This book symposium on *Freud's Theory of Dreams: A Philosophico-Scientific Perspective* consists of an overview of the book by the author, Michael T. Michael, together with two commentaries, by Bennett Holman and Timothy J. Fuller, and the author’s replies to these commentaries. As summarised in the précis, the book analyses and responds to major philosophical and scientific criticisms that have been brought against Freud’s theory of dreams, while also evaluating the scientificity of the theory and exploring its current relevance. In his review of the book, Holman raises questions related to the robustness of the reasoning by which Freud’s claims might be warranted and to the potential pseudoscience of psychoanalysis. In the second review, Fuller raises a number of challenges, concerning the fruitfulness of Freud’s theory, the author’s distinction between personal and general justification, and whether the metaphors that Freud uses can be cashed. The author responds to each of these questions, arguing in support of the book’s conclusion that Freud’s theory of dreams is unproven but still highly promising.
Précis of

*Freud’s Theory of Dreams: A Philosophico-Scientific Perspective*

Michael T. Michael

Although Freud’s theory of dreams is over a hundred years old, it continues to generate debate. An example of this is the formal debate that took place at the April 2006 “Toward a Science of Consciousness” conference in Tucson, Arizona, in which the neuroscientist Mark Solms—founder of neuropsychoanalysis, a movement which seeks to integrate neuroscience and psychoanalysis—took on the anti-Freudian psychiatrist J. Allan Hobson on the question of whether Freud’s dream theory should be abandoned. The audience of over hundred psychologists, neuroscientists, philosophers and other academics voted against the motion, but the issues raised in the debate are still without full resolution. In some quarters of academia, Freud’s theories and methods remain a by-word for pseudoscience.

*Freud’s Theory of Dreams: A Philosophico-Scientific Perspective* explores these controversies by engaging with the major philosophical and scientific criticisms that have been brought against Freud’s dream theory. Approaching the issue from the vantage of the history and philosophy of science, I attempt to offer perspective and nuance to the debate in place of the more usual polemics. I do this initially by narrowing the poles of the debate, asking not whether the theory is to be lauded or traduced, but whether it is more like seventeenth-century alchemy—misguided but vital to the development of science—or like eighteenth-century calculus—right-minded but lacking in proper foundations. I navigate my way to an
answer by means of three simple guiding questions: Is what Freud had to say about dreams reasonable? Is it scientific? Is it still relevant?

The first of these questions has two parts. The first part has to do with the degree to which Freud’s own arguments support his theory. The psychoanalytic scholar Rachel Blass has pointed out that little work has been done in reconstructing and analysing Freud’s reasoning about dreams (2001, 142). This remains the case, despite Blass’s own partial attempts to fill this gap. Thus Chapters 1 and 2 offer a long overdue attempt to reconstruct the main arguments by which Freud arrived at and supported his theory.

Within this context, I address one of the main criticisms that have been brought against Freud. This is that his theory is based on obsolete nineteenth-century neurobiology (McCarley & Hobson, 1977). Against this, I point out that the concepts of the mind as a reflex apparatus and of mental energy that underpin Freud’s metapsychology do not derive from and are not tied to neuroscience, something which Freud realized soon enough. Rather he adopted these concepts because they offered an explanatory framework for understanding the phenomena he was encountering in the clinic. Moreover, recent work in neurobiology suggests a correspondence between these notions and those developed by Karl Friston in relation to the Bayesian brain hypothesis (Carhart-Harris & Friston, 2010). Thus, I argue, McCarley and Hobson’s criticism is ill-founded.

The second part of the question about the reasonableness of Freud’s ideas, addressed in Chapters 3 and 4 of the book, concerns the validity of Freud’s method of dream interpretation. Numerous critics have argued that the method is inherently and overtly fallacious in that it requires inferring, without good reason, from the fact that a thought has been
associated to a dream element to the conclusion that this thought is thereby a “dream-thought”—that is, part of the causal sequence of thoughts that led to the dream. This criticism has been made in various ways by numerous critics, including Wittgenstein (1966), who observed that, while free associations may inevitably lead to the crux of our waking concerns, that they do so and that this thereby appears to make sense of the dream offers no support for the belief that these concerns were indeed causes of the dream. I address this criticism in the context of Clark Glymour’s (1983) more developed argument. I point out that even with the example that Glymour uses to illustrate his criticism, a strong case can be made for Freud’s interpretation on the basis that it better explains the dream content in light of the associations made with it. My defence builds on Freud’s own “jigsaw puzzle” analogy by articulating the ways in which dream content and associations often fit together too well to be best explained by coincidence. This defence is not an attempt to demonstrate that Freud’s method is reliable in general, but to show how a Freudian dream interpretation can be justified. The problem that remains is that the basis of this justification, which often includes subtle experiential states, is not readily available to anyone outside the analytical encounter. As such, though this defence addresses the most worrying criticism brought against Freud, it does not amount to a scientific validation of his method.

The second question addressed in the book, the subject of Chapters 5 and 6, concerns the scientificity of Freud’s theory and method. Here the dominant critiques are those of Karl Popper and Adolf Grünbaum. Popper’s famous criticism is that psychoanalysis is unfalsifiable. With respect to the theory of dreams, Popper (1983) emphasizes Freud’s attempts to defend his wish-fulfillment claim against the obvious
objection from nightmares and anxiety dreams. He regards Freud’s failure to face up to these “falsifiers” as rendering the theory methodologically unfalsifiable. In Chapter 5, I argue that Popper has misunderstood Freud in several ways, and that the apparent counterexamples are not falsifiers. I also articulate the well-known (among philosophers of science) limitations to Popper’s falsificationist philosophy, while illustrating how, in any workable sense of “falsifiability,” Freud’s major claims about dreams are indeed falsifiable.

While Popper attacks the scientificity of Freud’s theory, Grünbaum’s criticisms pertain to the method of interpretation, which he thinks does not conform to “time-honoured” canons of inductive reasoning. I argue that Grünbaum has misunderstood both Freud’s reasoning and scientific reasoning more generally. Using the example of Freud’s early thinking about hysteria, I show that substantive psychoanalytic claims can be defended through Inference to the Best Explanation. This defense involves taking on Grünbaum’s negative remarks about Inference to the Best Explanation by showing how Peter Lipton’s (2001) reply to Wesley Salmon on the subject illuminates the utility and ubiquity of this method of inference. Lipton explains not only how Inference to the Best Explanation is to be properly understood, but also how it can be compatible with Salmon’s favored Bayesianism, while being vastly more practicable.

Though I reject Grünbaum’s arguments, I nevertheless agree with his conclusion. Psychoanalysis as currently practiced is not scientific. This is not because of the form of inference it employs, but because it falls short in point of detail of the standards required of science. I believe there are two paths to scientificity available to psychoanalysis. One involves showing the discipline to be based on rigorous epistemic practices—that
is, practices that incorporate procedures to counteract sufficiently the errors and biases it would otherwise be prey to. Psychoanalysis has, as yet, failed to step up to this challenge. The second path involves showing that psychoanalytic interpretation constitutes a reliable technique. Models for the scientific acceptance of such techniques include the acceptance of the use of the telescope and of the microscope in the seventeenth century. Psychoanalysis, however, fails this test as well, not least because there is, as yet, no scientifically accepted way of checking the validity of any single interpretation. As such, Freud’s method of dream interpretation cannot, as things stand, be accepted as scientific.

Though Freud’s theory of dreams is not scientifically acceptable on Freud’s terms—on the basis of interpretative evidence—it may be so on other terms. For, contra-Popper, it is a candidate for scientific acceptance. This takes us to the third main question of the book, addressed in Chapters 7 and 8, of how well the theory stands up to recent empirical research on dreams. I argue that, despite often shrill pronouncements to the contrary by scientific critics such as J. Allan Hobson and William Domhoff, the theory stands up to the evidence remarkably well. The several versions of Hobson’s alternative “activation-synthesis” theory are far more compatible with Freud’s theory than Hobson makes out, and where they do differ, the evidence is either more favorable to Freud’s theory or insufficient to decide between the two. Domhoff’s (2003) exhaustive list of empirically-based objections to Freud’s theory fare little better. For example, recent work on children’s dreams is, if anything, favorable to Freud’s theory rather than contrary (Colace, 2010). Where Domhoff is at his strongest is in pointing out that there has been no scientific validation of the probative value of free associations. I acknowledge that Freudians must take up the burden of proof on this
issue, though point out practical reasons why this would be no easy matter.

I conclude that, overall, the empirical evidence is supportive rather than undermining of Freud’s theory. Mark Solms’s work, in particular, shows considerable promise. Evidence from multiple lines of research suggests that dreams are driven by particular dopamine circuits of the brain, corresponding well to Freud’s wish-motivating claim. Numerous other findings also support Freud’s claims. To be sure, the evidence is not yet sufficient for acceptance of any of these hypotheses, but it does at least indicate that Freud’s ideas deserve to be taken seriously and to be subjected to further scientific investigation. As I put it in my closing remarks, “Freud’s theory may be still unproven, but it is in much finer fettle than has been thought.”

References


American Psychological Association.


Review of Michael Michael's *Freud's Theory of Dreams: A Philosophico-Scientific Perspective*

Bennett Holman

Irrespective of whether Freud’s theories have any modern day validity, they have played a monumental role in shaping the discipline of psychiatry and discussions about Freud have shaped the philosophical understanding of what makes inquiry scientific. Thus, even if Freud’s theories were radically false, the historical study of Freud remains a central, and indeed understudied, topic in the history of science. Accordingly, *Freud’s Theory of Dreams*, is a welcome corrective to this lacuna, all the more so given Prof. Michael’s rich knowledge of the history of Freud’s thought.

In his book, Prof. Michael addresses three primary questions regarding Freud’s theory of dreams and method of dream analysis: Are they reasonable to personally accept; are they scientific; and finally, how well supported are they by modern science? The first question seems best answered by a historian of psychiatry, the second by a philosopher of science and the third by a scientist. Given my specialty, I shall leave the first and the third questions aside and focus on Michael’s arguments pertaining to the demarcation question. Namely, Michael’s argument that Freudian psychoanalysis is not science, but not for the reasons that have traditionally been adduced.

In chapter 5, Michael rebuts the two most famous arguments against the scientific status of psychoanalysis (i.e. those of Karl Popper and Adolf Grünbaum). His treatment of Popper recounts objections that are
well-known to philosophers of science, but which will be surprising to those who think that science begins and ends with falsification. Moreover, Michael brings to light a fact that often gets overlooked in Popper’s philosophy. Specifically, that Popper does not contest the truth of psychoanalysis, but only its status as a science. His critique of Grünbaum, rests on debates in the philosophy of science as to whether a theory that is arrived at from inference to the best explanation can be said to be confirmed by the evidence.

While Michael provides a good account of the dialectic, I am less convinced that Freud’s reasoning is actually sufficiently robust to merit the type of conformational support that Michael attributes to it. Most problematic is Michael’s account of inference to the best explanation and the evidential warrant of evidence used in crafting theories. While Michael makes a convincing case that the Freudian theories explain many important cases, it also seems that they were built with these cases in mind. If one adopts a use-novelty principle, that a theory does not (strictly speaking) explain any evidence that it was created to account for, it is much less clear how much explanatory power Freud’s theory retains. Whether there are any cases in Freud’s account that would not fall to this concern is an interesting question for future research.

In chapter 6, Michael builds his own case against the scientific status of psychoanalysis. Though the chapter is framed in terms of the demarcation question, it primarily addresses whether psychoanalysis is demonstrably reliable. He first provides good reason for denying its prima facie reliability and thus provides motivation for considering psychoanalysis as unreliable. This argument is a rich and nuanced one, especially given that Michael has provided a positive argument for the reasonableness of (personally) believing in psychoanalysis in chapters 3
and 4. Next Michael uses a set of interesting and accessible cases from the history of science to demonstrate how methods of questionable reliability can be substantiated. After abstracting from these cases a way to ground the reliability of disputed techniques, Michael argues that, at least in its current form, psychoanalysis is unscientific.

Michael concludes that psychoanalysis is neither science nor pseudoscience. From a historical standpoint Michael is right to focus on Popper and Grünbaum. However, later discussions and alternatives for the demarcation criteria are not considered. I suggest that this leaves his part of his primary conclusion unwarranted. Irrespective of whether there might be a stronger position available to Michael (viz. Psychoanalysis is pseudoscience), I agree with the thrust of his argument, that Freudian psychoanalysis has not been demonstrated to be epistemically reliable. Whether he is also successful in arguing that one might still reasonably believe in it nonetheless (the argument of the first four chapters) and whether modern neuroscience provides support for a modified version of Freud’s theory of dreams (the argument of the final two), I leave to others.
Review of Michael Michael’s *Freud’s Theory of Dreams: A Philosophico-Scientific Perspective*

Timothy J. Fuller

My overall assessment of Prof. Michael Michael’s *Freud’s Theory of Dreams* is very positive. This is an excellent book. It’s a fascinating look at Freud’s theory of dreams in light of the history of science, the philosophy of science, and some recent and central work in the cognitive neurosciences that bears on Freud’s theory.

The Positive

In general, I wish researchers in the cognitive sciences had more background in the history of science. It’s useful for judging cognitive theories and their frameworks, including what should be expected of them given that they’re potentially intermediate theories in the still nascent sciences of psychology, psychiatry and the cognitive neurosciences.

Professor Michael Michael’s book is an important contribution in this vein—it draws off the history of astronomy, the calculus, alchemy, the development of the telescope and microscope, and many other developments in science to understand the scientific status of Freud’s theory of dreams and the methods and evidence invoked to support it.

Moreover, Michael brings his background in the philosophy of science into his appraisal, drawing on his knowledge of Popper’s falsification criterion, on whether Freud makes valid (scientific) inferences when attempting to support the claim that dreams have meaning that are discoverable by processes of association, and on whether the support for
Freud’s theory might be rationally reconstructed in terms of inference to the best explanation. Michael’s perspective is especially important in assessing Freud’s work since so many of Freud’s critics are working with questionable and seemingly dated theories on the nature of science and scientific inference.

Michael’s appraisal of Freud’s theory is sophisticated, nuanced, careful, and I would say his assessment is not just balanced but fundamentally honest—this is an extremely fair-minded and therefore valuable assessment of Freud’s theory of dreams. The alternative is a polemic, with pratfalls of potential irrelevancies, misleading statements and cherry-picked arguments and evidence. That’s manifestly, on every single page, not what is motivating this author. There’s an integrity to Michael’s book that I would like to acknowledge because it’s an essential component of genuine scholarship.

Given Michael’s aims, this book is overall successful. I believe the book successfully argued that Freud’s theory is falsifiable, that core aspects of it are not pseudo-science, and indeed that there is some evidence in favor of the theory. In this respect, Michael successfully rebuts Freud’s most prominent philosophical critics—Sir Karl Popper and Adolf Grünbaum. Michael also acknowledges that many aspects of Freud’s theory are not supported, and that central evidence and central methods of gathering evidence used by Freud in his psychoanalysis are as yet not supported. The book clearly identifies the challenges Freud’s theory faces in this regard and outlines in reasonable detail the steps that would need to be taken to rescue Freud’s methods and evidence, should practicing psychoanalysts wish to more fully justify their practices as scientific.
Critical Reflections

In some sense, we shouldn’t be too surprised that Freud’s theory turns out not to be demonstrably false, and that we don’t have strong, decisive evidence against the theory, since cognitive science in general is an immature science, with enormous gaps, and with obviously fledgling and evolving research paradigms. Amidst such a sea of uncertainty, dreams are one of the least understood aspects of the mind. Really we don’t have a lot of understanding of sleep in general, including its reasons for existing.

With this in mind, I’d like to offer some additional challenges to Freud’s theory, which might be framed as steps that should be taken to make Freud’s theory more scientific. But I won’t frame them that way, I’ll simply level three objections.

1st worry: metaphors that can’t be cashed.

Freud’s theory of dreams relies on many metaphors, for example that ‘psychical energy’ and ‘a censor’ contribute to our understanding of dreams. My worry is that such metaphors may inhibit rather encourage progress towards a more mature theory.

The points perhaps apply to Freud’s hypothesis of dreams as wish-fulfillments, and the claims that a censor keeps repressed wishes at bay during waking life, then the mental censor is weakened during sleep, and wishes have a kind of mental force or psychical energy for wanting to be “expressed” where that “expression” would mean leading to some action. And in some cases we have a “disguiser” of dreams, the dream worker.

My worry is that many of these notions or metaphors aren’t likely to be part of a mature cognitive neuroscientific account of dreams;
moreover, I think it’s reasonable to worry not only that Freud’s theories in their current form aren’t likely to be part of such a theory, but that they’re not moving in that direction.

At least that’s one moral one might take from advances that have been made in the cognitive neurosciences since the so-called “cognitive turn” in the late 1960s. Here, mechanistic accounts or models of mechanisms are supposed to explain mental capacities and phenomena, and the explanations themselves aren’t supposed to invoke mental capacities or mental phenomena as explanations. So the worry is that a disguiser and a censor and the like are homunculi, which are proliferated rather than reduced in Freud’s theory.

Similarly, one might worry about talk of “energy” and its “dissipation”. On Freud’s theory, disguising a wish uses less energy than censoring a wish, but it isn’t clear why. With disguised dreams becoming increasingly bizarre, this looks quite active. And we know the brain in general is as active during REM sleep as during waking life. It’s unclear to me why a wish’s energy would “dissipate” when it is expressed only in such a disguised form.

Usually when there’s talk of cognitive resources being used, one has a specific proposal about how this might be measured or its effects. For example, if the cognitive resource is attentional resources, one would expect that distinct tasks that use attentional resources are more difficult to complete. But what use of “energy” by the censor is being drawn away from other cognitive systems? My worry is that we don’t have here a framework that’s moving towards participating in a mechanistic account of the interaction among cognitive systems.
2nd worry: personal justification isn’t valid.

Michael Michael contends that psychoanalysts may be “personally justified” in accepting Freudian methods in some instances, given experiences with dream interpretation in their own or in their patient’s cases. Here the notion of justification applies to Freud’s methods of supporting his theory, in particular his central claim that dreams have meaning that are discoverable by processes of association. Michael argues that practicing psychoanalysts and Freud himself might be/might have been personally justified in accepting an analysis of meaning that connects desires or past experiences with the content of dreams.

I maintain that there is no such valid notion of personal justification. Justification is an inherently non-internalist, non-private notion. If there are reasons available to an individual for accepting a hypothesis, even ones that are based on his or her own experiences, they should be available to anyone on the basis of testimony. Moreover, whatever reasons a third person might have to doubt an individual’s testimony should also be reasons for the individual to doubt her own experiences, including “a-ha!” moments of seemingly valid associations. Either Freudian methods are justified or they aren’t, I don’t accept that justification is context-dependent in the way Michael conceives.

3rd Worry: Freud’s theory of dreams isn’t fruitful.

The last critical point I wanted to raise is that I think Prof. Michael hasn’t considered the objection that Freud’s theory, though it isn’t directly contradicted by a lot of dream research, doesn’t do any work explaining many phenomena associated with dreaming. This suggests that Freud’s theory may be explanatorily inadequate in some way. If it’s merely compatible but doesn’t explain central phenomena in its domain, this
perhaps signals that it’s not a fruitful research program.

Here I have in mind that dreaming seems to have some fundamental relations to learning. For example, in 2010, researchers found that subjects were much better at getting through a complex 3D maze if they had napped and dreamt of the maze prior to their second attempt. Indeed, they were up to ten times better at it than those who only thought of the maze while awake between attempts, and many times better than those who napped but did not dream about the maze. Some researchers theorize that certain memory and learning processes can happen only when we are asleep, and our dreams signal that these processes are taking place.

To take another example, a prominent theory of dreaming, called “reverse learning,” holds that while sleeping, and mainly during REM sleep cycles, your brain reviews neural connections formed while awake and dumps the unnecessary ones. According to this theory, without this unlearning process—which results in many or most dreams—your brain could be overrun by useless connections and pointless thoughts might disrupt thinking you need to do while you’re awake.

Another potential “learning” aspect of dreaming is learning to cope with stressful events. Stress neurotransmitters in the brain are much less active during the REM stage of sleep, even during dreams of traumatic experiences. This has led some researchers to theorize that one purpose of dreaming is to process painful experiences to allow for psychological healing. Reviewing traumatic events in your dreams with less mental stress may grant an ability to process them in psychologically healthy ways. Those with PTSD often have difficulty sleeping, leading some researchers to believe that lack of dreaming may be a contributing factor to their illness.
All of these potential “learning” functions and uses for dreaming seem to at best have nothing to do with Freud’s theory of dreams. His theory might be compatible with this evidence, so I’m not claiming it disconfirms Freud’s theory. But I want to stress that none of these results are, at least on the face of it, enlightened by Freud’s theory—they aren’t expected, predicted or explained by his theory in any obvious way. Again, they’re at best compatible with the theory. To draw on the history of science, when central advances in a theoretical domain seem to occur outside of a given theoretical paradigm, that provides some reason to doubt that the paradigm is fruitful and that it will be central to further advances in the field.
Replies to Commentators

Michael T. Michael

I would like to thank Prof. Fuller and Prof. Holman for their generous positive comments about my book and for their polite, well thought-out, and constructive criticisms. It is, naturally, on the criticisms that I will focus in my replies.

Reply to Prof. Bennett Holman

Prof. Holman makes some intriguing comments concerning the middle section of my book, which addresses the scientific status of Freud’s theory. There are two main points, which I will address in reverse order.

1. Pseudoscience

Prof. Holman raises the objection that my view that psychoanalysis is not pseudoscientific is unwarranted. I agree that I have given no direct argument for this view, and I did not claim to have done (as can be seen from my conclusion to Chapter 5 (pp. 139-40), which is carefully worded in that respect). Instead, I have considered arguments made by the two most prominent philosophical critics of the scientificity of psychoanalysis, Popper and Grünbaum, and shown that these fall short of establishing that psychoanalysis is pseudoscientific.

I could, of course, have considered other accounts of pseudoscience. However, as this was not the main focus of my book and as there is as yet no consensus in philosophy of science on what constitutes pseudoscience, I did not consider this to be the best approach to the issue.
Instead, I chose to address what I regard as a more fundamental matter: to what extent psychoanalysis is reliable. Since the charge of pseudoscientificity is usually invoked to undermine the credibility of psychoanalysis, it seems more immediately relevant to address the question of reliability rather than to involve oneself in disputes about contentious philosophical theories of pseudoscientificity.

But though I did not directly argue that psychoanalysis is not pseudoscientific, there is in my discussion an indirect argument for this claim. I regard it as a condition of pseudoscientificity that a theory is not just unscientific, but that it is “antithetical to science” or “rightly regarded as disreputable” (pp. 139-40). One way of interpreting this condition is as the claim that for a theory to be pseudoscientific it needs to be severely lacking in epistemic reliability (cf. Hansson 2013, p. 70). While I have argued that psychoanalysis is insufficiently reliable to be considered a science, I hope that I have also shown that, when properly practiced, it does not fall so far short of reliability as to be “severely lacking.” All the points in which it falls short are correctable, and I have made suggestions as to how they can be corrected (Michael 2015, Ch. 6). Hence I do not think it meets the above-stated condition for being a pseudoscience.

2. Use-Novelty

Prof. Holman’s point about use-novelty is an interesting one, but I am not quite sure to which of my arguments he is applying it. One possibility relates to my argument of Chapters 3 and 4, in which I invoke Inference to the Best Explanation (henceforth, IBE) to argue that one can be personally justified in accepting a particular Freud-style dream interpretation. But I do not think Prof. Holman’s version of use-novelty can be a requirement for valid inference in such uses of IBE, as, if it
were, then it would invalidate much everyday reasoning based on IBE. Take, for instance, van Fraassen’s (1980, pp. 19-20) famous example, “I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears – and I infer that a mouse has come to live with me.” The hypothesis that there is a mouse in the house explains the evidence it was proposed to account for, so seems to violate Prof. Holman’s version of the use-novelty principle. But it surely provides a genuine explanation of the evidence, and in the right circumstances the inference drawn from it is valid. It is just such reasoning that is relevant to the argument of Chapters 3 and 4, so I think this argument withstands Prof. Holman’s objection.

It may be that Prof. Holman is raising the concern specifically in relation to the question of scientificity. His point, in other words, may be that the reasoning by which psychoanalytic claims were originally established violates the use-novelty principle, and so psychoanalysis should be considered a pseudoscience precisely because of this failing. This would be to assume, among other things, that a failure to conform to use-novelty is (part of) a sufficient condition for something to be a pseudoscience. Such an account of pseudoscience would be contentious, and Prof. Holman has not presented an argument for it. But in any case, I do not think his version of use-novelty would be suitable to such an account.

As a criterion of scientificity, use-novelty is controversial. Stated simply, the principle of use-novelty is that evidence being in accord with a hypothesis confirms the hypothesis only if the evidence was not used in the hypothesis’s construction (Mayo 1991, p. 523). One problem with this criterion is spelling out what is meant by “used in the hypothesis’s construction” in a way that does not render the notion psychologistic or
dependent on contingent historical events. For example, it is unclear to what extent the existence of vestigial organs was part of Darwin’s considerations when he came up with his theory of evolution, but even if it was, that seems irrelevant to the confirmatory value of that evidence for the theory. Whether a suitable notion of construction can be given is still a matter of debate. But even if it can be given, it needs to be argued that Freud’s explanations fail to meet the criterion, and Prof. Holman has said nothing that suggests that would be the case. My view is that core Freudian explanations would be as in the Darwinian example, failing only a simplistic version of the use-novelty principle, rather than a suitable version.

In any case, it seems to me that even on a simplistic version of use-novelty, much of Freudian reasoning would meet such a criterion. For example, in relation to Freud’s theory of hysteria, which I briefly discuss in my book and use as a model of IBE in support of Freud’s theory, the phenomenon of resistance and the behavioural details that accompany a patient’s struggle with and overcoming of (purported) resistance can be said to confirm the theory, though were not part of its construction (e.g. Freud 1893-95, pp. 296-97). They thus constitute an example of use-novelty confirmation. With regard to dream interpretations, Freud often makes use of subsequent confirming evidence to support a particular interpretation he has already proposed, as is the case in the “supper-party” dream example I discuss at length in Chapter 3 of my book (see especially pp. 77-78). Such evidence would thereby also be in accord with the principle of use-novelty. Moreover, Freud then uses such interpretations to confirm his wish-fulfilment claim—a claim constructed before the evidence from such dreams emerged. This is again in accordance with the principle of use-novelty. I think there are many more
such instances of use-novelty in Freud’s work. But, to re-emphasize my previous point, I do not think that such examples matter a great deal to the scientific status of Freud’s theory, as I do not think that a simple version of use-novelty can be a criterion of scientificity.

**Reply to Prof. Tim Fuller**

Prof Tim Fuller makes some good points and raises interesting questions. I will again address these in reverse order.

1. **Studies Relating to the Function of Dreams**

To address Prof. Fuller’s third worry, it is helpful to look more closely at the studies that he draws on to support his claim of a possible “learning” function of dreaming. For though I do not preclude such a function, one of the concerns I have about Prof. Fuller’s claim is that it is based on a conflation of the *function of sleep* (or a particular stage of sleep) and the *function of dreams*. Indeed, when one examines the studies that Prof. Fuller draws on more closely, one sees that though they may support a learning function of (some stages of) sleep, they do not support such a functional claim about dreaming. In fact, the authors of the studies do not claim that they do.

In relation to Prof. Fuller’s “reverse learning” claim, the paper on

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1) This is a distinction that dream researchers also make. For example, Revonsuo et al (2015, p. 2) write: “Modern theories of the functions of sleep are undoubtedly quite strong as scientific theories of sleep ... but they are not in any direct sense theories of dreaming. ... Thus, we would like to strongly emphasize that the merits and the predictions of theories of dreaming primarily have to be tested by using data that reflects subjective dream contents, not the objective features of sleep.”
which he draws concerns the hypothesis of the “clearing out” of spurious or redundant associations during sleep (Tononi & Cirelli 2006). The paper talks about this hypothesis as a function of NREM sleep,\(^2\) not as a function of dreams—indeed dreams are not mentioned at all throughout the article. The authors clearly do not intend their hypothesis as about the function of dreams.

With regards to the paper on learning to navigate a 3D maze, the issue is more subtle. At best, the results of this study show that task-related dream imagery and mentation during NREM sleep can facilitate memory consolidation and learning. But that conclusion falls considerably short of the claim that dreams \textit{in general} function to enhance learning. Indeed, the authors themselves point out that we cannot generalize these results, since the relevant mentation occurred only during the first two stages of NREM sleep, Stage 1 (i.e. sleep onset) and Stage 2 (i.e. light sleep). So they do not generalize even to all NREM sleep, never mind to all sleep. They say nothing at all, for example, about REM sleep, which is the stage of sleep in which dreams are most likely to occur.

A more accurate way of describing the results of this study is that reactivation of recently formed memories during some stages of NREM sleep serves to enhance learning, and that this is \textit{reflected} in sleep mentation and dream imagery.\(^3\) As the authors, with appropriate caution,

\[\text{\textsuperscript{2} NREM, or non-rapid-eye-movement sleep, alternatively known as slow-wave sleep, is a type of sleep that alternates with REM sleep in cycles over the course of the night. There are several stages of NREM, including both light and deep sleep.}\]

\[\text{\textsuperscript{3} The evidence in relation to the role dream imagery plays in learning during these stages of sleep is actually very minimal. In only one of the studied cases did dream imagery appear to play a role. As the authors write, “Three of these subjects reported task-related mentation at sleep onset, whereas the fourth reported a maze-related dream after awakening from stage 2 sleep at the end of}\]

These observations suggest that sleep-dependent memory consolidation in humans is facilitated by the offline reactivation of recently formed memories, and furthermore that dream experiences reflect this memory processing. (Wamsley et al 2010, p. 850; my italics)

So they are talking about a function of a certain stage of sleep that is reflected in sleep mentation and dream imagery, not specifically about the function of dreams.

Thus my response to Prof. Fuller’s charge that Freud’s theory fails to explain an important fact about dreams is that the purported fact is not actually an important fact about dreams—at least, the evidence falls considerably short of supporting it as such. While one would expect dreams to reflect in some way the function of the particular stage of sleep in which they occur—and Freud made numerous remarks that offer support for this expectation—this need have no bearing on the general function of the process of dreaming. Hence the proposal that dreams have “fundamental relations with learning” is, I think, premature, and not yet well-supported by the evidence.

the nap period” (Wamsley et al 2010, p. 850). Since sleep mentation is arguably not the same as dreaming, these studies offer little support for any claim about dreaming serving a learning function.

4) According to Freud, (most) dreams are a combination of unconscious and preconscious thoughts. In numerous places, Freud indicates that preconscious thoughts can reflect the current physiological state of the dreamer (cf. Michael 2015, pp. 172-73). This leaves ample room for thoughts that reflect the functions of the particular stage of sleep in which they occur.

5) It is also worth noting, as a somewhat related point, that Freud was well aware that dreams can serve a problem-solving function, but did not consider such to be a general function of dreaming. Indeed, he regarded the fact as trivial: “The fact that dreams concern themselves with attempts at solving the problems by
Similar points can be made about the proposal that a function of dreaming relates to coping with stressful events. The most plausible version of this is that described by Walker (2017), in which reduction in the anxiety-inducing molecule noradrenaline during REM sleep is said to allow for the reprocessing of upsetting memories in a way that dissolves the “painful emotional charge” associated with such memories. This is an interesting hypothesis, though also one that seems complementary to a Freudian account. The evidence in support of it, however, is, as far as I am aware, restricted only to REM dreaming, so we do not yet know if it can be a plausible hypothesis about dreaming in general. The reality is that there is as yet no consensus about the general functions of dreaming among dream researchers. So again, it is premature to claim that there are central advances in dream research that are unanticipated by Freud’s theory.

which our mental life is faced is no more strange than that our conscious waking life should do so; beyond this it merely tells us that that activity can also be carried on in the preconscious—and this we already knew” (Freud, 1900, p. 507, ft. 2; see also p. 579, ft. 1).

6) Freud’s function of dreams, stated more fully, is that dreams serve to preserve sleep by allowing the release of psychical energy. The lowering of anxiety associated with REM sleep (assuming it is a purely physiological phenomenon) may contribute to a greater (though not total) relaxation of censorship, thereby allowing more such energy to be released. This would be consistent with the dissolution of painful emotional charge associated with certain upsetting memories, since such emotional charge is, on Freud’s view, a form of psychical energy.

7) There is, though, some recent support for Freud’s proposal that dreams serve to preserve sleep. As Parrino & Vaudano (2017, p. 108) write, “In the general framework upon the role of sleep as a vital need for humans, recent evidence points towards a protecting role of dreams from sleep disruption. This hypothesis would support Freud’s original theory on the basic function of dreams. ... Recent psychophysiological studies support this view and suggest that especially NREM sleep dreaming protects sleep against external arousing stimulation.”

8) An anonymous reviewer has suggested that the empirical research cited by Prof.
With regards to the more general point that Prof. Fuller is making, though I agree that failing to explain important facts that fall under a theory’s purview should count against that theory, there are limitations on the strength of this consideration. First, particularly in relation to mental phenomena, it is unrealistic to expect that any single perspective, representing one (or a few) of several possible levels of understanding, should account for all facts related to that phenomenon. Second, one should also bear in mind that, as Kuhn and other historically-minded philosophers of science have pointed out, paradigms or theories always fail to account for numerous facts that fall under their purview, yet are profitably retained despite this. So there are qualifications on the extent to which a perceived lack of fruitfulness counts against a theory. Third, one would have to weigh any failures against the successes, and as I argue in my book, Freud’s theory potentially scores highly in the latter regard due to its correspondence with Solms’s well-supported hypothesis that the dopaminergic SEEKING system—“a non-specific motivational system engaged in looking for something to satisfy needs” (Solms & Turnbull 2002, p. 201)—is the driving force behind dreams. But these

Fuller opens up the possibility that dreams are merely a by-product of learning processes in these stages of sleep, and that dreams lack a function of their own independent of these learning processes. But just like the hypothesis of a learning function of dreaming, this possibility is as yet no more than a hypothesis, so does not, as things stand, support Prof. Fuller’s charge that Freud’s theory fails to explain important facts about dreams. To sustain the above-described hypothesis would require not only content analysis of a large number of dreams in order to establish the plausibility that all dreams are by-products of learning processes, but also evidence against positive proposals about a function of dreaming. In this respect it should be noted that while the evidence in favour of such positive proposals may not yet be strong enough for their acceptance, that in itself does not amount to evidence against there being a general function of dreaming. For evidence in favour of Freud’s proposal, see the previous footnote and Michael 2015, pp. 198-200.
remarks are anyway merely hypothetical, since, as I have argued, Prof. Fuller’s claim is not, as things stand, well-supported in relation to Freud’s theory of dreams.

2. Personal Justification

My notion of “personal justification” is not new. It is simply the mainstream epistemological idea that one can be justified in believing something. I further claim that one can be justified in believing something based on first-person experience. But again this is in accord with mainstream epistemology, in which it is widely accepted that one can be justified in believing on the basis of perceptual experience or introspection. So I am not saying anything new or radical here. To reject the idea that one can be justified on such grounds is, I believe, the more radical thesis, which thereby carries the greater burden of proof.

What I think Prof. Fuller is challenging, though, is my distinction between being justified in believing something and being able to justify this to others. I claim that one may be justified in believing, say, a particular Freud-style interpretation of one’s own dream, but not be able to justify this belief to others, because it depends on subtle private evidence, including possibly experiential states.

To see why the distinction pertains, consider perceptual beliefs. As stated above, most epistemologists accept that one can have justification for such on the basis of perceptual experience. But while I may have that justification, someone else who does not have that perceptual experience lacks that specific justification. There is, in other words, a disparity in our justificatory states, and there is no guarantee that this could be bridged—that is, there is no guarantee that I can provide the other person with an appropriate justification.
Here one might draw the distinction in two ways: in principle and in practice. I think both pertain, but I need not rest my case on that, as the “in practice” thesis would suffice. It is possible that one is justified in believing something that one cannot, in practice, justify to others.

There are many reasons why this might be true. It may be that I am justified in believing something because certain external conditions hold, but that I cannot justify it to others because I cannot justify to them that those conditions do indeed hold. Consider, for example, an amateur ornithologist who spots an ivory-billed woodpecker—a bird thought to be extinct—in the forest, and suppose that this ornithologist is sane, sober, sees clearly, and knows an ivory-billed woodpecker when he sees one (i.e. can reliably identify such birds). Then he may be justified in believing that he has seen an ivory-billed woodpecker. But he might not be able to justify it to others who lack evidence that those conditions do indeed hold. So it seems to me straightforwardly true that it is possible for one to be justified in believing something that one cannot, at least in practice, justify to others.

3. Metaphors

I agree with Prof. Fuller’s concerns about the metaphors Freud uses. If psychoanalytic theories are to gain an equal standing with cognitive and neuroscientific theories, then those metaphors would need to be cashed.9) Freud, as he himself well-recognised, was grasping to express ideas he

9) One should bear in mind, however, that psychoanalytic theories are not intended as would-be cognitive or neuroscientific theories. Though it is anticipated that psychoanalytic theories would be compatible with well-supported cognitive or neuroscientific theories, psychoanalysis involves a quite different, even if not competing, paradigm.
had not quite the language or the conceptual resources to fully articulate, hence the necessity for him to resort to metaphors. However, a few words of caution are in order, lest the point be taken too far. Recourse to metaphors in and of itself does not render a theory unscientific, as metaphors abound in science (cf. Brown 2008). Moreover, one must distinguish between the expository convenience of metaphors and their theoretical necessity. Talk of the censor in Freud’s writing, for example, can in principle be replaced by talk of repression,\textsuperscript{10} but that would make the exposition of the subject cumbersome and unengaging.

Nevertheless, Prof. Fuller’s worry that the metaphors Freud uses cannot be cashed needs to be addressed. Though I cannot offer a decisive response, there are, as I alluded to in my book, promising signs that the metaphors can be cashed. The most promising relates to Freud’s notion of psychical energy. The eminent neuroscientist Karl Friston has argued that this notion corresponds to his own information-theoretic concept of free-energy, which is central to his prominent version of the Bayesian brain hypothesis (Carhart-Harris & Friston, 2010; Michael 2015, pp. 37-38).\textsuperscript{11} If free-energy does indeed correspond to Freud’s notion of

\textsuperscript{10} As Laplanche & Pontalis (1973) put it, “whenever this term [censorship] is employed, its literal sense is always present: those passages within an articulate discourse that are deemed unacceptable are suppressed, and this suppression is revealed by blanks or alterations” (p. 66).

\textsuperscript{11} The Bayesian brain hypothesis is a highly influential theory within cognitive neuroscience which posits that the brain is a hierarchically-organized inference machine that generates predictions of sensory input and adjusts its models on the basis of prediction error (or free-energy) feedback. The hypothesis is supported by powerful theoretical arguments and by numerous applications, including explanations of vision, learning, reasoning, planning, and mental disorders such as autism, schizophrenia, and hysteria. Due in part to its impressive explanatory power, it is also gaining increasing attention from philosophers (Clark 2016; Hohwy 2013). For accounts of how prediction error might do the work that psychical energy does in Freud’s theory (as requested by an anonymous
psychical energy, then this raises the prospect of replacing the metaphors
Freud uses in regard to this notion, thereby potentially providing
considerably greater clarity and rigour on a number of issues, including
those that Prof. Fuller mentions in his response.  

As regards the “censor” metaphor, this is important to the expression
of one of Freud’s major claims, so I should say something about it. What
underlies this metaphor, as already stated, is repression, a purported
psychical process that pushes and keeps thoughts out of consciousness.
Prof. Fuller’s concern seems to be with whether this can be given a
mechanistic account. I think there is evidence that it can. This pertains to
recent research in the psychology and neurobiology of thought
suppression (Ryckman et al 2018; Anderson & Levy 2009; Anderson &
Levy 2006; Anderson et al 2004; Anderson & Green 2001). As
Anderson & Green (2001) write:

reviewer), see Hopkins (2016) and Michael (2018a).
12) Some of these represent, I believe, Prof. Fuller’s misunderstanding. For example,
Prof. Fuller’s statement that “disguising a wish uses less energy than censoring
a wish” is not an accurate portrayal of Freud’s view. What I think Prof. Fuller
is referring to is Freud’s account that the force of repression (corresponding to
executive processes) is reduced during sleep, but remains in place to the extent
that it necessitates “disguise” in order for dream-thoughts to find conscious
expression. Such disguise is the consequence of unconscious processes that,
according to Freudian theory, are not only mainly automatic (that is, not under
the control of executive processes), but also are not as subject to the
“de-energization” process of sleep as the processes related to conscious wakeful
thought. I think Freud’s account in this regard makes good sense within the
theoretical framework from which he was working.
13) It is conceivable that thought suppression can become so instinctive as to elude
awareness, something that would render it indistinguishable from Freud’s
(mature) notion of repression. As Berlin (2011, p. 15) argues, “If suppression by
executive control processes becomes habitual over time, inhibition may be
maintained without any intention of avoiding the unwanted memory, evolving
from an intentional to an unintentional process (i.e., repression).” See also
Research on memory and attention shows that people have executive control processes directed at minimizing perceptual distraction, overcoming interference during memory tasks, and stopping strong habitual responses to stimuli. Here we show that these mechanisms can be recruited to prevent unwanted declarative memories from entering awareness, and that this cognitive act has enduring consequences for rejected memories. (p. 366; my italics).

While there is some way to go before such research can be used to fully cash the metaphor of censorship, it nevertheless supports the view that, contrary to Prof. Fuller’s concern, there is a realistic prospect of providing a mechanistic account of this process.

Concluding Remarks

In conclusion, I would once again like to thank Prof. Holman and Prof. Fuller for their thought-provoking responses to my book. I greatly value their intelligent and constructive criticisms, and can only wish that the philosophical debate surrounding the validity of psychoanalysis be always conducted in such a courteous and open-minded way.

For my part, I have tried in my book and my replies to colleagues to convey the complexity of the epistemic status of Freud’s theory of dreams. There is a strong tendency among scholars to navigate to polar extremes—to a pro or con position, to acceptance or rejection of the theory. I have tried to resist this tendency. This is because I believe the evaluative situation is too complex to allow for such straightforward answers. Freud’s theory of dreams, as I have argued in my book, is unproven but still highly promising. Simple positions cannot do it justice. Part of the reason for this is that psychoanalysis is an original and unique
enterprise, and in so being throws up numerous challenges to philosophers of science and other scholars (cf. Michael 2018b). My word of caution to such scholars is that it could be to miss something immensely valuable to dismiss Freud’s theory on the basis of as-yet preliminary and disputed philosophical or scientific ideas. Regardless of scientific status, many of Freud’s insights into how the mind works are, in my opinion, too potentially important to ignore.


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프로이드 꿈의 이론에 대한 책 심포지엄:
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『프로이드 꿈의 이론: 철학-과학적 관점』에 대한 이 북심포지엄은 저자인 Michael T의 책 요약과 함께 Bennett Holman과 Timothy J. Fuller의 논평, 그리고 두 논평에 대한 저자의 답변으로 구성되어 있다. 요약에 나와 있듯이, 이 책은 프로이드의 꿈의 이론에 대하여 제기된 주요한 철학적, 과학적 비판들을 분석하고 그에 대응하며, 동시에 꿈의 이론의 과학성을 평가하면서 현재의 적합성을 탐색한다. 이 책에 대한 리뷰에서 Holman은 프로이드의 주장은 정당화하려는 추론들의 탄탄함 및 정신분석학의 잠재적 유사과학성과 관련된 질문들을 제기한다. 두 번째 리뷰에서 Fuller는 프로이드 이론의 생산성, 개인적 정당화와 일반적 정당화에 대한 저자의 구분, 그리고 프로이드가 사용하는 은유가 현급화될 수 있는지의 여부와 관련하여 여러 가지 이의를 제기한다. 저자는 이 질문들에 각기 대응하면서, 프로이드의 꿈의 이론은 아직 증명되지 않았지만 여전히 매우 유망하다는 책의 결론을 옹호한다.

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