When does 'Folk Psychology' Count as Folk Psychological?

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Abstract

It has commonly been argued that certain types of mental descriptions, specifically those characterized in terms of propositional attitudes, are part of a folk psychological understanding of the mind. Recently, however, it has also been argued that this is the case even when such descriptions are employed as part of scientific theories in domains like social psychology and comparative psychology. In this paper, I argue that there is no plausible way to understand the distinction between folk and scientific psychology that can support such claims. Moreover, these sorts of claims can have adverse consequences for the neuroscientific study of the brain by downplaying the value of many psychological theories that provide information neuroscientists need in order to build and test neurological models.

6. Conclusion

1 Introduction

During the 1980s and early 1990s, a battle was waged amongst philosophers of mind for the soul of modern psychology and cognitive science (although the first salvo in this battle was fired long before this). This battle was to determine whether traditional psychological vocabulary such as 'beliefs', 'desires', and 'intentions', had a legitimate place within the scientific study of psychology. The attribution or ascription of these propositional attitudes to agents as a means of predicting and explaining behaviour became widely known as 'folk' psychology, to be contrasted with the more mechanical explanations emerging from neuroscience and cognitive science regarding the transformation of sensory inputs into behavioural outputs. The question of whether folk psychology could find a place within this new scientific framework was a topic of much debate, with philosophers and psychologists taking up arms on different sides of the battlefield.

While the battle itself is not being fought to the same degree it was a few decades ago, its effects have been widespread, and continue to influence scientists and researchers working today in numerous different academic disciplines. From comparative and animal psychology (Papineau & Heyes [2006]; Shettleworth [2010]; Penn [2012]), to psychiatry (Murphy [2014]), to economics (Guala [2012]), to criminal law (Commons & Miller [2011]; Morse [2011a], [2011b]), the assumption that propositional attitude ascriptions (hereafter PAAs) are rooted in a folk, and not a scientific, understanding of the mind is commonplace. Meanwhile, the fact that

PAAs are frequently invoked as part of scientific theories within these domains has not discouraged many theorists from insisting that their usage in such contexts remains folk psychological. It is argued that because mentalistic terms such as 'beliefs' and 'desires' get their meaning from a pre-scientific conceptual framework, to import them into psychological theories is still to adhere to a folk psychological account of the mind. In order to truly be validated as scientific psychology, PAAs must first be shown to either successfully identify natural kinds, be mapped to cognitive/neurological states of the brain, or prove indispensable to scientific practice. Failure to meet any of these criteria is a failure to secure itself within legitimate scientific psychology, even if employed within psychological theories in domains like social or comparative psychology.

In this paper, I argue that such views are ultimately unsupportable, and can have adverse consequences for the neuroscientific study of the brain. My intention is not to reignite the war, but to demonstrate that PAAs need not validate their role within current scientific psychology in order to be scientific, as opposed to folk, psychology. Instead, their very presence within current psychological theories guarantees their status as scientific psychology. This is the case irrespective of whether they originated from a pre-scientific conceptual framework, or whether they will ever find a place within our best neuroscientific and cognitive accounts of the mind. The only criterion that is relevant for determining whether the usage of mentalistic terminology is folk or scientific is whether it is informed by scientific theories and experimentation in that context. So long as the application of a mentalistic term like 'belief' or 'intention' is in accord with legitimate scientific methodology and practice, then that usage is not folk psychological and is as scientific as any other term used in our best theories of psychology and neuroscience.

Moreover, I propose that classifying certain mentalistic terms as folk psychological when

they are embedded in scientific theories can result in the tendency to be dismissive of those theories, or to view them as having limited scientific value. Yet psychological theories which invoke PAAs often provide a wealth of information regarding the sorts of behaviours performed by cognitive systems, the conditions under which particular behaviours occur, and the time frame in which behaviours are carried out. This information puts essential constraints on how to build and test models needed in the study of the system's underlying neurological mechanisms. The mere fact that these psychological theories and models employ certain mentalistic terminology in the course of generating such data is not relevant to the value of this data in helping us to identify and study the causal mechanisms of the system.

In Section 2 of this paper, I examine the tendency amongst many theorists to view scientific theories which invoke traditional mentalistic terminology as still engaging in folk psychology. In section 3, I detail the different arguments that could be offered to defend such a claim, and demonstrate why each fails. In section 4, I outline a more helpful way of thinking about the distinction between folk and scientific psychology that better accounts for the usage of PAAs within contemporary psychology. Lastly, in section 5, I demonstrate why mislabelling PAAs as folk psychological in scientific contexts can have negative consequences for the neuroscientific study of the brain.

2 Propositional Attitudes in Scientific Theories

The idea that certain types of mentalistic descriptions, specifically those characterized as propositional attitudes, are part of folk psychology is a view that has been widely held in the philosophy of mind. As Matthew Ratcliffe notes:

> [There is] an account of 'commonsense' or 'folk' psychology [that] is routinely accepted, according to which its central element is the attribution of intentional states, principally beliefs and desires, in order to predict and explain behaviour. ([2007], p. 223)

For some explicit examples, consider E.J. Lowe who defines folk psychology as 'our propositional attitude vocabulary', and our 'belief-desire discourse' ([2000], p. 62). Or Stephen Stich, who defines folk psychology as the attribution of any psychological state that is 'characteristically attributed by invoking a sentence with an embedded "content sentence".' ([1983], p. 5). Likewise, John Heil defines folk psychology as 'the practice of explaining behavior by reference to the propositional attitudes.' ([2004], p. 152). Similar claims can be found in the works of Chuchland [1981]; Dennett [1987]; Ramsey, Stich & Garon [1990]; Morton [1996]; Haselager [1997]; Bickle [2003]; Godfrey-Smith [2005]; Gauker [2009], and many others.[1]

Interestingly, this idea has not been limited to philosophy, and in recent years has found its way into numerous other scientific and academic fields, such as criminal law, economics, social psychology, comparative psychology, and psychiatry. This is particularly noteworthy because many of these domains frequently employ PAAs as part of scientific theories and models. Is the application of PAAs in these scientific contexts still to be considered folk psychological, or does our propositional attitude vocabulary and belief-desire discourse count as

[1] It is worth noting that for the purposes of this paper, I neither presuppose, nor reject, the theory-theory of mind (as opposed to simulation-theory, or the more embodied accounts of folk psychology). The question of whether the way in which we understand others in daily life requires the use of PAAs is, for my present purposes, irrelevant. The relevant question is: why are so many convinced that the use of PAAs are part of a 'folk' understanding of the mind instead of a scientific one?

full-fledged scientific psychology in such cases, with the same scientific status as discourse about neurological mechanisms and chemical interactions? Many have proposed that the presence of PAAs within such psychological theories is insufficient to lift them out of the realm of folk psychology and into the domain of scientific psychology. For a straightforward example, consider the following claim from Derek Penn:

> Folk psychology has plagued every domain of comparative psychology (Penn and Povinelli [2007a]; Penn *et al*. [2008]). Research on animals' ToM [Theory of Mind] abilities has, however, been held hostage by folk psychology to a degree far beyond any other domain. Effectively, most comparative researchers in this domain are not practicing comparative cognitive psychology but rather 'comparative folk psychology'; that is, the study of nonhuman minds from a folk psychological perspective. ([2012], p. 257)

Penn defends this claim by arguing that comparative psychologists import mentalistic vocabulary from a pre-scientific conceptual framework of the mind into their analysis of animal behaviour, instead of providing an account of said behaviour in terms of more accurate computational or neurological theories of the mind. In Penn's own words:

> Comparative psychologists regularly claim that animals have an 'understanding of' or 'insight into' some folk psychological concept in order to falsify claims that the animal's cognitive processes are rule-governed and unconscious. […] Nearly all the most prominent claims in support of attributing a ToM to nonhuman animals are framed using folk psychological idioms (e.g., 'chimpanzees know what their groupmates do and do not

> know,' ' chimpanzees can distinguish between an experimenter that is unwilling or unable to give them food,' 'scrub jays can project their own experience of being a thief onto the observing bird') without any attempt to cash out these claims at a computational, algorithmic, or neural level of explanation. ([2012], p. 256)

Note that according to Penn, PAAs are still folk psychological when employed as part of explicit theories and models within comparative psychology. In order to reach the status of scientific psychology, they must first be a characterizable in terms of more acceptable neurological or cognitive accounts.

This assumption that PAAs remain folk psychological even when used as part of explicit theories within comparative or animal psychology can likewise be found in the work of Papineau & Heyes [2006]. They claim that:

> Perhaps nowadays we understand the question ['are animals rational?'] in terms of two styles of cognitive explanation. This certainly seems to be the way that many psychologists understand the issue. On the one hand lie 'rational' explanations of behaviour, explanations that advert to norm-governed reasoning involving belief-like representations. On the other side lie non-rational explanations, in terms of 'behaviourist' or (more accurately) associative psychological processes. […] We suspect that the rational–associative dichotomy is just Descartes dressed up in modern garb. In place of Descartes' immaterial mind we have the accolades of 'folk psychology', and in place of his brute matter we have 'associative machines'. ([2006], p. 188)

Papineau & Heyes go on to argue that this dichotomy is ultimately a false one, despite many advocates within psychology arguing for one side or the other. What's important to note for our purposes is that for Papineau & Heyes, many psychologists who adopt rational explanations of behaviour within their theories are still employing a folk psychological account of the mind. For instance, regarding the interpretation of the behaviour of birds in scientific experiments, they claim that…

> …it is easy, perhaps irresistible, to interpret sensitivity to demonstrator reward in folk-psychological terms. We naturally assume that the birds who imitated did so because they *wanted* food and *believed* that performing the same action as the demonstrator would enable them to get it. ([2006], p. 188)

But what is it exactly that makes mentalistic terms like 'wanted' and 'believed' folk psychological terms when employed in such a manner by comparative psychologists? Clearly comparative psychologists who come to such an 'irresistible' conclusion are still generating an interpretation of the data they've gathered by working within the confines of proper scientific practices, forming theories based on empirical observation and experimentation. So why then are these not clear cut cases of scientific psychological terms? What justifies Papineau & Heyes's insistence that this is a folk psychological interpretation of the behaviour of the birds? This is a question they never explicitly address, however it is implied that this 'folk psychological' interpretation of animal behaviour is at odds with the more mechanical understanding of the mind provided by cognitive science and neuroscience.

This view that PAAs need to do more than merely be part of scientific theories in order to count as scientific psychology can likewise be found in other academic domains. Consider criminal law. According to Stephen Morse:

> Brief reflection should indicate that the law's psychology must be a folk psychological theory, a view of the person as a conscious (and potentially self-conscious) creature who forms and acts on intentions that are the product of the person's other mental states such as desires, beliefs, willings, and plans. We are the sorts of creatures that can act for and respond to reasons, including legal rules and standards that are expressed and understood linguistically. ([2011a], p. 531)

In a similar vein, he claims that criminal law presupposes a 'folk psychological view' because it explains human behaviour by appealing to 'mental states such as desires, beliefs, intentions, volitions, and plans' (Morse [2011b], p. 378), and that certain defences in law 'involve folk psychology because they are based on mental states, including desires and beliefs.' ([2011b], p. 379).

Of course, it is certainly possible that criminal law is based on folk, and not scientific, psychology. However, it is something quite different to insist, as Morse does, that criminal law presupposes folk psychology because it attributes certain kinds of mental states to people. After all, many scientific theories in psychology are similarly based on mental states in exactly this way, often invoking mental states such as desires, beliefs, intentions, volitions and plans. What are we to say about these cases? Morse himself acknowledges that scientists do indeed appeal to such mental states in their theories and often debate how best to define them (Morse [2011a], p.

530). Yet, despite this, he insists that such usage still counts as folk psychological on the grounds that folk psychology requires 'only that human action is in part causally explained by mental states' (Morse [2011a], p. 530). Yet, what could justify such a claim?

According to Morse, the folk psychological status of such mentalistic terms stem from the fact that they originated from a pre-scientific understanding of the mind and do not fit with our current best theories from neuroscience and cognitive science. As he notes, 'many scientists and philosophers of mind and action consider folk psychology to be a primitive or pre-scientific view of human behaviour.' (Morse [2011a], p. 532). Thus even if psychologists employ PAAs as part of their current theories, given that their meanings are still rooted in a pre-scientific understanding of human behaviour, and are at odds with emerging neuroscientific/cognitive evidence, their usage remains folk psychological when imported into those scientific contexts.

This kind of argument is not uncommon, and can be found elsewhere in philosophy (Churchland [1981]; Ramsey, Stich & Garon [1990]), comparative psychology (Shettleworth [2010]), criminal law (Commons & Miller [2011]), and economics (Guala [2012]). To sum up, many have claimed that being part of explicit scientific theories and models in domains like social psychology and comparative psychology does not by itself grant PAAs the status of *scientific* psychology. I intend to demonstrate that such claims are incorrect, and that the intuitions which underlie them confused. In order to demonstrate why, I will examine in detail the different arguments that could be used to justify the claim that PAAs remain folk psychological when used as part of theories and models in domains like social psychology or comparative psychology.

3 Where the 'Folk' and the 'Scientific' Part Ways

I propose that there is no plausible way to carve the folk/scientific distinction so that PAAs, as used by current psychologists, fall on the side of folk psychology. In order to see why, let us consider the different ways one might try to argue for the claim that PAAs remain folk psychological even when embedded in psychological theories or models. One rather straightforward reason for believing PAAs are folk psychological can be understood as follows:

1. Even if descriptions which employ PAAs are used as part of theories in scientific domains like social psychology and comparative psychology, the meaning of mentalistic terms like 'beliefs' and 'intentions' are entirely determined by their place within the folk conceptual framework from which they originated.

Unlike scientific concepts, whose meanings are determined by their place within scientific theories, the meanings of the mentalistic terms found in PAAs appear to be determined by their place within the folk theories from which they sprang. This is why, even if they are used within scientific domains like social or comparative psychology, the vocabulary itself remains folk. Paul Churchland, for instance, argues that PAAs are folk psychological because they are rooted in a 'common-sense conceptual framework for mental phenomena' ([1981], p. 69). This point is echoed by Daniel Dennett, who argues that the folk status of PAAs is grounded in the fact that the mentalistic terms they employ originated from ordinary language:

> Since the terms 'belief' and 'desire' and their kin are parts of ordinary language, like 'magnet', rather than technical terms like 'valence,' we must first look to 'folk psychology' to see what kind of things we are being asked to explain. ([1987], p. 46)

But if this is indeed a good reason for labelling PAAs 'folk', then it would seem to commit us, at least *prima facie*, to the idea that most of contemporary physics is similarly folk, and not scientific, physics.

Consider: Long before there existed anything like a rigorous scientific account of physics, we commonly explained and predicted the behaviour of physical systems by employing terms like 'time', 'space', and 'motion'. These terms were rooted in a common-sense conceptual framework for physical phenomena, and were likewise part of ordinary language. From this, do we therefore conclude that any use of the terms 'time', 'space', or 'motion' within the context of physics today is thereby an instance of folk physics, in virtue of originating from an undeniably folk conceptual framework? If so, then the vast majority of contemporary physics would constitute folk physics given their common use of such vocabulary. And considering that even our best neuroscientific accounts of the mind similarly make use of the concept of 'time', then neuroscientific theories would be as much a part of folk psychology as PAAs are.[2] Yet most would deny that contemporary neuroscience is folk psychology. But if we are to deny this, then what spares terms like 'time' and 'space' from a folk fate when imported into scientific contexts, but not the mentalistic terms found in PAAs? Clearly the meanings of 'time' and 'space' originated from ordinary language usage just as 'beliefs', and 'desires' did.

[2] After all, such neuroscientific theories would be part of a psychology that predicts and explains human behaviour by employing concepts which are rooted in a folk conceptual framework (i.e. 'time').

One plausible response is that concepts like 'time', 'space', and 'motion' have been refined and altered by scientific practices, theories, and experimentation in physics. The notion of 'time' used by scientific physics is different from the folk notions that spawned it. The sorts of inferences we draw about time in folk contexts are different from the sorts of inferences we draw about time in the context of contemporary physics. For instance, in folk contexts it is common to believe that two events either happen simultaneously, or they do not. Yet we know that in the context of scientific physics, such a generalization does not apply. It is for this reason that concepts like 'time' are no longer folk when employed in contemporary physics.

This is certainly a reasonable response, but it is one that applies equally well to our use of the mentalistc concepts embedded in PAAs. Scientific domains like social psychology, comparative psychology, and developmental psychology often draw very different sorts of inferences from the ascriptions of propositional attitudes than do people in everyday folk contexts. As a straightforward example, consider the Theory of Planned Behaviour (TPB) in social psychology (Ajzen [1985], [1988], [1991]). The TPB is a model of human behaviour that has enjoyed a great deal of predictive success regarding a range of human behaviours.[3]

The TPB predicts human behaviour by ascribing propositional attitudes such as beliefs and intentions to agents, but also involves the ascription of things like subjective and cultural norms, as well as a perceived sense of behavioural control. It likewise takes various environmental conditions into account when generating predictions. In folk contexts, however, our use of PAAs to predict and explain behaviour is very different. Most people in folk contexts draw inferences about human behaviour directly from the ascriptions of propositional attitudes

[3] For empirical studies on the predictive successes of the TPB, see: Ajzen & Driver [1992]; Blue [1995]; Connor & Sparks [1996]; Godin & Kok [1996]; Hausenblas *et al*. [1997]; Armitage & Conner [2001].

while completely ignoring cultural and environmental factors (Ross & Nisbett [1991]).[4] The TPB on the other hand, is sensitive to both in a way that folk accounts are not. This means that we draw different inferences from PAAs in folk contexts than we do in scientific contexts. What this shows is that folk concepts like 'beliefs' have likewise been refined and altered by psychological practices, theories, and experimentation. As a result, just as in the physics case involving 'time', the notion of 'belief' used by scientific psychologists is different from the folk notions that spawned it.

Some may object that the notions of 'belief' used by the different branches of psychology are still similar enough to the everyday conception that it still warrants being considered folk psychological. But this line of reasoning quickly becomes problematic. Presumably, the notion of 'time' used by physics was refined and changed gradually. And so at what point did physics officially switch from being 'folk' to 'scientific' in its usage? How different did the concept need to become before it was no longer folk physics, and became a legitimately scientific term? Are engineers still doing folk physics when they make reference to time in the context of Newtonian mechanics (as opposed to employing the more accurate quantum mechanics)? There is still a family resemblance between the scientific notion of 'time' and the colloquial folk notion. If there were not, then why continue to use the term at all? Why not abandon it as we did 'phlogiston' and 'ether'?

The assumption that there must be some particular sufficient change to a concept in order for it to officially shift from 'folk' to 'scientific' ignores the important observation made by Wilfred Sellars that scientific terminology is an extension of ordinary language ([1956]). Scientific theories and practices are still generated within a natural language. Assuming that we

[4] This is why the Fundamental Attribution Error is so prevalent.

can draw a definitive and clear distinction between 'scientific' language and 'folk' language is therefore something we must be cautious of.

Perhaps one might argue that the physics case is importantly different from the psychology case in that a concept like 'time' still has an important role to play in our best scientific practices, while the same is not true of PAAs in scientific psychology. Thus, perhaps we can offer a second possible argument for the classification of PAAs as folk psychological, even when used as part of theories in psychology and cognitive science:

2. Even if PAAs are used as part of theories and models in scientific domains like social and comparative psychology, their meanings are (at least partially) determined by their place within the folk conceptual framework from which they originated, and such mentalistic terminology is not necessary for a correct scientific understanding of the mind.

There are different ways in which such a claim might be cashed out. For instance, we can make sense of this claim in the following ways:

2(a). PAAs do not correctly describe the actual cognitive states and processes of the mind. In other words, they do not denote natural kinds.

2(b). PAAs are embedded in theories of the mind which are known to be false.

2(c). PAAs are dispensable to the scientific study of the mind.

I propose that each of these possibilities, even if true, is unhelpful for making sense of the folk/scientific divide. Let us examine each in turn to see why.

According to 2(a), science is explicitly in the business of identifying natural kinds, and so psychological concepts must identify natural kinds in order to validate their place within a scientific psychology (for some examples, see: Sehon [1997]; Devitt & Sterelny [1987]; Griffith [2004]; Machery [2009]). Michael Devitt and Kim Sterelny, for instance, claim that 'if psychological kinds are not natural kinds, then folk cognitive psychology cannot be protoscience [or science]' ([1987], p. 242). If one accepts these sorts of views, then the question of whether PAAs should be included as part of scientific psychology depends on their ability to successfully denote natural kinds.

For the sake of argument, let us suppose that natural kinds genuinely exist (a topic of much debate in the philosophy of science), and let us also suppose that propositional attitudes do not denote natural kinds. Could this validate the claim that PAAs are folk psychological, even if they are used as part of theories in psychology and cognitive science? I propose not. This is because, even if natural kinds genuinely exist, it is simply false to suggest that science is primarily in the business of identifying them, or that theories and models are *only* considered scientific when they do so.

Examples abound in science of descriptions and models that do not attempt to characterize the natural kinds of systems. Take, for example, Hodgkin & Huxley's mathematical model of the action potential in the squid giant axon ([1952]). The equations developed by Hodgkin & Huxley have been extremely influential in physiology and neuroscience. As Carl Craver notes:

> [The equations] summarize decades of experiments. They embody a rich temporal constraint on any possible mechanism for the action potential. They allow neuroscientists to predict how current will change under various experimental interventions. They can be used to simulate the electrophysiological activities of nerve cells. They permit one to infer the values of unmeasured variables. And they constitute potent evidence that a mechanism involving ionic currents could possibly account for the shape of the action potential. ([2006], p. 363).

Despite providing these scientific virtues, the model does not attempt to identify the natural kinds that constitute the mechanisms producing the time course of the action potential. Instead, the model is merely a mathematical description that remains agnostic as to the possible underlying implementation of the system. As Hodgkin himself notes:

> As soon as we began to think about molecular mechanisms it became clear that the electrical data would by themselves yield only very general information about the class of system likely to be involved. ([1992], p. 291)

Despite this, Hodgkin & Huxley's model is hardly considered unscientific simply in virtue of not identifying natural kinds. On the contrary, Hodgkin & Huxley's model won them the Nobel Prize in physiology. Thus I propose that the question of whether PAAs correspond to natural kinds is largely irrelevant to the question of whether they are folk or scientific, since many uncontroversially scientific theories and models do not identify natural kinds at all (many

statistical models, for instance, do not). Perhaps the problem is that PAAs do not have the same value to scientific methodology that Hodgkin & Huxley's model did?

This brings us to 2(b). Perhaps the problem with PAAs is that they fail to be scientific in virtue of the fact that they belong to an incorrect theory of the mind? The problem with this idea is that the complexities involved in scientific representation often require idealizations, simplifications, and distortions which result in false theories that are nevertheless useful and important in science (for details, see: Teller [2001]; Batterman [2002]; Woods & Rosales [2010]). To suggest that incorrect theories are unscientific is to ignore a good deal of genuine scientific practice which knowingly works with false characterizations of systems for numerous pragmatic reasons.

More importantly for our purposes, however, is the fact that false theories are not necessarily folk theories. Quantum mechanics may have replaced Newtonian mechanics as our best physical theory, but this does not retroactively make Newtonian Mechanics folk physics. Likewise, if quantum mechanics is overturned at some point in the future, this would not *ipso facto* mean that quantum mechanics was a folk theory all along. Thus, even if PAAs are embedded in psychological theories that are false, this is insufficient to argue that the theory is folk; especially given the fact that, as noted above, the use of PAAs as part of theories and models in domains like social psychology and comparative psychology differ from their use in colloquial everyday contexts. They may prove to be part of false scientific theories, but they would still be part of scientific psychology nonetheless.

Let us turn finally to 2(c). Perhaps PAAs are not part of scientific psychology because our present scientific theories of the mind do not require that we posit such entities at all? It has

been argued by a number of philosophers that PAAs are likely dispensable to our scientific understanding of the mind. Christopher Gauker, for instance, claims that:

> What I don't see is any evidence that we can reliably predict what people will do in a way in which attributions of belief and desire [and other PAAs] play an ineliminable role. [...] Nor even have I ever heard of a single real-life example in which it was at least quite plausible that one person successfully predicted the behavior of another [and] it was not evident that the same prediction could have been made in other ways. ([2009])

Or consider John Bickle, who argues that there is 'no need to saddle our ontology of the mind with "folk psychology".' ([2003], p. 8). But even if Gauker and Bickle are correct, and PAAs are not indispensable to scientific practice, this would hardly be enough to justify the further claim that they are inherently unscientific as a result. Science is not in the business of only using theories and models that are shown to be indispensable. To emphasize this point, consider Hartry Field's argument that we could, in principle, do science without using numbers ([1980]). Field famously argues that it is possible to do science without quantifying over abstract objects like numbers or functions. Now, I do not propose to argue for Field's position here. Instead, let us suppose for the sake of argument that his project proves successful. In which case, numbers are dispensable to science. Would this therefore make all current mathematical equations in science folk, as opposed to scientific? That seems unlikely, or else virtually all of physics and neuroscience would be folk. Similarly, if scientific inclusion is based on indispensability, then we have little grounds to consider very much of current science to be genuinely scientific. We

have not shown that our best theories in neuroscience or quantum mechanics are, in principle, indispensable.

Lastly, let us consider a third possible explanation for the classification of PAAs as folk psychological, even when used as part of theories in cognitive science and psychology:

> 3. Science is a strictly descriptive practice. PAAs, on the other hand, are not descriptive but normative. Therefore, the application of PAAs cannot be scientific.

Many philosophers have argued that PAAs involve an inherently normative component (Quine [1960]; Dennett [1987]; Sehon [1997]), and that this puts them at odds with scientific practice. Sehon describes the problem as follows:

> First, there is a normative character to our practices of mental state ascription that is foreign to the theories involving natural kinds in the sciences. The normative aspect of mental state ascription can be seen in several related ways. Most generally, when we ascribe mental states, we do so against the background assumption that we are dealing with a *rational* agent; i.e., we attribute propositional attitudes to an agent against our background conception of what she *ought* to believe and desire. [...] Attributing a completely irrational set of beliefs to an agent defeats the purpose of belief attribution, and the attribution itself loses sense. [...] However, scientific theorizing does not appeal to overtly normative standards in the way that mental state ascription does. Our choice of theories is not guided by an ideal conception of how the world ought to behave. ([1997], p. 334).

Thus, perhaps the folk nature of PAAs is rooted in their normative aspect. To be genuinely scientific, a theory must eschew the sort of normativity inherent in PAAs.

The problem with this way of understanding the folk/scientific distinction is that it greatly misrepresents actual scientific practice. Sehon claims that 'our choice of theories is not guided by an ideal conception of how the world ought to behave.' Yet, this is simply not true in many cases. Idealized models in science often work by doing exactly this. Instead of describing the way the world genuinely is, idealized models in science predict and explain phenomena by creating models of how the world ought to behave given various idealizations we build into our model, and then using this to draw inferences about the behaviour of real systems. Angela Potochnik, for example, argues that 'the aim of [scientific] modeling is to indirectly represent a real-world system by describing a simpler, hypothetical system and investigating that simpler system, in order to draw conclusions about the actual system of interest' ([forthcoming], p.48). She likewise notes that:

> The consensus in the literature on model-based science is that idealized models can represent despite their false assumptions. […] Idealizations represent features of systems, for they represent those systems as if they possessed features that they do not. This qualifies as representation in virtue of similarities in the behavior of the fictionalized representation and of the represented system(s), and as such, it contributes to the search for causal patterns. Idealizations, and the fictional entities and properties they posit, are thus integral to scientific practice. ([forthcoming], p.58)

Consider, for instance, the role that optimality models play in evolutionary biology (Beatty [1980], [1981]; Orzack & Sober [1994], [1996]; Rice [2004]; Potochnik [2007], [2010]; Woods & Rosales [2010]). Optimality models are commonly used to investigate and predict the evolution of phenotypic traits within a given population not by describing how the evolutionary process actually works (often ignoring known genetic and epigenetic factors in the evolutionary process), but instead only by characterizing what sorts of traits would be optimal for the creature to have given the constraints of natural selection. In effect, these models predict real systems based on an ideal conception of which traits ought to be selected for if natural selection was the only causal factor in evolution, and always produced optimal results.

Or consider the study of phase transitions in physics, such as a fluid changing from a liquid phase to a solid phase. In order for statistical mechanics to model this phenomenon effectively, we must model the system as if it had an infinite volume, allowing for infinite degrees of molecular freedom (See: Bub [1988], p. 71; Callender [2001], p. 549; Batterman [2002], [2011]). Just as with our application of optimality models, we do not describe the way the world is when we employ such models, but instead predict based on an ideal conception of how the world ought to behave if it allowed for things like infinite volumes.

In this respect, many scientific models are normative in the same way that the attributions of PAAs are normative. One can dispute whether such models will eventually be displaced in science when better models become available, but it is simply false to claim (as Sehon seems to) that they do not exist in science. If PAAs are folk psychological because they contain a normative component to them, then it would appear that optimality models in evolutionary biology, and statistical mechanics in physics, would likewise constitute folk biology and folk physics respectively. Yet very few, if any, would grant such a claim. If such normative models

are part of legitimate scientific inquiry, then so too are PAAs when used as part of scientific theories and models.

How then are we to understand the distinction between folk psychology and scientific psychology? Should we abandon the distinction altogether? I propose not. Instead, I suggest that turning to our understanding of folk physics may yield a clue.

4 Grounding Scientific Terminology in Scientific Theory and Experimentation

Given that terms like ‘time’ and ‘space’ can be found in both everyday folk contexts, as well as rigorous scientific contexts, we can conclude that it is not the terminology itself that makes an account folk physics as opposed to scientific physics. So what is it that differentiates folk physics from scientific physics? For some insight, consider the following passage by Dennett:

> Folk physics is the system of savvy expectations we all have about how middle-sized physical objects in our world react to middle-sized events. If I top over a glass of water on the dinner table, you leap out of your chair, expecting the water to spill over the side and soak through your clothes. You know better than to try to sop up the water with a fork, just as you know you can’t tip over a house or push a chain. You expect a garden swing, when pushed, to swing back. [...] The truth in academic physics if often strongly counterintuitive, or in other words contrary to the dictates of folk physics, and we need not descend to the perplexities of modern particle physics for examples. The naive physics of liquids would not predict such surprising and apparently magical phenomena as siphons or pipettes (Hayes [1978]), and an uninitiated but clever person could easily

> deduce from the obvious first principles of folk physics that gyroscopes, the virtual images produced by parabolic mirrors, and even sailing upwind were flat impossible. ([1987], p. 8)

If this is true, then the division between folk physics and scientific physics is based on the sorts of generalizations we make about physical systems. The sorts of generalizations employed in folk physics are those that fit with our everyday intuitions about how physical objects behave, but which largely ignore scientific theorizing and experimentation. Scientific physics, on the other hand, forms generalizations based on the results of scientific experimentation and the application of scientific theories. As Bas Van Fraassen points out, 'to ask that [explanations] be scientific is only to demand that they rely on scientific theories and experimentation, not old wives' tales' ([1980], pp. 129-130). This would explain why terms like 'time' and 'space' can be part of both folk and scientific physics. It is the sorts of generalizations we draw about time and space that are either folk or scientific depending on whether they are relevantly informed by scientific practice or not.

If we apply this lesson to psychology, then we can conclude that the scientific or folk status of PAAs is likewise determined by whether their usage in a given context is informed by scientific theories and experimentation. If so, as is the case with the PAAs embedded within theories in social psychology and comparative psychology, then these mentalistic terms are not folk psychological, but scientific terms like any other. Note that this condition does not require that PAAs, nor the theories in which they are in embedded, first be vindicated by our best theories in neuroscience or cognitive science. All that is required is that our psychological

theories were generated in accordance with the tenants of scientific practice. If so, then it is not folk psychology.

Of course, it is still common for people to use PAAs in everyday folk ways by making psychological generalizations that are largely divorced from rigorous scientific theories and practices. In these everyday contexts, PAAs are part of folk psychology. But this should not be particularly surprising; we use 'time' and 'space' in folk physical ways all the time in daily life as well. Drawing a distinction between folk and scientific psychology can certainly be useful and beneficial. It becomes dangerously misleading, however, when we speak of mentalistic vocabulary as being folk psychological even when used as part of empirically informed scientific theories and practices.

5 Implications for the Neuroscientific Study of the Mind

Why is it important that we not mislabel PAAs as folk psychological when part of scientific theories? The issue here is not merely clarity for clarity's sake. This way of talking tends to downplay the value of scientific theories and models which invoke PAAs, or to brush them aside as merely 'folk' psychology. As Kathleen Wilkes notes, 'once [a theory] is so baptised [as folk psychology], it inevitably finds it hard to live down its folksiness. There is an inbuilt temptation to see it as a bit twee, a bit primitive' ([1993], p. 168). Dennett, for instance, tells us that we shouldn't take the application of PAAs in science '*too* seriously' ([1987], p. 350, emphasis in text). Likewise, Penn argues that psychological theories which employ folk psychological terminology (which he defines in terms of PAAs) have no place in modern cognitive science (2012, p.256). Yet, to downplay the value of psychological models and theories which invoke

PAAs for being folk psychological would be to risk trivializing data that is extremely important to the neuroscientific study of the brain. We know empirically that psychological models which employ PAAs can provide information about the ways in which cognitive systems behave in a variety of contexts, and can be predictive of a range of different behaviours (see footnote 5). This sort of behavioural data is essential to neuroscientific research given that it puts essential constraints on how to build and test our neurological models. Put simply, neuroscience needs to know what the behavioural regularities of the system are in order to determine how they can be produced by the underlying neurological mechanisms. As Patricia Churchland notes:

> Crudely, neuroscience needs psychology because it needs to know what the system does; that is, it needs high-level specifications of the input-output properties of the system. Psychology needs neuroscience for the same reason: it needs to know what the system does. That is, it needs to know whether lower-level specifications bear out the initial input-output theory, where and how to revise the input-output theory, and how to characterize processes at levels below the top. (Churchland 1989, p. 373)

On a similar note, Chris Eliasmith and Oliver Trojillo have recently claimed that:

> The 'top-down' approach [to generating large-scale brain models] allows us to use the vast knowledge gained through behavioral sciences to impose constraints on the model. This allows us to use the model to test hypotheses about the functions of different brain regions. (2014, p.4)

To illustrate, consider well-documented cognitive biases regarding judgments of probability, such as the 'gambler's fallacy' or the 'hot hand fallacy'. There is a great deal of evidence that people do not naturally think about probabilities in a way that accords with probability theory, and instead misrepresent probabilities based on the outcomes of previous unrelated events. Any complete neuroscientific account of cognition must account for these biases in reasoning. Thus, knowing the sorts of judgments we in fact make about probabilities, the contexts in which we make them, and the circumstances that influence them, tells us what sorts of conditions and constraints our neurological models must meet. If our neurological models are unable to match the actual behaviours that humans display in the real world, then they cannot be an accurate account of our cognitive processes. Thus, we can test our neurological models by comparing them against the behavioural data we have in order to determine if they can account for the sorts of behaviours we empirically observe.

Meanwhile, it is important to note that the majority of behavioural data we have on biases such as the gambler's fallacy and hot hand fallacy have been gathered using psychological theories and models that interpret behaviour in terms of 'beliefs' and 'expectations' (for just a small sampling, see: Tversky & Kahneman [1971], [1972]; Gilovich, Vallone & Tversky [1985]; Clotfelter & Cook [1993]; Keren & Lewis [1994]; Ayton & Fischer [2004]; Burns & Corpus [2004]; Sundali & Croson [2006]; Sun & Wang [2010]). Of course, the fact that this behavioural data was generated using psychological theories and models that happen to make reference to PAAs is irrelevant to whether or not they identify genuine biases regarding our evaluations of probabilities, and characterize relevant conditions under which these biases manifest themselves. In this respect, they provide essential data for neuroscientific research, even if they appealed to

PAAs in order to attain it. To dismiss or abandon all this data for being 'folk psychological' would be to needlessly obstruct our ability to neuroscientifically study such biases.

It is also worth noting that this is the case not only when learning about neurological mechanisms in humans, but also in animals. Recall Papineau & Heyes' insistence that it is easy, if not irresistible, to interpret animal behaviour in terms of PAAs. They note that we 'naturally assume that the birds who imitated did so because they *wanted* food and *believed* that performing the same action as the demonstrator would enable them to get it' (Papineau & Heyes [2006], p. 188). With this in mind, let us consider a similar such irresistible interpretation when it comes to the behaviour of frogs. It might likewise be easy, if not irresistible, for scientists to characterize frogs as 'believing' that any darting black object is food, and thus can be 'fooled' into 'believing' that small darting images on a computer monitor are food. Likewise, that frogs can only 'recognize' flies when they are in motion, and not stationary. Even though this way of describing the behaviour of frogs is couched in propositional attitude terminology, this sort of characterization provides all kinds of information regarding what sorts of stimuli frogs respond to, and under what conditions. This sort of behavioural data has proven extremely important in the study and understanding of motion sensitive neurons within the frog's visual system (see, for example, Lettvin *et al*. [1959]).

One might feel tempted to point out that such behavioural data in social psychology and comparative psychology could easily have be gathered without the use of mentalistic terminology, and so the presence of PAAs is not essential for generating the behavioural data that neuroscientists need. Yet even if this is true, it would be to miss the point. The point is not that PAAs are necessarily ineliminable or indispensable for gathering such data, but that it is a descriptive fact that a great deal of this data has been, and continues to be, generated using

models which do invoke such terminology. Thus, to be dismissive of such theories simply because of the terminology they choose to invoke would be to cut ourselves off from decades of psychological research that we know empirically provide exactly the sort of information that neuroscientists need in order to refine and improve their models. Whether we could have acquired this sort of behavioural data by employing different sorts of theories and models is simply not relevant to whether the models we have do provide this information, and whether it is valuable. By analogy, consider once again the Hodgkin & Huxley model. This model was hugely influential in neuroscience, and provided a great deal of information regarding the behaviour of the time course of the action potential. Yet Hodgkin and Huxley themselves noted that they did not need to employ those particular equations in order to generate the same behavioural data of the time course. Different sets of equations would have worked equally as well. As they themselves put it:

> An equally satisfactory description of the voltage clamp data could no doubt have been achieved with equations of very different form, which would probably have been equally successful in predicting the electrical behaviour of the membrane. ([1952], p. 541)

Do we therefore conclude that, because we could have used a different set of equations, the Hodgkin & Huxley model therefore did not provide us with essential behavioural data used in the study of the underlying neurological mechanisms? Ought we to have been dismissive of the scientific value of the model on the grounds that we could have used a different set of equations? Not at all. Likewise, the fact that scientific models which employ PAAs have generated, and continue to generate, important information regarding behaviour is not undermined by the fact

that different kinds of descriptions may have yielded similar data. To dismiss these models as mere 'folk psychology' because they employ PAAs as opposed to other forms of descriptions would be akin to dismissing the Hodgkin & Huxley model as unscientific because it uses the particular equations they do instead of using others. In both cases, we would be needlessly cutting ourselves off from important sources of empirical data. Psychological theories and models which employ mentalistic terminology provide essential insights into behaviour needed in the neuroscientific study of the brain, and their scientific contributions continue to be important.

6 Conclusion

There has been a tendency to view PAAs as folk psychological, even when employed as part of explicit psychological theories and models. In this paper, I have argued this view is problematic, and potentially harmful to neuroscientific practice.

All this does not imply that PAAs are indispensible to the sciences of the mind, nor does it imply that our best cognitive or neuroscientific theories will ultimately vindicate the existence of propositional attitudes. Instead, the point is that such issues are ultimately irrelevant to our understanding of whether a term is part of folk or scientific psychology. To insist that PAAs are folk psychological when embedded in scientific theories can result in a tendency to downplay the important role that psychological generalizations play in our scientific study of the mind, and this can hinder scientific progress.

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