Greater Cascadia History and Philosophy of Science Workshop

May 17th, 2019

Pre-workshop activity: May 16th, 3-5PM, Vancouver Aquarium engagement room. Tickets may be picked up for earlier entry to the Aquarium.

Contacts: Holly Andersen, handerse@sfu.ca, and Cody Brooks, cody_brooks@sfu.ca.

Program Schedule:

9AM: Keynote: Hasok Chang
   Chair: Holly Andersen

10-10:15: Coffee

10:15 – 12:45 (30 mins per speaker) Chair: Sina Fazelpour
   1. Paul Franco, “Ordinary Language Philosophy & the Historical Turn in Philosophy of Science”
   2. Megan Delehanty, “Epistemic Injustice in Psychiatry”
   4. Christopher Stephens, “Modus Darwin Redux”
   5. Benjamin Feintzeig, “Reductive Explanation and the Construction of Quantum Theories”

Lunch: 12:45-1:45 Nuba.

1:45PM: Keynote: John Dupre
   Chair: Alison Wylie

2:45-3PM: Coffee

3PM – 5:30PM (30 mins per speaker) Chair: Cem Erkli
   1. Alison McConwell, “The Relevance of Individuality to Scientific Practice”

Dinner: 6 PM, Brioche.
Abstracts: alphabetically by last name.

Bert Baumgartner, University of Idaho: The literature on the reproducibility crisis presents several putative causes for the proliferation of irreproducible results, including p-hacking and publication bias. Without a theory of reproducibility, however, it is difficult to determine whether these putative causes can explain most irreproducible results. Drawing from an historically-informed conception of science that is open and collaborative, we identify the components of an idealized experiment and analyze these components as a precursor to develop such a theory. Openness, we suggest, has long been intuitively proposed as a solution to irreproducibility. However, this intuition has not been validated in a theoretical framework. We use probabilistic arguments and examine how openness of experimental components relates to reproducibility of results. We show that there are some impediments to obtaining reproducible results that precede many of the causes often cited in literature on the reproducibility crisis. That is, even if, for example, p-hacking never took place, publication bias was absent, and other erroneous individual- or system-level practices were corrected, reproducibility is not ensured.

Megan Delehanty, University of Calgary: Attention to epistemic injustice in psychiatry has so far focused on the context of patient involvement in revisions to the Diagnostic and Statistical Manual of Mental Disorders (DSM). Other related work has attended to the way various forms of epistemic injustice arise in healthcare more generally, with a focus on somatic illnesses. Still other work has examined the difficulty people with mental illnesses have in communicating their experience and/or being taken to be credible speakers in everyday contexts. In this paper, I expand on this literature in two ways. First, I will examine a wider range of contexts within psychiatric practice (broadly construed) in which people experiencing mental illness often encounter forms of epistemic injustice. These include diagnostic interviews, decisions about treatment modalities, resolution of complaints about mental health professionals, and within the context of treatment (both individual and group therapy). Importantly, this latter context, in particular, shows the extent to which the conceptions of different mental illnesses in the social imagination produces a hierarchy among patients according to which some groups of patients are significantly more marginalized than others. Second, where most of the literature in this domain has identified either epistemic injustice in general, or has focused on testimonial and hermeneutic injustice, I attempt to provide a finer-grained analysis of the varieties of epistemic injustice that tend to be found in these settings. The end result of this is a fuller map of the terrain of epistemic injustice in psychiatry, one which will, I hope, lay the foundation for more detailed examinations of specific domains within this area.

Marc Ereshfsky, University of Calgary: Theories of natural kinds suffer from two defects. First, many of those theories are developed according to a priori considerations. As a result, such theories fail to capture why scientists actually posit natural kind classifications, and they fail to help us understand why classificatory practices in science are successful. Second, theories of natural kinds tend to be universal they claim that all natural kind classifications are posited to capture some overarching aim, such as highlighting the causal structure of the world. However, scientists have a variety of reasons for positing natural kind classifications, and extant philosophical theories fail to capture that variety. Given these two problems, there is a discrepancy between the philosophical literature on natural kinds and classificatory practices in science. This presentation highlights the above problems, and it offers an account of natural kinds that better reflects classificatory practices in science. The account offered does not suggest an overarching metaphysics of natural kinds, as theories of
natural kinds tend to do, but is attentive to the local metaphysics of different classificatory projects. The account of natural kinds developed is naturalistic. But at the same time, it offers two constraints on natural kind classifications, namely that such classifications serve the epistemic functions they are posited for and that they are grounded in the world. Hence, the title of the proposed account: "The Grounded Functionality Account of Natural Kinds." Consider two virtues of this account. First, a classification is judged whether it is a natural kind classification depending on whether the classification satisfies the epistemic function it is posited for. This makes the suggested account of natural kinds attentive to the local and varying epistemic aims of those doing taxonomy. Second, the proposed account requires that natural kinds be grounded in the world, but not in the sense that they are independent of human thought. Psychological and social kinds can in part depend on us and constitute natural kinds, so long as they are grounded in testable states in the world, and those states include psychological and social states. A side benefit of the proposed account of kinds is that it blunts recent pessimism about philosophical work on natural kinds articulated by Ian Hacking and David Ludwig. They argue that philosophical work on natural kinds is madly off in all directions, and that such work has no relevance to how scientists actually construct classifications. The Grounded Functionality Account answers both lines of pessimism. The account requires that natural kind classifications be grounded in the world - something that philosophers and scientists generally agree on. At the same time the account is sensitive to the varying epistemic reasons scientists have for positing classifications. Thus, it captures how scientists actually construct classifications.

Benjamin Feintzeig, University of Washington: It is common to require that a newly constructed physical theory explain the success of the theory it supersedes. But this requirement—that one can give a reductive explanation of the success of predecessor theories—is vague as it stands. I will show that in the context of the construction of quantum theories, one can make at least a part of this requirement precise and provide justification for its normative status. In the context of the construction of quantum theories, the requirement of providing reductive explanations becomes the requirement that a quantum theory explain the success of its corresponding classical theory. This is accomplished by investigating the classical limits of quantum theories. Physical theories—both classical and quantum—are composed of different pieces, including physical magnitudes, physical states, and dynamics. I will focus specifically on how quantum mechanics can be used to explain the structure of the state space of classical physics. In other words, I will focus on obtaining physical classical states as classical limits of quantum states. Once one focuses on the state space of a physical theory, an issue arises. The state space one employs in any physical theory is liable to contain unphysical states—ones that don't represent any physically reasonable possibilities. It can be especially difficult in quantum theories to pick out a distinguished subclass of physical states; it is often much more intuitive or clear in the corresponding classical theories which states to consider as physical. It is my contention that physical quantum states should be understood as just those states that can be used to explain the success of a corresponding classical theory. If one can pick out a distinguished subclass of physical quantum states in this way, then a theorem established in previous work shows that it is always possible to construct a quantum theory that allows for precisely the chosen collection of physical states in its state space. But why should all and only the quantum states whose classical limit is a physical classical state be considered physical? I propose an argument based on the requirement that a quantum theory explain the success of a classical theory. My proposal hinges on the claim that a necessary condition for a quantum theory to explain the success of a classical theory is that (i) every
physical classical state can be obtained as the classical limit of a physical quantum state and (ii) the classical limit of every physical quantum state is a physical classical state. For such a proposal to have any force, we must start with some prior conception of which classical states are physical. Then condition (i) amounts to the claim that the new theory can recover, at least in some approximate sense, the predictive and explanatory power of the old theory. Condition (ii) is more controversial, but I argue it is motivated by the requirement that a quantum theory explain the success of theoretical structure of classical physics, where that theoretical structure includes the structure of the state space.

Paul L. Franco, University of Washington: In "The Road Since Structure," Thomas Kuhn identifies a few "present at the creation" of "the historical[...]

philosophy of science": "Paul Feyerabend and Russ Hanson, in particular, as well as Mary Hesse, Michael Polanyi, Stephen Toulmin, and a few more besides" (1990, 3). At the same time this historical turn was created in the philosophy of science, a shift to studying ordinary language in the work of Ludwig Wittgenstein, P.F. Strawson, and J.L. Austin, among others, happened in the philosophy of language. These developments share in common a move away from understanding science or language as a set of logically related propositions to understanding science or language as consisting of practices governed by rules, habits, and conventions. Further, these rules, habits, and conventions resist reduction to a collection of descriptive propositions and their logical relationships. In this paper, I explore these parallel developments in philosophy of science and philosophy of language in the work of Toulmin and Michael Scriven. Toulmin and Scriven were trained or teaching in and around the hubs of ordinary language philosophy at the time, Cambridge and Oxford. I want to make the case that more than just being parallel developments, the ordinary language philosophers' emphasis on actual linguistic practice was influential in shaping Toulmin's and Scriven's respective historical and practice-based criticisms of the ahistorical and logical investigations of logical positivist-influenced philosophy of science. [1] With Toulmin, we see these parallels and lines of influence in his criticisms of formal approaches to philosophy of science in his 1953, Philosophy of Science: An Introduction, and his criticisms of formal logic in his 1958 The Uses of Argument. With Scriven, we see these parallels in his mid-1950s and early-1960s criticisms of Carl Hempel's deductive-nomological model of explanation, in which Scriven argues for an approach to explanation involving "meticulous examination of the circumstances in which [the meaning of terms or concepts or logical problems] occur" (1958, 100). In reconstructing these criticisms of ahistorical and logical philosophy of science, I aim to shed light on intersections between 1950s British philosophy of science and ordinary language philosophy. Moreover, while the philosophers of science I focus on are interesting in their own right, the paper also provides a fuller story about the move away from logical positivism in philosophy of science in the 1950s and 1960s. [1] I do not think it was just Toulmin and Scriven who were influenced in this way. I leave open the possibility of expanding or changing the roster to include the Wittgenstein-influenced Hanson; Feyerabend, who was invited by Scriven to study at the Minnesota Center for the Philosophy of Science in 1957 (Preston 2016, √3−β±2) and who was the colleague of the ordinary language-inspired Stanley Cavell and of Kuhn at Berkeley; and Hesse, given that her work on metaphor shares an interest with ordinary language philosophers on language not straightforwardly literal. I also leave open the possibility of narrowing the roster to some combination of any two of the figures mentioned.

Brian Hanley, University of Calgary: Mill's A System of Logic has shaped a literature on a resurgent philosophical topic. The topic is causal selection, or how we single out important
causes. Mill claims this reasoning is "capricious" and unscientific. By taking Mill as the origin, reactions to his pessimism frame much of the philosophical discussion of the topic. So much so that according to the Stanford Encyclopedia of Philosophy, his view has "won the field" (Schaffer, 2016, Metaphysics of Causation). Consilience with and challenges to Mill's claims guide much of the contemporary discourse on the problem of causal selection and the supposed terms of its solution. Despite Mill's influence, there is a weak grasp of the details of his actual view. There is lack clarity about what Mill means by 'capricious' and the arguments for his claims. His concern with the topic and how it fits into his project in Logic are interesting, but unexplored. This paper lays out overlooked details of Mill's thought and offers a new interpretation of Mill's ideas. I argue that Mill's thinking is more nuanced and the problem he identifies more challenging than is recognized. I argue that this new interpretation offers insights for contemporary philosophers of science interested in causal selection. The deeper philosophical challenge Mill identifies can be used to develop a new path forward for work on causal selection, one building on Woodward (2010) and others.

Alison McConwell, Stanford: Recently, there is criticism of the individuality debate's relevance to biological practice. Kovaka (2015) argues that the quality of empirical work is not affected by resolution of the individuality debate. That is, biological practice does not depend on an established individuality concept. Additionally, Ken Waters (2017) calls for a shift in focus. Rather than asking what is a biological individual, philosophers should ask how biologists conceive of individuals and in which contexts individuality concepts are useful. I will address this challenge. Consider the following example. A conservation biologist may be concerned with how to distinguish the production of something new from mere growth of the same. Reproductive criteria for individuality will be useful for the biologist to assess the number of organisms in a population. However, a clinical immunologist is concerned about causes of immunogenic response. And with the aims to promote human health and treat disease, clinical immunologists are starting to consider humans in combination with their microbial counterparts as one complex immunological individual (see Barin et al. 2015). These methodological differences in part define the value of both reproductive and immunological individuality concepts by their capacity to generate scientific knowledge in a given context. Inspired by a school of thought concerning pluralism, scientific explanations, and a turn to practice, one gains insight into the viability and relevance of individuality concepts by analyzing how biologists use them. My project makes use of some recent philosophical literature in addition to working with information gained from lab settings. For instance, Brigandt (2013, 85) argues that "it is vital for philosophers to take into account the particular explanatory aims underlying individual explanations developed and debated by scientists." And so, I will explore how the diverse interests of biologists calls for many operational concepts of individuality by working with different labs groups. Additionally, Woody (2015) offers a pluralistic account of scientific explanation more generally that analyzes the practice of scientific explanation - we can understand explanation by how it is used in practice. Similarly, I will explore how the value of biological individuality concepts might be realized in their use, rather than through analysis done in isolation from practical applications. As Woody (2015, 123) describes the philosophical turn to practice, I argue that analysis of individuality can be based on the reasoning of scientists in certain contexts. Individuation practices vary depending on the respective scientific communities in which scientists undertake their work, as well as the choices made concerning how to distinguish biological phenomena. Following Longino's work (2006, 2013), individuality concepts are operationalized and conceptualized according to the pragmatic aims that guide a particular approach. This project is distinctive because many
philosophers have focused on the ontological question of what an individual is and assumed a single answer. Alternatively, I will examine how practitioners in biology and medicine investigate and theorize about individuality, and how diverse individuality concepts produce scientific knowledge in a way that depends upon the aim of inquiry.

**Christopher Stephens**, University of British Columbia: How should we evaluate Darwin and Wallace's arguments for common ancestry over separate ancestry? Elliott Sober ([2019, 2008], [2011], [2015]) defends a likelihood reconstruction of Darwin's reasoning that he dubs modus Darwin: similarity, therefore common ancestry. Armadillos and extinct giant sloths are similar, therefore they share a common ancestor. The Galapagos finches are similar, therefore they share a common ancestor. Sober provides a rational reconstruction of this reasoning by explicating a set of probabilistic conditions (based in part on Reichenbach's principle of the common cause) that are collectively sufficient for an observed similarity to favour the common-ancestry hypothesis over the separate-ancestry hypothesis. Casey Helgeson [2018] criticizes Sober's argument on both historical and epistemological grounds. Helgeson raises two epistemic objections. First, he argues that 19th century uncertainty about the age of the Earth casts doubt on Sober's reconstruction of modus Darwin. Second, he argues that the space of trait possibilities on the separate ancestry model is not well defined. Consequently, if Darwin argued in the way that Sober suggests, his arguments would be flawed. However, Helgeson provides textual evidence to suggest that Darwin only defended the weaker comparative claim that species X is more similar to species Y than either is to species Z. According to Helgeson, we should think of modus Darwin as arguing for the more modest: greater similarity, therefore more recent common ancestry. I argue that both Sober and Helgeson neglect crucial features of the historical context, and that a better understanding of the history leads to a more defensible version of modus Darwin. In particular, one of the conditions that Sober specifies "that the two ancestors postulated by the separate ancestry hypothesis must have character traits that are probabilistically independent of one another" is problematic. I make two related points. First, a historical point: I present evidence to show that some of Darwin's most important 19th century targets, such as Geoffroy and Owen, would not have accepted this condition. A better representation of Darwin's (and Wallace's) reasoning would be to think of modus Darwin as an inference about both trait matching and biogeography (or fossil evidence). This leads to my second main point: if we understand modus Darwin in my alternative way, we can relax the problematic assumption about probabilistic independence to allow for some correlation between the traits of organisms on the separate ancestry model. I then prove that this new condition, combined with Sober's other conditions, are still collectively sufficient for an observed similarity to favour common ancestry over separate ancestry. This new set of conditions provides a more accurate picture of modus Darwin, in both its historical and contemporary guises. Once we have this account of modus Darwin, we can respond to Helgeson's epistemic criticisms. I also provide evidence to show that Darwin and Wallace were indeed arguing against separate ancestry, and were not content to defend Helgeson's more modest claim.

**Mark Tonelli**, University of Washington (note: co-authored work with Jon Williamson): While the importance of mechanisms in determining causality in medicine is currently the subject of active debate, the role of mechanistic reasoning in clinical practice has received far less attention. In this paper we look at this question in the context of the treatment of a particular individual, and argue that evidence of mechanisms is indeed key to various aspects of clinical practice, including assessing population-level research reports, diagnostic as well as
therapeutic decision making, and the assessment of treatment effects. We use the pulmonary condition bronchiectasis as a source of examples of the importance of mechanistic reasoning to clinical practice.

Kino Zhao, University of California, Irvine: In 1936, the magazine Literary Digest set out to predict the presidential election between Alfre Landon and Franklin D. Roosevelt. By analyzing the 2.4 million responses that were received, they concluded that Lando was going to win with 57% of the votes against Roosevelt's 43%. Instead, Roosevelt won with 62% against Landon's 38%. By contrast, George Gallup was able to predict the result with only 50,000 respondents. This case study highlights the importance of sample representativeness. In order to decide which addresses to send the mock ballots to, the Literary Digest used their own subscriber list, as well as addresses taken from automobile registration lists and telephone books. As a result, their sample consisted of people who were wealthy enough to own magazine subscriptions, cars, or telephones just a few years after the great depression. They were middle to upper class, which also meant that they tended to be Republicans. They were not representative of American voters at that time. The difference between a representative sample and a biased sample is a lot easier to intuitively understand than precisely define. A common myth is that a representative sample is one we would get if we faithfully practiced random sampling techniques. In fact, these two concepts are independent. A random sample may, by sheer chance, be non-representative; and researchers can, and often do, non-probabilistically select units to compose a representative sample. More importantly, an "almost random" sample will not be almost representative, and a non-representative sample cannot be made more representative by introducing more randomness. This paper is about sample representation in the social sciences — what it is; why it's difficult; and what to do about it. The paper addresses these three questions in its three core sections, outlined as follows. In section 1, I present and defend the core thesis of the paper, which is that a sample's representativeness can only be assessed with respect to a specific research purpose; reappropriation of the sample to a different purpose often does not preserve the justifications originally provided for the sample's representativeness. As a secondary aim, I highlight the effectiveness of sampling techniques that do not involve randomization in enhancing sample representation. In section 2, I discuss a few consequences of overlooking the contextual nature of sample representation and the overconfidence in the power of random sampling techniques argued above. In particular, I discuss the consequences from the perspectives of the hidden influence of the so-called non-epistemic values in exacerbating bias and injustice as well as the replication crisis facing the social sciences. In section 3, I offer a number of practical remedies aimed at mitigating some of the problems discussed in this paper. I argue that the traditional strategy of improving the randomization process is, for most research teams, neither realistic nor effective. By separating the methodological tool of random sampling from the epistemic goal of sample representativeness, we open up more productive conversations over realistic ways of improving research generality in the social sciences.