

The Reign of Prince Auto: Psychology in an Age of Science

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Introduction

The principle of autonomy (hereafter Prince Auto) is a doctrine which commits physicalistic philosophers to mechanical explanations of human behaviour. In this paper I argue that physicalism (in all its forms) presents a much too narrow account of scientific explanation. If we are to develop an adequate philosophy of psychology we must first free ourselves from the rule of a metaphysical picture which has dominated philosophy since at least the time of Descartes. We must free ourselves from the reign of Prince Auto.

(i) Royal Descent

Who is this Prince Auto who has ruled modern thinking of late? He is a slippery character who appears in many different guises; Fodor calls him methodological solipsism¹, Burge calls him individualism², but it was Stich who dubbed him the Principle of Autonomy.³ Dennett⁴ bends knee to him when he argues that physical stance predictions supreme and, to a still lesser extent, Davidson pays more limited homage with his proposal of psychophysical supervenience⁵. Prince Auto's central decree is that when we are engaging in a serious psychology we should be concerned only with the internal, efficient causes of behaviour. Or put another way, in doing psychology, we must discover what is inside the agent that is the immediate or proximate cause of its taking the action it does. He also says that anyone who does not pay him homage is not engaged in true or serious science. Let us quickly sketch the line of descent of our illustrious monarch.

Prince Auto succeeded to the explanatory throne during the seventeenth century when he replaced the ageing monarch Prince Telos. Prince Telos' decree was that in the explanation of all behaviour, both animate and inanimate, we should look for the purpose or end of the behaviour. While Auto emphasised efficient causes, Telos concentrated on final causes. Prince Telos was the ruler of the scholastics. And as we know it was these very scholastics who became the principal opponents of Renaissance science. Their failure in the competition with the new science in the explanation of the behaviour of inanimate objects brought a general disgrace to all teleological explanations.

The first damaging challenge to Prince Telos' rule came in 1543 when Copernicus' cosmological theory was first published. But the teleological world-view was most seriously challenged by Galileo, who became Prince Telos' principal opponent in the debate about falling-bodies. As we know the scholastics lost their duel with Galileo. Russell sums up the failures of teleological explanation by saying "though it might well seem admirably suited to explain the growth of animals and plants, it became in the event, a great obstacle to the progress of science" (Russell, 1945, p. 205, emphasis

mine). Behind Russell's words we can see the attitude which is also present in the minds of modern physicalists who think that only mechanical explanations are scientific. This attitude stems from Prince Auto's influence.

In his discussion of the history of physics, March announces that "The next major contribution to the development of mechanics came from the French philosopher Rene Descartes...His goal was to construct a general philosophy, as a replacement to that of the scholastics, by means of his own meditations and analytic methods that place great emphases on the discovery and use of first principles" (March, 1978, p. 27). It is crucial to an understanding of today's problems in the philosophy of psychology that we do not forget that while Descartes was laying down the foundations for talk of mental phenomena he was also contributing to the development of classical physics.

"[He] did develop his physics, a physics that is, at least in principle, nothing less than applied mathematics, or mechanics; a physics based on the clear and distinct ideas of extension and motion, a physics that reduces all material being to an endless interplay of movements, governed by strict mathematical laws, in the uniform space of the infinite universe" (Anscombe and Geach, 1969, p. xxviii).

We know that classical physics is a science which has gone on to achieve great success in its own domain. We also know that this new science of mechanics ascended to the throne partly by deposing Prince Telos' final cause explanations of the movements of matter. That is why for Descartes, and most thinkers of the seventeenth century, all matter was to be treated alike - and all explanations of the motion and behaviour of physical bodies could be described in purely mathematical terms. Descartes, under the sway of Prince Auto, thought of animals and all biological entities, including human bodies, as wholly mechanical. For him what separated humans from the beasts was to be found in their thinking essences.⁶

Modern physicalists are not very different from Descartes in this matter. They differ only in having dropped the thesis that there are separate entities, or substances, called minds. If there are any mental events then they must be physical in nature. Realists, who generally subscribe to psycho-functionalism or physicalism, obey Prince Auto by forcing the body to cast off its mental ectoplasm. Anti-realists, whether strong or weak, deny that mental entities have any reality rights at all, since content and consciousness cannot be squared with an ontology of purely physical entities, properties and relations. Thus, all physicalists feel the pressure to justify incorporate, e.g. modify, or deny the mental. In this physicalism is governed by the metaphysical monarch who has sat on the imperial throne of explanation since the seventeenth century.

What this simplistic historical sketch reminds us is the kind of difficulty that has arisen, in part, because Prince Telos is no longer respected on questions of matter and motion. His decree was disobeyed to allow for the birth of seventeenth century Renaissance science. While the teleological world view failed to provide a sound basis for all forms of explanation, it is important to query the unquestioned assumption that lies at the heart of modern physicalism which is that explanations should only respect only *one master*; since Prince Telos has been discredited, he has been replaced entirely by Prince Auto. This is the move that crucially needs to be

challenged. For if we adopt a physicalist point of view we will not only misrepresent the nature of psychology but also the nature of scientific explanation.

(ii) The Argument from Biology

The argument from science goes something like this; The physicalist likes to suggest that if we are agreed that there is nothing magical about the causes of action then we must admit that the immediate, efficient causes of action must be an internal physical event. This, so far, is just a statement of a commitment to the mechanistic world-view; a pledge of allegiance, if you like.

They also hold that to deny that reasons are, or are reducible to, mechanical causes is to be in conflict with the scientific world view. This conclusion derives from the allegedly undeniable truth that all modern of science is ultimately causal/explanatory in nature. Therefore, if we cannot describe reasons in terms of causes either straightforwardly or by appeal to a suitable notion of supervenience, then we must abandon reasoned explanations of actions altogether and seek a more appropriate causal explanation.⁷ If this worst case scenario should be realised our quandary would be: to be true to our selves or our science. Never the twain shall meet. Taking any other view would be to hold that our actions are necessarily mysterious. But it is crucial to this way of presenting matters that one accepts that the only proper scientific explanations are thus given in terms of efficient and proximate causes.

But surely this is not the case. After all, even some domains of modern physics rebel against the picture of causal determination - as Rosenberg tells us "Quantum mechanics rests on the rejection of a Newtonian principle that physicists and philosophers have spent two hundred years attempting to prove as a necessary truth of metaphysics" (Rosenberg, 1985, p. 7). To establish this would be enough to demonstrate that the physicalist claim concerning the link between mechanistic explanations and scientific respectability is undermined.

However, more relevant to the explanation of action is the fact that if we accept the physicalist criteria of scientific respectability we would have to hold that the majority of the biological sciences, i.e. those that rely on teleofunctional explanations, are not in good standing. As Gasper says, "biology is the best example we have of nonphysical science" (Gasper, 1991, p. 545, cf. Rosenberg, 1985, p. 13, 28, 32). But why is biology thought to be an exemplar of nonphysical science? It is, as Mayr suggests, because "The most characteristic aspect of evolutionary biology are the questions it asks. Instead of concentrating on what? and how? as does the biology of proximate causes, it asks why? ... [and t]he question 'why?' in the sense of 'what for' is meaningless in the world of inanimate objects" (Mayr, 1981, p. 72-73). Or in the words of John Maynard-Smith "[a biologist] asks not only how it works, but what is it for. This sharply distinguishes biology from physics and chemistry" (Maynard-Smith, 1990, p. 65). If it is true that teleofunctional explanations of this sort are not mechanical explanations, and that biology, of need, employs such explanations, then Mayr is right to conclude that "the assumption of ... naive mechanists that biology consists entirely of proximate causes is demonstrably wrong" (Mayr, 1981, p. 73).

If presented with this case, it is open for the physicalist to argue in one of three ways. Firstly, they could deny that biology really trades in such explanations at all. Secondly, they could deny the claim that functional explanations are irreducible to mechanical explanations. Thirdly, they could concede the impossibility of such a reduction, but nevertheless maintain that in itself this only provides evidence that biology is not a respectable science. None of these responses is satisfactory. I will not here give any attention to the first but I will examine the other two in turn.

Let us consider the second reply first. This is to adopt a strategy of *reductive provincialism* according to which the physicalist admits that biology and mechanical science do appear differ significantly in the explanations which they provide but, despite this they claim that with the introduction of bridge or reducing laws the explanatory value of biological theories can be preserved and captured by a more comprehensive, lower level science. However there are special problems, in principle, when it comes to the reduction of biological science, particularly evolutionary biology. I wish to draw attention to the three most important.

First, strong reduction requires that the higher level theory micro-reduce via bridge laws to the lower level theory. But as J.J.C. Smart points out "there are no biological theories, in [the sense of close-knit theories of physics and chemistry] and not even biological laws. (Though there are biological generalisations)" (Smart, 1961, p. 50). If this is true, that is if teleofunctional explanations resist axiomatisation, then there are no laws to reduce.

Second, another obstruction to this programme is that evolutionary biology, at least, does not admit of natural kinds (this is one of the reasons that there are no universal laws). As Hull puts it "biological species in general are best viewed as historical entities, not as they have been traditionally interpreted as natural kinds" (Hull, 1984, p. 19, Rosenberg, 1985, ch. 7, Mayr, 1981, p. 55-57, Dupre, 1983, p. 326-327, 342). In evolutionary theory species are quantified over and defined in terms of historical lineage. Hull says "The point I wish to argue is that genes, organisms and species, as they function in evolutionary processes, are necessarily spatiotemporally localised individuals. They could not perform the functions which they perform if they were not" (Hull, 1994, p. 195). He also insists that species names, as they appear in evolutionary theory, are best understood as proper names and claims "A taxon has the name it has in virtue of the naming ceremony, not in virtue of any trait or traits it might have" (Hull, 1994, p. 206) The point is that there can be no bridge laws if there are no natural kinds of the traditional sort to which such laws apply.

Third, the reductionist programme insists that the lower level theory holds all the explanatory cards, but explanation is not unidirectional in evolutionary biology. As Kitcher points out, "[u]nderstanding the phenotypic manifestation of a gene, [anti-reductionists] will maintain requires constant shifting back and forth across levels ... one sometimes uses descriptions at higher levels to explain a more fundamental level" (Kitcher, 1991, p. 568-569).

Still, some areas of biology are more susceptible than others to reduction. Successes within the area of molecular biology have often encouraged reductionists. But given that "molecular studies cannot cannibalize the rest of biology" Kitcher, 1991, p. 569)

unless we are given some reason to think that teleofunctional explanations can be so reduced we must reject reductive provincialism.

The third physicalist reply is to sponsor some form of eliminative provincialism. This is the most dramatic response as it attempts to deny that biology is a proper science. The argument is simple and suggests that if biology fails to reduce because it lacks proper laws then it is not of the right character to be considered a science. Eliminative provincialists say of biology that "Its credentials as a natural science are not in order" (Rosenberg, 1985, p. 33). The counter claim is simply that "animals and men are very complicated mechanisms" (Smart, 1961, p. 50). But to date there have been no mechanical laws proposed which could be applied usefully and universally to living creatures. And without such laws the bold statement that the behaviour of animals and men *should* only be explained mechanically is nothing other than sheer prescription.

We might have reason to take this claim seriously if it were supported by some convincing argument that physics, and only physics, yielded objective truth. Such an argument, however, would need to show that the categories of truth and reference were scientifically respectable in the narrow terms permitted by the reductive provincialist. Therefore, it is not an argument open to physicalist who supports any form of eliminativism or instrumentalism about the 'mental'. Nor can it be put forward persuasively by an intentional realist without some detailed account of how intentional phenomena (in general) can be incorporated into the causal-mechanical world view.⁸

Until reductive and eliminative provincialists can meet these objections the most reasonable response, which has been adopted by many post-positivist philosophers of science, is to accept that the many biological sciences are scientifically respectable despite the fact that they employ different kinds of explanations from that of the causal-mechanical sciences. One can simply hold that teleofunctional explanation in biology is autonomous; and this implies, *contra* physicalist claims, that mechanical explanation is not the only type of explanation, scientific or otherwise. It is because I am persuaded of this view that I hold that the arguments in favour of methodological solipsism, individualism, and the principle of autonomy all rest on a limited understanding of the nature of scientific explanation.

When an evolutionary biologist explains the behaviour or evolution of particular phenotypic property of an organism he is perfectly entitled to make use of the external features of the world; such as the organism's the evolutionary history and environment. In fact, their explanation wouldn't be worth much if they didn't. It is useful to ask: Why should psychologists be any more limited in the scope of their scientifically respectable explanations in this regard (cf. Millikan, 1993)? Why should we limit our explanations to the internal causes of behaviour or adopt elaborate, epicyclical theories designed to ensure that we have not done insult or injury to explanations in terms of causal-mechanical laws? If the answer is simply that we wish to be scientifically respectable then this is no answer at all. Science is more than classical mechanics.

Against this background it may seem surprising that physicalism has had the overwhelming popularity it has and escaped serious scrutiny for so long. For example, Sober tells us "Biologists are often surprised to learn how little Darwinism has

influenced philosophy of science in that last one hundred years. They frequently think that the philosophical consequences of evolutionary theory must be so profound that philosophers have chosen to stick their heads in the sand" Sober (1985, pp. 5-6). But he admits this analysis of the situation is not really not fair because "[p]hilosophers now assimilate the fact of evolution with as little difficulty as the fact the earth is not the centre of the solar system ... [but the source of the problem is that] ... philosophy of science this century has been shaped by an interest in physics and mathematics" Sober (1985, p. 6).⁹ And in another, hopeful and sobering quotation, he remarks: "It remains to be seen how radically the philosophy of science will be reinterpreted" Sober (1985, p. 7).

Physicalism, with its emphasis on the methods and explanations of mechanical science is just as limited and one-sided as the Scholastic provincialism which insisted that teleological explanations applied to all things. Given the difficulties with reductionism and the inappropriate nature of physics with regard to the explanation in many areas of biology we would be well-advised to adopt the pragmatic strategy of recognising a plurality of scientific methods and explanations. We must bear in mind that different explanatory strategies will be appropriate to different subject matters. No single science can, or should, claim supremacy in all areas of explanation.

(iii) Neo-Aristotelianism

Aristotle gave us the idea of four different types of causal explanations, efficient, formal, material, and final, because he realised by considering the accounts proposed by his predecessors that any singular explanation would be inadequate. It would not tally with our varied deployment of the notion of cause in our explanations.¹⁰ Thus, he maintained that we must concentrate our attention on one type or the other depending upon which type of question we wish to answer. No single response could fully explain why something really happened. No single type of explanation could claim supremacy (but he did believe that explanations in terms of final causes were the most important).

Even in ordinary case we find evidence for this. No single response to the question 'Why is the bird doing that?' makes sense without a relation to a context of inquiry. For instance, I might answer by saying its brain processes are causing the muscular operations in its wings. If I answered you thus, while we were out hunting, I would either have misunderstood your question or I might be making a joke.¹¹ This does not necessarily make the reply incorrect only inappropriate. It is inappropriate relative to the purpose for which you sought the explanation. Unless one is alert to this, one will misconstrue the nature of explanation. In any event, once we realise that there can be more than one response to 'Why-questions', and that these responses need not be in competition it will become clear that physicalism is too parochial on this score. As Ryle suggested long ago "Mechanism seemed to be a menace because it was assumed that the use of these terms in mechanical theories was their sole use; and that all 'why' questions are answerable in terms of laws of motion" (Ryle, 1949, p. 79).

There is bound to be an outcry from enraged physicalists. "What you done to our Prince?" In my book, Prince Auto is a seventeenth century tyrant. Why should we limit our explanations of behaviour to efficient or proximate causes inside the skin of the agent? Of course internal causes play a crucial role in making behaviour and

action possible but why should we limit our explanations to such causes alone? Where are the physicalist arguments to show that a serious psychology must limit itself in this way?

It is not through a philosophical referendum that physicalism crowned Prince Auto. He was crowned when classic physics became successful in the domain of explaining and predicting the behaviour of inanimate objects. But to concentrate on his achievements in this domain alone and insist, on this basis, that he should rule all others generates a one-sided view of the nature of scientific explanation. If you begin your philosophy of mind with Descartes it is likely you begin your philosophy of science with Descartes as well, in spirit at least. That means you will probably think that if your view is to be scientifically respectable it must (not just can) describe all things mechanistically - all bodies must be mere automata.

We must depose this tyrannical despot who has ruined our philosophical crops for the past three centuries. And in his place I would urge that we do not put another monarch. It is time for a more democratic and liberal explanatory leader - one which recognises autonomous contexts of inquiry. We must not react against mechanism the way mechanists reacted against teleology. Relative to a particular context of inquiry we can expect physics, the biological sciences and descriptive anthropology to play important and useful roles. We might even speak of human contexts of inquiry to remind ourselves that even the sciences themselves are dependent upon human culture and society. Our scientific and philosophical foundations are cultural not physical - human society is our given. Having said this, I wish to separate myself from those who would make linguistic or conceptual analysis their end point - for that, I believe, is to err too far in the other direction. Some questions simply are not about human culture and language although all questions stem from it.

However, resistance to what I will tentatively call Neo-Aristotelianism about explanation is provoked by the fact that philosophers and scientists alike believe that physics should have ontological supremacy even if does not have explanatory supremacy. It is for this reason that many philosophers of biology who accept the autonomy of biological explanations still hold that physics is the final arbitrator of all ontology. This is because no self-respecting biologist wishes to be accused of vitalism (Mayr, 1981, p. 60, 63-64, Kitcher, 1991, p. 567, Rosenberg, 1985, p. 24-25, Gasper, 1991, p. 546, Hull, 1984, p. 31). However even if we recognise that there are different types of explanation it is simply open for us to accept a physicalist ontology - or so it may seem. But this is a course I no longer recommend.¹² For without any methodological or explanatory unity between physics and biology, such a commitment looks like nothing other than an unwarranted pledge of ontological allegiance. In Putnam's terms it is metaphysical comfort. Rosenberg notes this as well.

"To conclude that all living systems are merely macromolecules in motion would be a merely spiritual consolation to materialist biologists if there are practically insuperable obstacles to explaining any part of their behaviour with all the power and precision that physics brings to bear on non-living things" (Rosenberg, 1985, p. 29).

But if we deny the metaphysics of physicalism are we not thereby committed to vitalism or dualism? No. Especially not if one views scientific theories pragmatically and instrumentally. If we are not convinced by the claims of scientific realists then we

need not be committed to vitalism in holding that the biological sciences, or commonsense psychology for that matter, are autonomous. A pluralist about explanation could hold that ontology is not objective in any case; they might hold that ontology is always relative to a theory or scheme. With these caveats in place I wish to look briefly at two different contexts of inquiry.

(iv) Teleology Revisited

Responses to the problem of explaining representational content have taken a promising turn in recent years. Many philosophers of psychology are turning away from the constraints of purely mechanistic theories of content towards the teleologically-based accounts. We can see this swing of attention in the recent work of Dretske (1988, 1990), McGinn (1989), Millikan (1984-1989), Van Gulick (1990) and Papineau (1984, 1987)

However, Fodor (1990) makes what appears to be a strong attack on teleological attempts to explain content. As he sees it they suffer from an inability to resolve what he call the disjunction problem.¹³ The disjunction problem arises because if one holds that the meaning of a subject's mental symbols covary lawfully with the external causes of such tokenings then whatever causes such tokenings must be included in the meaning. An example will help make this clearer. Mahoney tells the story in relation to thinking that "FELIX IS A CAT" on one particular evening. He is sitting watching television when his dog, Fido, rushes through the room. But it is dark and as far as he can tell it was Felix his cat that passed by. In fact, Fido causes him, in this case to think of Felix. And this generates the disjunction problem.

"For the point of a causal semantics of the mind is that a mental representation means whatever is its reliable cause. Since Felix's being a cat or Fido's being a dog is a more reliable cause of 'FELIX IS A CAT' than is Felix's being a cat, the content of 'FELIX IS A CAT' ought - by the causal theory's lights - to be that Felix is a cat or Fido is a dog [the disjunction]. And with but a little nudge it now follows from the causal theory that, though contrary to fact, simple mental representations can never misrepresent" (Mahoney, p. 4, emphasis added)

As Baker points out "the disjunction problem, first identified by Fodor himself, is both deep and pervasive" (Baker, 1991, p. 29). But, for whom? As Mahoney makes clear that it is a problem for defenders of crude causal theories of content. And I have already argued that to employ a purely causal account in the explanation of animal behaviour would be inappropriate. We must agree with Millikan in thinking that "on a teleological analysis, the belief box will have to be defined by its teleofunction, not by its causal disposition, not by its mechanofunction" (Millikan, 1991, p. 156).

We need to ask our questions about the purpose or functions of representations in the context of the larger role they play in an organism's response. Why is this so? Take the infamous case of a frog who can't tell the difference between flies, black dots, flee-bees, or bee-bees. In cases of what we would consider error the frog has failed to distinguish between what we know to be nutritious and what is not. And we can give an account from evolutionary theory to further explain what it was in the past that caused this targetted response in the frog's ancestors to be selected and handed down.

It is directed at flies and nothing else because only the consumption of flies will have benefited these creatures. Given its stomach-design, as per his lineage, the frog's tongue-action should be directed at flies. If he snaps at a black dot something has gone wrong from the historical point of view. The teleological approach allows us to define error by contrasting what the frog's is currently directed at (in extension) and contrast this with what it is supposed to be directed at. The later is decided by appeal to the evolutionary history and natural environment of the organism's forefathers. In short, perceptual error, of this sort, is best explained teleologically, where such teleology is historically described by the organism's evolutionary background.

Fodor is wrong to attack teleological accounts with the disjunction problem. Disjunction is a problem for those who support causal theories of content. It eloquently reveals the inadequacy of such accounts by exposing the fact that they leave no room for normative concepts such as error. And even Fodor admits the teleological story "is sensitive to the plausible intuition that errors are cases where something has gone wrong". Nevertheless, as Kim Sterenly who is convinced that teleological accounts are the right story to tell about content, recently writes "[i]t is one thing to give teleological accounts of innate structures; quite another to give teleological accounts of the propositional attitudes" (Sterenly 1990, p. 128).¹⁴ This is well observed.

(v) Humanity Revisited

I have argued that relative to certain contexts of inquiry mechanical and teleological explanations are pragmatically useful - but ultimately, I believe, that they are both inappropriate when employed as models for full-fledged explanations of human action. Some inclined towards causalism will argue, immediately, that there is a serious problem in this response which has nothing to do with science. For they hold that in ordinary language we do speak of beliefs and desires as being causally efficacious. They say that any examination of ordinary discourse shows that reasons are causes - especially if cause is to mean, as it does in the Oxford English Dictionary, "what produces an effect". For example;

Why did you hit that man? I thought he was poking fun at me and I wanted to teach him a lesson.

Why did you eat that cake? I wanted some chocolate and I thought the icing was chocolate.

These are paradigms of causal explanations if, by causal, we mean "what made something happen".

That we talk in this way and give these types of explanation is, I agree, beyond dispute. But this, in and of itself, does not mean that the causalist conclusion is correct. As I have already pointed out, Aristotle noted we use the concept of cause in different ways. This was in part an observation about everyday usage. One has but to examine the way in which the terms are used in these cases to discover that there is a difference between our talk of reasons and our talk of causes in the strict, mechanical sense. As Medlen says, "It is certainly true that we use 'cause' in speaking about the

actions of agents, but we can no more infer from this verbal consideration that actions are the Humean effects of events than we can from the etymological derivation of the term 'motive'" (Melden, 1961, p. 208)

What is overlooked by the causalist is that there are "two quite different senses in which we say that it happened 'because' so and so was the case" (Ryle, 1949, p. 89). Thus, "When we ask 'Why did someone act in a certain way?' this question might, so far as its language goes, either be an inquiry into the cause of his acting in that way, or be an inquiry into the character of the agent which accounts for his having acted in that way on that occasion" (Ryle, [1949, p. 89]). To cite the first type of response is to cite a cause, to cite the second type of response is to cite a reason. As Meldén notes "What this comes down to is that if we say that certain muscle movements take place - this is how the arm gets raised ... we are not so much saying what the agent is doing as describing what is taking place" (Meldén, 1961, p. 24). A physiologically indistinguishable arm movement could occur in a number of different contexts. I might be raising my arm on one occasion in order to answer a question, to get my hat from a high shelf, to wave to my friend, or to signal to my enemy to cease. Such contexts make all the difference to how we describe and explain the action when we speak of reasons. Viewed narrowly the physical movements, the efficient, proximate causes of my behaviour, are of no interest to us in the context of inquiry that asks after reasons. We could imagine our physiology was underpinned by an alternative physics with a different ontology and it would have no effect on the reasoned-explanation. Mele recently (1992) reminds us of the important distinction between narrow and broad behaviour. Narrow behaviour for him "is mere bodily motion, in abstraction from its causes and effects" (Mele, 1992, p. 18); broad behaviour takes into account the subject's environment, history and the context of his behaviour. He gives a host of examples to support this distinction. I will cite only one.¹⁵

"When I unlocked my office door this morning, I moved my right arm, wrist, and hand in some determinate way or other. However, those motions themselves, in abstraction from their relations to other things, do not constitute my action of unlocking my office door. Precisely those motions, narrowly construed, might have occurred without my having unlocked my office door." (Mele, 1992, pp. 17-18)

Citing a cause for my movements is not the same as citing a reason for my action. The fact that we can offer both explanations in the same situation by giving different answers to what is apparently the same why-question indicates that reasoned explanations and causal explanations is what is apt to confuse us. As Wittgenstein points out, "[t]he difference between the grammars of 'reason' and 'cause' is quite similar to that between the grammars of 'motive' and 'cause' ... [t]he double use of the word 'why', when asking for the cause and asking for the motive, together with the idea that we can know, and not only conjecture, our motives, gives rise to the confusion that a motive is a cause" (Wittgenstein, 1958, p. 15). But he also notes the further difference that to cite a reason for our action involves no hypothetical inference, no knowledge of the inner workings of our bodies. But to offer a causal explanation, in the mechanical sense, is in contrast precisely a conjecture; a hypothesis. Nor will it help to think of reason explanations as a kind of teleofunctional explanation. It would be as great a mistake to employ biological or evolutionary explanations to full-fledged human action as it would to attempt to employ mechanical explanations to fully explain the behaviour of animals. For as

Fodor critically inquires of the ambitious teleofunctionalist "[Does] doubting that the Dodgers will ever move back to Brooklyn have a Normal function[?]"¹⁶ Beliefs about the Dodgers have a particularly cultural flavour and we must treat them as such. To understand what I mean when I air a doubt about the Dodgers you have to understand a great deal about baseball, New York, and so on. You will not understand the content of this doubt by concentrating on my biological functions any better than you will by focusing on the proximate, physiological causes.

What would license us to seriously suppose that every belief or desire that we currently harbour has a function which was selected for a particular biological and survival related function? It would be a mistake for those sympathetic to teleofunctional accounts of basic intentionality to confuse the aspects of mind that concern the cultural and social with those that are best explained by the evolutionary sciences. A great deal of human action and behaviour cannot be understood, let alone explained, in terms of direct, unique biological ends. To force a harness on such explanations, as might be attempted by sociobiologists, is comparable to mechanist attempts to explain in strictly causal terms.

What is crucial to notice about reasons, and human attitudes in general, is that without a human context they are inappropriate. It is for this reason that Melden says "Here I can only repeat that these locutions are intelligibly employed only in the context of human action" (Melden, 1961, p. 216). Or in Ryle's words "An action on the part of one agent could not have been one of spying or applauding, unless it had to do with the actions of another agent; nor could I behave as a customer, unless you or someone behaved as a seller" (Ryle, 1949, p. 191). Consider again the case of the arm-raising as cited by Melden:

We need to know, in short, that we have an agent, a motorist, who is driving and whose action of raising the arm is to be understood in terms of the appropriate rule of the road as a case of signaling a turn as the crossroad comes into view. But in that case we have left behind all reference to hypothetical occurrences in the nervous system, for now we are back to the scene of human action (Melden, 1961, p. 210).

In the light of such cases we are able to see where our starting points are. We know what it takes to be a buyer or a law abiding motorist, and this is what allows us to explain our behaviour in the light of reasons. There is no scientific hypothesis here which will tell me the real reason I am writing this very page. Unless very much deceived I know why I am doing it already - there is no need to guess. If I advance a scientific hypothesis about the causes of my fingers tapping the keys I would be giving a very different response to the question "Why are you doing that?".

Here nothing is hidden; it is because I understand him, not because I am aware of events transpiring in some alleged mechanism of his mind or body, that I am able to say what he will do (Melden, 1961, p. 208).

We need to put the emphasis in the right place in our inquiries into human behaviour. We must remind ourselves of the purpose of our explanations. Reasons are not mechanically identifiable causes. Nor are they rational ones. They are not biological functions or dispositions. They are explanations that appeal to our character and

situation and they depend on a cultural context. When we cite the reason for someone's behaviour we are not interested in the penultimate causal factor in the series as we might if we were seeking a purely mechanical explanation. Nor are we interested in a person's biology. We are interested in the person. We must remind ourselves of the purposes for which we seek explanations. I hope in that doing so we are also reminded that that we are not mere machines after all. And although we are animals, we are not *only* such.

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ENDNOTES

1 Fodor describes methodological solipsism in Fodor (1981, p. 239) but denies that it is identical to Prince Auto in Fodor (1987, p. 42). Despite this it is clear enough that the spirit of central decree is the same.

2 cf. Burge (1986, pp. 3-4).

3 Stich (1982, p. 197) and (1990, p. 348).

4 Dennett (1985), (1987), (1991).

5 Davidson (1980).

6 "The mechanical outlook is as noticeable in biology as in physical science during the 17th century. The tendency was to regard living things, particularly animals, as machines - to study them with reference to the new principles of hydrostatics and dynamics...Descartes regarded the bodies of men and animals as nothing more than elaborate machines, entirely subject to physical law." (Hull, HPS, p. 270).

7 cf. Stich (1983, 1990), Churchland (1987), Fodor (1968, 1975), Dennett (1985), Davidson (1980).

8 Putnam makes these points quite strongly in Putnam (1989).

9 Descartes great achievements in science lie in physics not biology. Thus it should not really surprise thinkers like Dawkins that "[p]hilosophy and the subjects known as 'humanities' are taught almost as if Darwin had never lived" (Dawkins, 1989, p. 1).

10 "...the truth is that the doctrine of the four causes does not consist in a recondite theory of fundamental metaphysical principles...we are in fact confronted with the results of an analysis of linguistic usage. Cause (aition) has several meanings in ordinary usage (195a29). Strictly speaking, therefore, we are dealing here not with four causes, but with four senses in which we speak of causes." (Wieland, 1975, p. 147).

11 Think of the old joke "Why did the chicken cross the road?". What makes it possible to give more than one answer to this question. As Wittgenstein says " - Let us ask ourselves: why do we feel a grammatical joke to be deep? (And that is what the depth of philosophy is.)" (Wittgenstein, PI 111).

12 I wrongly favoured this response in Hutto (1991).

13 Fodor, (1990, p. 71).

14 As Stich tells us, that "we cannot conclude that evolution will result in systems that are optimally well designed (or nearly so)". This is so because according to evolutionary theory nature is frugal - if there is no pressure to develop a perceptual system to discriminate between flies and black dots then such a perceptual ability will not be selected. Or put another way, frogs with this crude perceptual mechanism will do no worse than frogs with a more sophisticated perceptual ability in environments where flies are not generally black dots.

15 Mele gives many more such examples in Ch. 1 of Springs of Action. Another good example of the same narrow behaviour differing from broad behaviour can be found in Melden's example of the boy who can be described as both voluntarily and involuntarily surrendering himself to the police, depending on perspective we adopt (cf, Melden, 1961, pp. 219-221)

16 Fodor (1990, p. 65).

