

## From the Editor

This is the second issue of the *AAPP Bulletin* structured around a target article and commentaries. With all due immodesty as Editor, I will call it a great success. I doubt that any reader will come away from this issue thinking that we have resolved the core questions of understanding psychiatric symptoms and their putative relation to what, if anything, underlies them. On the other hand, I *hope* that readers will come away thinking, ‘my goodness, this is really complicated, I had better keep thinking about it. Claire Pouncey has certainly set that challenge for us. But she also says that we really don’t have to deal with these questions, the clinicians among us can choose to be happily content treating our patients without asking, what is a symptom? I like to think that anyone who has taken the trouble to read this discussion will readily choose the path of further reflection.

Along with this Bulletin we are including the next target article, *Digital Psychiatry: Promises and Perils*, by Phoebe Friesen. That Bulletin issue will appear in the summer with commentaries and response and promises to generate a vigorous discussion.

What follows below is my own commentary on Claire’s target piece.

### Where is Psychiatry’s Psychopathology

Claire Pouncey has offered us a brilliant illustration of psychiatry’s perennial problem – the nature of the psychopathology underlying surface symptoms. She invokes Carl Hempel’s model of theory formation as a possible model for theory formation in psychiatry, and she cites the recent confirmation of black holes and event horizons as an example of successful Hempelian theory formation. She questions whether the Hempel model can work with psychopathology, and she expresses doubt as to whether psychiatry will

## President’s Column

One attractive feature of the *AAPP Bulletin* is that it reliably offers thoughtful overviews on targeted topics in the philosophy of psychiatry. Our topic this issue, in part, concerns a question about what norms we should use to evaluate the adequacy of psychiatry as a science (or if you prefer, evaluate the adequacy of the science of psychopathology). Will a successful scientific psychiatry confirm that clusters of psychiatric symptoms are primarily the result of causal mechanisms in the brain, or are the relevant causes also extra-neuronal? If the relevant causes are mostly brain mechanisms, would a successful scientific psychiatry confirm that the mechanisms of valid disorders are always disease mechanisms, never mechanisms that represent normal variation? Can we empirically decide what is and is not a disease using brain science only?

There are many ways to approach these questions in philosophy and in her target article Claire Pouncey takes them in an interesting direction. If we assert that psychiatric symptoms are indicators of diseases that are yet to be confirmed, then said diseases are hypothetical entities whose existence is being predicted by psychiatrists just like Einstein predicted the existence of the hypothetical entities that he named ‘black holes.’ Claire asks: given that physicists have recently confirmed the existence of black holes, should we be expecting that by analogy a successful science of psychiatry will confirm the existence of the disease mechanisms causing psychiatric symptoms. Let’s say that a medical model-inspired scientific common sense declares ‘yes,’ there are disease mechanisms out there waiting to be scientifically confirmed.

Here is where the philosophy of science comes in. Philosophers excel in showing that common sense is often common, but not always sensible upon further reflection. For instance, in the 17<sup>th</sup> century, scientific common sense hypothesized that there was a specific day when creation began. It turns out that we no longer consider the day on which creation began to be a legitimate hypothetical event for science to confirm, let alone a hypothesis based on scientific common sense.

It would be a mistake to make too much of a superficial analogy between the disease model and creationism, other than to say that predictions about hypotheticals are not better off or improved in any way by gaining the obviousness of scientific common sense. In his arguments against seeing science as a process of confirming hypotheses, Karl Popper advocated for predictions that contradicted our learned expectations. The prediction that the hypothetical mechanisms are disease mechanism is still a live option – but not one that should be treated as ‘scientific common sense.’ Treating a prediction as scientific common sense can lower the bar for what we consider confirmation.

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ever have its event horizon. I agree with her doubt, and in what follows I will offer further argument to support the doubt.

Let’s start with symptoms. We certainly think of them as symptoms of something else – a disorder, an underlying psychopathology, a whatever, or as with Claire’s metaphor, the penumbra of a psychiatric event horizon. The problem of dragooning symptoms into this role as penumbræ is that they are way to messy. Take anxiety, for instance. It can, of course, be a symptom of an anxiety disorder, but it can also be a symptom of many other disorders – depression, schizophrenia, you name it.

*(continued on page 10)*

## What psychiatry can learn from the Event Horizon Telescope

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In April 2019, astrophysicists used the Event Horizon Telescope to visually represent a black hole for the first time, thus providing indirect but substantial empirical support for Einstein's general theory of relativity. It was a thrilling confirmation of an unobservable theoretical object first predicted over 100 years ago in Einstein's theory of general relativity. The event horizon telescope – the means of “seeing” – was comprised of eight different radio telescopes erected around the globe, whose recordings over ten days were temporally synchronized and confirmed to be so. The millions of data points collected from the eight telescopes over those ten days were collated on a single supercomputer using a single, human-designed, -tested, and -selected mathematical algorithm that synthesized those radio transmission data and represented them as a “photograph” of the shadow created by the black hole in galaxy Messier 87.<sup>1</sup> This shadow is visually represented as a lopsided ring of light (rather than darkness), which is the intensified radio signal emitted as gas and dust gravitate toward the black hole, and are accelerated and heated by it. The theoretical border between the black hole and the swirling matter accelerating around it is called the “event horizon,” the border between the black hole and the light and mass that has not yet entered it. The images created from the radio transmissions were not of the black hole itself, nor of the event horizon, but of the hypothesized penumbra of emitted light that physicists predicted would surround a black hole if black holes exist. This confirmation of the theory of general relativity led CalTech astrophysicist Kip Thorne to conclude that “[t]here can be no doubt this really is a black hole at the center of [galaxy] M[essier] 87, with no signs of deviation from general relativity.” The Event Horizon Telescope project director, Shep Doeleman, reported that “We have seen what we thought was unseeable.”<sup>2</sup>

Although the press reported these representation as “photographs” or “images” of black holes, these summaries are overstated. By definition, light cannot escape black holes, so they cannot be seen or photographed. Black holes are also too large and too distant to be ascertained without multiple layers of inference and mathematical calculations – they are not mid-sized objects that lend themselves to the sort of “red here now” immediacy of empirical confirmation. The real scientific excitement of last April was not about the images themselves, rather, it was about the predictive power demonstrated by the theory of general relativity. That is, the images produced by the supercomputer that collated the millions of data points were not traditional empirical observations. But the fact that the theory of general relativity had consistently matured over a century to the point that such technology could be developed and used to deliver those images indicated the robustness, and some would say, the semantic truth or validity of the theory.

This is the hope Carl Hempel held out for psychiatry at the American Psychopathological Association meeting in 1959. In addressing stages of theory maturity and the taxonomy of mental disorders, Hempel encouraged operational definitions for mental disorders until psychiatry could develop mature theories of its own. But Hempel assumed that all theories mature, and that their unobservable entities, like black holes, would ultimately be confirmed and characterized. He told us: “[S]ystematic progress [theory development] ... will call for the formulation of principles expressed in theoretical terms, which refer to various kinds of unobservable entities and their characteristics. In the course of such development, classifications defined by reference to manifest, observable characteristics will tend to give way to systems based on theoretical concepts.” In short, Hempel urged psychiatry to use operational definitions for mental disorders until such time as the fuller meanings for those theoretical, unobservable entities could be provided by more mature theories.

Hempel would be thrilled by the black hole representations of last April, perhaps not just because they confirm general relativity, but because they also confirm his view of scientific theory maturation. Astrophysics started with the theoretical entity – the black hole – and used general relativity, theories of radio detection and signaling, computer synchronization, etc., to predict what else would be true of the world if that entity exists. They got what they predicted: a computer-generated image of a distorted penumbra of light surrounding a round area with no light at all.

But this particular scientific success highlights a disanalogy with psychiatry. Hempel suggested that mental disorders are to psychiatry as black holes are to physics: unobservable theoretical entities. Unlike astrophysics, however, psychiatry starts by observing the immediate effects of the theoretical entity. Our “penumbra” are the symptoms of what we assume to be mental disorders. The psychiatric black hole – the posited psychopathology – is meant to explain the immediately given feelings, cognitions, and behaviors we readily perceive, or that clients report. Psychiatry “sees” symptom penumbra all the time, and posits theoretical mental disorder entities to explain them, without substantive theory about what those entities might be. But there is no reason to assume that psychiatric science will mature beyond this point. We hope it will, certainly. We would like to confirm the existence of mental disorders that explain symptom clusters, but there is no *prima facie* reason to adopt Hempel's optimism that a classification of unobservable mental disorder entities defined by observable characteristics will eventually yield a rich, scientifically developed theory about how those characteristics arise.

The good news is that we don't need an event horizon telescope for psychopathology. The bad news is that psychiatric nosology may always be ad hoc. In clinical work this may not matter, since what we treat are symptoms and behaviors in individuals – treatment addresses what we already know, not what we theorize. If I treat Mr. A's anxiety symptoms, he might feel better even if there isn't *really* an underlying disorder that explains his anxious feelings. This nominalism is not very scientifically satisfying, of course, and I would not go so far as to argue that we should not continue to look for generalizations and law-like regularities about

the mental disorders we presume underlie symptoms. But the way I see it, the Event Horizon Telescope's representation of the light contrast around the Messier 87 black hole creates doubt that Hempel's promise will come true for psychiatry. Psychiatry already has the penumbra, but we have no reason to assume a black hole.

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## What are Unobservable Theoretical Entities in Psychiatry?

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I would like to thank Dr Pouncey for her insightful and stimulating reflections on the state of psychiatric science spurred by the indirect visualization of a black hole by Event Horizon Telescope. Her reflections also provide an opportunity to us to revisit Hempel's highly influential ideas. I don't disagree with Dr Pouncey in a substantive way, however, I do feel her reflections could perhaps be teased out in greater detail, and this is the task that I am undertaking here. I will do so by:

- 1) clarifying Hempel's ideas regarding unobservable entities
- 2) drawing a hypothetical parallel between physics and psychiatry
- 3) discussing unobservable entities in psychiatry

### 1) Hempel and Unobservable Entities

Hempel sees a pattern in the progression and maturation of scientific theories, especially those related to classification.<sup>1</sup> In the early days of a scientific discipline, the discipline is

attempting to adequately describe phenomena under scientific investigation with reliability and precision. At this stage, patterns of observable behaviors are also noted and are described with some statistical detail. As science progresses, the task moves on to explaining, predicting and scientifically understanding the phenomena by means of general theories. The characteristic of theories at this stage is that they refer to various theoretically postulated entities which are not directly observable – at least at first – and these theoretically postulated entities provide a framework with which to classify and explain the observables. For these theories involving theoretically postulated entities to be scientific, they have to make testable predictions. Hempel gives the example of periodic table of elements; the early classifications were based on the chemical behavior of elements (observables), however, subsequent mature classification was based on the atomic structure of the elements (unobservable). Another example Hempel uses is that of biological species: early on, species were identified using observable morphological features, however, as the science of biology matured, the emphasis on morphological features was replaced by an emphasis on phylogenetic basis (unobservable).

### 2) If Physics had Disorders

How does the indirect visualization of black hole fit into the framework offered by Hempel? Theory of General Relativity provided a formulation of principles governing gravitational motion of objects on a cosmic scale, expressing these principles in theoretical terms referring to various kinds of unobservable entities, such as the curvature of space-time. These unobservable entities such as space-time curvature in the framework of the theory implied the existence of further unobservable entities, such as black holes, and the theory was scientifically mature enough that it also predicted conditions under which the existence of black holes could be empirically tested.

Where does psychiatry stand in comparison? Let's go back to the Newtonian era and reflect on the observation that the orbit of Mercury

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deviates from the predictions of Newtonian mechanics. When the orbit of a solar object deviates from predictions of Newtonian physics, we'll consider it to be an instance of "Orbital Movement Disorder". The presence of a "disorder" requires only the observation that there is a mismatch between observation and prediction; it is agnostic about what causes the discrepancy. Orbital Movement Disorder therefore is simply a descriptive label about observables; it posits no unobservable entities. The next step in progress would be a formulation of hypothesized reasons for this observed behavior which refer to unobservable entities.

One cause of Orbital Movement Disorder could be within the framework of Newtonian physics, for instance, hypothesizing that there is an undiscovered planet or solar object in orbit between mercury and the sun whose force of gravity is influencing the orbit of Mercury, causing the discrepancy (unobservables here include the force of gravity and the undiscovered planet). Another cause of Orbital Movement Disorder could be in terms of Sun causing the space-time continuum to curve in a specific fashion which leads to the discrepancy (space-time curvature is the unobservable here). The next step is that these hypothesized explanations need to be so rigorously formulated that they can generate empirically testable predictions, and confirmation of these predictions will then provide support for the existence of these unobservables.

We can imagine here that Newtonian mechanics corresponds to a common-sense, folk understanding of human behavior. Behavior that doesn't match up with these expectations gets identified as something out of the ordinary. A description of that behavior gets labelled as a "disorder". At this stage, however, we are still in the realm of the observable; we have not posited any unobservable entities.

### 3) Unobservable Theoretical Entities in Psychiatry

There are unobservable theoretical entities in psychiatry but "mental disorders" are not among them. This is because mental disorders – based on our current post-DSM-III under-

standing – are descriptive constructs that are not understood in terms of theoretical unobservable entities. What would it mean to confirm the existence of "schizophrenia" the way we confirmed the existence of black holes? Schizophrenia is simply a label for a cluster for symptoms. The label *by itself* does not refer to any unobservable theoretical entity. One can debate about how the descriptive boundaries should be drawn for the purpose of the labeling them, but the debate is not be about whether schizophrenia *exists*.

Unobservable theoretical entities emerge as we attempt to explain how the symptoms captured by the label schizophrenia can arise. We can talk about psychological conflicts that threaten the integrity of the ego; we can talk about excessive dopaminergic activity in certain regions of the brain; we can talk about infectious insults; we can talk about gene-environment interactions, etc. These are all unobservable theoretical entities, with competing theories positing different unobservable entities.

In order to make scientific progress, these hypothesized unobservable theoretical entities need to be present in the context of a larger theoretical framework (such as force of gravity is posited within Newtonian mechanisms or space-time curvature is posited within General Relativity), this larger theoretical framework needs to have the explanatory power to explain not just one observable (such as orbit of Mercury) but many other observables (orbits of other planets; orbits of comets that come very close to the Sun, etc.), and then this theoretical framework needs to be able to generate testable predictions.

In psychiatry, we have our observable entities (symptom clusters labeled as disorders). We have a smattering of hypothesized unobservable theoretical entities. But we have no larger theoretical framework which utilizes unobservable theoretical entities to explain multiple observable entities and is rigorous enough to generate testable predictions. That is the scientific barrier that psychiatry has been unable to cross in the last several decades.

Dr Pouncey writes: "If I treat Mr. A's anxiety symptoms, he might feel better even if there isn't *really* an underlying disorder that explains his

anxious feelings."

In what sense can we say that there is no disorder underlying the anxiety symptoms? I conceptualize "disorder" as a descriptive entity, so in my view it would be a category mistake to think that an anxiety disorder underlies and explains anxiety symptoms. We can, however, utilize different vocabulary to make the point that I think Dr. Pouncey is making. We can posit that anxiety disorder (and all mental disorders) are the result of an underlying "dysfunction", where dysfunction is understood as some biological or psychological mechanism which is not working *as it ought to be*. This dysfunction is an unobserved theoretical entity. This broad definition lacks scientific rigor and is not very helpful. We need to be able to outline the sense in which mechanism is not working as it ought to be. For the sake of discussion, let's take the example of Wakefield's understanding of dysfunction as a mechanism that is not working as it was designed to work by natural selection.<sup>2</sup> The challenge here is that we don't know the mechanisms underlying anxiety disorder and we don't know what our mental mechanisms are designed to do by natural selection (and we don't seem to care that much about evolutionary design when it comes to our psychological lives, so is it really a relevant construct?). Furthermore, we can plausibly think of situations in which anxiety disorder arises from situations other than a failure of a mechanism, such as design-environment mismatch in which the mechanisms underlying anxiety are working exactly as designed but they are operating in the 21<sup>st</sup> century digital world for which they were not designed. If we expand the notion of mental disorder from being purely descriptive to one that posits unobservable entities by necessitating that the mental disorder must be the result of an underlying dysfunction, then it's very plausible (perhaps even likely in some cases) that there is no underlying dysfunction to be found. Mental disorders in that case would indeed be penumbras without black holes.

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## Explanation and Intervention is Psychiatry

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I enjoyed reading Dr. Pouncey's paper and agree with the conclusions she draws from her comparison of the Event Horizon Telescope and research in psychiatry. In this commentary, I want to draw out some implications of a couple of points she makes toward the end of the paper. First, she says, "[w]e would like to confirm the existence of mental disorders that explain symptom clusters", similar to the way that the images created using the Event Horizon Telescope confirmed the existence of black holes. Second, she says that even if this is not possible (and there is reason to doubt that it is), psychiatrists do often succeed in treating the "observable" manifestations of these disorders – the symptoms and symptom clusters themselves – even in the absence of an explanation of their etiology.

Pouncey thus highlights a tension in psychiatry and psychiatry research between explaining the nature and characteristics of mental disorders (whether understood as pathophysiological mechanisms or as psychological entities) and intervening in the purported manifestations of the disorders, i.e., symptoms or symptom clusters. This tension also raises important questions about the allocation of resources for psychiatric research. On the one hand, it is important to better understand how to use current knowledge to inform treatment choices – how, in Pouncey's example, to better treat Mr. A.'s anxiety symptoms. Even at this "surface" level, there will be important generalizations to be identified, as well as important differences among patients with similar, though not identical, characteristics and circumstances. On the other, many people share the optimism that Pouncey attributes to Hempel: that we can discover a better way of thinking about mental disorders and, based on this deeper understanding, can do a better job of diagnosis and of treatment. The NIMH's RDoC project (Insel et al., 2010) is an example of this optimism: it aims to radically restructure

biological research in psychiatry with the explicit hope of gaining a better understanding of mental illness, and, ultimately, a better way to diagnose and to treat them.

Like Pouncey, I don't want to argue against continuing to do research that might give us a better understanding of mental disorders. But this should not happen at the expense of making the best use of our current understanding by conducting research that aims to refine existing diagnostic and therapeutic practices. Looking for a deeper explanation of mental disorders is a big gamble, with no guarantee of a big payoff, while working with, in Pouncey's terms "what we already know, not what we theorize" is a less risky way of gaining knowledge, and one that is more immediately applicable to clinical practice.

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## The Source of the Problem.

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On the broad scale, I agree with Pouncey, although I have to confess that hearing about the discovery of the Event Horizon Telescope did not make me think about psychiatric science and hearing about other discoveries of observational/theoretical science rarely brings psychiatry to mind for me. So I am grateful for the comparison and contrast that Pouncey draws between psychiatry and cosmological science. Hempel's hopes for psychiatric science achieving maturity, like so many of the optimistic claims made for the field, look in hindsight that they were aiming high. Pouncey does not rule out the possibility that we will have a mature science of the mind that explains psychopathology in similar ways to other forms of explanatory

science, but she points out, very appropriately, that we may never get there. She does not say much about what might make it impossible for psychiatry to achieve maturity as a science. One way that philosophy of psychiatry can be productive is to investigate what it is about psychiatry that makes it so difficult to achieve the status of a precise science.

This is a big topic, and one that many have written on at length. Here, I will focus on what three categories of reasons there are to be skeptical that psychiatric science can achieve the same status as other spectacularly successful areas of science. I will also comment on what we can learn about the future of psychiatry from these considerations.

## Inductive conclusion from past history.

One striking feature of the history of psychiatry, and clinical psychology too, is how often important figures have claimed to have made discoveries that will transform the discipline into a precise and successful science, and how uniformly those predictions have proven to be overblown. There are various ways to interpret this history of psychiatry, and they range in their assessment of the progress of psychiatry. Some see psychiatric progress as real and impressive, while others see no significant progress at all, and only changing methods of trying to treat mental illness, often with economic or political explanations being far more important than scientific ones. Often the history of psychiatry is done carried out with pre-existing ideological convictions and they see the history through their own interpretive lens. We can probably agree that some psychiatric conditions have successful treatments for them, and that in other cases we have very limited progress. There will remain dispute about the success of many other conditions. A middle-ground approach will have to admit that even if there has been progress on some fronts, it has been generally limited and there has not been any massive transformation in knowledge that has provided a psychiatric revolution. The study of the brain has been progressing and there have been claims of a biological revolution in our understanding. Similarly there have been claims for pharmacological revolutions. Nevertheless, a sober look at

these grandiose claims reliably reveals them as exaggerated. Sometimes pessimistic inductive inferences about the future based on the past turn out to be wrong. Sometimes revolutions do really occur. So we can still retain hope for the future of psychiatry even with its past. But this history should at least cause us to be cautious and even skeptical about the current and future claims.

### **The impossibility of psychophysical laws.**

Psychology generally has struggled to be a science, and many philosophers have been skeptical about the possibility of any strict psychological science. Kant famously argued that empirical psychology cannot be a true science. More recently, Donald Davidson argued that due to the very different nature of psychological and physical concepts, it is impossible to have strict laws that combine them. Some eliminativist materialists have argued that some or all psychological concepts will have no place in a future science of the brain, since those concepts are fundamentally confused and laden with mistaken assumptions, so a future science of what we used to call the mind would need to be fundamentally neurological. Yet current psychiatry is thoroughly enmeshed in thick psychological concepts. If the kinds of critiques of psychological science I have referenced are on the right lines, then psychiatry could never achieve more than rough generalizations and associations. It is hard to imagine an understanding of human life that completely avoids psychological concepts, or a psychiatric practice that is purely neurological. Along similar lines, some have argued that concepts of disorder and psychopathology are thoroughly evaluative and normative in ways that concepts of science are not. Thus any attempt to understand fundamental psychiatric concepts in scientific terms could never have complete success. So it may be that the very nature of psychiatry as we currently practice it just makes it impossible to have the kind of precision and clarity that is required for discoveries comparable that of the Event Horizon Telescope.

This source of pessimism about psychiatry as a science is based on very general, largely *a priori* arguments. Furthermore, the arguments themselves are all vigorously debated and contro-

versial within philosophy, and have not curtailed the enthusiasm for researchers in psychology and psychiatry in seeking to make significant discoveries. We might especially worry that they rely on overly narrow conceptions of what "true" science is, and to what extent it relies on universal laws. Yet these arguments may also point to some of the difficulties of making psychiatry more ripe for the observational discoveries we see in other sciences: it marries a wide variety of concepts and approaches, and there are challenges in getting them to mesh with each other -- they tend to make bad marriages! Since psychiatry is a large field, combining many areas, we might look to those areas that employ a more homogenous set of concepts that mesh well with each other to be more ripe to deliver significant observations discoveries. One obvious place to look would be in neurology and more pure brain science.

### **Psychiatry is difficult and we are not that smart.**

The final sort of reason for the difficulties of progress that Pouncey highlights is more pedestrian than the former category. Rather than it being about the *a priori* impossibility of psychiatric science, it is about the sheer difficulty of the project and our own human limitations. Psychiatry requires examining and treating humans in a family and social context over significant periods of time, with very many other factors complicating what happens. This is a vastly complex system, and our attempts are inevitably simplistic. Even if we make significant breakthroughs in localized areas, and arguably we have, these will only be small parts of the overall system. We are not going to be able to have any grand overarching theories of psychiatry in the way that we can have grand theories of space and time or gravitation. There is just too much complexity to contend with and our cognitive abilities are too limited.

Strangely, this is not a source of difficulty that I have seen much articulated within philosophical or psychiatric literature. Yet it strikes me as a plausible source of the problems that Pouncey highlights. We still need to assess to what extent we are incapable of understanding the over-

all phenomena of psychiatry in a scientific way. We could compare the claims of some in philosophy of mind that human consciousness, the "hard problem," is beyond human understanding because of our cognitive limitations -- labelled as the "new mysterianism." That view says the problem is forever beyond our solution. A more optimistic take would be that through the time tested methods of splitting phenomena into smaller parcels and analysing each, and then eventually synthesizing our understanding of each, we will be able to have the kind of confirmed psychiatric theories and associated discoveries that would compare with that of the Event Horizon Telescope. Even though, that stage could be a long way off.

To wrap up, it is important to remember, as Pouncey makes clear, that psychiatry does not need overarching theories and dramatic confirmation of those theories in order to proceed. It is currently a piecemeal enterprise, with a focus on helping individuals. It is a very practical interaction between people, and while we like to entertain big theories and we need categories of disorder for guidelines of policy and treatment, it is fine if these categories are nominalistic rather than getting at essences of underlying reality. In the hope of future big discoveries, we can investigate what makes psychiatric science so difficult, and this may help to guide where we put our energies for our research programs.

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## **Predicting History**

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### **I Classification**

Does conceptual realism depend upon predictability? Does failure to predict therefore entail nominalism for the concepts employed in psychiatric diagnosis? That is the question at issue in Claire Pouncey's response to Hempel's application of "The Fundamentals of Taxonomy" to psychiatry. The answer depends upon what sorts of concepts are involved and upon whether Hempel's theory of taxonomy

provides an appropriate standard of their relationship to reality and their role in psychiatric diagnosis.

Hempel sets out to provide a "systematic background for a discussion of the taxonomy of mental disorders" and explains that by "taxonomy" he means "the *theory* of classificatory procedures and systems." He simply assumes that diagnosis is a classificatory procedure. In order for class concepts to enable predictions, they must supply a foundation for logical deduction, which requires necessary and sufficient conditions of class membership so that one can be sure that all the individual members of the class will be included in the predicted outcome. If any individual member of a class fails to perform as predicted that would invalidate the definition of the class, since the confirmation of the class definition depends upon successful prediction. (If the mass of the Higgs Boson had not fallen within the predicted range in experiments at CERN, for example, its existence would have fallen into doubt and its theoretical definition would have had to be revised, which would have created serious problems for the "standard model" of particle physics.) Most of Hempel's essay elaborates upon the roles of the scientific laws, operational definitions and theoretical postulates that Pouncey cites, which are necessary to mediate between classificatory concepts and confirming predictions.

However, the theory that concepts reflect or rest upon precise sets of necessary and sufficient conditions has since been seriously challenged by the prototype theory of concept formation introduced by Eleanor Rosch in the 1970's under the influence of Wittgenstein's account of concepts as only providing "family resemblances". (The standard illustration is that the concept "bird" does not rest upon any precise specification of all the necessary attributes of class membership, but on a prototypical example, such as a robin, that other instances of the class more or less resemble in some way.) Rosch's theory arose from empirical research into the color concepts of the Dani people of Papua, New Guinea and was extended to other domains of concept formation by later research

Since Hempel doesn't appeal to any empirical sources, Rosch's inductively grounded account prompts questions about the inductive source of his taxonomic theory, which claims to

define the necessary and sufficient conditions of mature science. Indeed, Hempel's theory places a heavy burden upon solving the problem of induction. How could one ever arrive inductively at sufficiently precise class definitions to satisfy the demands of his theory of taxonomy?

Hempel confronts this problem in the concluding section of his essay where he puts his theory to the test by applying to the particular case of mental illnesses. And, as Pouncey argues, the rigorous predictions required by deductive science seem beyond the reach of psychiatric diagnosis and do not yield the predictions needed to validate their categories. The compilers of the APA's Diagnostic and Statistical Manuals have sought to establish biomarkers that would supply extra-psychological grounds for their categories, thereby relieving clinicians of the burden of depending upon what their patients say and do. If diagnosis could be tied to neurological or genetic indices, it could borrow the theoretical basis and criteria of class membership from biology instead. But biomarkers have proved elusive and clinicians have had to rely upon what people say and do, after all.

Of course, Hempel can simply reply that this only shows that psychiatry is not a mature science. But although he does not abandon that theory of scientific maturity and returns to it in his final paragraph, he concedes that his taxonomic account does not describe the sorts of concepts available for psychiatric diagnosis and admits that it often doesn't work for other scientific research:

Classification, strictly speaking, is a yes-or-no, an either-or affair: A class is determined by some concept representing its defining characteristics, and a given object falls either into this class or outside, depending on whether it has or lacks the defining characteristics.

In scientific research, however, the objects under study are often found to resist a tidy pigeonholing of this kind. More precisely: those characteristics of the subject matter which, in the given context of investigation, suggest themselves as a fruitful

basis of classification often cannot well be treated as properties which a given object either has or lacks; rather, they have the character of traits which are capable of gradations, and which a given object may therefore exhibit more or less markedly.

Proceeding more inductively by attending to actual scientific research and concepts of mental illness, then, Hempel sets aside taxonomic classes and sketches a theory of concepts that presages the prototype theory that Eleanor Rosch introduced a dozen years later. He evokes Max Weber's "typological" method, which describes class membership in terms of proximity to ideal types, much as Rosch described proximity to particular prototypes that other examples "more or less" resemble in some way. Hempel characterizes the result as a more "dimensional" account of concepts:

Since each of the types distinguished in a typological theory will represent at least one quasi-linear ordering, typological systems usually provide for an arrangement of individuals along several axes, and thus replace classificatory schemes by reference "spaces" of several "dimensions."

Jung's polarity of introverted and extraverted types provides a familiar example of such dimensions. Introversion is not a "yes or no" property that could support a deductively inferred prediction. An individual may fall anywhere along such a continuum - and at different locations at different times or in different circumstances. And if he or she must be diagnosed based upon "spaces" of several "dimensions", prospects for prediction are lost in space. Bear in mind, also, that when diagnosing mental illness, one is attempting to assess individuals who often diverge from typical norms and that one must also reckon with the effects of therapeutic intervention. As Pouncey points out, the psychiatrist has to begin from a "penumbra" of symptoms, not with precisely defined classes that could support deductively inferred predictions.

But does predictive failure entail diagnostic nominalism - or only the failure or irrelevance of Hempel's taxonomic model of concepts? What

is there about starting from symptoms that thwarts taxonomic classification?

## II Interpretation

The answer is that symptoms are not simply empirical facts or causal events or conditions. They are *signs*, as I pointed out in an earlier edition of this bulletin, and signs of a specific sort. Standard medical usage distinguishing between signs and symptoms, using “signs” to refer to objective evidence observed by the physician and “symptoms” refer to subjective evidence observed by the patient. But the physician’s observations are also subjective experiences while the patient’s subjective experiences are only available for diagnosis through some form of overt expression observable by the diagnostician. But both are signs in the generic sense that their use and function depends upon their significance, their meaning. Still, symptoms are a distinct kind of sign, Susanne Langer noted in “The Logic of Signs and Symbols.”

There is a fine distinction between sign and symptom, in that the object signified by a symptom is the *entire condition* of which the symptom is a proper part; e.g., red spots are a symptom of measles and “measles” on the other hand, may be one part of a total condition which we associate with another separate part. Thus, a ring around the moon is part of a weather condition, but what it signifies is rain – another proper part – and not the entire state of “low-pressure” weather.

Nothing is a sign in or by itself. It can only signify for an interpreter. Diagnosis is therefore an interpretive or “hermeneutic” process. Moreover, since a symptom signifies the entire condition of which it is a part, the meaning of any symptom depends upon other symptoms, much as the meaning of a single word depends upon the sentence in which it stands and to which it contributes, so that one must wait until the last word to be sure of the meaning of the rest. Yet each following sentence may decisively alter the meaning of *its* predecessors. So, too, each session may alter the clinician’s interpretation of previous sessions and the symptoms noted therein. Moreo-

ver, the patient’s interpretation may not agree with the therapist’s – and the therapist’s interpretation must take that different interpretation into account as symptomatic. The task of diagnosis therefore cannot escape the problem of how to interpret signs correctly. Diagnosis is thereby drawn into the “hermeneutic circle,” the method of critical interpretation imposed by the fact that the interpretation of single words or signs, including behavioral and medical symptoms, depends upon forming an initial, tentative “sense” of the meaning of words or other signs and then circling back to revise that initial, hypothetical interpretation in light of other signs and revising that, in turn, in light of still other signs, much as one passes from single words to sentences to paragraphs to an entire statute or poem or ballet – and thence to wider and wider contexts of other signs and circumstances. This is especially evident in history, sociology and the other human studies since the significance of single actions or events often depends upon how historical agents interpreted one another’s words and actions and, retrospectively, upon the further course of events. “The great war,” the “war to end all wars” became only “World War I” with the beginning of “World War II.”

This means that the hermeneutic circle is always open or can only be closed by fiat, since life and history always go on to produce new signs and interpretations. The psychiatrist’s initial diagnosis is liable to revision by the patient’s responses to therapeutic intervention, including response to medication.

Consequently, understanding is always tentative and retrospective and cannot warrant or depend upon successful predictions. Karl Marx thought that his understanding of history enabled him to predict the withering away of the state and the end of private ownership of the means of production. His predictions failed egregiously, and no one has done any better. But the failure to predict history does not entail historical nominalism, the metaphysical conclusion that there really was no history, nor even that we can’t find out what really happened. It

only shows that inquiries based upon the interpretation of symptoms cannot yield the unambiguous class concepts required to deduce validating predictions. However, the lack or failure of predictions based upon diagnostic concepts doesn’t warrant nominalism or show that clinicians cannot find out what’s really amiss in their patients. But it does show why, when confronted with the reality of understanding mental illness, Hempel had to abandon the taxonomic theory that he had advertised as a criterion of scientific maturity.

## Appendix: Understanding and Explanation

Hempel’s discovery that his theory of taxonomy does not fit psychiatric diagnosis and his shift to Weber’s typological method echoes the distinction between explanation and understanding that Dilthey and Weber introduced at the end of the 19<sup>th</sup> century and that Jaspers appropriated in his *General Psychopathology*. Like Hempel, all three of these thinkers found that the methods and standards of the natural sciences did not fit the modes of inquiry and validation of the emerging historical, social and psychological disciplines, the human sciences or *Geisteswissenschaften*. Logical positivism – or as Hempel called his version, “logical empiricism” – only led to the conclusion that at best, they were only “young sciences.”

However, one must be careful not to take the contrast between explanation and understanding to be an exclusive disjunction. Successful prediction does not conclusively validate the hypotheses or categories of the natural sciences. Einstein’s theory of relativity is still being tested. The “image” of a black hole does not put an end to experiments that seek to verify or falsify it, much as the Michelson-Morley experiment revealed the limits of the Newtonian mechanics that had been mistaken for absolute and final truth.

Jaspers urged that a variety of methods should be welcomed for the diversity of perspectives they provide since the several perspectives complement the limits of each. Semiotics adds “significant dimensions” to physical and biological existence, whereas perspectives and meanings only exist in

embodied interpreters, without which the signs, symbols and symptoms would be mute.

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### Responses to Commentators

Claire Pouncey

I want to thank my commentators – Robyn Bluhm, Christian Perring, Awais Aftab, Melvin Woody, and James Phillips – for joining me in considering what is admittedly an unconventional route to old problems in philosophy of psychiatry. I appreciate the different perspective each author brought to the discussion: Woody challenges my conclusion about mental disorder nominalism; Phillips questions whether psychiatry even has a symptom penumbra in the sense I am willing to grant; Bluhm calls attention to the different foci on explanation and intervention in psychiatric science, and whether explanation should be the primary focus of research. Aftab recon-

siders whether my problem might be resolved by framing it in terms of dysfunctions rather than disorders; and Perring thoughtfully pushes the dialogue about why psychiatry may never be a mature science in the sense Hempel envisioned. Since I have no direct challenges for my discussants, each of whom addresses important aspects of theory in psychiatry, I will respond to them by developing the themes I identify in their remarks.

To clarify one point, my own focus on Hempel’s view of theory maturity and predictive power was not to endorse or dismiss Hempel’s view myself. I had hoped to call attention to psychiatry’s explicit adoption of that view circa 1960, and to admonish psychiatry as a whole for taking that one position so seriously, yet failing to challenge or modify it in response to psychiatry’s own predictive failures, and also significant changes in both psychiatric science and philosophy of science in the decades since Hempel addressed the APA. I deliberately steered clear of the relationship between identifying psychiatric symptoms and positing underlying causal disease entities, and what implications that might have for psychiatric taxonomy. I have argued elsewhere that Hempel and others’ focus on taxonomy as the identification of tokens of types has misdirected how both psychiatry and philosophy of psychiatry approach problems of psychiatric nosology, so I will not repeat that argument here.

That said, my initial inquiry addressed the question of whether psychiatry has any reason to assume that underlying mental disorders cause, explain, or otherwise ground psychiatric symptoms. I will respond to my commentators by addressing themes they raise in their discussions.

Several commentators thought my remarks entail a nominalism about disease entities. I suppose this is true, but not in the sense that typically lends itself to debates about mental disorder realism and antirealism. My essay meant to challenge psychiatry’s Big Nosological Premise – that discrete mental disorders exist and cause psychiatric symptoms – without addressing whether mental disorders should be understood as (in some sense) real, ontic, theoretical entities that are based in dysfunctions (or some other sense). Hempel’s address to the American Psychiatric Associa-

tion in 1959 did not question the Big Premise in any way, so perhaps my use of Hempel’s theory to reflect on the findings of the Event Horizon Telescope was misleading. Woody asks, “What is there about starting from symptoms that thwarts taxonomic classification?” This is an important and interesting question, to be sure, but it is not the one I was trying to address. My concern was that in psychiatry we can be realists about symptoms (as interpreted by those who experience them as well as by the professional to whom they are described), but we do not have any evidence for the Big Premise that where there are symptoms there must be an underlying mental disorder. I would describe my position as being a realist about symptoms experienced by individuals, and agnostic about realism versus nominalism with respect to mental disorder disease entities.

Aftab wonders whether, in the absence of a psychiatric theoretical framework to posit mental disorder entities, I might be satisfied to conceptualize mental disorders as dysfunctions, whereby mental symptoms arise when natural functions depart from what they are naturally selected to do. I find that renaming “disorders” as “dysfunctions” creates more problems than it solves, given my own view that “natural function” and “dysfunction” can only be determined with respect to a specific explanatory context, thus adding a good dose of relativism to my skepticism about mental disorders as separate entities that cause psychiatric symptoms. Indeed, Aftab comes to something like this conclusion on his own.

To Phillips’ challenge that in psychiatry we do assume underlying disease entities, and shape treatment accordingly, my response is that there may be practical reasons for doing so, and patients sometimes benefit from treatment decisions made under this assumption. However, this does not confirm, or even support, psychiatry’s Big Premise. Successful treatment choices, although hopefully based in good psychiatric science and clinical judgment possible, do not support the premise that mental disorders exist and cause symptoms. At best, they suggest underlying causes of and associations with symptom penumbra, and provide direction for investigating a presumed disorder. But many treatments in psychiatry, pharmacologic

and otherwise, are neither precise nor specific.

I think Bluhm would join me in reminding Dr. Phillips that the pharmacologic choices he offered for ‘neurotic’ versus ‘psychotic’ anxiety are neither fixed algorithmic options, nor do they work preferentially and consistently on the underlying kinds of anxiety he suggests. That is, a person with (what we assume to be) neurotic anxiety sometimes responds better to an antipsychotic medication, and many people with (what we assume to be) paranoid, psychotic anxiety often respond to antidepressants and benzodiazepine medications. Since these pharmacologic treatments have been developed without ever proving an underlying disorder call ‘anxiety,’ and because they all provide some relief for both neurotic and psychotic anxiety, I hold my stance that we have no reason to assume a black hole exists either for individual mental disorders or for the concept of underlying, causal mental disorders in general. I can use a pharmacologic strategy and only speculate that the symptom consists in some state of affairs – not necessarily the mental disorder Anxiety – that corrects with stimulation of certain neuroreceptors. If I treat the anxiety with cognitive behavioral therapy, I would only have to commit to the existence of a symptom penumbra in which cognitive and behavioral changes affect anxiety symptoms. To bring this point home, I remind us all that millions of people are self-treating the anxiety created by opiate withdrawal with more opiates. Psychiatry does not treat this anxiety penumbra with methadone or buprenorphine because of realist assumptions about the reality of the disorder underlying the anxiety penumbra, but only to alleviate the symptom.

Perring surmises, and I confirm his suspicion, that part of my agnosticism (Perring calls it “pessimism,” but that’s too strong) about finding mental disorder entities as underlying causes of mental symptoms is that “Psychiatry is difficult and we are not that smart.” I would phrase the position differently. Psychiatry encompasses many fields of inquiry – medical science, psychological science, cognitive science, philosophical assumptions, plus others – as Perring describes, and I do doubt that we can find unifying psychophysical laws that govern all the ontic possibilities these various disciplines posit. Any such

attempt would fumble with category errors, the problem of induction, and difficulties in generalization that scientific laws require. However, if we are not looking to prove the Big Nosological Premise and find a unifying theory for all mental disorders (and I argue that we shouldn’t be), differences in ontological, epistemic, and methodologic perspectives are not the biggest problem psychiatry faces. We don’t need to agree on what the “true” science of psychiatry is (more on this below). If our piecemeal findings and theories ever converge (and again, I’m not saying they won’t, simply that we can’t bank on it), the epistemic virtue of consilience, which is widely held in psychiatry, would lead us to consider that a good thing, if not actually a path toward truth.

For me, this piecemeal approach to psychiatric inquiry does seem to entail Perring’s posit that my agnosticism about disease entities stems from the fact that psychiatry is difficult and we’re not that smart. To reframe, I would say that psychiatry is complicated and its theory is already complex, so even smart, thoughtful people struggle with how to approach questions that can be addressed in many ways by many disciplines. We cannot possibly know what we don’t know, and this is true for our own cognitive limitations as much as any other question in psychiatry. Calling psychiatry an “immature science” in unnecessarily derogatory if the heart of the problem is that it isn’t actually a single science, or that it can only be explained by unreducible theories (i.e., not by understanding mental disorders as dysfunctions). We can respect psychiatry for being complicated and challenging without deciding in advance either that psychiatric symptoms cannot be explained, or that there will never be a unified theory of explanation. What we can’t do is *assume* that the Big Nosological Premise is true, though who knows? Maybe it is.

I do not think the complexity of what we encompass under the umbrella of psychiatric science is a reason not to try to study the distress caused by mental symptoms and how to treat them. I am a realist about symptoms, after all, despite the problems of reference, causation, interpretation, and subjectivity that Phillips and Woody well note. Bluhm reminds us that psychiatry’s philosophical assumptions have practical consequences, especial-

ly when research funding is directed to understanding and explaining our ostensive black hole in lieu of, or in preference to, seeking treatments for the symptom penumbra. It may be that we cannot develop successful and safe treatments without confirming and understanding a black hole, as the NIMH RDoC project assumes, but without understanding the black hole we still have successful treatments for symptoms, however limited or imperfect they may be. In this regard, I maintain both my realism about symptom penumbra and their treatments, and my agnosticism about whether underlying mental disorders cause those symptoms.

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*(continued from page 1, Editor)*

DSM-5 deals with this symptom promiscuity with what it calls “Cross-Cutting Symptom Measures.” The adult version of the cross-cutting measure consists of 23 questions that cover 13 areas of psychiatric concern: depression, anger, mania, anxiety, somatic symptoms, suicidal ideation, psychosis, sleep problems, memory, repetitive thoughts and behaviors, dissociation, personality, and substance use. Each of the areas of concern has 1 to 3 questions. The 23 questions are asked, and each response is scored on a frequency/severity scale (0=not at all; 5=severely or daily). Items with a score of 2 (mild) or more, require more intensive evaluation, which is designated as Level 2. These measures are complicated and very time-consuming; it’s difficult to imagine anyone using them except, perhaps, researchers. But aside from these concerns over practicality, there’s another oddity with the cross-cutting measures, that of – to exaggerate a bit – everything being a symptom of everything. If anxiety can be a symptom of depression and depression can be a symptom of anxiety, just how different are anxiety and depression disorders from one another? Some have suggested that we should have diagnoses of anxiety, depression, and anxiety-depression, and it is unclear why the latter category was rejected in the DSM-5 developmental process. In any case, if anxiety is to serve as a penumbra of underlying psychopathology, we have no idea of what that psychopathology might be.

That question leads from symptoms to diagnostic categories. If symptoms are too messy to qualify as the penumbrae of underlying psychopathologies, how well do categorical diagnoses qualify as underlying psychopathologies? It turns out that they're just as messy as symptoms. The diagnostic categories of the DSM and the ICD are a series of boxes, and we all know that the goal of the diagnostic manuals is to fit each presenting patient neatly into one of the boxes. The failure of the DSMs to accomplish this fit goes all the way back to DSM-III with its Hempelian operational definitions. We have just seen that symptoms don't behave themselves and agree to reside in just one box. The cross-cutting measure allowed them to find a home in several boxes.

Where does this leave the diagnostic categories? Answer: all over the place, with comorbidities, fuzzy boundaries, and failed etiologies. Moving toward DSM-5, the failure of previous manuals to establish diagnostic validity [read *boxes*] culminated in the 2002, pre-DSM-5, white paper, *A Research Agenda for DSM-V* (Kupfer, First, and Regier 2002), in which the editors wrote in the Introduction:

In the more than 30 years since the introduction of the Feighner criteria by Robins and Guze, which eventually led to DSM-III, the goal of validating these syndromes and discovering common etiologies has remained elusive. Despite many proposed candidates, not one laboratory marker has been found to be specific in identifying any of the DSM-defined syndromes. Epidemiologic and clinical studies have shown extremely high rates of comorbidities among the disorders, undermining the hypothesis that the syndromes represent distinct etiologies. Furthermore, epidemiologic studies have shown a high degree of short-term diagnostic instability for many disorders. With regard to treatment, lack of treatment specificity is the rule rather than the exception (2002, xviii).

Returning now to the event horizon metaphor, we can recognize that psychiatry can claim neither a clear penumbra nor a clear black hole. Claire argues that psychiatry's Hempelian

failure need not be a problem. She writes:

The good news is that we don't need an event horizon telescope for psychopathology. The bad news is that psychiatric nosology may always be ad hoc. In clinical work this may not matter, since what we treat are symptoms and behaviors in individuals – treatment addresses what we already know, not what we theorize. If I treat Mr. A's anxiety symptoms, he might feel better even if there isn't *really* an underlying disorder that explains his anxious feelings...Psychiatry already has the penumbra, but we have no reason to assume a black hole.

This is perhaps a little too simple. We would, for instance, treat Mr. A's anxiety symptoms differently depending on his "underlying" condition. If we think he suffers from simple anxiety, we might use a benzodiazepine or an SSRI antidepressant, while, if we think his anxiety is part of a psychotic condition, we might prescribe an antipsychotic. We do in fact assume underlying psychopathology, however messy, unclear, and unHempelian it might be. So, finally, although we fail at any effort to apply Hempel's model, we do work with a very crude simulacrum of it. That does work much of the time, and we continue in this manner without any expectation that we are heading toward a full Hempelian psychiatry.

JP

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(continued from page 1, President)

How to think about the hypothetical mechanisms is one of the big and interesting questions in the philosophy of psychiatry. If you want to have some fun thinking about this question and work on your own views, Claire's target article and the many fine commentaries that follow offer an accessible way to begin.

Peter Zachar, Ph.D.

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