficent cause for that effect. (3)

If we want to get from the completeness of physics itself to the imperialist phsyicalist conclusion that everything is physical, we need an argument. However, the general shape of such an argument is not hard to find. As I put it in the last section, if the completeness of physics is right, and all physical effects are due to physical causes, then anything that has a physical effect must itself be physical. Or, to put it the other way round, if the completeness of physics is right, then there is no room left for anything non-physical to make a difference to physical effects, so anything that does make such a difference must itself be physical.

Some version of this line of thought underlies the writings of all the philosophers who started arguing for physicalism in the 1950s and 1960s. Thus, for example, consider Smart's thought that we should identify mental states with brain states, for otherwise those mental states would be "nomological danglers" which play no role in the explanation of behaviour. Similarly, reflect on Armstrong's and Lewis's argument that, since mental states are picked out by their causal roles, including their roles as causes of behaviour, and since we know that physical states play these roles, mental states must be identical with those physical states. Or, again, consider Davidson's argument that, since the only laws governing behaviour are those connecting behaviour with physical antecedents, mental events can only be causes of behaviour if they are identical with those physical antecedents. (4)

There is much to say about these arguments, and I shall say some of it below. But the point I want to make here is that none of these arguments would seem even slightly plausible without the assumption of the completeness of physics. To see this, imagine that the completeness of physics were not true, and that some physical effects (the movements of arms, perhaps, or the firings of the motor neurones which instigate those movements) were not determined by law by prior physical causes at all, but by sui generis non-physical mental causes, such as decisions, say, or exercises of will, or perhaps just pains. Then (1) contra Smart, mental states wouldn't be "nomological danglers", but directly efficacious in the production of behaviour; (2) contra Armstrong and Lewis, it wouldn't be necessarily be physical states which played the causal roles by which we pick out mental states, but quite possibly the sui generis mental states themselves; and (3) contra Davidson, it wouldn't be true that the only laws governing behaviour are those connecting behaviour with physical antecedents, since there would also be laws connecting behaviour with mental antecedents.

5. Comments on the Causal Argument for Physicalism

The interesting historical question, to which I shall turn shortly, is why these completeness-of-physics-based arguments started appearing when they did. But first it will be useful to clear away a bit of philosophical undergrowth. Those readers who are more interested in history than philosophical niceties may wish to skip ahead to the next section.

There are significant differences between the completeness-based arguments put forward by Smart, Armstrong, Lewis, and Davidson and other physicalist writers. However, rather than getting entangled in detailed comparisons, let us focus on one

canonical form of this argument, which I shall call the "causal argument". (Crane, 1995, Sturgeon, 1998.) This will enable me to make some general structural points.

Premise 1 (the completeness of physics):

All physical effects are fully determined by law by a purely physical prior history.

Premise 2 (causal influence):

All mental occurrences have physical effects. (5)

Premise 3 (no universal overdetermination):

The physical effects of mental causes are not all overdetermined.

Conclusion:

Mental occurrences must be identical with physical occurrences.

Some comments:

(i) The ontology of causes. The force of this causal argument is extremely sensitive to how you think about causation. If, like Donald Davidson (1980), you think of the relata of causation as events, and think of events in turn as basic particulars, then the argument concludes only that mental and physical descriptions pick out the same events, not that there is any constitutive relationship between mental and physical properties. On the other hand, if you think of the relata of causation as instantiations of properties, or more generally as facts (Mellor, 1995), then the argument promises to establish the stronger conclusion that mental properties are identical with physical ones. Since the stronger version is the more interesting, and since facts in any case seem to me the better candidates for the relata of causation, I shall read the argument in this way henceforth.

(ii) Accepting overdetermination. The causal argument seems pretty clearly valid . (6) So those who reject the conclusion must reject one of the three premises. All three moves are found in the literature. The status of premise 1, the completeness of physics, will occupy most of what follows. This leaves premises 2 and 3. Let us first consider rejecting premise 3, the premise of no universal overdetermination.

To reject this presmise is to accept that the physical effects of mental causes are always overdetermined. This "belt and braces" view is defended by Gabriel Segal and Elliott Sober (1991) and D.H. Mellor (1995, pp. 103-5). In response to the worry that this view seems to imply that your arm would still have moved even if you hadn't felt a pain (because your C-fibres would still have fired, say), these philosophers argue that the distinct mental and physical causes may themselves be strongly counterfactually dependent. Still, this then raises the question of why such causes should always be so counterfactually dependent, if they are genuinely distinct. Possible causal mechanisms underpinning this dependence can be imagined, but there seems to me no good reason to believe in them.

(iii) Epiphenomenalism and Pre-Established Harmony. What about premise 2? The possibility of denying this premise is familiar enough, under the guise of "epiphenomenalism" or "pre-established harmony". If you are prepared to accept that mental states do not have physical effects, and are indeed "nomological danglers"

with respect to the causation of behaviour, then the above argument for physicalism will not move you, for you will not embrace its second premise. I leave it to readers to decide whether this denial of the efficacy of the mental is a price worth paying to avoid physicalism. (7)

While we are on this point, it is worth noting that one of the most popular versions of physicalism, namely, functionalism, is arguably a closet version of epiphenomenalism. By functionalism I mean the view that identifies a mental state with a "second-order state", that is, the state-of-having-some-state-which-plays-a-certain-role, rather than with the first-order physical state which actually plays that role. Since the second-order mental state cannot be identified with the first-order physical state (rather, it is "realized" by it), it is not clear that it can be deemed to cause what that first-order state causes, such as items of behaviour. So functionalism threatens the epiphenomenalist denial of premise 2, the claim that mental states have physical effects.

The recognition of this difficulty has put functionalism under some pressure recently. One option is to turn away from functionalism, and insist that mental states are first-order states after all, and so strictly identical with physical states. (Lewis, 1980.) This option in effect upholds a strong version of premise 2, and allows it to argue for the full identity of mental with physical properties.

Another option is to read "causation" generously in premise 2, and assume only that mental states cause physical effects in the weaker sense that either they cause them directly or they have realizers that cause them directly. That is, we might allow a state to "cause" in virtue of having a realizer which causes. If we do this, then the causal argument will no longer require us to identify conscious states with strictly physical states, but it will still give us an argument for identifying them with second-order states which are physically realized. For unless we suppose this identification, we won't even be able to satisfy the weaker version of premise 2, and will be in danger once more of having our behavioural effects overdetermined by two ontologically quite unrelated causes. (8)

(iv) Non-causal realms. This discussion of epiphenomenalism shows that the causal argument for physicalism only applies to non-physical occurrences that do have physical effects. Without premise 2, there is no argument, since it is only on the assumption that the non-physical occurrences in question are not "causal danglers" that we need to identify them with something physical.

This shows that there are limits to this form of argument for physicalism. At the beginning of this paper I characterised physicalism as the doctine that "everything is physically consituted". However, this ambitious claim outstrips anything that can be delivered by the causal argument. For the causal argument has no grip on putative realms of reality that are outside the causal realm altogether, and so a fortiori don't have physical effects. I particularly have in mind here the realms of mathematics, and of moral and other values. While some philosophers have supposed that mathematical or moral facts do have physical effects, this is not the normal way to think about them. And, if we do deny that moral or mathematical facts have physical effects, then our causal argument will provide no basis for identifying them with physical facts. (9)

I myself think that this limitation to the causal argument constitutes a genuine boundary to the proper ambitions of physicalism. I think that physicalism is best

formulated, not as the claim that everything is physical, but as the significantly weaker claim that everything which interacts causally with the physical world is physical. This leaves it open that there may be non-causal realms of reality which are not physically constituted, such as the realm of moral worth, or of beauty, or of mathematical objects.

Of course, there may be other problems with such non-physical realms. For example, it is not clear how we may come by knowledge of such realms, if they can have no physical effects on our sense organs. But these further arguments are by no means clear-cut, and there is no special reason why they should be accepted by everybody who accepts the causal argument. Because of this, I shall use "physicalism" in the rest of this paper specifically for the doctrine that everything with causal powers is physical, whatever may be true of non-causal realms.

(v) What is "Physics"? In a moment I shall turn to the history of the completeness of physics. But first we need to address a terminological issue, one that may have been worrying readers for some time. How exactly is "physics" to be understood in this context of the causal argument? An awkward dilemma may seem to face anyone trying to defend the crucial first premise, the completeness of physics. If we take "physics" to mean the subject matter currently studied in departments of physics, discussed in physics journals, and so on, then it seem pretty obvious that physics is not complete. The track record of attempts to list all the fundamental forces and particles responsible for physical effects is not good, and it seems highly likely that future physics will identify new categories of physical cause. On the other hand, if we mean by "physics" the subject matter of such future scientific theories, then we seem to be in no position to assess its completeness, since we don't yet know what it is.

This difficulty is more apparent than real. If you want to use the causal argument, it isn't crucial that you know exactly what a complete physics would include. Much more important is to know what it won't include. Suppose, for example, that you have an initial idea of what you mean by "mental" (the sentient, say, or the intentional, or perhaps just whatever events occur specifically in the heads of intelligent beings). And suppose now that you understand "physical" as simply meaning "non-mental", that is, as standing for those properties which can be identified without using this specifically mental terminology. Then, provided we can be confident that the "physical" in this sense is complete, that is, that every non-mental effect is fully determined by non-mental antecedents, then we can conclude that all mental states must be identical with something non-mental (otherwise mental states couldn't have non-mental effects). This understanding of "physical" as "non-mental" might seem a lot weaker than most pre-theoretical understandings, but note that it is just what we need for philosophical purposes, since it still generates the worthwhile conclusion that the mental must be identical with the non-mental -- given, that is, that we are entitled to assume that the non-mental is complete.

The same point applies if we want to apply the causal argument to chemical, or biological, or economic states. As long as we can be confident that all non-chemical effects are fully caused by non-chemical (non-biological/non-economic . .) states, then we can conclude that all chemical (biological/economic . . .) states must be identical with something non-chemical (non-biological/non-economic . . .).

We might not know enough about physics to know exactly what physics does include. But as long as we are confident that it excludes such-and-such special categories, then we can use the causal argument to conclude that these special categories are in fact identical with other kinds. I shall suppose this indirect understanding of "physics" in what follows: it should simply be understood as that set of properties which can be specified without appeal to whichever special vocabularies (mental, biological, . .) we are interested in. Correspondingly, the completeness of physics will be the doctrine that such non-special effects are always fully accounted for by non-special causes. (Cf. Papineau and Spurrett, 1999.)

6. Descartes and Leibniz

Let us now concentrate on the history of the completeness of physics. The important question, as we have just seen, is whether any non-special effects are produced by sui generis special causes. True, the exact content of this question will be relative to which special categories we are interested in, for the reasons just explained. Still, we can take it for the moment that we are interested in a relatively strong version of the completeness of physics, and in particular one which would rule out sui generis mental causes, let alone biological, economic, social, or other even more special causes. So let us focus for now on the question of whether there are any non-mental effects which cannot be accounted for without reference to sui generis mental causes.

When I first became interested in the causal argument a few years ago, I recognized that there were many points where it could be queried. However, I assumed that the completeness premise itself was quite uncontentious. Surely, I thought, everybody agrees that the movements of matter, such as the movements of molecules in your arm, can in principle always be fully accounted for in terms of prior physical causes, such as physical activity in your nerves, which in turn is due to physical activity in your brain, . . . and so on.

To my surprise, I discovered that some people didn't agree. They didn't see why some physical occurrences, in our brains perhaps, shouldn't have irreducibly mental causes. My first response, when presented with this thought, was to attribute it to an insufficient education in the physical sciences. Sometimes I went so far as to communicate this diagnosis to those who disagreed with me. However, when they then asked me, not unreasonably, to show them where the completeness of physics is written down in the physics textbooks, I found myself in some embarrassment. Once I was forced to defend it, I realized that the completeness of physics is by no means self-evident. Indeed further reading has led me to realize, far from being self-evident, it is an issue on which the post-Galilean scientific tradition has changed its mind several times.

My original thought was that the completeness of physics would follow from the fact that physics can be formulated in terms of conservation laws. If the laws of mechanics tell us that important physical quantities are conserved whatever happens, then doesn't it follow that the later states of physical systems are always fully determined by their earlier physical states?

Not necessarily. It depends on what conservation laws you are committed to. Consider Descartes' mechanics. This incorporated the conservation of what Descartes called "quantity of motion", by which he meant mass times speed. That is, Descartes held that the total mass times speed of any collection of bodies is guaranteed to remain

constant, whatever happens to them. However, this alone does not guarantee that physics is complete. In particular, it does not rule out the possibility of physical effects that are due to irreducibly mental causes.

This is because Descartes' quantity of motion is a non-directional (scalar) quantity, defined in terms of speed, as opposed to the directional (vectorial) Newtonian notion of linear momentum, defined in terms of velocity. Because of this, the direction of a body's motion can be altered without altering its quantity of motion. As Roger Woolhouse explains the point, in an excellent discussion of the relevance of seventeenth-century mechanics to the mind-brain issue (1985), a car rounding a corner at constant speed conserves its "quantity of motion", but not its momentum.

This creates room for sui generis mental causes to alter the direction of a body's motion without violating Descartes' conservation principle. Descartes' conservation principle means does mean that, if one physical body starts going faster, this must be due to another physical body going slower. But his principle doesn't require that, if a physical body changes direction, this need result from any other physical body changing direction. Even if the change of direction results from an irreducibly mental cause, the quantity of motion of the moving body remains constant.

According to Leibniz, Descartes exploited this loophole to explain how the mind could affect the brain. As Leibniz tells the story, Descartes believed that the mind nudges moving particles of matter in the pineal gland, causing them to swerve without losing speed, like the car going round the corner. This then explained how the mind could affact the brain without violating the conservation of "quantity of motion". (Leibniz, 1898 [1696], p 327.)

Now, there is little evidence that Descartes actually saw things this way, nor indeed that he was particularly worried about how the laws of physics can be squared with mind-brain interaction. Still, whatever the truth of Lebniz's account of Cartesian theory, his next point deserves our attention. For Leibniz proceeds from his analysis of Descartes to the first-order assertion that the correct conservation laws, unlike Descartes' conservation of quantity of motion, cannot in fact be squared with mind-body interaction.

Leibniz's conservation laws were in fact a great improvement on Descartes'. In place of Descartes' conservation of "quantity of motion", Leibniz upheld both the conservation of linear momentum and the conservation of kinetic energy. These two laws led him to the correct analysis of impacts between moving bodies, a topic on which Descartes had gone badly astray. And, in connection with our present topic, they persuaded him that there is no room whatsoever for mental activity to influence motion of matter.

In effect, the conservation of linear momentum and of kinetic energy together squeeze the mind out of the class of events that cause changes in motion. Leibniz's two conservation laws, plus the standard seventeenth-century assumption of no physical action at a distance, are themselves sufficient to fix the evolution of all physical processes. The conservation of momentum requires the preservation of the same total amount of quantity of motion in any given direction, thus precluding any possibility of mental nudges altering the direction of moving physical particles. Moreover, the conservation of energy, when added to the conservation of momentum, fully fixes the speed and direction of impacting physical particles after the collide. (10) So there is no

room for anything else, and in particular for anything mental, to make any difference to the motions of physical particles, if Leibniz's two conservation laws are to be respected.

We can simplify the essential point at issue here by noting that Leibniz's conservation laws, unlike Descartes', ensure physical determininism. They imply that the physical states of any system of bodies at one time fix their state at any later time. Physical determinism in this sense is certainly sufficient for the completeness of physics, even if the possibility of quantum-mechanical indeterminism means it is not necessary (cf. footnote 2 above). So Leibniz's dynamics, unlike Descartes', makes it impossible for anything except the physical to make a difference to anything physical.

Leibniz was fully aware of the implications of his dynamical theories for mind-body interaction. (Cf. Woolhouse, op. cit.) However he did not infer mind-brain identity from his commitment to the completeness of physics. Instead he adopted the doctine of pre-established harmony, according to which the mental and physical realms are each causally closed, but pre-arranged by the divine will to march in step in such a way as to display the standard mind-brain correlations. In terms of the canonical causal argument laid out in section 5 above, Leibniz is here denying premise 2, about the causal influence of mind on matter. He avoids identifying mental causes with physical causes, in the face of the completeness of physics, by denying that mental causes ever have physical effects.

7. Newtonian Physics

Some readers might wonder why this isn't the end of the issue. Given that Leibniz established, against Descartes, that both momentum and energy are conserved in systems of moving particles, then why wasn't the history of the mind-brain argument already over? Of course, we mightn't nowadays want to follow Leibniz in opting for pre-established harmony, as opposed to simply embracing mind-brain identity. But this is simply because we favour a different response to the causal argument laid out in section 5, not because we have any substantial premises Leibniz lacked. In particular, the crucial first premise of the causal argument, the completeness of physics, would seem already to have been available to Leibniz. So doesn't this mean that everything needed to appreciate the causal argument was already to hand in the second half of the seventeenth century, long before the rise of twentieth-century physicalism?

Well, it was -- but only on the assumption Leibniz gives us the correct dynamics. However, Leibniz's physical theories were quickly eclipsed by those of Newton, and this then re-opened the whole issue of the completeness of physics.

The central point here is that Newton allowed forces other than impact. Leibniz, along with Descartes and all other pre-Newtonian proponents of the "mechanical philosophy", took it as given that all physical action is by contact. They assumed that the only possible cause of a change in a physical body's motion is the impact of another physical body. (Or more precisely, as we are telling the story, Descartes supposed that the only possible non-mental cause of physical change is impact, and Leibniz then argued that mental causes other than impact are not possible either, if the conservation of momentum and energy are to be respected.)

Newtonian mechanics changed the whole picture. This is because Newton did not take impact as his basic model of dynamic action. Rather his basic notion is that of an impressed force. Rather than thinking of "force" as something inside a body which might be tranferred to other bodies in impact, as did all his contemporaries (and indeed as did most of his successors for at least a century (11)), Newton thought of forces as disembodied entities, acting on the affected body from outside. An impressed force "consists in the action only, and remains no longer in the body when the action is over." Moreover, "impressed forces are of different origins, as from percussion, from pressure, from centripetal force." (Newton, 1960 [1686], Definition IV.) Gravity was the paradigm. True, the force of gravity always arose from the presence of massive bodies, but it pervaded space, waiting to act on anything that might be there, so to speak, with a strength as specified by the inverse square law.

Once disembodied gravity was allowed as a force distinct from the action of impact, then there was no principled barrier to other similarly disembodied special forces, such as chemical forces, or magnetic forces, or forces of cohesion (cf. Newton, 1952 [1704], Queries 29-31) -- or indeed vital and mental forces.

Nothing in classical Newton thinking rules out special mental forces. While Newton has a general law about the effects of his forces (they cause proportional changes in the velocities of the bodies they act on), there is no corresponding general principle about the causes of such forces. True, gravity in particular is governed by the inverse square law, which fixes gravitional forces as a function of the location of bodies with mass. But there is no overarching principle dictating how forces in general arise. This opens up the possibility that there may be sui generis mental forces, which would mean that Newtonian physics, unlike Leibnizian physics, is not physically complete. Some physical processes could have non-physical mental forces among their causal antecedents. (12)

The switch from a pure impact-based mechanical philosophy to the more liberal world of Newtonian forces thus undermined Leibniz's argument for the completeness of physics. Leibniz could hold that the principles governing the physical world leave no room for mental acts to make a difference because he had a simple mechanical picture of the physical world. Bodies preserve their motion in any given direction until they collide, and then they obey the laws of perfect elastic impact. The Newtonian picture is far less pristine, and gives no immediate reason to view physics as complete.

You might that think the conservation laws of Newtonian physics would themselves place constraints on the generation of forces, in such a way as to restore the completeness of physics. But this would be a somewhat anachronistic thought. Conservation laws did not play a central role in Newtonian thinking, at least not in that of Newton himself and his immediate followers. True, Newton's mechanics does imply the conservation of momentum. This falls straight out of his Third Law which requires that "action and reaction" are always equal. But it is a striking feature of Newtonian dynamics that there is no corresponding law for energy. (13)

Of course, as we shall see in the next section, the principle of the conservation of kinetic and potential energy in all physical processes did eventually become part of the Newtonian tradition, and this does impose a general restriction on possible forces, a restriction expressed by the requirement that all forces should be "conservative". But this came much later, in the middle of the nineteenth century, and so had no influence

on the range of possible forces admitted by seventeenth- or eighteenth-century Newtonians. (Moreover, it is a nice question, to which we shall return at length below, how far the principle of the conservation of kinetic plus potential energy, with its attendant requirement that all forces be conservative, does indeed constitute evidence against sui generis mental forces.)

In any case, whatever the significance of later Newtonian derivations of the conservation of energy, early Newtonians themselves certainly saw no barrier to the postulation of sui generis mental forces. In a moment I shall give some examples. But first it will be helpful to distinguish in the abstract two ways in which such a Newtonian violation of the completeness of physics could occur.

First, and most obviously, it could follow from the postulation of indeterministic mental forces. If the determinations of the self (or of the "soul", as they would have said in the seventeenth and eighteenth centuries) could influence the movements of matter in spontaneous ways, then the world of physical causes and effects would obviously not be causally closed, since these spontaneous mental causes would make a difference to the unfolding of certain physical processes.

But, second, it is not even necessary for the violation of completeness that such sui generis special forces operate indeterministically. Suppose that the operation of mental forces were governed by fully deterministic force laws (suppose, for example, that mental forces obeyed some inverse square law involving the presence of certain particles in the brain). Then mental forces would be part of Newtonian dynamics in just the same sense as gravitational or electrical forces: we could imagine a system of particles evolving deterministically under the influence of all these forces, including mental forces, with the forces exerted at any place and time being deterministically fixed by the relevant force laws. Even so, this deterministic model would still constitute a violation of the completeness of physics, for the physical positions of the particles would depend inter alia on prior mental causes, and not exclusively on prior physical causes.

Did I not say at the end of the last section that determinism is sufficient for the completeness of physics (even if not necessary, because of quantum mechanics)? No. What I said was that physical determinism (the doctrine that prior physical conditions alone are enough to determine later physical conditions) is sufficient for the completeness of physics. However, we can accept determinism as such without accepting physical determinism, and so without accepting the completeness of physics. In particular, we can have a deterministic model in which mental forces play an essential role, and in which the physical sub-part is therefore not causally closed.

You might feel (indeed might have been feeling for some time) that a realm of deterministic mental forces would scarcely be worth distinguishing from the general run of physical forces, given that they would lack the spontaneity and creativity that is normally held to distinguish the mental from the physical. And you might think that it is therefore somewhat odd to view them as violating the completeness of physics. I happily concede that there is something to this thought. But I would still like to stick to my terminology, as stipulated at the end of section 5, which assumed an initial sense for "mental" (as sentient, or intentional, or intelligent), and then defined the "physical" as whatever can be identifed without alluding to such mental properties -- which then makes even deterministically governed sui generis mental forces come out

"non-physical", since they can't be so non-mentally identified. This is the terminology which best fits with our original interest in the causal argument for physicalism. We don't want deterministic mental forces to be counted as consistent with the "completeness of physics", precisely because this kind of "completeness of physics" wouldn't be any good for the causal argument: if mental forces are part of what makes "physics" complete, then we won't be able to argue from this that mental forces must be identical with some other (non-mental) causes of their effects.

So far I have merely presented the possibility of special Newtonian forces as an abstract possibility. However, the postulation of such forces was a commonplace among eighteenth-century thinkers, particularly among those working in anatomy and physiology. Many of the theoretical debates in these areas were concerned with the existence of vital and mental forces, and with the relation between them. Among those who debated these issues, we can find both the indeterministic and deterministic models of mental forces. (14)

Thus consider the debate among eighteenth-century physiologists about the relative roles of the forces of sensibility and irritability. This terminology was introduced by the leading German physiologist Albrecht von Haller, Professor of Anatomy at Göttingen from 1736. Haller thought of "sensibility" as a distinctively mental force. "Irritability" was a non-mental but still peculiarly biological power. ("What should hinder us from granting irritability to be a property of the animal gluten, the same as we acknowledge gravity and attraction to be properties of matter in general . . .", Haller, 1936 [1751].) Haller took the force of sensibility to be under the control of the soul and to operate solely through the nerves. Irritability, by contrast, he took to be located solely in the muscle fibres.

In distinguishing the mentally directed force of sensibility from the more automatic force of irritability, Haller can here be seen as conforming to my model of indeterministic mental forces. Where the force of irritability is determined by prior stimuli and is independent of mental agency, the force of sensibility responds to the spontaneous commands of the soul.

Haller's model was opposed by Robert Whytt (1714-66) in Edinburgh. In effect Whytt can be seen as merging Haller's distinct mental and vital forces, irritability and sensibility. On the one hand, Whytt gave greater power to the soul: he took it that a soul or "sentient principle" is distributed throughout the body, not just in the nerves, and is responsible for all bodily activities, from the flow of blood and motion of muscles, to imagination and reasoning in the brain. But at the same time as giving greater power to this sentient principle, he also rendered its operations deterministic. He explictly likened the sentient principle to the Newtonian force of gravity, and viewed it as a necessary principle which acts according to strict laws. Whytt can thus be seen as exemplifying my model of deterministic mental forces: the sentient principle is simply another deterministic Newtonian force, just like gravity and the others, in that its operations are fixed by a definite force law. (Whytt, 1755.)

8. The Conservation of Energy

In this section I want to consider how the principle of the conservation of energy eventually emerged within the tradition of Newtonian mechanics, and how this bears

on the completeness of physics. It will be useful to separate some different aspects of this emergence.

(i) Rational Mechanics

Through the eighteenth and early nineteenth centuries a number of mathematician-physicists, among the most important of whom were Jean d'Alembert (1717-83), Joseph Louis Lagrange (1736-1813), the Marquis de Laplace (1749-1827) and William Hamilton (1805-65), developed a series of mathematical frameworks designed to simplify the analysis of the motion of interacting particles. These frameworks allowed physicists to abstract away from detailed forces of constraint, such as the forces holding rigid bodies together, or the forces constraining particles to move on surfaces, and concentrate on the effects produced by other forces. (See Elkana, 1974, ch. II, for the history, and Goldstein, 1964, for the mathematics.)

These mathematical developments also implied that, under certain conditions, the sum of kinetic energy and potential energy remains constant. Roughly, when all forces involved are independent of the velocities of the interacting particles and of the time (let us call forces of these kinds "conservative"), then the sum of actual kinetic energy (measured by 1/2mv2) plus the potential to generate more such energy (often called the "tensions" of the system) is conserved: when the particles slow down, this builds up "tensions", and, if those "tensions" are expended, the particles will speed up again.

We now think of this as the most basic of all natural laws. But this attitude was no part of the original tradition in rational mechanics. There were two reasons for this. First, the Newtonian scientists in this tradition were not looking for conserved quantities anyway. As I explained earlier, conservation principles played little role in classical Newtonian thinking. True, Leibniz himself had urged the conservation of kinetic energy (under the guise of "vis viva"), but by the eighteenth century Leibniz's influence had been largely eclipsed by Newton's. Second, the conservation of potential and kinetic energy in any case only holds under the assumption that all forces are conservative. We nowadays take this requirement to be satisfied for all fundamental forces. But this again was no part of eighteeenth-century thinking. Some familiar forces happen to be conservative, but plenty of other forces are not. Gravitation, say, is conservative, since it depends only on the positions of the particles, and not on their velocities, nor the elapsed time. But, by contrast, frictional forces are not conservative, since they depend on the velocity of the decelerated body relative to the medium. And correspondingly frictional forces do not in any sense seem to conserve energy: when they decelerate a body, no "tension" is apparently built up waiting to accelerate the body again.

For both these reasons, the tradition in rational mechanics did not initially view the conservation of kinetic and potential energy in certain systems as of any great significance. On the contrary, it was simply a handy mathematical consequence which falls out of the equations when the operative forces all happen to fall within a subset of possible forces. (Cf. Elkana, 1974, Ch 2.)

(ii) Equivalence of heat and mechanical energy

In the first half of the nineteenth century a number of scientists, most prominently James Joule (1819-89), established the equivalence of heat and mechanical energy, in the sense of showing that a specific amount of heat will always be produced by the

expenditure a given amount of mechanical energy (as when a gas is compressed, say), and vice versa (as when a hot gas drives a piston).

These experiments suggested directly that some single quantity is preserved through a number of different natural interactions. They also had a less direct bearing on the eventual formulation of the conservation of energy. They indicated that apparently non-conservative forces like friction and other dissipative forces need not be non-conservative after all, since the kinetic energy apparently lost when they act will in fact be preserved by the heat energy gained by the resisting medium. (15)

The stage was now set for the formulation of a universal principle of the conservation of energy. We can distinguish three elements which together contributed to the formulation of this principle. First, the tradition of rational mechanics provided the mathematical scaffolding. Second, the experiments of Joule and others suggested that different natural processes all involve a single underlying quantity which could manifest itself in different forms. Third, these experiments also suggested that apparently non-conservative forces like friction were merely macroscopic manifestations of more fundamental conservative forces.

Of course, it is only with the wisdom of hindsight that we can see these different strands as waiting to be pulled together. At the time they were hidden in abstract realms of disparate branches of science. It took the genius of the young Hermann von Helmholtz (1821-94) to see the connections. In 1847, at the age of twenty six, he published his mongraph Uber die Erhaltung die Kraft ("On the Conservation of Force"). The first three sections of this treatise are devoted to the tradition of rational mechanics, and in particular to explaining how the total mechanical energy (kinetic plus potential energy) in a system of interacting particles is constant in those cases where all forces are familiar "central forces" independent of time and velocity. The fourth section describes the equivalence between mechanical "force" and heat, referring to Joule's results, while the last two sections extend the discussion to electric and magnetic "forces", showing again that there are fixed equivalences between these "forces", heat, and mechanical energy. (16)

(iii) Physiology

At the end of his treatise Helmholtz touches on the conservation of energy in living systems. Helmholtz was in fact a medical doctor by training, and had been a student in the Berlin physiological laboratory of Johannes Müller in the early 1840s, along with Emil Du Bois-Reymond (1818-96) and Ernst Brücke (1819-92). Together these students were committed to a reductionist programme in physiology, aiming to show that phenomena like respiration, animal heat, and locomotion could all be understood to be governed by the same laws as operate in the inorganic realm.

This physiological context undoubtedly played a fundamental role in Helmholtz's articulation of a universal principle of the conservation of energy. Because of his physiological interests, Helmholtz was interested in a principle that would cover all natural phenomena, including those in living systems, and not just such manifestly physical phenomena as mechanical motion, heat, and electromagnetism. Thus he took the crucial step of asserting that all forces conserve the sum of kinetic and potential energy; superficially non-conservative forces like friction are simply macroscopic manifestations of more fundamental forces which preserve energy at the micro-level. This then enabled Helmholtz to view the equivalences established by experimentalists

like Joule, not just as striking local regularities, but as necessary consequences of a fundamental principle of mechanics. All natural processes must respect the conservation of energy, including processes in living systems.

It is noteworthy that neither the experimentalists like Joule, nor the mathematician-physicists in the rational mechanics tradition, made this crucial step to a universal principle. None of the scientists working experimentally on numerical equivalences between different processes, like Joule, generalized their discoveries into the claim that there is one quantity, energy, preserved in all natural interactions whatsoever. While it is true that a number of different scientists at the time were investigating such numerical equivalences (thus the historical thesis of the "simultaneous discovery" of the conservation of energy), there is no reason to suppose that these scientists were generally inspired by any vision of the underlying unity of different natural processes. Similarly, there was nothing to attract mathematical physicists in the tradition of rational mechanics to the conclusion that all forces are conservative, for the reasons given above. They simply thought of such forces as the mathematically tractable special case where changes in kinetic and potential energy happen always to balance out.

Without the desire to bring living systems under a unified science, none of these scientists had any motive for synthesizing the different strands pulled together by Helmholtz. It was Helmholtz's combination of physiological interests and sophisticated physical understanding that precipitated the crucial step. He saw that, if we assume that all fundamental forces are conservative, then this guarantees that a certain quantity, the total energy, will be preserved in all natural processes whatsoever, including the organic processes that formed the focus of his interest.

(iv) Vital Forces

Helmholtz was part of a tradition in experimental physiology which set itself in opposition to the previous generation of German Natuurphilosophen. During the eighteenth century the Newtonian categories of "irritability" and "sensibility" had gone through various transformations, and by the end of the century were widely referred to under the heading of "Lebenskraft" or "vital force", though there was continued disagreement on the precise nature of such forces. Meanwhile, within the tradition of German idealism, the notion of vital force had broken loose from its original Newtonian moorings, and became part of a florid metaphysics imbued with romanticism and idealism.

According to the Natuurphilosophen, organic matter was infused with a special power which organized and directed it. Following Blumenbach and Kant, Schelling took up the term"Bildungstrieb" ("formative drive"), because of what he saw as the excessively mechanical connotations of "Lebenskraft". Scelling and the other Natuurphilosophen viewed this formative drive as having a quasi-mental aspect, which enabled it to to mediate between the "archetypical ideas" or "essences" of different species and the development of individual organisms towards that ideal form. (See Coleman, 1971, ch. 3, Steigerwald, 1998.)

The experimental tradition which included Helmholtz can be seen as a reaction to these extreme doctrines. However, it is striking that many of those associated with this tradition, though not Helmholtz himself, continued to admit the possible existence of vital forces, both before and after the emergence of the conservation of energy.

This is less puzzling than it may at first seem. These physiological thinkers did not think of vital forces as the mystical intermediaries of the Natuurphilosophen, imbued with all the powers of creative mentality. Rather these thinkers were reverting to the tradition of eighteenth-century physiology. They simply viewed vital forces as special Newtonian forces, additional to gravitational forces, chemical forces and so on, and which happen to arise specifically in organic contexts. Justus von Leibig (1803-73), the leading physiological chemist of the time, and Müller, Helmholtz's own mentor, are clear examples of experimental physiologists who were prepared to countenance vital forces in this sense. (Cf. Coleman, 1971, ch. VI, Elkana, 1974, ch. IV.)

(vi) Does the Conservation of Energy Rule out Vital (and Mental) Forces?

The interesting question, from the point of view of this paper, is how far this continuing commitment to vital forces is consistent with the doctrine of the conservation of energy. There is certainly some tension between the two doctrines. It is noteworthy that Helmholtz himself, and his young colleagues from Müller's laboratory, were committed to the view that no forces operated inside living bodies that were not also found in simpler physical and chemical contexts. (Coleman, 1971, pp. 150-4.) Even so, there is no outright inconsistency between the conservation of energy and vital forces, and many late nineteenth-century figures were quite explicit, not to say enthusiastic, about accepting both.

In order to get clearer about the room left for vital (or mental) forces by the conservation of energy, recall how I earlier distinguished two ways in which early Newtonian theory left room for such special forces to violate the completeness of physics. First, such forces might operate spontaneously and indeterministically: nothing in early Newtonian theory would seem to rule out spontaneous forces ungoverned by any deterministic force law. Second, even if the relevant forces are governed by a deterministic force law, they may still be sui generis, in the sense that they may be distinct from gravitional forces, chemical forces, and so on, and may arise specifically in living systems or their brains.

The conservation of energy bears differentially on these two kinds of special forces. It does seem inconsistent with the first kind of special force, a spontaneous special force. But it does not directly rule out the second, deterministic kind.

Why should the conservation of energy rule out even a spontaneous special force? (Think of a spontaneous mental force that accelerates molecules in the pineal gland, say.) Why shouldn't such a force simply respect the conservation of energy, by not causing accelerations which will violate it? But this doesn't really make sense. The content of the principle of the conservation of energy is that losses of kinetic energy are compensated by build-ups of potential energy, and vice versa. But we couldn't really speak of a "build-up" or "loss" in the potential energy associated with a force, if there were no force law governing the deployment of that force. So the very idea of potential energy commits us to a law which governs how the relevant force will cause accelerations in the future.

However, nothing in this argument rules out the possibility of vital, mental, or other special forces which are governed by deterministic force laws. After all, the conservation of energy in itself does not tell which basic forces operate in the physical universe. Are gravity and impact the only basic forces? What about electromagnetism? Nuclear forces? And so on. Clearly the conservation of energy as such leaves it open

exactly which basic forces exist. It only requires that, whatever they are, they operate conservatively.

9. The Death of Emergentism

So a commitment to the conservation of energy by no means settled the question of whether sui generis mental or vital forces should be rejected and physics declared complete. True, some few thinkers, like Helmholtz himself, conjoined the conservation of energy with a denial of such special forces. But this was by no means mandated by the conservation of energy itself, for the reasons I have just explained. Accordingly, many other thinkers in the late nineteenth and early twentieth centuries took the opportunity to posit special forces of the kind allowed by the conservation of energy. So I still owe an explanation of what finally created a scientifically informed consensus against such special forces.

The issue is not straightforward, and there is no question of dealing with it fully here. But in this final section I would like to offer some outline conjectures. I shall proceed as follows. First, I shall take it as given that the conservation of energy at least was a settled doctrine. Of course there is a story to be told about this as well. But, for whatever reasons, the doctrine of the conservation of energy did win widespread acceptance within a decade or two of its initial formulation, and certainly none of the developments I am about to consider questioned its validity. Second, I shall lump mental and vital forces together. There are of course considerations that bear differentially on the existence of such forces, but I shall be proceding at a level where these are not significant.

(i) Two Arguments

My central suggestion will be that two rather different lines of evidence contributed to the demise of special forces. The first was an abstract argument based on theoretical physics, while the second was a more direct empirical argument based on physiological research. The first, abstract argument involves considerations to do with the conservation of energy, and was available from the time of Helmholtz onwards (even though it was not incontrovertible, and many were not persuaded). By contrast, the second, more direct argument does not follow from the conservation of energy alone, and indeed did not really gain force until the twentieth century.

At the end I shall argue that both arguments can be seen as contributing to the general modern acceptance of the completeness of physics. But the precise timing of this acceptance, and in particular the arrival of a general consensus in the second half of the twentieth century, seems to call for explanation in terms of the build-up of direct evidence for the second argument, rather than in terms of the more abstract argument which had been available since the middle of the nineteenth century.

Let me begin by presenting the two arguments in outline.

(1) The Argument from Fundamental Forces. The first argument is that all apparently special forces characteristically reduce to a small stock of basic physical forces which conserve energy. Causes of macroscopic accelerations standardly turn out to be composed out of a few fundamental physical forces which operate throughout nature. So, while we ordinarily attribute certain physical effects to "muscular forces", say, or

indeed to "mental causes", we should recognize that these causes, like all causes of physical effects, are ultimately composed of the few basic physical forces.

(2) The Argument from Physiology. The second argument is simply that there is no direct evidence for vital or mental forces. Physiological research reveals no phenomena in living bodies that manifest such forces. All organic processes in living bodies seem to be fully accounted for by normal physical forces.

I take both of these to be empirically-based arguments, and both to have the same conclusion, namely, that there are no special mental or vital forces. But note that the evidential basis for the two arguments is quite different. The second argument appeals directly to the evidence uncovered by physiological research. It notes that observations made inside living bodies never reveal any accelerations that cannot be attributed to normal physical forces. The first argument, by contrast, appeals to the investigation of forces in general. It rests on evidence that many apparently different kinds of forces turn out to be composed of a few fundamental forces, and then applies this lesson to vital and mental forces in particular. So it need not appeal directly to any evidence about what goes on in living bodies. Instead it can infer the general conclusion inductively from the study of other forces, and then project it to the special case of mental and vital forces.

(ii) The Argument from Fundamental Forces

Let me now explain the first argument more fully. I shall return to the second below. I take the materials for the first argument to have been available from the middle of the nineteenth century, and to relate to the reasoning which led up to the acceptance of the conservation of energy. It is true, as I have stressed, that the doctrine of the conservation of energy is itself consistent with the existence of special forces, as long as those forces are themselves conservative. At the same time, it seems to me that the thinking which supported the conservation of energy also weighed against special mental or vital forces.

At its simplest, my thought here is that the arguments behind the conservation of energy give inductive reason to suppose that all forces reduce to a small number of fundamental forces. We have already seen how Helmholtz's formulation of the conservation of energy hinged on the assumption that friction and other dissipative forces are non-fundamental forces, macroscopic manifestations of processes involving more fundamental conservative forces. For it is only if we see macroscopic forces like friction as reducing to fundamental conservative forces that we can uphold the universal conservation of energy. Now, this point can be viewed as providing inductive support for the general thesis that all apparently special forces will reduce to a small stock of fundamental forces. The special forces which have been quantitatively analysed, like friction, turn out to reduce to more fundamental conservative forces. So this provides inductive reason to conclude that any other apparently special forces, like muscular forces, or vital forces, or mental forces, will similarly reduce. (17)

This is of course not a knock-down argument. Vital or mental forces could themselves figure among the fundamental forces of nature, even if they are only generated in the special circumstances associated with life or sentience. But this position does not sit happily with a inductively based commitment to the universal conservation of energy. An insistence on the independent existence of sui generis special forces inside bodies

threatens to remove the inductive reasons for believing in the conservation of energy in the first place. For there are no obvious grounds for expecting such sui generis special forces to be conservative.

After all, what argument was there, in 1850, say, for believing that forces operating inside bodies do not violate the conservation of energy? I am suggesting that the most persuasive argument hinged on the assumption that all forces operating in special circumstances reduce to a small stock of fundamental conservative forces. However, suppose now that it is explicitly specified that vital and mental forces do not reduce to other forces. Now we need independent evidence for supposing they are conservative, and it is not clear where it is to come from. In effect, then, positing sui generis vital or mental forces threatens to undermine the inductive grounds for upholding the conservation of energy in the first place. For it makes the assumption of their conservativeness an independent assumption, an assumption for which we lack any independent evidence.

I suspect that something like this line of though lay behind Helmholtz's and his younger contemporaries' conviction that there were no special vital forces. Consider how Helmholtz argues in Uber die Erhaltung die Kraft. He takes pains to stress how it is specifically central forces independent of time and velocity which ensure the conservation of energy. This emphasis on central forces (by which Helmholtz meant forces which act along the line between the interacting particles) now seems dated. Nowadays, conservativeness is normally defined circularly, as a property of those forces which do no work round a closed orbit, and which are therefore the gradient of a scalar that depends only on position. This definition does not require a restriction to central forces. However, Helmholtz was in no position to adopt our circular definition of conservativeness. He was aiming to persuade his readers of the general conservation of energy, and so needed an argument. It wouldn't have served simply to observe that energy is conserved by those forces which conserve energy. Helmholtz's actual claim was that energy is conserved by a wide range of known forces, namely, central forces. Still, this by itself doesn't show energy is conserved by all forces, unless all forces are central. Why should this be? Well, as above, the most plausible though is surely that there is a small stock of basic central forces, and that all causes apparently peculiar to special circumstances are composed out of these.

To repeat, this is not a conclusive argument. Those thinkers who remained convinced, for whatever reasons, that there must be irreducible special forces inside living bodies, could still respect the universal conservation of energy, by maintaining that these extra forces must themselves operate conservatively. In support of this they could have offered the alternative inductive argument that, since all the other fundamental forces so far examined have turned out to be conservative, we should infer that any extra vital or mental fundamental forces will be conservative too.

I am not sure how far these alternative lines of inductive reasoning can be found explicitly laid out in the nineteenth-century debates. But they offer one possible explanation for the two different views on sui generis special forces which coexisted after the emergence of the conservation of energy. The thought that all apparently special forces reduce to a small stock of fundamental forces can account for the rejection of irreducible vital or mental forces by thinkers like Helmholtz and his young colleagues. Yet there were at least as many who wanted to maintain that vital and mental forces are sui generis, and they had the option of arguing that, even if

these forces are fundamental and irreducible, the nature of other fundamental forces provides inductive reason to suppose these sui generis forces will be conservative in their own right. (18)

In connection with this latter school of thought, I have already mentioned Leibig and Müller, two eminent physiologists of the older generation, who continued to accept vital forces, even after the conservation of energy had won general acceptance. And Brian McLaughlin, in his excellent article on "British Emergentism" (1992), explains how the philosophers J.S. Mill and Alexander Bain went so far as to argue that the conservation of energy, and in particular the notion of potential energy, lends definite support to the possibility of non-physical forces. (19) (The "British Emergentists" discussed by McLaughlin were a philosophical movement committed precisely to non-physical causes of motion in my sense, causes which were not the vectorial "resultants" of basic physical forces like gravity and impact, but which "emerged" when matter arranged itself in special ways. The particular idea which attracted Mill and Bain was that these "emergent forces" might be stored as unrealized potentials, ready to manifest themselves as a causes of motion only when the relevant special circumstances arose.)

(iii) The Argument from Physiology

McLaughlin explains how British Emergentism" continued to flourish into the twentieth century (20). This highlights the question with which I began this paper. Given that thinkers continued to posit special mental and vital forces until well after the Great War, why has the idea of such forces now finally fallen into general disfavour?

Here I think we need to refer to the second line of argument against such forces, the argument from direct physiological evidence. We can view this second argument as operating against the background provided by the earlier argument from fundamental forces. The earlier argument suggested that at least most natural phenomena, if not all, can be explained by a few fundamental physical forces. This focused the issue of what kind of evidence which would demonstrate the existence of extra mental or vital forces. For once we know which other forces exist, then we will know which anomalous accelerations would indicate the presence of special mental or vital forces. Against this background, the argument from physiology is then simply that detailed modern research has failed to uncover any such anomalous physical processes.

The relevant research dates mostly from the twentieth century. While important physiological research was carried out in the second half of the nineteenth century (see Coleman, 1971), it did not penetrate to the level of forces operating inside bodies. At most it identified the chemical inputs and outputs to various parts of the body, and showed that animals are subject to general conservation principles. (See in particular Coleman, p. 140-3, for Max Rubner's elaborate 1889 respiration calorimeter experiments, showing that the energy emitted by a small dog exactly corresponds to that of the food it consumes.) Experiments of this kind, however, failed to provide compelling evidence against vital or mental forces. That normal chemicals are moved around, and that energy is conserved throughout, does not in the end rule out the possiblility that some accelerations within bodies are due to special vital or mental forces. It may still be that such forces are activated inside cells, but operate in such a way as to "pay back" all the energy they "borrow", and vice versa. (21)

In the first half of the twentieth century the situation changed, and by the 1950s it had become difficult, even for those who were not moved by the abstract argument from general reducibility, to continue to uphold special vital or mental forces. A great deal became known about biochemical and neurophysiological processes, especially at the level of the cell, and none of it gave any evidence for the existence of special forces not found elsewhere in nature.

During the first half of the century the catalyctic role and protein constitution of enzymes were recognized, basic biochemical cycles were identified, and the structure of proteins analyzed, culminating in the discovery of DNA. In the same period, neurophysiological research mapped the body's neuronal network and analysed the electrical mechanisms responsible for neuronal activity. Together, these developments made it difficult to go on maintaining that special forces operate inside living bodies. If there were such forces, they could be expected to display some manifestation of their presence. But detailed physiological investigation failed to uncover evidence of anything except familiar physical forces.

In this way, the argument from physiology can be viewed as clinching the case for completeness of physics, against the background provided by the argument from fundamental forces. One virtue of this explanation in terms of two interrelated arguments is that it yields a natural explanation for the slow advance of the completeness of physics through the century from the 1850s to the 1950s. Suppose that we rank different thinkers through this period in terms of how much specifially physiological evidence was needed to persuade them of completeness, in addition to the abstract argument from fundamental forces. Helmholtz and his colleagues would be at one extreme, in deciding for completeness on the basis of the abstract argument alone, without any physiological evidence. In the middle would be those thinkers who waited for a while, but converted once initial physiological research in the first decades of this century gave no indication of any forces beyond fundamental forces found throughout nature. At the other end would be those who needed a great deal of negative physiological evidence before giving up on special forces. The existence of this spectrum would thus explain why there was a gradual build-up of support for the completeness of physics as the physiological evidence accumulated, culminating, I would contend, in a general scientific consensus by the 1950s. (22)

10. Conclusion

The problem I set myself at the beginning of this paper was to explain the rise of physicalist doctrines in the second half of this century. My argument has been that this is due to contemporary agreement on the completeness of physics. In the main body of this paper I have sought to show that this consensus is not just a fad, but a reflection of developments in empirical theory. Though it has not always been so, there is now good reason to believe the empirical thesis that all physical effects are due to physical causes. In particular, by the 1950s there was enough physiological evidence to persuade even those scientists who were unmoved by the abstract argument from fundamental forces.

The rise of physicalism among philosophers can be seen as a reflection of this development within science. Without the completeness of physics, there is no compelling reason to identify the mind with the brain. But once the completeness of physics became part of established science, scientifically informed philosophers

realized that this crucial premise could be slotted into the various alternative versions of the causal argument for physicalism. There seems no reason to look any further to explain the widespread philosophical acceptance of physicalism since the 1950s.

Of course, as with all empirical matters, there is nothing certain here. There is no knock-down argument for the completeness of physics. You could in principle accept the rest of modern physical theory, and yet continue to insist on special mental forces, which operate in as yet undetected ways in the interstices of intelligent brains. And indeed there do exist bitter-enders of just this kind, who continue to hold out for special mental causes, even after another half-century of ever more detailed molecular biology has been added to the inductive evidence which initially created a scientific consensus on completeness in the 1950s. Perhaps it is this possibility which Stephen Clark has in mind when he doubts whether any empirical considerations can "disprove" mind-body dualism. If so, there is no more I can do to persuade him of the completeness of physics. However, I see no virtue in philosophers refusing to accept a premise which, by any normal inductive standards, has been fully established by over a century of empirical research. (23)

Footnotes

(1) Though see section 5 (iv) below for some necessary qualifications.

(2) Or, even more precisely, to accommodate quantum mechanical indeterminism: the chances of all physical occurrences are fully determined by a purely physical prior history. I shall ignore this qualification in nearly all that follows, since it would only complicate the issues unnecessarily.

(3) Note, however, that while this is just a doctrine about physics, it does implicitly distinguish physics from other realms, since most other realms aren't complete in this sense. The mental isn't complete, for example, since there is no mental cause for the pain I feel when I sit on a drawing pin. Nor is the ecenomic, since there is no economic cause for the economic costs occasioned by a hurricane. (This is why we don't find arguments aiming to show that everything is mental, or economic, parallel to the completeness-based argument that everything is physical).

(4) In other writings, the relevance of the completeness of physics does not need to be excavated, since it lies on the surface. Thus see Feigl, 1958; Oppenheim and Putnam, 1958.

(5) Equally: all chemical/biological/social occurrences have physical effects. The causal argument provides a schema that delivers physicalism for other subjects as readily as for the mental. In the historical discussion below, various differnet special categorieswill be at issue at diferent points. But it will often be expositorily convenient to let the mental stand for the other cases, especially when addressing issues of argumentative structure rather than historical substance. The context should make it clear when the category of the mental is so being used.

(6) However, Sturgeon (1998) argues that an equivocation between a quantum-theoretical sense of "physical" (in premise 1) and an everyday sense (in premise 2) invalidtes the argument. This raises a number of interesting issues which I shall not be able to dicuss here. But see Noordhof (1999) and Witmer (forthcoming).

(7) Of course, many philosophers are moved to pay this price because they cannot believe that conscious occurrences in particular can be identical with physical occurrences. I do not think this is a good motivation. However, I do accept that physicalists owe some explanation of why conscious occurrences seem so very different from physical ones, if they aren't. (See Papineau, 1993, ch 4: 1998).

(8) Won't we have two causes anyway, namely, (a) the role property with which we are now identifying the conscious property, and (b) the physical property which directly causes the behavioural result? Well, we might in a sense have two "causes", but they won't overdetermine the result, if the role property is

present only in virtue of the physical property's presence. (Note that in this case the behavioural result wouldn't still have occurred if the physical property had been absent, for then the role property would have been absent too; and similarly, if the role property had been absent in any particular case, so would the physical property have been absent.)

(9) Conversely, those philosophers who do not think that mathematical or moral facts have physical effects (in our brains, say) will come under pressure from the causal argument to identify them with physical facts.

(10) Leibniz took it that all basic material particles are perfectly elastic, and that no kinetic energy is lost when they collide. He explained the apparent loss of kinetic energy when inelastic macroscopic bodies colide by positing increased motion in the microscopic parts of theose bodies. (Thus he explains, in the fifth paper of the Leibniz-Clarke Correspondence, H. Alexander (ed.), 1956: "The author objects, that two soft or un-elastic bodies meeting together, lose some of their force. I answer, no. 'Tis true, their wholes lose it with respect to their total motion; but their parts receive it, being shaken (internally) by the force of their concourse.")

(11) Cf. Papineau, 1977.

(12) Throughout the rest of this paper I shall talk in terms of "forces". However, the issues will arise in just the same way if you regard forces as otiose, and instead think of the circumstances which "cause forces" as themselves directky causing the resulting accelerations. In that case, you will replace the question of whether there are "mental forces" with the question of whether specifically mental initial conditions (conditions of sentience, or intentionality, or intelligence, depending on how you wish initially to pick out the mental) make a differnece to accelerations, in the sense of entering as antecedents into special laws about accelerations which do not follow from other laws about accelerations. More simply, are there special accelerations in brains which aren't predicted by other laws about accelerations? (Cf. McLaughlin, 1992, pp. 64-5).

(13) One barrier to the formulation of any energy conserevation principle by early Newtonians was their lack of a notion of potential energy, the energy "stored up" after a spring has been extended or compressed, or as two gravitating bodies move apart. Given this, there was no obvious sense in they could view two gravitiating bodies, for example, as conserving energy while they moved apart: after all, the sum of their kinetic energies would not be constant, but unequivocally decreasing. And even in the case of impact, where the notion of potential energy is not immediately needed, early Newtonians displayed no commitment to the conservation of (kinetic) energy. Most obviously, Newton and his followers were perfectly happy, unlike Leibniz, to allow unreduced inelastic collisions, in which both bodies lose kinetic energy without transmitting it to their internal parts. It is also worth remarking that there is nothing in Newton's Laws of Motion to rule out even "superlastic" impacts, in which total kinetic energy increases. If two bodies with equal masses and equal but opposite speeds both rebounded after collision with double their speeds, for example, Newton's three Laws of Motion and the conservation of momentum would be respected. True, any such phenomenon would provide an obvious recipe or perpetual motion, but the point remains that Newton's Laws themselves do not rule it out. (It is also worth noting that perpetual motion was by no means universally rejected by seventeenth and eighteenth-centru physicists. Cf Elkana, 1974, pp. 28-30.)

(14) Here I am closely following Steigerwald, 1998, ch.2.

(15) One model for this preservation was the kinetic theory of heat, which took the macroscopic kinectic energy apparently lost to be converted into internal kinetic energy at microscopic level (cf. Leibniz's explanation for the apparent loss of kinetic energy in inelastic impact mentioned in footnote 9 above). But the abstract point at issue did not demand aceptance of teh kinectic theory, since the lost kinetic energy could alternatively be viewed as being stored in the "tensions" of whatever force might be assocaited with heat.

(16) Helmholtz used the word "Kraft". This is now standardly translated as "force" rather than "energy", but these two concepts were not clearly distinguised at the time, in either English or German. The general expectation at the time was that any conservation law would involve "force" ("Kraft", "vis"), where this was though of as a directed quantity ("force of motion") rather than as a scalar like energy. (Here again we see the dominance of the Newtonian tradition, whose only conserved quantity was the vectorial momentum.). One of Helmholtz's most important contributions was to make it clear that even

within the Newtonian tradition of rational mechanics it is the scalar energy that is conserved, rather than any vectorial "force". Even so, the confusions persisted for some time, as shown, for example, by Faraday's 1857 paper "On the Conservation of Force". (Cf. Elkana, 1974, pp. 130-8.)

(17) This conservation-of-energy-based argument from fundamental forces raises some interesting questions in connection with quantum mechanics. (I am grateful to Barry Loewer for pressing these points on me.)

(1) An initial query relates to my continued presentation of the issues in terms of forces. How does this fit in with modern quantum mechanics, which is normally formulated in terms of Hamiltonians rather than forces, that is, directly in energetic terms? But there is no substantial issue here, since the Hamiltonians themselves can be seen as depending on the relevant forces. (Cf. McLaughlin, 1992, p54.)

(2) On some interpretations, quantum systems do not always respect the conservation of energy. While nergy is conserved in the "Schrodinger evolution" of quantum systems, it is appaerntly violated by "wave collapses". Some, including myself, take this to argue against "wave collapses". But even if you don't go this way, it doesn't matter for this paper, since (a) the argument from fundamental forces to completeness will still have weight even if conservation is restricted to Schrodinger evolutions, and (b) completeness itself is consistent with the indeterminacy of collapse utcomes, since the chances of those outcomes are still fixed by prior physical forces alone. (Cf. fn. 2 above.)

(3) On some, but not all, collapse interpretations, however, sui generis factors do seem to fix whether a collapse occurs or not (even though the subsequent chances of the various possible outcomes then still depends entirely on the prior physical forces). I am thinking here of interpretations which say that collapses occur when physical systems unteract with consciousness (or indeed which say that collapses occur when there are "measurements", or "macroscopic interactions", and then refuse to offer any physical reductions of these terms). On thes interpretations, the completeness of physics is indded violated, since collapses don't follow from more basic physical laws, but depend on "emergent" causes. It would seem an odd vistory for non-physicalists, however, if the sole locus of sui generis mental action were quantum wave collapses.

(18) I have the impression that scientifically-infomed late nineteenth-century philosophers were not particularly exercised by our issue of whether or not there are speacial vital or mental forces. Understandably enough, they were far more interested in the determinism which, as I have pointed out, is required by the conservation of energy even if we admit speacil mental forces. Cf. Tyndall 1898 (1877).

(19) Indeed this line of thought seems to have become extremely popular in the late nineteenth century. The idead that the brain is a repository of "nervous energy", which gets channelled in various ways, and is then realeased in action, is a commonplace of Victorian thinkersfrom Darwin to Freud.

(20) Not all emergentists were as sophisticated as Mill and Bain. In Mind and its Place in Nature (1923), C.D. Broad addresses the issue of whether independent mental causation would violate the conservation of energy (pp 103-9). But instead of simply claiming that any mental force would operate conservatively, he insists that the principle of the conservation of energy does not explain all motions, even in physical systems, and so leaves room for other causes. He draws an analogy with a pendulum on a piece of string, where he says that the "pull of the string " is a cause which operates independently of any flows of energy, and he suggets that the mind might operate as a similar cause. While it is not entirely clear how Broad intends this analogy to be read, it is difficult to avoid the impression that he has mastered the letter of the principle of the conservation of energy without grasping the wider physical theory in which it is embedded.

(21) Indeed, and somewhat paradoxically, this species of "bookkeeping" experiment may even have weighed in favour of postulating sui generis vital forces. This is beacuse these experiments offer a counter to the argument from fundamental forces. That argument, remember, hinged on the claim that there is no direct inductive reason to suppose that any sui generis vital forces are conservative, if it is denied that they reduce more fundamental forces. But experiments like Rubner's do offer just such direct inductive reason, in that they show that any special forces operating inside bodies must always "pay back" just as much energy as they "borrow", even if they don't reduce to more fundamental forces. (I owe this point to Keith Hossack.)

(22) McLaughlin (1992, p.89) attributes the end of British Emergentism, and therewith the rise of contemporary physicalism, to the 1920s quantum-mechanical reduction of chemical forces to general physical forces between sub-atomic components. But it seems unlikely that this could have been decisive. After all, why should anybody be persuaded against special mental causes just because of the reduction of chemistry to physics? (Why should it matter to the existence of sui generis mental forces exactly how many independent forces there are at the level of atoms?) At most the reduction of chemistry to physics would have added weight to the argument from fundamental forces, by showing that yet another special force reduces to more basic forces. But it was irrelevant to the argument which I claim swayed thinkers in the twentieth-century, the argument from physiology.

(23) I would greatly like to thank Barry Loewer, Keith Hossack, Joan Steigerwald, Scott Sturgeon and Davis Spurrett for comments on drafts of this paper.

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