AA P&P Association for the Advancement of Philosophy and Psychiatry

# Bulletin

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## *From the Editor*

Welcome to another issue of the AAPP Bulletin. We continue with the format of target article, commentaries, and response by author. Our target article is Awais Aftab's Experimental Philosophy of Psychiatry, which has led to a stimulating and informative discussion.

In addition, in this issue of the Bulletin we honor the memory of two recently deceased AAPP members, Osborne Wiggins and Louis Charland.

As is now our routine, this issue of the Bulletin will be accompanied by a target piece for the next issue. We have invited Dan Stein, Professor and Chair of the Department of Psychiatry, University of Cape Town, South Africa, to provide us with a chapter, The Pleasures of Life, from his recent book: *Problems of Living: Perspectives from Philosophy, Psychiatry, and Cognitive-affective Science.*

We look forward to commentaries and a lively discussion of this chapter.

I begin below with my own commentary on Awais Aftab's target article.

### Concepts and Facts

In commenting on Awais Aftab's challenging paper, let me start with a general comment about X-phi. X-phi research does not study conceptual issues; rather, it studies people's reactions or thoughts about them. It takes a vote of who thinks this and who thinks that. And the 'who' may be a professional or it may be a layperson. To someone with a skeptical bent like myself, this kind of analysis evokes a first response such as 'who gives a hoot what so and so thinks, what does that tell me about the conceptual issues in question'.

Let me pursue this line of thought with a couple core conceptual issues: empirical data and disease. At the beginning of his article, Aftab proposes getting beyond conceptual disputes by bringing in empirical data (or empirical research). When Awais writes at the beginning of his article of leaving one's conceptual armchair and seeking empirical data to "advance the debate," a naïve reader like myself expects more from 'empirical data' or research than a vote.

This of course requires agreeing about what qualifies as empirical data. What I think is empirical may not fit your idea of empirical. If we don't want to decide this conceptually, from the armchair, we're left to decide it, X-phi-wise, with empirical data or research – that is, taking a vote.

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## President's Column

Christian Perring

### Remembering Ozzie Wiggins and Louis Charland.

This year we lost former Executive Council member Osbourne Wiggins and current member Louis Charland. Ozzie a founder member of AAPP, while Louis had been involved in more recent years. We mourn their loss. Here I share some memories of both men. There will also be pieces about them published in PPP.

**Osborne Wiggins**

I only met Ozzie Wiggins a few times. He had been faculty at the New School in Manhattan, and he then moved to the University of Louisville. As he makes clear in his Psagacity interview with John Sadler from November 2011, his work in philosophy of psychiatry was from the start a collaboration with Michael A. Schwartz, which was a lifelong partnership. It was rooted in the phenomenological tradition of Husserl and Heidegger. He describes himself as providing the theory, with Schwartz tying it down to psychiatric reality. Wiggins explains that his personal motivation is to develop an adequate theory of consciousness that can accommodate and even explain phenomena of psychopathology. His interview captures his careful manner, his immersion in his scholarly project over decades, and the dynamics of his collaboration with Schwartz. While he mentions at one point that their dialectic would occasionally involve them having passionate disagreements until they moved to a point of agreement. But most people found Wiggins very calm and steady.

*Ed Hersch* writes, "I didn't know Ozzie Wiggins very well but I did meet him on a number of occasions at AAPP meetings in the 1990's and early 2000's. What I do recall about him though was that he was always a very warm and friendly presence at these meetings and he was very welcoming and encouraging to me, particularly in my early days as a newcomer to the group. I enjoyed his presentations and written work a lot as well."

*Nancy Potter* writes, "I first met Ozzie when I went to the University of Louisville for a campus interview. Ozzie took me on a tour of the city and, while driving around, I asked him about choral opportunities. He popped in a recording of the Bach Society as an introduction. It turned out that Oz was a music scholar and had been a jazz musician himself; he was knowledgeable about all

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# Experimental Philosophy of Psychiatry

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> … the very notion that we could develop a theory of health and disease without any reference to empirical data strikes me as quite weird if not absurd, and yet seems to be the dominant view in the philosophy of medicine. Walter Veit (1)

There is considerable debate within philosophy of psychiatry regarding how to best conceptualize psychiatric conditions and how to approach related issues such as the naturalism/normativism debate, the nature of psychiatric classification, and the boundaries of medicine in mental health. So far, this debate has predominantly taken the form of "conceptual analysis", where reason, intuition, and argumentation are applied to clarify the concepts and better understand what they refer to. There is no essential need in the project of conceptual analysis to leave one's armchair and seek empirical data to advance the debate. Conceptual analysis does make progress by weeding out views that are inconsistent, incoherent, or otherwise incompatible with other generally accepted philosophical views. It also helps in articulating with greater philosophical precision what coherent views are available to us. However, conceptual analysis frequently results in stalemates between differing conceptualizations. Different accounts of "disease", for instance, agree on what constitutes "paradigm cases" of what is disease and what is not (cases which are uncontroversial and almost universally accepted), but disagree on how to categorize "controversial cases". This disagreement can come in essentialist or non-essentialist flavors. In an essentialist setting, the disagreement arises because the conceptual accounts offer different necessary and sufficient conditions for what constitutes a case or instance of the concept under question. In a non-essentialist setting, necessary and sufficient conditions are not available because they do not exist (there is no objectively correct answer "out there"), and the controversial cases represent differences in how pragmatic considerations and value judgments are weighed and applied.

Such stalemates have proven extremely difficult to resolve. Lemoine argues that it is "hopelessly unlikely" that conceptual analysis can decide between "two reasonably successful definitions of 'disease'" (2) because conceptual analysis by itself does not offer us definitive reasons to favor one coherent, consistent account over another coherent, consistent account (1).

Dissatisfaction with this state of affairs has resulted in increasing work being done in the area of "experimental philosophy" (X-phi), which applies the methods of psychology and social sciences to theoretical debates in philosophy (3). X-phi doesn't claim that it can provide definitive resolution of these philosophical debates (neither can conceptual analysis, for that matter) but that we cannot make *progress* unless we utilize empirical methods.

In this article, my goal is two-fold.

1)Provide a brief review of empirical research on conceptual issues in psychiatry.

2)Using insights gleaned from the X-phi literature, discuss how empirical methods can inform conceptual debates in psychiatry.

I will note here that X-phi is not without fierce critics in philosophy, and the legitimacy and the relevance of empirical methods has been subjected to much critique from philosophers. Engaging with that critique is not a primary focus of my discussion here but is otherwise a necessary endeavor for the broader defense of X-phi of psychiatry.

Jerome Wakefield on numerous occasions has also indicated that empirical studies have an important role to play as an adjunct to conceptual analysis. He is very conscious, however, of the need to design, execute, and interpret them with great care.

He writes:

> Moreover, empirical study of conceptual issues is challenging, especially when it comes to designing the experimental manipulation for testing rival hypotheses to yield relatively unambiguous outcomes. This is because concepts interact in a variety of ways with the background web of beliefs to yield classificatory judgments, so judgments in response to a target vignette can represent many different things (pg. 76) (4).

As an illustration, he gives the example that he observed while testing vignettes with students in social work that if the vignette contained a history of sexual abuse as a child, it enormously increased the attribution of mental disorder, irrespective of other variables. The attribution was not driven by the underlying concept of mental disorder but a widely prevalent belief at the time among mental health professionals that childhood sexual abuse almost invariably leads to mental illness later in life.

I would like to emphasize this need for caution highlighted by Wakefield at the outset.

## Empirical Research on Conceptual Issues in Psychiatry

In this section I will review the findings of some empirical studies that have been conducted to investigate conceptual issues in psychiatry. Although the relevant body of literature is relatively small, this is neither a comprehensive nor a systematic review; my intention is only to highlight select

findings for the purposes of illustration [see Ralston 2013 for another review of empirical perspectives in philosophy of psychiatry (5)]. Readers are referred to the original studies for details regarding study methodologies and statistical details of the results.

### Survey Studies and Questionnaires

Much of existing empirical research in conceptual psychiatry takes this form. It is the least philosophical of the empirical studies, and as we'll see, subject to numerous constraints, but they are easier to conduct, and can still be informative. Consider my own survey study on conceptualization of mental disorder at a US academic medical center, recently published in *Journal of Nervous and Mental Disease* (6).

We conducted a survey study of how healthcare professionals at UCSD, a large teaching hospital system, understand the notion of mental disorder, with a particular emphasis on contemporary themes related to 'biological psychiatry'. The participants were: medical students; trainees and faculty in the departments of psychiatry, neurology, family medicine and geriatric medicine; nurses working on the inpatient psychiatry units; and social workers in the department of psychiatry. The survey included nine conceptual statements inquiring about respondents' philosophical understanding of mental disorders and a list of 12 conditions accompanied by the statements "[This state of being] is a disease" (this statement was taken from the FIND study, see below) and "The etiology of [this state of being] is best explained in terms of biological mechanisms". Respondents were asked to rate their agreement or disagreement using the same five-point Likert scale. We received survey responses from 209 respondents, with over-all response rate around 17-18% (209/~1200). Respondents in the fields of psychiatry and psychology together constituted about half of the sample, and about half of the sample was still in training.

The survey results showed interesting patterns of consensus as well as lack thereof. At face value, the range of responses suggests considerable lack of consensus: responses to every item (with one exception) in the survey ranged from "strongly agree" to "strongly disagree"; to put this into perspective, this (surprisingly) also includes disease status attribution for homosexuality. The sole exception was the item about biological etiology attribution for schizophrenia, which ranged from "strongly agree" to "disagree to some extent".

Despite the range, certain statements did generate substantial consensus. The conceptual statement with the most consensus was: "The diagnosis and classification of mental disorders is influenced by social, cultural, moral, and political values." (93.3% agreed). No surprises there, as the statement was deliberately designed to apply very broadly and was expected to generate consensus. The conceptual statement with the least consensus was: "All mental disorders are diseases." Opinions were almost equally split, resulting in over-all neither agreement nor disagreement. Nearly 75% agreed that "Mental disorders must cause distress or functional impairment to be considered disorders."

Among those who agreed or strongly agreed with "all mental disorders are diseases", 57% (43/57) disagreed or strongly disagreed with "For a condition to be a mental disorder, there must be an underlying biological abnormality", suggesting that the presence of biological abnormality was not seen by many as a necessary criterion for "disease" attribution.

The following conditions were considered diseases with >75% of the respondents in agreement (strongly agree or agree to some extent): schizophrenia, alcoholism, gambling addiction, binge eating, social anxiety, and pedophilia. Ho-

## 2022 AAPP Annual Conference

### *Open Meeting*

**May 21-22, 2022**
**New Orleans, LA, USA**

Although past conferences have been organized around a specific theme, for the 2022 annual meeting AAPP invites abstracts for presentation on any topic that addresses philosophical issues relevant to psychiatry or psychiatric issues relevant to philosophy. Our aims for this conference are to bring together thinkers from a variety of disciplines who are interested in philosophy of psychiatry and to provide a forum for the presentation of work on both emerging and classic questions in 'philosophy of psychiatry'.

**Conference Organizers**

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Abstracts of 500-600 words, prepared for anonymous review, should be sent to Jon Tsou (jtsou@iastate.edu) and Peter Zachar (pzachar@aum.edu). Questions about the conference should also be addressed to either of them. Abstracts are due on November 15, 2021. Notices of acceptance or rejection will be sent by January 15, 2022. Presentations will be strictly limited to 20 minutes, followed by 10 minutes for discussion.

mosexuality and transgender identity weren't considered diseases by >75% of respondents. The disease status of grief, occupational burnout, absence of sexual desire, and narcissistic personality was more contested.

There was a statistically significant correlation between disease status attribution and biological etiology attribution for all conditions except homosexuality and transgender identity. That is, perceptions of whether the etiology is best explained in terms of biological mechanisms correlated with tributions of disease status (the influence is likely bi-directional).

Responses were generally similar for respondents from various fields, and differences were generally of small magnitude and reflected differences in emphasis rather than polar opposite views. Respondents from psychology were less likely to characterize conditions as diseases, and were less likely to agree with the statement "all mental disorders are diseases", a difference that was statistically significant.

Let's take a look at some of the other studies.

In the Finnish Disease (FIND) study (7), the largest such survey, responses were collected from more than 3000 respondents, consisting of physicians (including psychiatrists), nurses, lay persons, and members of the Parliament of Finland. The respondents were asked to what extent they considered 60 conditions to be "diseases"; twenty of these 60 conditions were related to psychiatry or mental health concerns. Similar to our survey (which offered a replication of sorts of the FIND survey), there were meaningful patterns of consensus and variance. Schizophrenia and autism were considered to be diseases by >75% while grief and homosexuality were considered not to be diseases by >75% in each group. In contrast, there were large differences in perceptions for alcoholism, work exhaustion, insomnia, drug addiction, gambling addiction, and social anxiety disorder.

(It is interesting to note that in our study the disease status of grief was rather contested, while in the FIND study there was widespread consensus that it is not a disease.)

Harland et al. (8) looked at how psychiatric trainees (N=76) conceptualize mental illness using Maudsley Attitude Questionnaire (MAQ). The MAQ investigates the application of eight models of mental illness (biological, cognitive, behavioral, psychodynamic, social realist, social constructionist, nihilist, spiritual) to four psychiatric disorders (schizophrenia, major depressive disorder, generalized anxiety disorder, and antisocial personality disorder). With the exception of schizophrenia, where biological model was most strongly endorsed, there was no exclusive commitment to any particular model. Analyses revealed that the views of trainees were often organized across three dimensions: biological vs non-biological, eclectic (a mix of different models), and psychodynamic vs sociological.

Survey studies, such as the ones discussed above, are difficult to interpret philosophically for a variety of reasons.

*i) Varied Interpretations*

An important limitation is that such surveys do not typically explore the *reasoning* behind the answers. Particularly they do not distinguish between intuitions (classically a view held based on instinct, without much conscious deliberation) vs deliberate, conscious reasoning. In fact, it is quite possible that the respondents may intuitively have felt a certain answer to be correct, but selected a different answer based on their education, conscious theoretical commitments, or conscious reasoning.

Interpretation of the question or the scenario can differ substantially, such that in a manner of speaking, the respondents are answering different questions rather than the same. For example, when asked "Is grief a disease", if respondents hold widely divergent views of what constitutes disease, the answers may not be "commensurable". As our survey shows, it cannot be taken for granted that people are referring to the same concept when they refer to something as "disease". Furthermore, not only is the interpretation of "disease" subject to interpretation, but so are the individual conditions. For instance, what exactly did respondents have in mind with regards to "grief". Did they interpret grief *broadly* to include states of grief that are severe enough to be labelled as complicated grief or meet criteria for major depressive disorder, or did they interpret grief *narrowly* to refer only to 'uncomplicated' grief which does not reach 'clinical significance'?

Thus, we have at least three sources of variance, which the survey cannot disentangle:

Varied interpretations of what "disease" means.

Varied interpretations of what "grief" means.

Varied intuitions/views on whether "grief" is a "disease".

*ii) "Heisenberg effect"*

Based on feedback from respondents, I realized that in some instances the very act of filling out the survey may have modified the views held by the respondents. This is because the succession of questions was perceived by some as a subtle form of "Socratic questioning" which led respondents to critically analyze their own views, in an effort to make them more coherent or consistent. This was particularly so where the previously held views were poorly articulated or because the participants had never deliberated on these questions in any meaningful way.

*iii) Disconnect between theory and practice*

Respondents can express certain theoretical commitments, but these commitments may not carry over to their practice, and an examination of how they interact with patients and

make decisions in clinical settings may reveal different views.

## Vignette-Based Experiments and Qualitative Studies

*i) Ahn*

Woo-Kyoung Ahn and her research group (9) have conducted a number of X-phi studies (including collaborations with Joshua Knobe, a leading experimental philosopher) examining beliefs held by clinicians related to causal understanding of psychiatric symptoms. Their research studies typically consist of presenting subjects with a variety of carefully designed vignettes in which specific details have been altered to assess the impact of those changes on resulting responses. They have summarized their rich body of research in a book chapter (10).

In various studies they have found:

- Lay people (such as undergraduate students) tend to have an essentialist view of mental disorders with the symptoms being produced by an underlying common cause, while practicing clinicians express more ambivalence, neither clearly agreeing with them nor disagreeing. The exact reasons are unclear, but the authors speculate that perceptions of heterogeneity of disorders on part of the clinicians may be a likely reason.
- Even though DSM symptoms are descriptive and generally given equal weight in criteria, clinicians often approach DSM symptoms using theories of causation according to which some symptoms are seen as more causally central and others as seen as more peripheral (for instance, in anorexia, distorted body image was seen as more causally central and given more importance, while amenorrhea is rated as more peripheral and diagnostically least important).·

In the absence of a mind-brain dualism, biological and psychological explanations are best understood as explanations at different levels of explanations, but clinicians (as well as laypeople), tend to see them as complementary. The authors discuss that this appears to be reflective of an *explanatory* dualism (not *metaphysical* dualism), that is, there are situations where psychological constructs or biological constructs are seen as providing a better form of explanation compared to the other. This is not by itself irrational, but their research shows that it does tend to generate an irrational bias, where biological and psychological explanations are seen to have an inverse relationship. That is, if psychological causes are considered to be more relevant by the respondents, then biological causes tend to be discounted, and vice versa.·

Presenting behaviors as concrete, as pertaining to a specific, named individual increases the likelihood of being understood as psychologically based, and in contrast, presenting behaviors in the abstract, not pertaining to any specific individual, increases the likelihood of being understood as biologically based. This applies to mental disorders, everyday behaviors, and also downstream to perceptions of appropriate treatments.·

*ii) Wakefield*

Wakefield has conducted several X-phi studies to determine support for the harmful dysfunction notion of mental disorder. The studies Wakefield conducted utilized vignettes about conduct disorder and studied them in samples of clinicians (clinical psychology and clinical social work) and lay persons (typically nursing or social work students who had not yet taken any mental health courses and had no clinical mental health experience).

Wakefield created three sorts of vignettes for conduct disorder, all of which satisfied DSM-IV criteria; vignettes with basic details and symptoms only, vignettes where additional environment context is provided and symptoms are presented as arising in response to sexual trauma or gang violence, and vignettes which did not provide information about symptoms occurring in response to life events and implied that behaviors seemed out of proportion or beyond normal range (Wakefield intended these vignettes to represent "internal dysfunction") (11, 12).

In both clinical and professional samples, the youth was considered to have mental/psychiatric disorder more often in the "internal dysfunction" vignette compared to the environmental context vignette. (As one example, in one of the specific vignettes, 54% of clinicians attributed mental disorder to symptoms -only version, 13% to environmental context vignette, and 91% to "internal dysfunction" vignette (12). Bear in mind that all three vignettes satisfied DSM-IV criteria.)

Although this was not included in the published studies, Wakefield has revealed in a recent book (4) that he and other researchers also collected additional data which asked clinicians if the problematic behaviors were a likely result of "dysfunction of some cognitive, affective, or other mental mechanism in the youth" and asked lay people if it seems likely that "something is wrong with this youth's mind". The attribution of "dysfunction" or "something wrong with the mind" was much higher in the internal dysfunction vignette compared to environmental vignette (82% vs 24% and 76% vs 17% respectively).

The experiment does seem to suggest that an inference of "dysfunction" is involved in disorder attribution, but there is little indication that "dysfunction" is understood in the evolutionary and essentialist manner that

Wakefield understands it (failure of a mechanism to perform a function for which it is naturally selected). It could very well be that respondents are employing an understanding of dysfunction that Kendler has referred to as "common-sensical" in his *Psychiatric Times* interview:

> The general idea of dysfunction is common-sensical – that the relevant psychobiological system is not doing what it is supposed to do. Examples might include providing your higher centers with an approximately veridical sense of the world around you, keeping levels of anxiety roughly appropriate to the real dangers being confronted, producing mood states approximately congruent to the environmental situation, etc. (13)

Wakefield's experiments suggest that at least some DSM criteria may not fulfil DSM's own requirement for the presence of a dysfunction. If intuitions of dysfunction are intricately linked to environmental considerations (particularly the idea that symptoms are in some sense out of proportion to or unrelated to the situation), then such considerations may need to be incorporated within formal diagnostic criteria.

*iii) Ralston*

Inspired by Fulford's ordinary language philosophy approach as well as X-phi, a study by Alan Ralston et al, presented in his 2019 PhD thesis (14), is an elaborate study designed to uncover the philosophical assumptions inherent in psychiatric practice (N= 30 psychiatrists). They collected empirical data using audio tapes and psychiatrists' written reports of clinical encounters with patients, MAQ (Dutch translation), semi-structured interviews, and final case reports characterizing the philosophical views of each psychiatrist (with comments and corrections sought from the psychiatrists for validation). The study findings are difficult to describe here with any justice, but the overall picture that emerges is characterized by:

Theoretical pluralism: use of multiple forms of reasoning and explanations without necessarily leading to an integration in the form of a singular diagnosis or case formulation (DSM played a relatively marginal role). Hence heterogeneous ontologies of disorder were produced.

Causal dualism: a dualism between biological and psychological explanations (similar to findings by the Ahn group).· Values-oriented pragmatism: clinical actions and conceptualizations were guided by supposed contribution towards a desired outcome..

Clinical realism: as a basis for their legitimacy, psychiatrists prioritized the reality of the clinical situation and their role as professionals in navigating this reality towards a beneficent outcome.·

## Using Empirical Studies to Inform Philosophical Debates

Veit argues that empirical studies can reveal substantive agreement or disagreement when it comes to concepts of health and disease, and each has different implications for the conceptual debate at hand (1).

If the studies show a lack of consensus, this can potentially be due to methodological sources of variance we have discussed above. But if such sources of variance are controlled or accounted for in future studies, this can potentially cast doubt on the notion that the concepts in question have a singular nature, paving the way instead for conceptual pluralism. For instance, “disease” may refer to different notions in different individuals or different communities. The important task in such a situation would be clarify the different notions of disease that may be at play, and to determine the roles they serve. An alternative route may be for the philosopher to argue that the diversity of intuitions should be dismissed, because the intuitions of some ought to be privileged over the rest. Such a view, however, is not without its challenges.

If the empirical studies show substantive agreement in their characterization of cases as instances of “disease” (or not “disease”), this suggests that we have more paradigm cases at our disposal which can be used to “test” philosophical theories. Philosophical theories which are better able to account for the expanded set of paradigm cases would be at a distinct advantage.

As we have seen above, studies in psychiatry don’t reveal straightforward consensus or lack of consensus, rather they tend to show a mixed pattern of consensus. Available studies, therefore, provide support for both conceptual pluralism as well as possible expansion of paradigm cases. This is relevant to philosophical reasoning in psychiatry with regards to concepts of health and disease.

For instance: if a scholar were to argue that anorexia nervosa is not a “disease”, she will have to take into account that >75% of psychiatrists, physicians, and nurses, and >50% of lay persons and members of parliament considered anorexia to be a disease in the FIND survey study; consensus of such magnitude, especially if it is reflective of differences in underlying concepts, cannot simply be dismissed in favor of one’s own preferred concept. At the very least, one is forced to take into account the reasons why so many consider it to be a disease. That is, one either has to contend with the reasons for which majority of people consider anorexia to be a disease, and show that those reasons are erroneous and inaccurate, or one has to acknowledge the possibility that the notions of “disease” held by the respondents differ from the critic’s notion of “disease”. If multiple notions are at play, then what are the grounds for privileging one notion over the others? X-phi, therefore, has the potential to challenge the sort of unquestioning, often essentialist, views of disease adopted by many in the philosophical community.

The argument is contingent on the "validity" of the empirical literature, i.e. the findings of the literature do indeed say something meaningful about underlying concepts and intuitions, and are not instead reflective of judgments peripheral to underlying concepts. A hardline essentialist can also argue that if that she is right, and "disease" does exist "out there in nature", then whether clinicians think that is the case or not is beside the point. Such a critic may indeed be unconvinced by X-phi, as such a critic is not concerned with the concept of "disease" as it exists in clinical practice but as it exists in nature. Most philosophers, however, I would like to think, are as concerned about the clinic as they are about nature, and a concept of disease that has no relationship to the concept of disease as it exists in clinical practice is not likely to be of much use.

It also appears to me that "disease", "disorder", "illness", "medical condition", and "psychological condition" all have very different connotations and are likely to produce different intuitions. Even though many commentators such as Wakefield treat "disorder" and "disease" synonymously, it would be interesting to design vignettes with different terms while controlling for other variables.

An important consideration is when studies show obvious signs of irrational thinking (for instance, viewing biological and psychological explanations as having an inverse relationship). This can suggest several things. One is the need for clinicians to make explicit their implicit philosophical assumptions, so that they can be examined [what G. Scott Waterman and I have described as "conceptual competence" (15)]. Another course of action may be for philosophers to examine the views of the clinicians in such instances and determine if beneath the veneer of irrationality there are any insights that are worth preserving and articulating in a more rational manner. Thirdly, if clinicians and laypersons are susceptible to such biases, there is no reason why authors working in the area of philosophy of psychiatry cannot be. And this provides reason to be on the lookout for signs of such biases in professional literature. My own informal impression is that causal dualism is not uncommon in the conceptual literature.

This body of literature also shows that philosophers should be cautious in broad characterizations of psychiatric clinicians. For instance, non-clinicians tend to over-estimate the degree to which clinical practice is guided by DSM. As Ralston's study shows, DSM plays a relatively marginal role, and clinicians typically rely on theoretical pluralism. Other broad mischaracterizations can include thinking that psychiatric practice is predominantly based on a "disease model" or that psychiatric clinicians are overwhelmingly biological reductionists.

X-phi literature suggests that it also appears to make a difference whether the question is framed as an abstract question versus a concrete one. For instance, in X-phi research on compatibilism, intuitions regarding compatibilism differ if the scenario is referring to an abstract universe versus if the scenario refers to the universe we live in. Respondents are more likely to endorse compatibilism in the latter case (16).

Studies by the Ahn group show that a similar abstract vs concrete effect is present with regards to how psychiatric clinicians employ causal explanation. Behaviors of a specific, named individual are more likely to be viewed as psychologically based, compared to the same behaviors in the abstract.

This may present a particular problem for the philosophers who, by the very nature of their work, are more predisposed to abstract thinking. If the intuitions differ in abstract vs concrete cases, are intuitions in one setting more correct than another? Is this a systematic difference in intuitions, or is this a form of cognitive bias?

X-phi literature also shows a curious phenomenon of fixed views regardless of the details of the scenario presented. In X-phi research some individuals show an extreme insensitivity to details, with rigid and pervasive expression of beliefs. For instance, there are some individuals who express a belief in free will even in cases where the scenario presented is too fatalistic to allow free will, with this phenomenon being described as "free will no matter what" (17).

An analogue of such thinking may have been present in my survey study as well: eight respondents in the sample (3.8%) agreed that the etiology across the board for all 12 conditions (even "occupational burnout" or "grief") was best explained in terms of biological mechanisms, suggesting that views reflective of extreme biological reductionism are present but uncommon. We can perhaps consider it "biological no matter what".

A prominent chunk of X-phi has been devoted to what has been called "the negative project" (3). This sort of research suggests that we cannot rely uncritically on intuitions, because these intuitions are distorted or influenced by factors in a manner that raises doubts about their validity. For instance, intuitions related to knowledge and epistemology can differ systemically based on cultural background, gender, or temperament. Furthermore, intuitions can demonstrate sensitivity to "contextual factors" that have no apparent philosophical relevance. My perception is that philosophy of psychiatry relies less on "intuitions" compared to some of the other fields of philosophy (such as epistemology or ethics), but to the extent that intuitions are involved (and it's hard to argue that intuitions are not involved in attributions of health and disease), the force of the X-phi "negative project" may also be felt in philosophy of psychiatry in the coming years.

I look forward to the commentaries from my colleagues.

*Acknowledgement: I am grateful to Alan Ralston for his comments on this article.*

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# Commentaries

## A Fruitful Experimental Philosophy of pPsychiatry: Some brief Carnapian reflections

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Dr. Aftab's piece on experimental philosophy (X-phi) of psychiatry sets a welcome (and needed) agenda for philosophers and mental health researchers alike. Indeed, I found myself largely in agreement throughout. X-phi provides a fresh opportunity for collaboration between these oft siloed groups. Drawing in part on the philosophy of Rudolf Carnap, I briefly consider three ways in which we might make these efforts even more fruitful.

My first comment concerns the standards used to evaluate X-phi. Aftab rightly observes the limitations of conceptual analysis, which "frequently results in stalemates between differing conceptualizations". X-phi offers something of a path forward, by bringing empirical results to bear on conceptual disagreements. As has been pointed out elsewhere (1, 2), this resembles and is complemented by Carnap's methodology of explication (3).

For Carnap, explication was a tool by which vague, imprecise, or otherwise informal concepts could be made more formal and precise. In Carnap's terms, we are replacing *explicandum* (the former) with an *explicatum* (the latter). Where Carnap's methodology aids X-phi (and X-phi more generally) is by providing principled standards by which we can determine if we have created adequate explicatum for our explicandum. More simply, are our new concepts actually improvements over our current ones? His four requirements are: 1) similarity (i.e. that an explicatum be sufficiently similar to the explican-

dum, such that the former can be used in at least some of the cases where the latter has been used), 2) exactness (i.e. increased precision), 3) fruitfulness (empirical or logical success), and 4) simplicity (i.e. the explicatum should be as simple as possible). Concepts transformed and engineered by X-phi should arguably be judged by how well they meet Carnap's four desiderata.

My second comment concerns the types of concepts targeted within X-phi studies. "Mental illness" and related terms, such as "madness", are highly heterogenous, resembling something of a black box (4). Statements and questions centered on the qualities and characteristics of these terms (e.g. *"All mental disorders are diseases"* or *"[All] mental disorders must cause 'x' to be considered disorders"*) will likely yield little consensus. Similarly, existential questions (e.g. *"Does mental illness exist?")* may prove trivial, if they are *internal* to the linguistic framework of mental illness, and (perhaps) vacuous and intractable if *external* to it (5). Instead, X-phi studies might more fruitfully focus on specific cases (e.g. anorexia nervosa, schizophrenia, depression, autism, etc.) for study and/or explication. This narrows the scope of inquiry and saves resources (see below), while still permitting investigations into beliefs about, for example, etiology and care.

My final comment consists of a few pragmatic, if oft-recited suggestions. For X-phi to be successful, it will need to be rigorously conducted and appraised. Samples will need to be representative and large enough to detect differences, where they exist (i.e. having sufficiently powered study designs). Researchers will need to resist the urge to selectively report results or to "torture" their data, in an effort to yield significant findings (i.e. "p-hacking") (6). This is especially true, as the field begins to establish itself. Positive results, while welcome, should be treated with an appropriate level of skepticism and subject to subsequent replication attempts. Failure to do so will only muddy the conversation about psychiatric concepts further, leaving us back where we started (or worse) and may potentially undermine this nascent and exciting area of research.

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## The intuitions of diagnosed individuals

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Awais Aftab's interesting article addresses the question of, firstly, what role conceptual analysis can play in understanding disease and, secondly, what concepts are actually held by clinicians and non-clinicians. In this response I will consider a third group which Aftab does not discuss. I consider the role of diagnosed individuals in experimental philosophy on disorder. I suggest they are a far from unproblematic source for studying intuitions or for offering a set of intuitions of elevated importance on diseases.

Disability studies has long called for people with disabilities to be involved in research on people with disabilities. This has recently become a topic of interest in philosophy following the application of notions of epistemic injustice to psychiatry (Bueter 2019). In principle, I support this general idea. It seems very likely that people who receive psychiatric diagnoses could have some types of knowledge which are useful and are absent in psychiatrists and the general public. I think that there should be some type of role for people with diagnoses within philosophical research on psychiatry. Following this line of thinking it is easy to imagine someone considering Aftab's paper as an instance of epistemic injustice for not incorporating the views of diagnosed individuals in his study. However, I suggest that the value of diagnosed individuals in this type of research is far from problem free.

Firstly, epistemic injustice focuses upon knowledge. Let's assume it is indeed an injustice to not include diagnosed individuals in forming knowledge. However, are intuitions knowledge? Aftwabs describes intuitions as "a view held based on instinct, without much conscious deliberation" and contrasts them with "deliberate, conscious reasoning". This definition, which seems unobjectionable to me, seems to suggest that intuitions are either not knowledge or extremely unreliable pre-reflective knowledge. Since epistemic injustice relates to knowledge it appears that epistemic injustice relates to a quite different issue compared to intuitions. Alternatively, if intuitions do have a knowledge component then it seems it relates to pre-reflective knowledge which is really not of much value. As such, epistemic injustice does not seem to extend into intuitions. There appears to be no obvious reason to think that diagnosed individuals have a special insight into some elevated set of intuitions which non-diagnosed people lack. It is not an instance of epistemic injustice to not incorporate diagnosed people in such studies or to not place greater value upon the intuitions of diagnosed individuals compared to anyone else.

Secondly, some of the specific problems with experimental philosophy on disease which Aftabs mentions seem highly applicable to patients. In my following discussion I draw upon my own experience of being a diagnosed autistic individual who is very interested in how other autistic individuals perceive the neurodiversity movement. Aftabs mentions that people often seem to hold quite different notions of disease and can have quite different views about whether any particular diagnosis is a disease. This heavily fits my observations of the views of autistic individuals. In my experience, autistic people who have views upon neurodiversity usually fall into a strongly pro-neurodiversity position or an anti-neurodiversity position. Pro-

neurodiversity individuals typically reject claims that it is a disability or only consider autism to be a disability on an implicit or explicit social model of disability whereas anti-neurodiversity individuals typically strongly think autism is an intrinsic disability. This shows strong divergence in views and these views seem partly driven by different conceptions of disability (disability and disease are not interchangeable philosophically but seem closely related in lay terminology). This shows that the intuitions of autistic people are unlikely to provide a consensus of intuitions over the status of autism or indeed conceptions of disease.

Another source of problems relates to psychological vs biological causes. Aftabs outlines how intuitions on causation suggests that psychological causes are often considered to have an inverse relationship to biological causes. When psychological causes are present then this is taken to mean biological causes are not. He also outlines how framing a question in a concrete rather than abstract manner, such as naming a person rather than just presenting a general person in experiments, increases seeing causes as psychological. It would be interesting to know if this was found to be the case in diagnosed individuals but I will mention some possible problems which would follow if diagnosed individuals also followed these trends. A particular diagnosed individual typically has better access to their psychological states than an outside observer. Also, a diagnosed individual will, by being a particular person, be a non-abstract particular instance rather than simply an abstract class. Both these factors could mean that diagnosed individuals might have an inclination to see causes as psychological. If this is accompanied by taking psychological causes as having an inverted relationship to biological causes (a yet to be established empirical question in diagnosed individuals) then diagnosed individuals may have intuitions which downplay the role of biological causes.

None of this is to discount the role of diagnosed people in experimental philosophy. If it is worth doing experimental philosophy on psychiatrists and the general public then it is also worth doing experimental philosophy on diagnosed people. However, it does mean that there seems no good reason to elevate the intuitions of diagnosed individuals over the intuitions of others. Also, Aftab critically addresses why and indeed whether intuitions are philosophically relevant. If the intuitions of diagnosed individuals are not an unproblematic source for experimental philosophy and if the intuitions of diagnosed individuals do not have an elevated status then this further increases the importance of questioning if intuitions have any legitimate role in philosophy of disease.

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## Transformational Conceptual Analysis and Progress in Philosophy of Psychiatry

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"Tell me: is a doctor—in the precise sense, the one you mentioned before—a moneymaker or someone who treats the sick?" Socrates in Plato's *Republic, Book 1,* 342d

Awais Aftab endorses two statements in "Experimental Philosophy of Psychiatry" that are the focus of this inquiry. The *armchair thesis*: "There is no essential need in the project of conceptual analysis to leave one's armchair and seek empirical data to advance debate." The *progress thesis:* "X-phi doesn't claim that it can provide definitive resolution of these philosophical debates (neither can conceptual analysis, for that matter) but that we cannot make *progress* unless we utilize empirical methods." (Aftab's emphasis).

The armchair thesis is a familiar jab against conceptual analysis. I do not find it convincing. First, there is no essential need in the project of X-phi to leave one's armchair. As long as you have a computer in your lap, you can create and analyze X-phi "experiments" (surveys) from the armchair. With remote library access, it's easy to obtain empirical data in the top journals from your laptop. So, like conceptual analysis, there is no essential need in X-phi to leave one's armchair and seek empirical data to advance debate. Thus, the armchair thesis is just as much of a problem for X-phi as it is for conceptual analysis.

Second, I think that many people find the armchair thesis to be convincing, because they maintain that conceptual analysis is just a matter of formulating (non-trivial) necessary and sufficient conditions for the correct application of a term in ordinary language and that conceptual analysis is an *a priori* activity that does not require empirical evidence. This familiar conception of conceptual analysis is overly narrow and excludes conceptions of conceptual analysis that reveal its central importance to philosophical inquiry. Indeed, conceptual analysis has an important ethical dimension that is eclipsed by this familiar conception of it.

To illustrate this ethical dimension of conceptual analysis, I turn to Socrates' cross-examination of his interlocutors in the *agora*, or central marketplace, of Athens for examples of conceptual analysis at its best. The Socratic *elenchus* (which, in Greek, means, "putting to the test" or "refutation") characteristically involved Socrates asking questions of the form "What is X?" where 'X' refers to conventionally recognized moral virtues. He would deploy the elenchus with persons who were considered experts in professions that centered on X. For example, in *Euthaphro,* he asks a professed religious expert "What is piety?", and in *Laches* he asks two distinguished Athenean generals, "What is courage?" During elenctic examination, Socrates asks his interlocutors to state what they know or believe to be true about the subject since they are considered to be wise in the relevant matters. Socrates then standardly refutes them by showing that their definitions are inconsistent with other convictions that they hold dear. Although Socrates never arrives at satisfactory definitions of these terms, the process of elenctic examination is at the same time a process of self-examination that seems to have a therapeutic intent. His interlocutors come to see that the conventional understandings of these ethical concepts are deeply confused and that in order to become better people, they need to revise their concepts. The public setting of these examinations also leads to the professed experts losing some credi-

bility and being deeply humbled. Critics of Socrates's elenctic examination, such as Callicles in the *Gorgias* and Thrasymachus in *Republic*, contend that Socrates does not have benevolent intentions and that he uses these conversations in a competitive spirit to outdo others and show that he is wiser than anyone else. Regardless of whether Socrates' motives in these conversations involve therapeutic intent towards his respondents or competitiveness, the transformational experience that results from this type of conceptual analysis is a feature, and not a bug, of it. If we take elenctic examination as a paradigm case of conceptual analysis, then we are not committed to the narrow view of conceptual analysis as a non-empirical and purely *a priori* activity. Next, I turn to my reservations with the progress thesis.

The progress thesis overstates the value of X-phi. We can make progress on the central questions of Philosophy of Psychiatry without recourse to surveys. What kind of progress are we talking about? It helps to distinguish the *personal progress* that we can accomplish in our own philosophical inquiries and the *progress of the field* of Philosophy of Psychiatry. I'll briefly examine how each type of progress bears on the progress thesis.

Elenctic examination of the question "What is a mental disorder?" can reveal the conceptual confusions in our own conceptions of mental disorders, and can lead to personal progress in understanding the central questions in this domain of inquiry. The relevance of X-phi data to your personal progress will depend on the nature of your inquiry and what is relevant to you. Since Aftab is a psychiatrist with serious philosophical interests, the survey data that he has collected may provide information about the medical culture of his workplace that leads to personal progress in his own philosophical inquiries. However, a philosophy professor at a small, liberal arts college with a theoretical interest in Philosophy of Psychiatry that stems from their research in metaphysics may not make personal progress in their inquiries by examining that survey data. The progress thesis does not seem plausible when one considers cases like the second example.

The nature of progress in the field of Philosophy of Psychiatry is more difficult to characterize. One source of that difficulty arises from the challenge of addressing the general questions: What is philosophical progress? What is progress in psychiatry? X-phi strives to offer a unified framework for progress in both domains, but the integrity of that structure is questionable. My (unargued) stance is that X-phi survey data is one way to increase knowledge (and progress) in these fields but is not necessary.

I enjoyed Aftab's paper and hope these arguments spark further discussion.

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## Problems with Concepts

Christian Perring, Ph.D.

Thanks to Awais Aftab for his extremely helpful discussion and summary of some of the relevant literature on experimental philosophy around the concept of mental disorder. I am rather pessimistic about some of the goals for his project, but also open minded where it might lead.

In an imaginary world, the hope for **conceptual analysis** of mental disorder is to inspect the concept and thus find necessary and sufficient conditions of what counts as a mental disorder. This will then tell us whether conditions like grief are mental disorders or not.

However, for reasons both about the nature of concepts, and also about the particular case of mental disorder, it is clear that this is not going to work. What are these reasons? Let me list some.

1. It turns out that in ordinary language, most concepts **do not have precise conditions**. There is considerable haziness around the borders, and concepts are not static. Since our interest is precisely around the borders, we can't look to conceptual analysis to provide clarity where none exists. Even for cases which are apparently clear now, it can turn out that concepts have evolved so that judgments about how it applies were different in the past or may change in the future. So even if conceptual analysis does provide answers, there's no guarantee that these answers will continue to hold.

2. We can of course **stipulate** some concepts in a precise way, but clearly there are many different ways to stipulate the extent of a concept. So we need some good justification for holding with one stipulation rather than another. Then we are no longer doing conceptual analysis, and are engaged in a different sort of project.

3. The concept of **disorder** is especially murky. Presumably the use of "disorder" to describe a medical condition is relatively new, and chosen precisely because it is not well defined. Alternative related concepts are also not precisely defined: illness, disease, malady, medical problem, pathology are all somewhat related but not the same. None of them are going to be any easier to analyze. They will be both hazy around the borders and also will evolve with time.

4. The concept of **mental** in mental disorder is notoriously difficult to specify if one wants to capture something like the existing core concept. It is not that the main **cause** of the disorder is mental, nor that the main **symptoms** of the disorder are mental. It is certainly not that the **therapy** for the disorder is mental. Indeed, it is far from clear that we have any clear distinction between mental and physical properties in general, so the idea that we might be able to distinguish in a principled way between mental and physical disorders seems especially unlikely.

With this background, we can see that getting clearer on the diversity of concepts and intuitions in the general population will not help to settle the question of what a mental disorder is. They can provide better data than the armchair intuitions of philosophers, so the question is how to use this data productively. The results discussed by Aftab certainly show the mixture of concepts, intuitions and opinions related to mental disorder and even with some views being widely shared, it seems that for most possible views, someone endorses them. Aftab writes "But if such sources of variance are controlled or accounted for in future studies, this can potentially cast doubt

on the notion that the concepts in question have a singular nature." But my assumption is that these concepts do not have a singular nature. I would be surprised to find that any of the concepts under examination here have anything close to a singular nature. I would be suspicious of the methodology of any experiment that indicated that there was a singular nature.

Conceptual analysis is thus not a method for us to achieve the "correct" theory of mental disorder. Instead, it provides a constraint on possible theories. No theory can depart so far from our current concepts of mental disorder without changing the subject and failing to be a theory of mental disorder in the first place. Any theory needs to do a good enough job of matching with our current concepts. What is good enough? That's impossible to quantify. Sometimes new theories come along which don't do a great job of matching our current concepts, and it might be then that they lead to a shift in how we think about a subject. When theorizing about concept X, sometimes there isn't enough continuity for us to say that this is still a theory of X, but just a very different one. We might judge that instead, the concept of X has been replaced by a new concept.

There used to be careful distinctions between concepts of X, conceptions of X, theories of X, and so on. But this carefulness didn't pay dividends. There may be some analytic truths, but the status of analytic truths just doesn't have the same authority as they used to. We try to formulate a way to understand X, but there's a lot of bleeding through between levels of theorizing. At this stage, while getting as clear as we can on concepts still seems like a good idea, we should not place too much hope in the project of using conconceptual analysis as a guide to how things should be.

So we need to move to some other way of deciding on what should count as a mental disorder. Rather obviously, it is not going to come purely out of **science** -- pace those who think that the concepts of mental disorder and normal function are built into the science of humans. I won't rehearse that debate here.

The main option left is to find an approach that works well for our society. We can either be relatively **conservative**, aiming to use a definition of disorder that fits relatively well with our existing concepts, or we can aim to be **radical** and ditch our current set of concepts in favor of different ones.

If we take the conservative approach, we will still need to decide what to do with conditions such as grief: then our decision is relatively pragmatic. We would try to predict the effects of including it as a disorder versus not doing so, and then opt for the approach which will work better for our society. One of the factors in favor of a conservative approach is that it builds on what existed before as a social institution, and so, to the extent that society has accepted psychiatry previously, it will continue to do so.

If we take a radical approach, we could aim to do away with the concept of mental disorder altogether, and just have a system of psychiatry that helps people when they want or need help without labeling them. This might have the advantage of avoiding the shaming and social control that has been associated with psychiatry in the past. But it also seems very idealistic and virtually impossible to implement. We might also suspect that despite our good intentions, problems of labeling and shaming would still be associated with those who received psychiatric help, even when the idea of mental disorder had been banished. So the radical approach may not achieve as much as its proponents would hope.

One central worry with the approach I am suggesting is that abandoning the pretense that we are just sticking with an existing concept of mental disorder will make people think that psychiatry is just engaged in social engineering. We might compare it to conceptions of what the US Supreme Court does. Some like to think that the court is just applying the law as it exists -- some form of "originalism." But it doesn't take much thought to realize that this conception of what is happening is unrealistic, and the judges are, at least to some extent, creating new laws, and are using their judgment about what is best. While some factions regard this as problematic, most don't. Similarly, it should be possible for the general public to understand that in reformulating the concept of mental disorder, psychiatric institutions are going through an inevitable process that should lead to progress, at least if it is done well.

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## Uses and Limits of Experimental Philosophy

John Z Sadler

We should all be grateful for Awais Aftab's very thoughtful and well-organized discussion about experimental philosophy as applied to disease/disorder concepts in psychiatry. I have a few substantive comments to make In response to his discussion.

1. At the outset I must disclose my annoyance at the opening quotation from Veit. I want to know what empirical evidence supports his sweeping generalization about the 'dominant view' in the philosophy of medicine. As an editor and reviewer, I'm always looking for unsupported sweeping generalizations. The evidence for the sweeping generalization may be simply cited, which, to be fair, Veit may have provided in the original publication. Such evidence may also be a generalization about superfamiliar empirical facts which everyone accepts, such as 'the sun rises in the east'. Veit's claim though is not the latter type.

2. Conceptual analysis, strictly defined, involves the identification of a set of conditions which are, ideally, necessary and sufficient for the application of a concept (Margolis & Laurence, Stanford Encylopedia of Philosophy https://plato.stanford.edu/entries/concepts/#ConConAna ). The method involves the use of the search for counterexamples, and in the search for counterexamples lies the utility of surveys in 'X-phil'. When trying to refine the concept of bachelors as unmarried men, I may forget about widowers and priests as counterexamples, and an empirical survey of the public about what kinds of men are bachelors may find examples that my own (empirical) experience has overlooked.

3. The last point above points to the occult, presupposed nature of empiricism in conceptual analysis. The armchair philosopher doing conceptual analysis cannot escape empirical facts which serve as examples and counterexamples. These are de-

rived from experience, and the armchair philosopher who ignores relevant social science addressing the concept is doing shoddy conceptual analysis. If the relevant social science has not been done, then more power to whomever wants to do it, including 'experimental philosophers'!

4. I don't think conceptual analysis, alone, is a very good method for answering the question about whether a given condition warrants the designation of 'mental disorder'. Dr. Aftab makes this evident in mentioning Kendler's 'commonsense' account. Designation of a mental disorder is a complex matter involving normative judgments, identifiication of metaphysical and other assumptions, taxometric and related considerations, political and economic concerns, the practical interests regarding mental disorders, among others. Surveying people about whether this or that condition is a disorder is useful for finding out a range of opinions, but adds little to refinement of a concept pointing to a disorder condition. Moreover, understanding concepts by vote can be fallacious and prone to misuse. Asking a group of white male Southern plantation owners in 1860 whether drapetomania is a mental disorder, or if blacks are inferior, or if slavery is good, would likely result in the affirmative on all three counts. These findings would be important only in being horrific in contemporary eyes, indicating the bias of historical and cultural moments, and the limits of popular understandings.

5. I found the Ahn work to be of most interest, but not because it informed philosophical questions, but because the work informs how to educate clinicians about clinical reasoning, and how to educate the public about mental illess.

6. The Wakefield vignette study of conduct disorder asks subjects to respond to vignettes of a tautological nature, e.g., internal dysfunction vignettes are stripped of environmental causes, and environmentally-caused cases have relevant context included. Rather than affirming the internal dysfunction/environmental cause distinction, the study affirms the ability of laypersons to respond appropriately to narrative/ hermeneutical evidence provided (or not provided, as the case may be). If I don't have any evidence of environmental provocation, then of course I will more likely attribute the condition as internal to the individual, and vice versa.

7. I want to remind our new-to-the-field readers that the philosophy of psychiatry casts a huge net, and however important the problem of defining mental disorders is, the field addresses many, many more domains, questions, and methods. These can be recognized by viewing other issues of the *Bulletin*, as well as our affiliated journal, *Philosophy, Psychiatry, & Psychology*.

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## Explanatory Dualism and Irrational Bias in Psychiatry

Allison Arp, M.D.

Aftab's lucid essay proposes that Experimental Philosophy (X-phi) offers ways to advance the debate within Philosophy of Psychiatry concerning how to best conceptualize psychiatric conditions. His target is "conceptual analysis," which proceeds by eliminating views that are inconsistent and incoherent via the armchair, and so reaches greater precision in coherent accounts, but ultimately is powerless when impasses are reached between alternative coherent accounts. He claims that in contrast to being stuck in such logically bound stalemates, the insights of X-phi can proffer progress in this regard.

One of the ways that X-phi can promote progress, according to Aftab, is by illuminating biases or instances of irrational thinking in practitioners' and lay persons' views that can inform the respective concepts at hand. He describes one such finding of an X-phi study as follows:

> In the absence of a mind-brain dualism, biological and psychological explanations are best understood as explanations at different levels of explanations, but clinicians (as well as laypeople), tend to see them as complementary. The authors discuss that this appears to be reflective of an *explanatory* dualism (not *metaphysical* dualism), that is, there are situations where psychological constructs or biological constructs are seen as providing a better form of explanation compared to the other. This is not by itself irrational, but their research shows that it does tend to generate an irrational bias, where biological and psychological explanations are seen to have an inverse relationship. That is, if psychological causes are considered to be more relevant by the respondents, then biological causes tend to be discounted, and vice versa.

What "irrational bias" is generated when a clinician selects a form of explanation (biological, psychological) that is most relevant in a clinical scenario? It is questionable whether there is a bias or some form of irrationality at play in holding multiple levels of explanation and judging one that seems most relevant in a particular situation. If a multi-level approach to explanation is correct, then there is no bias present. If the assumption of explanatory dualism is itself irrational, is that due to something inherently irrational about the view? Many epistemologists distinguish two kinds of irrationality: structural and substantive. Structural irrationality is attributable to some kind of inconsistency in one's beliefs. Substantive irrationality is due to other factors, such as endorsing a view that is impossible, unusual or unethical. It is more theoretically difficult to establish that a view is substantively irrational than to show that it is structurally irrational (which will depend on consistency with the other beliefs that a person holds). Aftab appears to claim that explanatory dualism is structurally irrational based on the inconsistency of respondents favoring one level of explanation over another while at the same time endorsing the view of explanatory dualism. Again, what is inconsistent about this? To favor one more relevant explanation over another is not inconsistent with explanatory dualism (that there are two different and complementary ways of explaining events in the mind/brain).

It is not explained by Aftab or by the study he cites in this case (Ahn) what exactly consists of such an irrational bias, until the next, separately described example is given regarding the finding that how a symptom is framed, whether in concrete or abstract terms,

determined judgments about different explanations for behaviors. Even if we use this example as a candidate of an irrational bias, to say that this amounts to "inverse dualism" seems misleading. In the clinic, a patient's symptoms and behaviors are assessed in their context, which includes onset, duration, history, stressors, family history and social history, among others. To frame this situation into a dichotomy of abstract versus concrete descriptions misrepresents how clinical judgment functions. For example, the symptom "grandiosity" can belong to a manic episode of bipolar disorder, to narcissistic personality disorder, or as a feature outside of a mental disorder. The context, established through careful history taking, will determine judgments of how the symptom is understood, including its biological and psychological bases.

Two of the major merits of X-phi studies for Aftab appear to be its support for conceptual pluralism and the ability to reveal irrational biases. Given the unclear claim of irrational bias in the example above, I worry that these studies are more likely to generate irrational biases instead of capture them. The purpose of tracking responses in these studies is to inform the project of theoretical clarity, however without knowing the reasons and context behind the survey responses, they seem to convolute more than clarify. Aftab acknowledges that a limitation of X-phi studies is that they do not provide the reasoning behind responses, however this leaves out what is most valuable for theoretical clarification. Further, to cogently investigate how conceptual pluralism or explanatory dualism are practiced in the clinic, including whether irrational biases are present, more than survey responses are needed. Studies could be designed that include blank spaces for qualitative responses, which would be more likely to capture the rationale of respondents. However, this would make the surveys less easy to complete (and potentially reduce the response rate) and more challenging to interpret.

I do think there is an important, informative role for empirical data for the debates of Philosophy of Psychiatry, as Aftab maintains. However, given the limitations of X-phi studies, in particular their exclusive provision of responses without reasoning and context, which are essential for both clinical judgment and conceptual analysis, these studies are severely restricted in their ability to contribute to conceptual progress. Perhaps we should instead turn to an alternative empirical resource, clinical practice, to inform these debates. One way to integrate clinical experience and philosophical dialogue is to hold interdisciplinary meetings where clinicians and philosophers work in partnership to have a dialogue with each other. Instead of talking past one another, the aim is to have clear and jargon-free conversations (as much as that is possible) concerning topics in the Philosophy of Psychiatry.

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## Response to Commentaries

*Awais Aftab, MD*

It is an honor for one to be the recipient of thoughtful critiques and deliberations. I am grateful to the commentators who took the time to engage with my article on X-phi of psychiatry, whether this engagement took the form of critiquing my ideas or using them as a stimulus for further reflections and suggestions.

These seven commentaries have offered me an opportunity not only to enhance and expand my own understanding of the subject, but also to clarify certain misunderstandings regarding the goals of the X-phi project and what can be gained from it. The commentaries collectively reveal different aspects of the debate around X-phi and share interesting connections and overlaps.

### "Taking a vote" and "Who gives a hoot" criticisms of X-phi

***Response to Phillips (some remarks of relevance to Sadler and Moseley)***

I'll start with Jim Phillips's commentary, as it provides a convenient way to address common misconceptions regarding X-phi.

X-phi is not fundamentally about conducting surveys and "taking a vote." In my article I began by highlighting selected survey studies, particularly my own, because such studies are readily available and offer relevant empirical data, but they are **not** paradigm examples of what constitutes X-phi. In fact, they might be best seen as precursors of X-phi research, something that can provide us with preliminary information and set up for better designed experiments to tease out the intuitions underlying the views expressed. Research by Ahn's and Wakefield's groups discussed in the article would constitute more typical examples of X-phi research.

What makes this body of research different from a vote? In a vote we begin with a plurality of views and then the majority opinion emerges as the *winner* after the views have been tallied. Nothing like that is involved either in the survey studies or in X-phi research. There is no assumption or illusion that the majority opinion is the "correct" one, nor is it intended that the majority opinion be seen as the de facto winner an adopted as the preferred view, or that there is one winner at all. If surveys reveal a strong consensus, that deserves to be taken seriously, but it can still be questioned and rejected.

What X-phi researchers are really after are not the views or opinions held by people but the intuitions beneath (or behind) those views. Different individuals may have the same sort of intuitions with regards to a particular concept but may provide widely divergent answers in a particular case if the information available to them or their beliefs about relevant facts are very different.

Both Phillips and Sadler mention drapetomania. While drapetomania was certainly proposed by the physician Samuel Cartwright as a mental illness and while it certainly had adherents and received a lot of political attention, it is my understanding that there is historical uncertainty regarding the degree to which the diagnosis was readily accepted by the medical community. By some accounts, Cartwright's suggestion was criticized, mocked and satirized by other physicians, especially in the North [1]. However, setting aside that issue, we have to go further here and consider that Cartwright's proposal was in the context of horrendously erroneous scientific views about the black race. He believed that slavery was the "natural" state of black individuals, and their physiology and psychology was such

that they were only fit for servitude. It is in the context of such grave scientific errors about the very physical and psychological constitution of the black race that the diagnosis acquired any degree of legitimacy. Our rejection of drapetomania, not only as an instance of illness but also as a valid category to begin with, is based less on a rejection of the intuitive concept of illness that was employed by Cartwright and others, and is based more on the rejection of the pseudoscientific body of beliefs to which the intuitive concept had been applied.

Phillips comments on the irony that what qualifies as "empirical" is itself up for debate as a conceptual matter, and if we want to decide it empirically by taking a vote, "we are now in the funny position of using an empirical method to decide what is 'empirical'." As I have expressed, X-phi is not in the business of *deciding* or *voting*. What constitutes as "empirical" is a non-trivial conceptual question and is therefore one that can be investigated by X-phi methods and *informed* by its results. It is not self-contradictory or paradoxical to maintain that empirical methods may help clarify the boundaries of what constitutes the concept of "empirical".

I have, however, arrived at a point of agreement with Jim about the validity of a contrast between conceptual and empirical inquiry. I don't think that a sharp contrast between conceptual analysis and empirical investigations is valid; there is indeed much empirical in conceptual analysis and much conceptual in empirical work. However, it does not mean that one can be collapsed into the other, or one can be disregarded in favor of the other. On the contrary, a recognition that there is empirical in the conceptual calls for a more rigorous approach to the empirical-in-the-conceptual, and that is what X-phi offers.

As Shepherd & Justus put it, "Replacing speculation about the conceptual judgments people *would* make with data about the judgments people *actually do* make is the overriding agenda." [2]

Phillips ends on the note about two senses of the "empirical" and questions why X-phi restricts itself to one. With regards to the particular issue of perception of biological etiology, my survey does ask about that, but not because what people think about the biological etiology of schizophrenia settles the question one way or the other. It does not and it cannot. What the survey attempts to do is examine the link between perceptions of biological etiology and attributions of disease status (and such a link does seem to be there). Secondly, there is a pretty good reason that X-phi isn't focused on the determination of empirical facts about reality. That is the purview of science, and science does it quite well. X-phi is interested in how facts about our intuitions and semantic intentions have a bearing on philosophical questions.

This is relevant to the "who gives a hoot" criticism or what has also been referred to in the literature as the "eye-rolling" response. It is perfectly legitimate to roll our eyes if we encounter the claim that by asking ordinary folk what they think, we can determine the *metaphysical or scientific* nature of things such as morality, knowledge, free will, abstract objects, or disease. Why? Balaguer elaborates on this point in an article: "That's because with these questions, there's no plausible story to tell about how folk intuitions could be tracking the relevant facts. But… there's one kind of philosophical question for which folk intuitions can plausibly be seen as tracking the relevant facts…" [3] This is a question of the form *'What is C?'* the answer to which captures the *ordinary-language meaning* of the corresponding expression 'C'. That is, "it picks out the concept that ordinary folk have in mind when they use the term 'C'." It is a question about "our usage, intentions, conventions, practices, and so on." [3]

Facts about semantic intentions and mental representations are at least among the facts that determine what our words mean. Understood in this way, X-phi is an attempt to gather data "regarding the applicability of our concepts, and these intuitions can be used as data points to confirm and falsify theories about what the folk mean by their words – i.e. about their semantic intentions, mental representations, and so on." [3]

## Conceptual Analysis, X-Phi and Progress

### *Response to Moseley (some remarks of relevance to Phillips and Perring)*

Daniel Moseley takes us for a walk through the Athenian *agora* to highlight the social and ethical nature of conceptual analysis *a la* Socrates. It appears Moseley takes the expression "leaving one's armchair" a bit more literally than is intended, but I agree that there is value in recognizing that conceptual analysis possesses an element of social and public engagement which is not without ethical ramifications. I would like to think that our present exchange and other similar exchanges in the AAPP Bulletin also carry some of that value and offer authors of target articles such as myself an opportunity of being humbled by the commentators!

The notion of conceptual analysis as an armchair activity relates to the distinction between conceptual analysis and X-phi. As I mention above, I don't think a sharp contrast between conceptual analysis and empirical investigations can be defended. There is a tendency in X-phi literature to present an impoverished account of conceptual analysis. To the extent that my article relied on and reinforced such a view of conceptual analysis, it is mistaken. That said, the difference between conceptual analysis and X-phi is perhaps the manner in which empirical considerations are taken into account, with X-phi seeking to do it in a much more systematic way, using the kinds of experimental methods that have traditionally been associated with psychology.

I did not intend for the "progress thesis" to be a central thesis of my article nor did I intend it in such a strong form, but my (poor) phrasing in the article certainly suggests that, and Moseley is right to point out that the progress thesis is unconvincing in its strong form. I do think there are many ways of making progress, that X-phi is one avenue of making progress, and that we may be hindering our progress by not taking X-phi methods seriously. Like any method, X-phi doesn't guarantee progress, only the possibility of it, and it is perfectly reasonable, as Christian Perring puts it in his commentary, to be pessimistic about that possibility while being open-minded.

It is worth examining briefly how X-phi can help advance philosophical debate. A good illustration of it comes from philosophical work on compatibilism [4]. Among other things, the philosophical position of incompatibilism has been justified by an appeal to alleged intuitiveness of the incompatibility of determinism and free will. X-phi work has demonstrated that ordinary folk appear to have a mix of both compatibilist and incompatibilist intuitions, and that these intuitions are influenced by nuances of the account of determinism that is presented (whether determinism emphasizes the *predictive* nature of the present and the future – everything is in principle predictable from

past events – or the *causal* nature of determinism everything is caused by what previously happened), whether we talk about specific individuals or abstract questions, whether we talk about *our* universe or an alternate hypothetical universe, and whether it is erroneously believed that causal determinism implies that our mental states are devoid of causal efficacy (it appears that this confusion about determinism is a common one) [4].

Of course, this doesn't tell us whether compatibilism is *true*, but that is not what X-phi sets out to do, and to think that it does is to misconstrue X-phi as taking a vote on reality.

Philosopher Agnes Callard wrote in a blog about progress in philosophy:

> There are many great philosophical arguments and ideas available [to a contemporary philosopher] to engage with. She has better interlocutors to think with than people did 10 or 1000 years ago….For example, nowadays if you want to go ahead and assert, in a philosophical context, that there aren't any true contradictions or that what didn't but could've happened is unreal, or that you are sometimes morally responsible for some of the things you do, there are philosophers who have made it hard for you to do that. Graham Priest and David Lewis and Galen Strawson have, respectively, raised the cost of saying what you're reflexively inclined to say. They've made you work for it— made you think for it… What one had before encountering them was, now one sees, nothing more than a way of vaguely gesturing at the idea in question. Engaging with them introduces order into one's thinking as to what exactly is meant by claiming, e.g. that one is morally responsible. [5]

I think X-phi does something similar with regards to intuitions and appeals to intuitions in philosophical arguments. X-phi has now made it *harder* for us to appeal to intuitions. It has raised the cost of doing so. We can no longer think of intuitions in the same way, or we can no longer appeal to intuitions in the same way as we did prior to X-phi, and that, by Callard's account, is a form of progress.

There is more to say about X-phi and progress, to be brought up in my response to Perring and Dunleavy.

### Wakefield's Vignettes
### *Response to Sadler*

John Sadler puts it very well that the "armchair philosopher doing conceptual analysis cannot escape empirical facts." My response to Phillips addresses Sadler's point that "understanding concepts by vote can be fallacious and prone to misuse," so I won't repeat that here.

Sadler points out that in the Wakefield experiments "internal dysfunction vignettes are stripped of environmental causes, and environmentally-caused cases have relevant context included." This is an important point but it's only partly true. This wasn't very clear in my article: while information presented about the environment was deliberately skewed in a certain way in the "internal dysfunction" vignette, it was not entirely lacking.

Wakefield describes the "internal dysfunction" vignette in one of his articles [6]: "additional information indicated that Carlos's aggression was disproportionate in intensity and duration to environmental threats, that it was directed relatively indiscriminately at those in his own as well as opposing gangs, and that the problem continued unabated even when he spent several months in a more benign environment." So, the information about environment in this case is presented in such a manner that the reader is left with a clear impression that the behavior is disproportionate to whatever is going on in environment. The point of the experiment is not whether the participants attribute the condition as being internal to the individual or being the result of environmental causes (since different vignettes are intended to convey that impression differently by design), but whether the relationship of the behavior to the environment influences intuitions of disorder attribution, in cases which otherwise fulfill official diagnostic criteria. The purpose is not to affirm the internal dysfunction/environmental cause distinction, as Sadler comments, but rather to illustrate that the intuitiveness of disorder attribution is dependent on the relationship (such as proportionality) of symptoms/behaviors with environmental stressors. The vignettes suggest that if behaviors are seen as understandable/expectable/proportionate consequences of environment threats, they are generally not seen as "disorders," *even if they meet DSM criteria*. This doesn't tell us what disorder *really* is or how we *ought* to define it, but it does tell us something about our intuitions.

### X-phi and Individuals with Psychiatric Disorders
### *Response to Sam Fellowes*

Fellowes offers a thought-provoking commentary on how we should approach the intuitions of diagnosed individuals within the broader project of X-phi. Fellowes's commentary is generally in the spirit of the "negative project" of X-phi, which I briefly mentioned at the end of my article, but did not discuss further. The negative project is critical of philosophy's reliance on intuitions and expresses skepticism that intuitions can serve as a reliable basis for philosophical inquiry. Fellowes argues that the intuitions of individuals diagnosed with psychiatric conditions do not serve as an unproblematic source and that there doesn't seem to be a good reason to elevate the intuitions of diagnosed individuals over the intuitions of others.

I do think that diagnosed individuals constitute an important and essential group to be included in X-phi studies, and that it would be a form of injustice to exclude them. This is because:

i) We do not yet know if there are systematic and reproducible differences in the intuitions of diagnosed individuals compared to clinicians and the general population; we have to remain open to the possibility that there might be. Fellowes's discussion gives us reason to think that there might be such differences.

ii) While I do not think we can privilege the intuitions of diagnosed individuals (or any other group, for that matter) on an a priori basis (it is however *possible* that we may have good reasons to privilege intuitions of certain individuals in certain specific contexts), they do constitute an important source of information, the exclusion of which may very well be detrimental to our philosophical and

scientific understanding. Such a view is consistent with standpoint epistemology. It is the case that individuals with psychiatric disorders, particularly serious mental illness, constitute a marginalized group and this marginalization socially situates them such that they are likely to be aware of things and to ask questions that others may be less likely to be aware of and to ask.

I really liked Fellowes's discussion of how existing X-phi findings, if extrapolated, suggest that the intuitions of diagnosed individuals may be skewed towards viewing causes as psychological since they have better access to their psychological states and constitute non-abstract particular instances. It is plausible, and if demonstrated in future inquiries, has important practical ramifications.

I am grateful to Fellowes for his commentary and applaud his contribution.

### Beyond the Goals of Conceptual Analysis
### *Response to Christian Perring & Daniel Dunleavy*

Christian Perring's commentary is a wonderful reflection on the nature of concepts and limitations of conceptual analysis. With a clarity I greatly admire, Christian goes to the heart of the matter and discusses possible ways forward for us as a society. I can't say that I am in strong disagreement with anything he has written.

Perring writes: "We can of course **stipulate** some concepts in a precise way, but clearly there are many different ways to stipulate the extent of a concept. So we need some good justification for holding with one stipulation rather than another. Then we are no longer doing conceptual analysis, and are engaged in a different sort of project."

That is correct, but I also don't see it as that much of a problem. Why should conceptual analysis not serve as a prelude to some other philosophical project? In fact, this wonderfully ties in with Daniel Dunleavy's excellent commentary in which he brings up Carnap's method of explication, by which vague, imprecise, and informal concepts are made more formal and precise. One of the articles by Shepherd and Justus that he references is a helpful one in this regard.

Shepherd and Justus [2] argue that X-phi is well-placed to assist the process of Carnapian explication. They identify the following ways in which it can do so (and offer examples of each in their article):

i) clarify areas of uncertainty and vagueness in concepts
ii) uncover conceptual pluralism
iii) discover sources of bias that influence intuitions
iv) discover factors that influence conceptual judgments (not necessarily biases), such as the role of personality traits
v) identify a concept's central features and its dependence relationships with other concepts

They write, "Of course, the contribution x-phi makes will not determine, in any particular case, how explication should go. Explicative choices (e.g., choices about which features of concepts to preserve and which to abandon) will be guided in part by theoretical aims particular to the case at hand. Even so, x-phi's contribution to such choices secures a positive philosophical payoff independent of contentious debates about intuition's evidential status… The more x-phi facilitates explication by helping clarify explicanda, the more x-phi participates in a compelling philosophical methodology." [2]

There is a tendency for us to assume that X-phi should restrict itself to and judge its success by the goals conceptual analysis has set, but there is no reason for us to do so. X-phi methodology can and should go beyond the goals of conceptual analysis. Daniel has shared with me (personal correspondence) a new article by Samantha Wakil [7] that makes very similar points with regards to "conceptual engineering." Wakil argues that "(1) evaluating the success of engineered concepts necessarily involves empirical work; and (2) the Carnapian Explication criterion precision ought to be a methodological standard in conceptual engineering." I think this line of thinking opens up new ways in which X-phi methods can be fruitful in philosophical work.

Dunleavy's other suggestions in the commentary are also important, and I am in agreement with them, particularly on the need for methodological rigor in X-phi studies.

Perring's discussion of finding a way of deciding what should count as mental disorder based on what "works well for our society" is worth commenting on briefly. Although we have recognized that concepts such as mental disorder do not have a singular nature, we often fall into the trap of thinking as if that were the case. This is particularly so when we consider the social, scientific, and pragmatic functions of mental disorder. It is not necessary that a concept of mental disorder that works adequately in one context will also work adequately in another. This is more clear-cut in the legal arena, for instance, where "mental disorder" may be defined in a more circumscribed fashion for involuntary commitment, or when determining who should be considered not guilty by reason of insanity or who is competent to stand trial. When it comes to society's expectations, "mental disorder" often stands proxy for things such as who can validly claim the "sick role," who should get access to care and medical treatment, what conditions should public money be spent on, when should care be reimbursed by insurance companies, etc. It is not reasonable to expect that a concept which adequately meets these social needs will also be a concept that adequately guides scientific research into mechanisms and causal explanations of relevant phenomena or provides the theoretical rigor necessary for good clinical work. A way forward may therefore require a collective recognition that different ends require different variants of a concept.

### The Irrationality of Inverse Dualism
### *Response to Allison Arp*

Arp's commentary offers an opportunity to clarify the nature of irrationality pointed out by the Ahn group. I want to emphasize that there is nothing questionable or irrational about clinicians selecting one form of explanation (psychological or biological) as being the most relevant for a particular patient. In most clinical circumstances, that is a quite reasonable thing to do. What is irrational, or at least lacks a prima facie justification, is the attitude that psychological explanations and biological explanations have an *inverse* relationship. That is, when psychological explanations are deemed to be salient, there appears to be a tendency to assume that biological explanations cannot simultaneously be relevant, that their relevance decreases as

the relevance of psychological explanations increases. Biological explanations and psychological explanations are seen to be in competition with each other. It is this zero-sum-game framing, this inverse relationship, that is questionable if the psychological and biological are understood as being different levels of explanations. A phenomenon may be better explained at one level than another, but explanatory power at one level is not necessarily acquired *at the cost of* another level. If we explain a patient's anxiety in terms of on-going interpersonal conflict, it does not mean that we cannot also explain it in terms of neurobiological phenomena such as activity of brain circuits or actions of neurotransmitters. As Ahn et al. describe, "… any given mental activities may sometimes be better explained using biological constructs and may at other times be better explained using psychological constructs. Explanatory dualism is therefore not necessarily irrational." [8] They go on to explain that what is irrational is the idea that explanations in terms of biological constructs and explanations in terms of psychological constructs are inversely related, that one wins at the expense of the other.

Perhaps one reason why we have a tendency to think so is that in general medicine we are influenced by the idea that disorders have single causes [9]. *If* we assume that psychiatric conditions and psychological phenomena have single causes, and *if* we assume that these causes are *either* categorized as biological or as psychological, then perhaps an inverse relationship between biological and psychological explanations makes sense. But everything we know about the causes of psychiatric conditions [10] and the relationships between levels of explanations [11] suggests that these assumptions are not viable.

Arp writes, "In the clinic, a patient's symptoms and behaviors are assessed in their context, which includes onset, duration, history, stressors, family history and social history, among others. To frame this situation into a dichotomy of abstract versus concrete descriptions misrepresents how clinical judgment functions." That is correct. It is not clear that real-world clinical judgments can be dichotomized into abstract vs concrete in any simple way. In Ahn's studies they have been able to do so by means of carefully designed vignettes, but clinical practice is not like that. So how the influence of abstract vs concrete distinction applies to actual clinical practice is, I think, an open question.

Arp expresses the worry that X-phi studies may generate their own biases instead of, or in addition to, capturing biases in clinical practice. That is a legitimate worry, and not one I would minimize or discount. Any new methodology brings its own possibilities of bias and error. X-phi is particularly vulnerable, considering that X-phi research in psychiatry is still in infancy and its methods so far have been severely restricted. That gives us reason to be cautious in our embrace of results from X-phi research and to be justified in demanding greater methodological rigor from future studies.

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*(References continued on page 22)*

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*(Continued from page 1, President)*

things musical and had one of the most extensive CD collections I've ever seen (some of which were still in their wrappers).

"We also shared a love of philosophy and psychiatry. I recall him pushing me on whether postmodernism could add anything to our understanding of madness; on how we could know when we have gotten it right about giving uptake to others; and whether the distinction between civil disobedience and defiance in those with mental disorders could be upheld. His work on schizophrenia ranged from the disorder itself, to conceptions of the self, and whether those diagnosed with this disorder would be served by people thinking of it as a problem of living. In other words, he was a keen listener and a philosophically deep thinker whose ideas challenged my own and others' research. Ozzie's contributions to philosophy and psychiatry are immeasurable.

"He and I also met together with a few other colleagues to discuss some philosophy books such as Martha Nussbaum's *Frontiers of Justice*. He was a terrific discussant: he brought to our discussions a fresh perspective and a solid ability to connect the selected text to others' research. I always left those meetings with a better understanding of the section of the text we currently were reading.

Ozzie was soft-spoken, kind, and thoughtful. He was a good friend to me and a great colleague. Knowing him enriched my life."

*Jim Phillips* writes, "My relationship with Ozzie went back to the mid-eighties. Maurice Natanson, Ed Casey, Dan Daniels, and I had developed a philosophy/psychiatry group in New Haven, and Ozzie, who was at the New School, joined us and did his own presentations. This group eventually ran its course, and for me folded into AAPP, which was starting in 1989 and '90. Over the ensuing decades I had a pleasant relationship with Oz, now in the context of AAPP. I have fond memories of our times together."

*John Sadler* has written a memorial to Ozzie in the September 2021 issue of PPP.

**Louis Charland**

I first encountered the work of Louis Charland when he wrote a target piece, "Cynthia's dilemma: Consenting to heroin prescription" for the American Journal of Bioethics in 2002. We met soon after that and he gave a talk at the NYC local group of AAPP, which I was running at the time. He also came to give a talk at my college in Long Island. We spent some time together and got to know each other. He was preoccupied with sorting out paperwork with the Canadian government so his wife Anna, who was living in Queens could move to be with him in Ontario. We met regularly at AAPP events, and he would often follow up afterward with a heartfelt emailed note, which was unusual.

Louis could be warm and encouraging, but he also was disappointed and frustrated with some of the institutions he had to deal with. He was a complex person, ambitious but gracious, in some ways a little nostalgic for the past, and immensely curious. He was particularly eloquent in describing his experience of China in his travels there with Anna, where he had learned a good deal about the practices of Chinese psychiatry.

Louis left a big impression with all. *Robyn Bluhm*, who took classes with him as a graduate student, remarks on his kindness which "came out in his classes, as well - we had a mix of undergraduate and graduate students, and Louis always made sure that everyone contributed to the class discussion and felt like their ideas were valued. His own deep engagement with and enthusiasm for the material also came across really clearly."

His obituary mentions a side of life he didn't discuss much with academic peers: he had a long involvement with Alcoholics Anonymous, and was a mentor to many. He had a rich life, and most of us only saw him from one perspective, making it difficult to appreciate the many facets of his experience. To a large extent, his contributions to AAPP were in his personal connections with others.

*Doug Porter* writes, "Louis Charland's warmth and charm and graciousness created such a great presence, it is hard for me to believe he is gone. Louis first introduced himself to me after I had given my initial presentation at an AAPP conference. During my presentation I had invoked the significance of David Healy's work for a critical theory of psychopharmacology. Louis let me know that he had interned with David Healy in Toronto and had been impressed with his clinical work and bedside manner. As a clinician it meant a lot to me to know how highly Louis valued clinical work. I would later come to discover that concern for quality clinical work was evident in Louis's philosophical writing. Louis was a careful historian, and I was impressed with the way he could demonstrate the significance of historical concepts such as "the passions" for developing a deeper understanding of psychopathology such as that which occurs in anorexia. But it was Louis's work on capacity and consent in the context of addiction that I found the most compelling. The concept of compulsion in addiction is understandably contestable, but Louis's work made it clear that nuances in interpretation of the meaning of compulsion and autonomy directly impact the practice of informed consent and therefore require careful reflection and analysis. It seemed to me that Louis's work here was guided by a clinical concern for the unique vulnerabilities created by the pathology of compulsive substance use. His ideas in this regard were the subject matter of a recent AAPP bulletin and this prompted an email exchange between us. Louis was not so much concerned with the content of our philosophical arguments in the bulletin during this email exchange as he was with the fact that an upcoming AAPP conference was to take place in New Orleans. Louis felt a special attachment to my adopted city of New Orleans because he was born there. An appreciation for the unique charms of the city was part of our kinship and he was looking forward to being able to share some time together here in New Orleans, as was I. Louis will be sorely missed."

*Ed Hersch* writes, "Louis Charland was also an extremely friendly and always welcoming person. He had one of those "larger than life" personalities that made people feel right at home when they were around him. I first came in touch with Louis around 2006 or 7 when I responded to a Call For Papers about the edited book on "Fact and Value in Emotion" which he and Peter Zachar published in 2008. Louis' response to my proposed contribution was so enthusiastic and positive at a time when I had had a few discouraging discussions with some nitpicky journal editors that it really encouraged me and revitalized my ensuing writing efforts.

"On subsequent occasions it was always a pleasure to see Louis, mainly at AAPP meetings and we had some memorable talks together, including a very nice hike with him and my wife Ann in Utah at our EC meeting a few years ago. A good picture that Ann took of Louis among some of our group there is attached below. I also recall a time when Louis, who taught at

*AAPP Executive Council Meeting in Utah, 2017. Photo by Ann Hersch.*
*Back row: Scott Waterman, James Phillips, Robyn Bluhm, Ed Hersch, Christian Perring, Louis Charland, Aaron Kostko, Melvin Woody, Doug Porter.*
*Front row: Claire Pouncy, Brent Kious, Peter Zachar.*

Western University, about 120 miles west of Toronto, came to present his work on "Anorexia as a Passion" at our Psychology Graduate Student Colloquium at York University (where I am affiliated) and joined us for dinner afterward. Louis was his usually friendly self and both he and his very original work were well received there. He is missed already."

*Jim Phillips* writes, "I had of course known Louis for many years in the context of AAPP and had always admired his work on emotion and passion. Earlier this year we spent a lot of phone and text time planning the recent issue of the Bulletin that highlighted his work. In one of these conversations we discovered a mutual interest in the early phenomenologist, Gabriel Marcel. When I mentioned that Merleau-Ponty had written a review of some of Marcel'work, Louis unfamiliar with the Merleau piece, was anxious for me to send him a copy of the review. Before I got that done, I heard the news of Louis' death."

*Tania Gergel* (Welcome Trust, Senior Research Fellow, King's College London) writes, "I had been inspired by Louis' work on decision-making and mental illness for many years before he made contact with me. There was something extraordinary about his breadth of vision and the way in which he could understand interrelationships, for example, between values and emotions within cognition. He was not afraid of courting controversy – most recently perhaps in his work on impairments of capacity and control within addiction, an area where I found his views very compelling. He was slightly puzzled as to why these ideas should seem so controversial to many others – we shared some fascinating and extremely enjoyable discussions speculating about this – but he always listened and responded to those who challenged him with exceptional humility, patience, and grace.

He was an incredible man and a wonderful friend both to me and to countless others – a brilliant mind, so vibrant and full of humour and warmth – yet so humble and with such generosity of spirit, despite his phenomenal achievements. We became close very quickly and our exchanges have been one of the highlights of my academic career. Louis was overflowing with ideas and excitement in the last months of his life – full of extraordinary creativity, joy, and energy until the end. I had been looking forward so much to hearing his new ideas develop and to working with him – but he leaves behind him an incredible legacy and, amongst the sadness, I feel immensely privileged to have known him as both a scholar and a friend."

*Jennifer Radden* writes, "Louis Charland was a memorably vivid and appealing character, and it is somewhat difficult to distinguish memories of his marvellously French gestural language, his cosmopolitan air, his elegant scarves, and his very Gallic intellectual verve and excitement from his substantial contributions to the philosophy and history of psychiatry. I have written before about his arrival in Boston with an enormous bouquet of roses after my cancer diagnosis in 2007. But equally, I could describe his presence at a long conference about anorexia at Duke University some years later, where he charmed the assembled collection of hard-boiled medical experts with his insistence that the disorder is best understood as a "passion," in the eighteenth century sense, long since (although perhaps unwarrantedly), dismissed by medical psychiatry. Or his profound excitement over unearthing a copy of what was I think the second edition of Pinel's great *Treatise on Insanity* - rare and ground-breakingly special, he'd excitedly explain, and not even comparable to the first edition!

"I cannot claim to have followed all the ins and outs of those many investigations, particularly those into the history of French and eighteenth century psychiatry. But by the time in the late 1990s, I was choosing authors for what came to be the *Oxford Companion to the Philosophy of Psychiatry* (2004), I had come to recognize the originality of Charland's approach, learned both from his early writing on the dilemmic form of some ethical decisions around incapacity, and from his wonderful talks at those AAPP Annual Meetings in the 1990s. And I still regard his contribution to that volume, where he sorted the character disorders according to their fit for moral treatment in eighteenth and early nineteenth century terms, as one of the volume's, and his finest achievements.

"Later, as an external reader for his tenure decision at the University of Western Ontario, which I wholeheartedly supported, I noted the consistently high quality, innovative nature and importance of his work. Of it, I wrote: "'Charland is equally familiar with French and English-speaking traditions in the history of psychiatry, as he is with contemporary psychological and philosophical accounts of emotion and the affective life. In addition to these matters of historical and psychological fact, Charland speaks with authority on the ethical and value elements that infuse all concepts and categories in psychiatry and psychology. The fusion of these several approaches and ideas makes for a particularly compelling, insightful and intriguing theorizing.' It did, in polished, careful work that was wide-ranging, informed, and expansive in its implications.

"The history and philosophy of psychiatry are fields in their infancy – with almost all yet to be learned and discovered. Charland's research added immeasurably to what we know. But it also provided a model of how we go about acquiring such knowledge. I will miss him."

*Bob White* (Emeritus Professor, Uniiversity of Western Australia) writes, "We all have our rare 'light bulb' moments which suddenly change the way we conceptualise our own subject. My most recent one came in 2012 attending Louis's first lecture as Distinguished Visitor to our Australian Research Council Centre of Excellence for the History of Emotions (CHE). It was intensely engrossing though modestly delivered, analysing the distinction between a fixed 'passion' (of long duration like an addiction or *idée fixe*) and '[é]motions' as fleeting, affective feelings responsive to fluctuating experiences. His analysis proceeded breathtakingly from antiquity, through Medieval, Renaissance and Enlightenment philosophers, down to his beloved Pinel and Ribot's revival, concluding with comments on his application of these idea to anorexia. In its astonishing inter-disciplinary scope and breadth, effortlessly synthesising medical history, philosophy, psychology and the art of healing hurt minds, it was mind-blowing to me, causing me instantly to rethink radically my own field of literary history, especially

Shakespeare's plays. At tea I plucked up courage to tell him so. This led on to many excited exchanges during that visit to CHE and two more in 2013 and 2016, and a published, co-written essay on *The Winter's Tale*, a play filled with medical imagery which fits like a hand in the glove of Louis's analysis of healing a dangerously destructive passion. We went on to discuss another essay on jealousy in *Othello* and even a future, ambitious set of psychological 'case studies' of all Shakespeare's characters. The sadly uncompleted project took its place in his applications for future travel grants to return to Western Australia, a place he seemed to have fallen in love with. News of his death came as a terrible shock, as the interruption of a delightful and compelling conversation. I miss his smiles, quietly vibrant voice, kindness and intellectual generosity, as I'm sure do many around the world. But somehow and somewhere the conversations still go on, as lively and irresistible as his life.

"Louis's distinctive and unique emails resembled haiku poems which encompass the tumbling immediacy of all experience, like this one, which was among his latest:

> Dear Bob
> I found my way through the maze of comments.
> One way.
> Two papers.
> All good.
> Beautiful sunny fall day here.
> Magical.
> Resting then swimming.
> Reenergizing.
> Online teaching is so fast paced.
> Tech issues etc
> But on the whole going great
> Just very demanding
> Free style lectures to power point
> Take care
> Louis

Thank you for the gift of your company and thoughts, Louis. And 'Take care'."

*Peter Zachar* writes, "I met Louis Charland at my first AAPP conference in 2003 in San Francisco. Sitting together at the lunch table, we were both beguiled by Bill Fulford's description of the founding of the AAPP. Louis became a good friend and I have many happy memories of time spent with him during conferences in the U.S. and abroad, including several variations on one of Louis' own passions – dinner at a high-end steakhouse.

Something that became evident in reading the many reminiscences written in response to Louis' death was how much time he spent maintaining connections to others. For my part, one or two times every semester he would call me on the phone to update me on what is happening in his and Anna's lives and to ask how I was doing.

Louis put considerable effort into his work and was confident about its quality and at the same time, invariably open to the views of those who might disagree with him. I would go so far as to say that he was unruffled by disagreement – and did not take offense or dismiss it. This same philosophical demeanor was expressed in his presentation style – which was calm and conversational. His talks came off as if he knew what he wanted to say but did not decide how he wanted to say it until that moment – and it was well articulated.

The son of a diplomat (the Canadian Ambassador to France), Louis had an extensive breadth of experience and one could have endless 'Really, you did?' reactions after discovering something new about him. Before returning to philosophy full time, Louis was a (Parisian) musician (classical guitar) but had to give that life up due to chronic dystonia in one hand. He never indicated any bitterness about that loss to me. In this respect, the email he sent to Bob White captures Louis perfectly. He continued to have physical ups and downs, but always returned to his poetic sense that today the sky is blue and life can be delightful."

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*(Continued from page 1, Editor)*

So, we're now in the funny position of using an empirical method to decide what is 'empirical'.

Let's take a look at another core notion in this discussion – the notion of disease. Aftab's own study at UCSD, along with the Finish Disease (FIND) study, are both strongly focused on disease attribution. Awais makes it clear that in evaluating whether the research subjects consider some disturbance a disease (grant it disease attribution), it's critical to know what they mean by 'disease'. In fact, failure to define 'disease' renders all further research on disease questions otiose. Now, if disease is a concept like other conceptual issues, with differing views of what disease is, the X-phi approach would be to bring in empirical research. But all that will get you is a vote on who thinks 'disease' is this and who thinks 'disease' is that.

In reality, the best we can do is further conceptual analysis and the development of a consensus on how to define disease. The consensus will state an arbitrary line: on this side 'disease', on the other side no 'disease'. This is a bit like the contrast between reliability and validity. As with that contrast, the consensus definition of 'disease' will produce reliability but not validity.

It goes without saying that all further analysis on the disease status of any candidate disorder will suffer from the arbitrary nature of our definition of disease. And the X-phi approach of taking a vote will not relieve us of this dilemma of arbitrariness. If E-phi research suggests moving the cut-off in one way or the other, will that really make the decision less arbitrary?

One could certainly make another proposal at this point, that of experimenting with the effects of moving the consensus cut-off in different ways. This of course wouldn't provide us with a final definition of disease, but it would educate us on the consequences of altering the definition of disease.

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In my contrast of conceptual analysis and taking a vote, am I being overly simplistic. Aftab's discussion certainly suggests this. At the least, his discussion of surveys offers a lot of interesting information. However close they get to something like truth, it's inherently interesting to learn what different groups of people think about psychiatric phenomena. And some of the results are quite challenging. For instance, if 75% of psychiatrists, physicians, and nurses, and 50% of laypeople consider anorexia nervosa to be a disease, that presents quite a challenge to someone who wants to ignore that vote. But then there's the counter-challenge, illustrated by the nineteenth century consensus view (vote) on drapetomania.

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In this commentary I want to suggest finally that the putative contrast between conceptual and empirical analysis may be unnecessary and even incorrect. In so-called armchair, conceptual analysis, I doubt that any such analysis, however pure, is free of bias, and the bias is at least in part the product of circulating empirical findings. Here I could invoke Hans-Georg Gadamer's hermeneutic argument that to think is to think from a point of view, from one's own personal bias.

What about empirical findings? Can they be free of concepts? Of course not. They have to be findings or data about something, and that something will always be some conceptual notion.

I end this commentary with a final note on X-phi. You might note in the above paragraphs that I'm using the term, empirical findings, in a broader sense than we find it in X-phi. For the latter, 'empirical findings' refers only to surveys of what people think about an issue. If, say, however, there is strong evidence that schizophrenia has a biological etiology, we are left with two senses of 'empirical' – empirical evidence of what percentage of a selected population agrees with the biological etiology, and empirical evidence that the biological etiology is the fact (or truth) about schizophrenia. The great limitation of X-phi is that it only considers empirical findings in the first sense.

JP

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*The Association for the Advancement of Philosophy and Psychiatry was established in 1989 to promote cross-disciplinary research in the philosophical aspects of psychiatry, and to support educational initiatives and graduate training programs.*



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