A Brief Critique of Contextual Empiricism

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Helen Longino argues for a form of empiricism that takes into consideration how the social and political context that scientific knowledge arises affects the knowledge itself, primarily through the linking of evidence to hypothesis. She calls this position ‘contextual empiricism’. In this paper I try to remedy the necessity she takes such a context to have with other intuitions we have about the objectivity or non-contextual nature of scientific inquiry. In particular, I deal with the objection that though science may be run through with social and political values, and though the context that scientific research arises in informs and causes the resultant knowledge, science ‘ought’ to screen-out such factors and consider only rationally warranted judgments. I consider such an objection to be impractical in its prescriptions, as it ignores the necessity of social factors on science.
Social factors have long been thought to affect the production of scientific knowledge\(^1\).

Particularly pressing is the question of the role of ideology in shaping what can be known through scientific methods and practices, as well as what relation this has to questions about the truth of scientific postulates and theories and the role science should have in society given these considerations. I will try to assess the possibly pernicious force of ideology in science by giving a critique of a popular account of feminist epistemology, namely Helen Longino's contextual empiricism as espoused in her book, *Science as Social Knowledge*. In particular, I will consider one objection I take to be pressing, namely that of Stephanie Ruphy, which claims that Longino's empiricism is mere description and social factors can be removed from science on normative grounds. My final conclusion is that Longino survives this objection, as the prescriptions Ruphy makes for science are impractical.

In regarding the influence social factors have on scientific knowledge, Longino argues for what she calls contextual empiricism. By her estimation the social/cultural context in which scientific knowledge emerges is necessarily bound up with the knowledge produced. She begins by making a distinction between contextual and constitutive factors in the production of knowledge. Contextual factors can include political orientations, moral judgements and cultural prejudices; in general, they consist of a wide range of normative judgements stemming from or the particular socio-historical circumstances researchers exist in. Constitutive factors are those methods that scientists use (generally consciously and explicitly) in scientific practice. They include norms about what constitutes proper experimentation, publishing of results and general academic conduct. For Longino, this distinction is merely apparent, as for her what constitutes

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\(^1\) For a survey of the ongoing debate, see Longino (2001) Chapters 2 and 3.
good science is itself the result of contextual factors. More specifically, she argues that a particular piece of evidence being evidence for a particular hypothesis is only determined by what the scientists in question take as evidence for said hypothesis based on their background assumptions about what gets to count as evidence. Her point is that observation is underdetermined; there is no “mere observation”. All researchers must discriminate between different phenomena presented to them in order to count a particular observation as evidence. To do discriminate in this way, these must use assumptions about what is being observed that are not determined by the observations themselves alone. She calls these “background assumptions” (Longino 1990:13), to emphasize that they are only indirectly involved in the production of scientific knowledge and may not be explicitly stated by the researchers.

She thinks further that it being the case that contextual factors are necessarily bound up with the production of scientific knowledge via inferences from evidence to hypothesis, ideology (or those cultural, social and political values we take to be separate from and generally pernicious to science) has a space to play a role as well. She goes into a complex discussion of contemporary research on behavioural endocrinology to make her case. For our purposes, we can use a simpler and more general example: say some scientist wants to prove their hypothesis, and in doing so accepts a given standard for what counts as evidence for said hypothesis. In accepting the standard, the scientist may also be implicitly accepting cultural values, such as sexism, racism or homophobia. Longino believes this to be the case in contemporary behavioural endocrinology as she believes it to be a background assumption for a particular model that there are essential gender differences that can be studied and measured. Thus she thinks that, in order for science to be the truth-seeking enterprise that many commentators purport it to be, it should be open about its background assumptions and only use those

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2 See Chapters 6 and 7 of Longino (2001).
cultural/social values that can be independently justified (e.g. feminism rather than sexism).

A pressing objection against contextual empiricism has been made by Stephanie Ruphy (2006). She questions Longino’s prescriptions for science on the basis of a rejection of the claim to the necessity contextual empiricism describes. She thinks that Longino, in her discussion of behavioural endocrinology, has shown the opposite of what she intended to. What was intended to be shown was that “[no] theoretical perspective can muster constitutively based arguments sufficient to exclude [others]” or, in other words, that arguments from constitutive factors (above) will not suffice to decide the argument, and that neither approach she looks at is doing bad science (where bad science is the act of violating agreed upon scientific methods and practices) (Longino 1990:161). Ruphy thinks that all the examples Longino raises can be dismissed on constitutive grounds, and, further, that background assumptions, including ideological ones, can be similarly dismissed (2006:196). Her meaning seems to be that in the cases of problematic science that Longino brings up, one or more of the approaches can be dismissed as violating agreed upon scientific practice, and wouldn’t be tolerable in other contexts. Further, if the research in any way presupposed cultural or political ideas we took to be problematic, these could be dismissed using agreed upon practices for debate (and possibly even scientific theories). Thus what it is to do good science, for Ruphy, is to screen out those contextual factors Longino thought necessary.

Ruphy is here understanding the distinction between contextual and constitutive as mirroring the distinction between context of discovery and context of justification. The distinction is, briefly, between those states of affairs that produce or cause us to believe certain things (context of discovery) and those normative claims about what we ought to take as knowledge. The motivating idea is that the fact that Newton realised his theory of gravity after an apple hit his head (according to the myth) says nothing about
whether or not we are rationally justified in believing said theory. What would make us rationally justified would be an assessment of Newtonian gravity along constitutive grounds (i.e. if it is proper science). She intends to say that there is a plethora of possible causes for scientific knowledge and the development of scientific theories, but that the causal roles played by different factors say nothing of the justification of particular theories. Ruphy seems to deny the necessity of social/cultural factors to science on the grounds that they are part of the context of discovery, or at least that they say nothing of justification. An extreme example of how context of discovery can be separated from context of justification might be the dismissal of general relativity on the basis that it was espoused and thought up by a Jewish man. I think that Longino would readily agree that this sort of justification doesn’t follow (as it is clearly false). I think that she would also agree that background assumptions and contextual factors could be assessed using constitutive factors as well (e.g. gender essentialism could be denied using rational thinking and evidence).

What is at issue between these two philosophers is rather the epistemic integrity of science, or, in other words, the trust that we as rational individuals as well as we as the general, non-scientific public should place in scientific theories and practices. Longino seems to be arguing that such epistemic integrity should only be conferred on those theories that have as their background assumptions values we take to be permissible. Ruphy, on the other hand, thinks that such values should be screened out, as they are at best neutral in producing true scientific theories. This ‘screening out’ Longino takes to be impossible.

The strength of Ruphy’s objection lies in its intuitive appeal. It seems at first obvious that social accounts of scientific knowledge are descriptive, in that they aim to describe actual scientific practices and can explain certain common errors, but that when we speak normatively about what ought to be taken as scientific fact they fall short, and a view of knowledge that transcends the
particular context of the scientist’s research is ostensibly more preferable. After all, what influence could social factors have on scientific knowledge but a corrosive one? Even if the background assumptions at play involve values we agree with, does it not seem as though this will only screen out potential sources of error? Further, can’t certain embarrassing periods in scientific history (e.g. phrenology) be dismissed as bad science, rather than just science-laden with values we disagree with? My concern with Ruphy’s objection is that it is impractical. I will explain this first by way of analogy: in the case of ethical dilemmas, it is sometimes the approach of thinkers to stamp their feet and demand that the situation be better. When confronted with the choice between saving one or many lives, the argument is that a better situation must exist (though there are more sophisticated ways of making this mistake). The problem with this line of argument is that though it is correct, in that better situations do exist, and they would in fact be morally preferable, the fact of these situations’ existence fails to take into account the problem at hand, which is, when limited to a few options, no matter how ethically unsatisfactory, the best choice is that which lies within the domain of our freedom. It is impractical to demand a better situation that the one at hand because it fails to deal with the problem properly. My claim is that Ruphy is doing this same foot-stamping in her objection. The picture of science she is arguing for fails to take into account what goes on in actual scientific practice. Given that researchers are human beings prone to bias and error, and that they respond to certain pressures, and must do their research within a certain social context that may push them one way or another through funding of projects, academic job applications, it seems as though social factors are in some sense necessary to doing science, good or bad. Denying them is thus mere foot-stamping: imagining a more ideal situation without taking into consideration the given circumstances. Longino, however, needs to establish that social factors are necessary in order to refute Ruphy.

She does this in two ways. First, she argues explicitly that science is a social practice (Longino 1900:75). By this she means that
scientific practice is not something done by isolated individuals, but by groups of people with particular values and particular ends in mind (some examples of this might be certain processes taken to be necessary for good science, like peer-review or replication of experiments). Scientific knowledge, to even be considered as such, needs to be recognized by the community of experts as established fact. Thus there is a sense in which scientific practice is necessarily social. However, it is a still a stretch to say that scientific practice, being social in this way, permits us to infect the knowledge given to us by science with social and cultural values of the researchers or the institutions they belong to.

To put this point differently, we might say that, in ideal circumstances, the community would act rationally and only consider some piece of knowledge scientific if they were properly justified in such a belief. This would undermine the link between scientific practice being necessarily social and the necessary relevance of social factors to scientific knowledge that Longino wants to claim. She would again be relegated to mere description.

In fact, Longino herself takes it that the question of whether or not social factors can play a positive role in science is the “wrong question” (1990:218). She says that “social and contextual values do play a role, and whether it is positive or negative depends on our orientation to the particular values in question” (Longino 1900:218). For example, when considering research into sex differences, Longino claims that certain scientists presuppose essential sex differences, then try to find out what they are. The question of whether or not such presuppositions are negative or positive depends on what we think about essential sex differences in the first place. What Longino seems to disagree with here is the idea that such presuppositions (or, as she calls them, background assumptions) are necessarily one way or the other (i.e. negative or positive in their effects on the conclusions of scientific research). What is at issue is whether or not we take these assumptions to be worthy of being assumed or believed in at all.
The normative character of this line of argument is to say that scientists ought to be open about its presuppositions both with itself, and with the general public. On the one hand, researchers should be clear about what sort of contextual values they being the research by doing (e.g. what their political orientation is, what their cultural bias is, what sorts of pressures on them from their funding) to themselves in order to produce more accurate results that are honest in their scope. They should try to be clear about what sorts of biases they have going in, so as not to appear wholly objective and divorced from any social context when in fact they just are well ingrained in a dominant one, and so as to be closer to the truth. In the example given above, the researchers should have concluded that, if it is granted that there are essential sex differences, then it follows given the data that they can be described in such a way (particular to the research in question). To conclude that from the study that it is scientific fact that there are essential sex differences not only begs the question, but fails to take into account the presuppositions of the researchers going into the study.

On the other hand, scientists ought to be more open with the general public as well. Often arguments in popular discourse are settled by one side claiming that their position is supported by ‘Scientific Fact’, defined as whatever scientist say is true. Science is presented in these debates as being the ultimate authority on truth; if science is as laden with contextual values as Longino seems to suggest, then this is a serious problem. Scientists, for whatever reason, have done nothing to stop (and in some cases have even aided) propagations of their practice as producing transcendent, unadulterated truth that can simply not be objected to. Further, what is taken to be scientific fact is often used to shape social values (a good example of this is climate change). So the negative effects that a popularized view of science has are twofold: it leads us to the possibility of erroneous belief, and to the possibility of problematic cultural values. Thus honesty about contextual values should be undertaken more readily by
scientists in order to avoid such problems. What is more is that these goals are attainable by scientific practitioners. Being more honest about the role contextual values play in scientific research is something that scientists could implement, and is not too lofty a goal to demand of scientists, as I claim Ruphy’s is.

A possible rejoinder to the impracticality of Ruphy’s normative claims that I have made here is the idea, often deployed in in ethics, that having a stringent ideal, even if ultimately unattainable, can still have positive effects. In ethics, the claim is that if we adopt the moral rule ‘Never Lie’, say, it might cause us to lie less than we would have if the rule had been ‘Don’t Lie Much’, even though such a rule would be more attainable. Even if the rule were wholly out of our grasp, like ‘Give To Charities All Wealth In Excess Of That Needed For Bare Survival’, the effects of adopting such a rule might be more beneficial than asking individuals to donate a more modest amount. In the case of science, the argument goes that adopting a highly stringent ‘Screening-Out’ rule like Ruphy’s might lead to better effects than a more modest normative claim. These could include more objective scientific practice, and less chance of being led away from proper results by biases or prejudices ingrained in a culture.

My response is that the case of scientific objectivity is not analogous to ethics. First, there are good reasons to suggest that adopting such a rule as regards charity will not be beneficial, that can be applied generally to claims about stringent moral ideals (see Arthur 1984). The argument is that rules, if too imposing, would have the effect of making us ignore the rules entirely rather than follow them more feverishly. Second, the necessity that Longino is claiming for the sociality of science might make the comparison to ethics problematic. In the ethical case, what we have is a given set of circumstances contrasted with an ideal,

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3 Thanks to Alex Koo for raising this objection to me.
4 It should be noted that I am making an empirical claim about human behaviour here, that could potentially be undermined by empirical data.
along with the assertion that implementation of the given rule would be bring us closer to the ideal. In the case of scientific objectivity, such an assertion simply cannot be made. Ruphy’s normative claims would not serve to make scientists more objective, since what is at issue here is whether or not science is an objective a method of attaining knowledge at all. What is being challenged is objectivity so conceived of as an ideal. If, as Longino contends, scientists cannot but impart contextual factors in their practice, and that to be objective is in some sense to do this imparting, then adopting an imposing rule regarding their practice will have no positive effects whatsoever, and plenty of negative ones, since it will ignore a relevant feature of what it is trying to impose on, namely this imparting.

Bibliography


Sacred Rejection, Pathological Desire: Sociohistorical Transformations in Beliefs about Anorexia Nervosa

Jacob Lang

Anorexia nervosa (AN) is characterized by persistent diet restriction, intense fear of weight gain, and distortions of self-perception. These diagnostic parameters have contributed greatly to popular beliefs about the motivations and personalities of AN patients. The aim of my analysis is to contextualize the contemporary medical definition of AN and identify core assumptions that bleed into our etiological theories and treatment models. Case histories spanning from the Middle Ages to the implementation of the medical model in the 19th century were consulted with respect to psychological theory (e.g. psychoanalytic interpretation). As the intellectual and cultural climates in Europe transformed across the centuries, so too did causal beliefs about extreme self-starvation; from a rejection of beauty ideals or of the corporeal body entirely, to a desire for the ideal. My discussion culminates with an overview of competing relational, neurobiological, and culture-based models of the syndrome, introducing the risks of relying on standardized diagnostic constructs.


**Background**

*Anorexia nervosa* (AN) is a syndrome characterized by persistent diet restriction, intense fear of weight gain, and distortions of self-perception (American Psychiatric Association [APA], 2013). These diagnostic parameters have contributed greatly to popular beliefs about the motivations and personalities of AN patients. The aim of this analysis is to contextualize the contemporary medical definition of the syndrome and identify core assumptions that bleed into our etiological theories and treatment models. I will be tracking changes in the intellectual and cultural environments in which the disorder was defined. Case histories will be presented, spanning from the Middle Ages to the implementation of the medical model in the 19th century. Elements of these cases will be deconstructed with respect to psychoanalytic interpretation and other relevant psychological theories. My discussion will culminate with an overview of the dialogue between relational, neurobiological, and cultural models of the syndrome. This approach allows for an exposition into the etiological transformations of AN while tracing the roots of our assumptions about disordered patients that pervade medicine and society.

Prior to the medicalization of mental illness in the West, it was often reported that European women who engaged in self-starvation were motivated to *reject* beauty ideals as an aversion to marriage, or reject their corporeal bodies entirely for religious reasons (Berg et al., 2003). In contrast, by the 21st century, secular Euro-American culture has come to define AN as a medical condition that requires the patient to express anxiety associated with weight gain (APA, 2013). The trouble with this contemporary criterion is that it imposes an assumption that only those with an intense pull towards an ideal of thinness have AN. Consequently, when a physician applies this diagnosis, they are imposing that it is the patient’s *desire* to have a slender body. This core bias restricts the scope of our etiological theories and treatment models. It has been argued that due to the prominence
of thinness as an ideal in Euro-American countries, the proportion of AN diagnoses in the West massively overshadows the amount of cases in any other culture (O'Connor & Van Esterik, 2008). Indeed, it is the case that cultural ideas of beauty romanticize and aggravate self-starving behaviour significantly (Garner & Garfinkel, 1980, as cited in Bryant & Bates, 1985; Lindberg & Fjern, 2003), but it cannot be inferred that AN is entirely a disorder of Western culture. Beauty ideals are culturally-constructed, but pathological self-starvation may not be.

Assuming a patient’s motivations arouses a host of ethical issues. Even the presupposition that a physician can judge a certain level of fasting to be pathological has been contested on the grounds of patient autonomy (Bruni & Singh, 2012). In 1993, a case was reported that a physician’s order to “force feed” a patient with severe AN had been overturned in favour of the patient’s right to self-determination (British Medical Journal). Another related ethical dilemma is the conflation that exists between an eating disorder patient’s symptoms with aspects of their character. Prominent stereotypes among Canadians include: blaming the patient for their behaviours, assuming that it is the patient’s conscious choice to engage in self-starvation, and therefore supposing it should be easy to eliminate these behaviours (Dimitropoulos, McCallum, Colasanto, Freeman, & Gadalla, 2016). The same study investigated the attitudes of women with AN in an inpatient care program; researchers exposed a disturbing reality that these stereotypes are “internalized,” and are negatively correlated with self-esteem and recovery outcome (Dimitropoulos et al, 2016). In this sense, the application of a diagnostic label can isolate and stigmatize patients.

Some critics, including O’Connor and Van Esterik (2008), emphasize that our assumptions about individuals with AN (and the associated stigma) infiltrate the ways medical interventions are structured. It is imperative to evaluate the beliefs that permeate our medical model. As such, in addition to the Medieval
and contemporary material, I will be examining case histories from the 17th to 19th centuries. At this point in Western European history, mystical interpretations of self-starvation were gradually replaced by a medical paradigm (Berg et al., 2003). What makes this transitional period integral to defining AN was the novelty of secular medical discourse on the subject. Diagnostic categories and stereotypes that interfere with 21st century conceptions of AN were absent. This period serves as a reference point from which we can compare contemporary diagnostic criteria and extract biases.

The precarious ethics surrounding our definition of AN demand an investigation into the sociohistorical roots of the diagnosis. In the following analysis, I will be drawing heavily from psychological theory to bolster or rebut aspects of the Medieval, transitional, and contemporary paradigms that have served to define self-starvation and determine the course of its treatment.

**History and Etiology**

**Lovers, Ascetics, and Rejection of the Body**

It is rare that Ancient and Medieval European accounts of abnormal behaviour are comparable to contemporary constructs of severe mental illnesses. Although records of behavioural patterns like psychosis and depression are evident even in Ancient Greek and Roman literature, there is an absence of specific constellations of symptoms that would constitute a disorder by modern standards (Evans, McGrath, & Milns, 2003). This is perhaps tied to Socratic notions of unusual behaviour (c. 400 BC), whereby individuals presenting with what we would refer to as “symptoms” were conceived as peculiar and mystical. Some archetypes applied to “abnormal” individuals were: “the wise fool, the holy fool, the lover, and the poet” (Dols, 1987, as cited in Sabuncuoglu & Ismail, 2007). This conception of mental illness appears to have been dominant in European civilization in
the Ancient and Medieval periods. In contrast, a medical model of mental disorders was present in northern Africa 1000 years before Socrates (Khalil & Richa, 2014). In the Middle Ages (c. AD 1284), while Europe was still concerned with mystical accounts of abnormal behaviour, village-like hospitals (māristān) were accessible to the mentally ill in Muslim Anatolia and Egypt (Dols, 1987). Nonetheless, there was little discussion of pathological disturbances in body image or eating habits among these cultures, apart from religious asceticism (Berg, Dermont, McSherry, & Strange, 2003). In the area of eating disorder pathology, the most robust Medieval sources are chronicles of the lives of European Catholic saints.

The chronology I will be presenting begins with case histories that resemble disorders of eating habits. Based upon these cases, it can be deduced that the first major European psychology of eating disorders was that self-starvation was motivated either by a rejection of a beauty ideal (avoidance of marriage) or a rejection of the corporeal body (asceticism). One of the earliest examples was the chronicle of the Portuguese princess, St. Wilgefortis (c. AD 900). In protest to a betrothal to a Saracen king of Sicily, the distraught young Christian withheld from eating in order to “make herself as unattractive as possible” (Berg et al., 2003, p. 7). She considered this her religious duty—much to the disapproval of her pagan father. Ultimately, the saint was crucified under her father’s orders. Iconographic representations of the crucified St. Wilgefortis depict her with facial hair, which is likely related to the growth of lanugo associated with emaciation (Berg et al., 2003).

While the narrative of St. Wilgefortis emphasizes protest to real-world pressures, attitudes towards abnormal eating habits based principally on Christian mysticism were also present in Medieval Europe. Anorexia mirabilis refers to the ascetic practice of self-starvation; a demonstration of denial of the material world in the name of God (Forcen, 2013). Accounts of this phenomenon were composed in such a way to inspire awe in the Medieval Christian
(Berg et al., 2003). Although motivations for abnormal eating behaviours were attributed to religious discipline, modern diagnostic criteria for AN appear to apply to many cases. A systematic review of 170 Medieval Italian saints found that 50% of females exhibited symptoms of AN (Bell, 1985, as cited in Reda & Sacco, 2001). One of the most poignant examples of the parallel between anorexia mirabilis and AN was St. Catherine of Siena (AD 1347-1380), depicted in Figure 1. In some accounts, the Italian saint’s childhood was dominated by her mother, Lapa Piagenti. She is described as “a strong woman” who “always had a strongly competitive and intrusive relationship with Catherine” (Reda & Sacco, 2001, p. 41). St. Catherine was prioritized above her siblings in terms of her mother’s physical affection and care. At the age of 12, Catherine began rejecting her meals following a visit to her sister, Bonaventura, and her “rough and brutal” spouse (Uboldi, 1995, as cited in Reda & Sacco, 2001, p. 41). Lapa insisted she eat, but with the help of Bonaventura, Catherine continued her self-denial in secrecy. She also avoided

Figure 1
Anonymous artist from Lesser Poland. (c. 1500). St. Catherine of Siena besieged by demons [Black and white photograph of artefact. Tempera and gold on panel]. National Museum, Warsaw, Poland.
marriage, attributing her change in behaviour to religious inspiration (Dorcy, 1983). After Bonaventura died in childbirth, St. Catherine was betrothed to her sister’s widower. The saint’s body weight depleted significantly, and she began vomiting at every attempt to eat. She also engaged in self-flagellation. It was recorded that when Catherine’s experiences worsened to the point of inexplicable, excruciating body pain, her family believed her to be possessed by a demon (Reda & Sacco, 2001). When she reached adulthood, St. Catherine’s condition stabilized, and she began promoting Christian notions of peace across European kingdoms (Libreria Editrice Vaticana, 1999). Her denial of food became an example of faith and charity.

St. Catherine’s life narrative could be interpreted as a possible eating disorder in light of object relations theory. Psychoanalyst Marilyn Lawrence (2002) posits that the quality of the mother-daughter relationship is an important moderator in the genesis of AN. In adolescence, the special intimacy and affection Catherine shared with her mother was distorted by the saint’s secret rejection of food and care. She was actively splitting her identity (Lawrence, 2002). In one life, Catherine embodied the perfect daughter for her mother, but there also existed a hidden life of spiritual introversion and self-destructive behaviour (Reda & Sacco, 2001). Distressing events throughout adolescence, such as the loss of her sister, could have intensified her symptoms. This theoretical framework also entails that an anxiety of intrusion is a sign of AN (Lawrence, 2002). Catherine’s rejection of Lapa’s care, avoidance of romantic relationships, and inability to ingest food could be interpreted as an aversion to intrusive objects.

Beyond psychoanalysis, the relational perspective of psychopathology in general posits that tensions between the subject and their primary caregiver can set the stage for abnormal responses to emotionally-charged situations (Jewell, Collyer, Tchanturia, & Simic, 2016). Specific interpersonal events that are characteristically difficult for individuals with AN include: role transitions, grief, and arguments with close others (McIntosh,
Bulik, McKenzie, Luty, & Jordan, 2000). In contrast to psychogenic theories, proponents of a purely biological model could interpret Catherine’s behavioural symptoms as evidence of a genetic (Bryant & Bates, 1985) or brain-based (Via et al., 2014) disease. There also exists a third etiological perspective that AN is entirely an epidemic of 20th century Euro-American society (Berg et al., 2003). Medieval cases would challenge the veracity of this model. Body image and attractiveness do not appear to be central themes in St. Catherine’s narrative. Unlike St. Wilgefortis, whose starvation was a conscious rejection of beauty ideals (Berg et al., 2003), the Italian saint primarily justified her harsh asceticism with accounts of mystical experiences (Libreria Editrice Vaticana, 1999; Reda & Sacco, 2001). Catherine’s asceticism was perhaps a sequence of dissociative responses to interpersonal experiences.

St. Catherine of Siena is venerated in the Roman Catholic Church as one of the patron saints of Europe for her extreme self-denial and charity (Libreria Editrice Vaticana, 1999), but psychoanalytic hermeneutics reveal that her relational experiences are reminiscent of those found in AN. Psychogenic and biogenic theories of AN liken Catherine’s self-destructive behaviour to severe psychopathology. This case exemplifies the possibility that eating disorders may well have existed in Europe as early as the Middle Ages, hidden behind ideas of religious devotion.

**Emergence of the Diagnosis**

As the centuries progressed, strictly religious narratives of self-starving individuals gave way to a more medical approach. However, the contemporary assertion that a fear of weight gain is necessary to AN (APA, 2013), along with the stereotypes of disordered patients (Dimitropoulos et al., 2016) had not yet been fabricated. I will argue that the medical judgments that occurred within between the transitional period of the 17th and 19th centuries were less biased than our modern construct of AN.
More than 400 years after St. Catherine of Siena’s anorexia mirabilis, Robert Willan (1790) recorded one of the earliest cases of an AN-like syndrome in a male. In this case history, a “melancholic” and “studious” youth suddenly began restricting his diet to sips of water due to unexplained “pains in the stomach and a constant sensation of heat internally” (Hunter & MacAlpine, 1963, as cited in Berg et al., 2003, p. 7). In addition to these somatic complaints, the young man became fixated on religion. From the onset of his fast until his death (60 days later), the youth devoted himself to transcribing and annotating a Bible (Berg et al., 2003). Because of the patient’s obsessional behaviour, this case could be of particular interest to contemporary scholars who ascribe to biogenic theories of AN. Research in psychiatry and genetics has demonstrated the possibility of a relationship between AN and symptoms characteristic of obsessive-compulsive disorder (Hecht, Fichter, & Postpischil, 1983; Blinder, Cumella, & Sanathara, 2006) as well as obsessive-compulsive personality traits, such as perfectionism (Serpell, Livingstone, Neiderman, & Lask, 2002). A transdisciplinary review by Jarry & Vaccarino (1996) comparing studies on eating disordered and obsessive-compulsive (OCD) patients ascertained phenomenological and neurochemical similarities between the diagnoses. Personality traits, depression, and anxiety levels were found to be parallel between AN and OCD patients. Serotonin (5-HT) receptor activity associated with avoidance and impulse inhibition were also comparable (Jarry & Vaccarino, 1996). In addition, preliminary research by Mas et al. (2013) suggests shared genetic risk variants (symptom-related single nucleotide polymorphisms, or SNPs) between AN and OCD patients.

Like the Medieval cases presented, the plausibility of Dr. Willan’s patient truly having AN is bolstered by brain-based and genetic theories of the syndrome’s etiology. The simultaneous onset of self-starvation, somatic complaints (inexplicable stomach pain), and religious obsession (Berg et al., 2003) might indicate the presence of a psychiatric diagnosis beyond AN. If it is the case that OCD-like behaviours are related to AN symptoms, it is possible that by contemporary diagnostic standards, Willan’s
patient might have had comorbid AN and OCD (APA, 2013). The missing criterion for a diagnosis of AN would be the controversial fear of weight gain. If Willan was correct that there was no evidence of a physiological disorder (Berg et al., 2003), then the patient’s somatic complaints might have been produced by a number of psychogenic causes. For instance, his body complaints may have been a manifestation of OCD—a persistent “urge” associated with religious notions of self-denial, or alternatively, a health-related obsessional fear (Leckman et al., 2010).

My dichotomous proposition that Medieval psychology necessitated a rejection of the body as the prime motivation for self-starvation as opposed to the contemporary assertion that AN requires a desire for beauty does not apply perfectly to Dr. Willan’s case. The uncertainty surrounding the meaning of the symptoms reinforces that, even in a more secular Europe, a drive towards a slender body was not necessarily the motivation of self-starving individuals. Furthermore, the distinctions between certain diagnostic criteria (e.g. AN and OCD) may be more arbitrary than they are useful. This case serves as an exemplar for my argument that this transitional period from a paradigm of rejection to one of desire allowed for the existence of less biased medical inquiry. Since the diagnostic construct was not yet established, the physician’s judgements were not bound in a concrete set of beliefs about the patient’s motivations. This flexible medical model provides a frame of reference to which the present definition of AN can be compared.

The first formal Western medical definition of the disorder had already begun to take shape when the British physician Richard Morton published Phthisiologia, or, a Treatise of Consumptions in 1694. Though he is more commonly regarded for his account of tuberculosis, Dr. Morton recorded two detailed cases of an AN-like syndrome, which he called “nervous consumption” (Morton, 1694, as cited in Berg et al., 2003, p. 8). In his first case, Morton describes an 18-year-old woman with an unfamiliar disorder characterized by complete lack of appetite, paleness and looseness
of the skin, poor digestion, and fainting spells. The patient demonstrated no medical symptoms apart from those associated with emaciation. She passed away within three months (Pearce, 2004). In a second example, Morton presented the case history of an 18-year-old man who had been fasting for two years for no apparent medical reason. The physician concluded that the young man should spend time away from his stressful studies, practice a relaxing pastime like horseback riding, and drink milk regularly (Morton, 1694, as cited in Berg et al., 2003). The patient recovered, and thus, this case is regarded as one of the earliest successful treatments of AN.

Morton’s second case was an early demonstration of the association between anxiety and self-starvation. Regardless, Morton did not ascribe to a purely psychogenic etiological model. In his thesis, Morton outlined two possible causes of nervous consumption: (a) “a morbid disposition of the blood,” related to “the system of the nerves and fibres,” and (b) “violent passions of the mind... sadness and anxious cares” (Morton, 1689, as cited in Bhanji & Newton, 1985, p. 590). In Phthisiologia, Morton predicted the fundamental impasse between biogenic and psychogenic theories of AN that remains unresolved (Bryant & Bates, 1985; Via et al., 2014). With Morton’s diagnostic conception of self-starving behaviour, medical professionals were given the opportunity to identify a syndrome that was previously inexplicable. The construct was crystallized in Sir William Gull’s seminal work, Anorexia Nervosa: Apepsia Hystérica, Anorexia Hystéricus (1874; as cited in Berg et al., 2003). In this period, there were no explicit, mandatory diagnostic parameters. Indeed, the patient was not required to be motivated by a desire for a beautiful body.

**Mind, Body, and Desire for the Ideal**

With the synthesis of a medical model for AN, seemingly anomalous historical cases could be given repose. For example, in her adolescence, Mary Queen of Scots (1542-1587) was struck
with a reduction in body weight, a capricious appetite, episodic vomiting and diarrhea, lightheadedness, fainting spells, and amenorrhea (McSherry, 1985). There was no known physiological cause for this illness, and throughout its course, Mary continued to practice dancing, horseback riding, and hunting. There appears to have been no religious motivation in Mary’s behaviour (Berg et al., 2003), which sets her apart from the earlier Medieval cases I presented. In 1985, J. A. McSherry postulated that it is appropriate to apply a diagnosis of AN to Mary’s condition. He believed that Mary’s syndrome was in accordance with several criteria, including: significant weight loss, the absence of another primary psychiatric disorder, poor insight into the severity of the emaciation, and episodic vomiting (McSherry, 1985). However one item renders the syndrome incomplete by contemporary standards—a fear of weight gain.

This problematic diagnostic criterion was introduced with the publication of the third edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-III; APA, 1980). Prior to the development of the DSM-III, psychiatric symptoms like those found in AN were interpreted as pathological reactions to psychic conflicts. Throughout the 1940’s and ‘50’s, etiological theories of eating disorder pathologies were largely informed by psychoanalytic discourse. Symptoms were viewed as largely unconscious defenses to disturbances of the inner world. According to the early analysts, self-starvation was a somatic reaction to the mental discomfort caused by a deeply-seated socially inappropriate desire, or phantasy (Waller, Kaufmann, & Deutsch, 1940, as cited in Bryant & Bates, 1985). Unsatisfied with these ideas of inappropriate drives or urges, analysts in the latter half of the 20th century focused on the symbolic function of food as a mode of communicating power dynamics between the infant subject and the mother (Bruch, 1976, as cited in Bryant & Bates, 1985). When the child chooses not to eat when fed, the rejection of the food-object represents a desire for independence. This signals a mental readiness for separation from the mother and the individuation of the subject as a self-determining being (Shainess, 1979, as cited in Bryant & Bates, 1985). If the
separation-individuation process is disturbed by a traumatic or unpredictable parent-child relationship, then pathological aversions to food may arise. This is consistent with the object relations approach (Lawrence, 2002), which I applied in my account of the life of St. Catherine of Siena.

Psychoanalytic dialogue and related etiological ideas lost their authority when an emphasis on empiricism, statistical reliability, and uniformity of diagnostic criteria became the dominant force in Western psychiatry (APA, 2016). With the development of strict criteria, such as the requirement that AN patients express a fear of weight gain (APA, 1980, 2013), alternative interpretations of patients’ motivations are shut down. In addition to the fact that the “relentless drive for thinness,” or “fear of fatness” is bound to Western beauty ideals (Goyal, Balhara, & Khandelwal, 2012, p. 290), this DSM criterion fails to encompass etiological theories that are integral to some of the most effective treatment models of the early 21st century. In particular, interpersonal psychotherapy (IPT; McIntosh et al., 2000), family therapy (Evans, Turner, & Trotter, 2012), and mentalization-based treatment (MBT; Jewell et al., 2016) all address the impact of self-definition and family dynamics on the development of psychiatric symptoms, and are all optimized for the treatment of AN.

MBT is predicated on the belief that the development of a mature, healthy ego usually requires a loving and secure attachment relationship with one’s primary caregiver in early childhood (Fonagy, Bateman, & Bateman, 2011). The focus of the therapy is to develop the capacity to accurately interpret emotional meanings and intentions of close others, and to improve affect regulation. The treatment model was originally developed for borderline personality disorder (BPD) symptoms. MBT is fundamentally tied to object relations theory, as attachment dynamics trace their roots to psychoanalysis (Bowlby, 1988). MBT-developer Peter Fonagy and colleagues apply a form of symbolic interpretation to the treatment of eating disorders. In this theory, the ingestion of food represents the patient’s...
overcoming of an avoidant behaviour:

[MBT for eating disorders] suggests an initial focus on the body, to stimulate the patient to investigate his/her experiences with body and food and gradually connect them with emotional, cognitive and relational experience, with the aim to translate them into a language which reflects upon them both as physical reality and as metaphor for mind (Fonagy, Bateman & Bateman, 2011, p. 104).

The MBT interpretation of AN symptoms is closely tied to an etiological approach that emphasizes the role of the body as an important source of metaphors for the anorexic’s lived experience. This is a view shared by psychoanalysts (Lawrence, 2002), anthropologists and sociologists alike (McNamara, 2002). In the first of a three-part study, psychiatrist Finn Skarderud conducted a phenomenological analysis of AN patients’s conceptions of the relationship between the mind and body (2007). Themes such as “emptiness/fullness,” “heaviness/lightness,” “purity,” “removal,” and “vulnerability” were identified as common body-related metaphors that patients used in self-referencing comments (Skarderud, 2007, pp. 167-169). This is consistent with the psychoanalytic postulation that the form and actions of the body have a special symbolic value to individuals with AN (Bryant & Bates, 1985). Skarderud conceptualizes of the anorexic’s “fear of eating and of gaining weight” as but one manifestation of a more generalized feeling of being overwhelmed by difficult emotions, which stimulate “the urge for emptying” (p. 167). The proposition relates to the dynamics of affect regulation described in Fonagy’s mentalization theory (Fonagy, Bateman, & Bateman, 2011) as well as the anxiety of intrusion put forth by object relations theorists (Lawrence, 2002). These frameworks do not entirely rebut the DSM criterion of a fear of weight gain. Rather, restructuring the criterion to address a more dynamic, core anxiety related to food and the body might confront a deeper layer of psychopathology — one that avoids the culture-bound supposition that all individuals with AN harbour a desire for a
The existence of a basic anxiety that transcends cultural values would be consistent with biological models of self-starvation. Genetics research did not yield significant findings in much of the 20th century (Bryant & Bates, 1985), however contemporary studies in the cognitive neuroscience literature have revived biogenic hypotheses. In 2004, clinical psychologist M. Strober proposed that “excessive fear of weight gain in AN may be mediated by a predisposition to fear learning, with neurobiological and genetic underpinnings” (as cited in Kidd & Steinglass, 2012, p. 420). Genetic studies have linked AN to other diagnoses characterized by anxiety and obsession (Blinder et al., 2006; Serpell et al., 2002). MRI studies using diffusion tensor imaging techniques have revealed abnormal white matter pathways across the brain in AN patients as compared to subjects with normal weight and eating habits (Travis et al., 2015; Via et al., 2014). Contemporary researchers have also given significant attention to the role of perinatal risk factors in the genesis of AN. One Italian cohort study, including 114 AN patients, 73 bulimia nervosa (BN) patients, and 554 healthy controls, investigated the role of obstetric complications in the emergence of eating disorder pathology later in life. Complications in the womb that were correlated with a diagnosis of AN included maternal anemia, diabetes mellitus, placental infarction, preeclampsia, neonatal cardiac problems, and hyporeactivity (Favaro, Tenconi, & Santonastaso, 2006). These biological models are compatible with the notion that the fear of weight gain is a manifestation of a core anxiety that transcends culture. It is possible that both religious practices (Middle Ages) and beauty ideals (21st century) could serve as conduits for the expression of this anxiety.

The etiological debate between psychogenic and biogenic theories of AN has also been tested directly in larger-scale studies. From a cohort of 989,871 individuals born in Sweden from 1973 to 1982, 1122 patients were hospitalized for AN-related medical emergencies in the 1990's (Lindberg & Fjern, 2003). Multivariate
analyses aimed to reveal the relationship between the following factors and the expression of AN symptoms: (a) Demographic and socioeconomic factors, (b) psychosocial variables, and (c) perinatal risk factors, including preeclampsia and low gestational age. The variables that were most significantly associated with increased risk of AN were gender (89.2% female) and whether the patient had European-born parents (49.3%). Psychosocial variables accounted for 7.6% of AN risk, and perinatal complications appeared to have minimal contribution to the development of AN (Lindberg & Fjern, 2003). The disorder was also associated with higher socioeconomic status.

This data undoubtedly reinforces an approach to AN that emphasizes the cultural climate in which the subject was raised. However the variables considered do not have enough depth to rule out the effects of the social climate adequately. The only psychosocial factors considered were parental psychiatric diagnoses, international adoption, and parental addictions (Lindberg & Fjern, 2003). These elements of the child’s social world do not capture the psychological and interpersonal factors implicated in AN (Jarry & Vaccarino, 1996; Jewell et al., 2016). I would argue that the inclusion of medical records documenting experiences of trauma and the existence of comorbid psychiatric diagnoses (e.g., OCD, anxiety, and OCPD traits) would have strengthened the validity of the study.

Conclusions

Case histories from the Middle Ages exemplify the restrictions that the religious paradigm imposed on how society conceived of self-starving individuals. The chronicles of Sts. Wilgefortis and Catherine of Siena demonstrate the Medieval European belief that self-starving women were actively rejecting their bodies so as to minimize attractiveness or to distance themselves from their corporeal being (Berg et al., 2003). The medicalization of AN demarcated a transition in Western thought that aimed to capture self-starvation within a diagnostic construct and develop a cure
(Pearce, 2004). Case histories from this transitional period, along with the flexible diagnostic approaches of the early 20th century, exposed that the strict association between AN and a fear of weight gain is a recent assumption about patient motivations (APA, 1980). This was the result of a shift away from an interpretive model of psychiatry towards empiricism (APA, 2016). The contemporary definition is problematic because it imposes that all AN patients must desire a slender body, which is specific to cultures in which thinness is interwoven with ideas of beauty (primarily Western countries). The influence of beauty ideals in Euro-American countries are inextricably linked to self-starvation (Garner & Garfinkel, 1980, as cited in Bryant & Bates, 1985; Lindberg & Fjern, 2003). However, it is immensely important to strip away the culture-bound presuppositions about AN, and address a more general symptom—a core, body-related anxiety—such that we can incorporate historical cases and broaden our etiological theories.

It is evident that several insights into the genesis of AN have been revealed by unbiased clinical explorations into the personal narratives of AN patients. Some of the most effective therapy models of the 21st century are focused on the patient’s phenomenal experience of the social world, with an emphasis on the dynamics of emotion and family life (Evans, Turner, & Trotter, 2012; Jewell et al., 2016; McIntosh et al., 2000). The effects of these treatments significantly outweigh the capabilities of current pharmacological interventions (David, Broft, & Walsh, 2012). A transcendence of the assumptions that pervade the rigid medical definition of AN and the associated stigma (Dimitropoulos et al., 2016) requires the critical lens of cross-cultural, patient-focused research, not the limiting categories of mainline psychiatry. A complete vision of AN demands the careful observation and interpretation of how body-related anxieties are expressed on a case-by-case basis, in a variety of cultural contexts. Research that operates within the current paradigm is inevitably subject to its limitations.
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Feyerabend v Brown: 
Dawn of the Justified use 
of the Scientist’s 
Consensus Veto 
Power 

Spencer Knibutat

In an era where all so-called "elites" face a sustained political backlash, and alternative facts exist parallel to actual ones, the “institution of science” has come under increasing fire. This paper seeks to pit the ultimate critic of the “institution of science”, Paul Feyerabend, against one of its staunch advocates, James Robert Brown, on the question of whether and in what circumstances the opinion of a consensus of scientists on a particular scientific question should count as a “fact”. The author provides an answer by modifying Brown’s Scientists’ Consensus Veto Power in reaction to Feyerabend’s critiques, to create a justified Scientists’ Consensus Veto Power that can better stand up the criticisms of its critics.
Section I: Introduction

“How are we to use the sciences and who decides the matter?” is a question of importance in Paul Feyerabend’s (in)famous Against Method. In this paper, I reconstruct and analyze two conflicting answers to this question: one by Feyerabend himself, and one by James Robert Brown. In Section II of this paper, I reconstruct Feyerabend’s position, that an elected council of ‘hard-working’ laypersons should decide issues impinging upon science, and contrast it to Brown’s Scientists’s Consensus Veto Power. In Section III of this paper, I more carefully specify the extent to which Scientists’s Consensus Veto Power applies to questions at the vertex of science and society. In Section IV, I identify the major tension between these two positions by levelling a number of Feyerabend’s arguments against Brown’s. In Section V, I respond to the main force of Feyerabend’s arguments, after which I utilize the remainder of Feyerabend’s critique in Section VI to create an improved conception of the Scientists’s Consensus Veto Power.

Section II: Contrasting the Position

In Science and a Free Society, Feyerabend argues that scientists should not be granted a decision-making privilege when it comes to how science develops and affects social issues. Instead, a duly elected committee of hard-working laypeople should be called in to decide on how or if to apply scientific knowledge to these various contentious social issues. On this model, scientists take a role analogous to expert witnesses in a jury trial, providing testimony that the “hard-working” jury members can then decide whether or not to incorporate into their decision. He supports this model of scientific decision-making by claiming that scientists are not much better equipped to decide on these issues

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3 Ibid., 96.
than are laypeople, noting how scientists are often only partial authorities on these largely interdisciplinary issues, being only experts in one of the relevant fields which therefore consigns them to a purely information-dispensing role.⁴

In contrast, James Robert Brown argues for a larger role of scientific experts on the civilian juries called in to decide on these contentious social issues.⁵ More specifically, in cases where the scientific community has reached a consensus, as on the question of whether evolution is real, Brown argues that scientists should be able to overrule the civilian jury’s decision.⁶ If the majority of the committee of laypeople were to decide that evolution should not be taught in schools, then Brown would claim that this position should be rejected due to the consensus of the scientific community. I will call this ability to override the laypeople’s decision the Scientists’s Consensus Veto Power (SCVP).

To demonstrate the intuitive pull of Brown’s argument for the SCVP and its widespread acceptance among parts of the populace, one can see how John Oliver, on a popular episode of Last Week Tonight, conceptualizes the consensus of scientists and the SCVP. In this episode, John Oliver⁷ points to studies⁸⁹ that show that 97.1% of scientists say anthropogenic climate change is real, a number that could reasonably be considered a consensus. Equating this scientific consensus with “fact”, Oliver cites a

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⁴ Feyerabend, Against Method, 250.
⁶ Ibid., 194.
study\textsuperscript{10} that says “1 in 4 Americans are skeptical on Climate Change...” which he argues “doesn’t matter. You don’t need people’s opinions on a fact”.\textsuperscript{11} The general argument looks something like this: if a position is widely endorsed by the scientific community, then it is objectively true (a “fact”) and therefore the public’s opinion on that issue should be irrelevant.

**Section III: The Content and Direction of Science**

Feyerabend and Brown’s contrasting views can be better understood by their differing attitudes on the direction and content of science. The direction of science primarily involves decisions surrounding how certain sub-disciplines of science should develop, which is affected by how society ends up spending its resources (e.g., money, scientific labour, supercomputer access). On the other hand, the content of science is the substance of the theories in a scientific sub-discipline that is studied.\textsuperscript{12}

While Brown concedes that direction can be impacted by social factors, he does not believe that the content of science can be impacted by these social factors — if a certain sub-discipline of science is thoroughly studied, it will produce the same scientific knowledge no matter what the social/political factors surrounding its study are.\textsuperscript{13,14}

It is also important to make the further distinction between the questions that can be answered with an appeal to the content of

\textsuperscript{11} John Oliver, *Last Week Tonight*.
\textsuperscript{13} Ibid., 198.
science and the political/social questions that are associated with questions about the content of science. For example, while the question “Is anthropogenic climate change real?” is a question that can be answered by an appeal to the content of science, it will spark further political and social questions like “should anthropogenic climate change be stopped?” With these distinctions in mind, the limitations of the SCVP are better defined and Brown’s position can therefore be better understood going forward in the argument.

Section IV: Feyerabend’s Criticism of the SCVP

Feyerabend provides three criticisms of the SCVP. First, Feyerabend does not believe in the existence of a scientific community upon which to rest the SCVP. Second, Feyerabend argues that even if one were to exist, the scientific community would have no special privilege to knowledge that laypersons would not possess. Finally, Feyerabend can be seen as arguing that external factors can influence the content of science itself.

The first criticism is Feyerabend’s belief that the scientific community does not exist, arguing that “the assumption of a coherent world-view that underlies all of science is either a metaphysical hypothesis trying to anticipate a future unity, or a pedagogical fake.” Issues supposedly resolvable with sole appeal to the content of science should therefore be better understood as a debate between experts with views along a spectrum. Returning to the jury analogy, since the scientific community is divided, the committee of laypeople should be specially placed to decide between the conflicting experts, since they can provide unbiased judgment from a source external to the conflicting scientists. However, Feyerabend’s conception of the scientific community is perhaps beside the point when it comes to the question at play, since it requires a higher standard for the

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15 Feyerabend, Against Method, 245.
16 Ibid., 251.
The scientific community required to support the SCVP does not “require a coherent world-view that underlies all of science” but just an empirical consensus (or overwhelming majority) in support of a single scientific theory or subdiscipline, as is the case with climate change.

However, this restricted conception of the scientific community does not avoid Feyerabend’s challenge that the scientific community is a “pedagogical fake”. Feyerabend’s second criticism directly challenges Brown’s assertion that the content of science is unaffected by social factors, by arguing that the scientific community’s unanimity on certain areas of science is a “result of shared prejudices: positions are taken without detailed examination of the matter under review and are infused with the same authority that proceeds from detailed research”\(^{17}\). These prejudices arise because it is not from personal and detailed research (and direct engagement with the science) that scientists develop their opinions on various theories/subdisciplines, but from appeal to the works of other scientists such as respected colleagues or teachers/mentors. Thus, a scientist’s opinion on a theory is not necessarily scientifically accurate, and therefore there is room to change it (and the content of science relied upon for the SCVP) through social factors.

This second criticism is supported by the third criticism: not only do scientists inadequately form their opinions based on actual science, but the authorities they rely on instead can also be unreliable.\(^{18}\) For example, Feyerabend argues that scientists often “try to cover up any source of uncertainty that might reduce the credibility of their ideas”\(^ {19}\). While Feyerabend has a number of his own examples in support of this point, I believe Eddington’s famous solar eclipse experiment to “prove” Einstein’s general


\(^{18}\) Ibid., 88.

\(^{19}\) Ibid., 97.
relativity to be an exemplar of his concern. \(^{20}\) In this experiment, some authors claim that Eddington committed intellectual fraud (some even going so far as to charge Eddington with throwing out two-thirds of his data that supported Newton) for the purpose of vindicating Einstein’s theory; yet, despite incomplete experimental evidence and quite apparently dubious statistical assumptions, Eddington succeeded in persuading a large part of the English physics community of the validity of Einstein’s view.\(^{21,22}\) The concern for the SCVP therefore appears to be that certain scientists can mislead the vast majority of the scientific community through their force of character and apparent knowledge such that the content of certain scientific sub-disciplines will be founded, not on science, but on faith in specialists in the field.

**Section V: A Response to Feyerabend**

I will provide two different arguments to support the claim that, even if it is true that the content of science is affected by non-scientific factors (especially the values of scientists), the scientific community should still possess the SCVP. The first argument is a response to Feyerabend’s claim (as mentioned at the beginning of this paper) that “important problems often lie across the boundaries of various sciences so that scientists don’t have the needed experience either.”\(^ {23}\) While Feyerabend ultimately thinks the layperson is equally up to the task of assessing a specific issue as an expert in one aspect of an interdisciplinary field, I will argue that this is false.

First, persons of technical training in science are likely to have a basic understanding of many domains within science in a way

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\(^{20}\) Ibid., 90.


\(^{23}\) Feyerabend, *Against Method*, 251.
that most people do not, in both a substantive and a procedural sense. In a substantive sense, scientists have likely encountered a number of other scientific disciplines, as students who specialize in one scientific discipline are often required to have a basic education in all of science. In a procedural sense, much of science shares similar methodological components. Therefore, a scientist familiar with one area of science will also likely have an understanding of methodological components that will allow them to better analyze and criticize the use of confounding variables, complex statistical equations and the overall methodological quality of studies outside of their chosen field in a way that a layperson would be unable to do.

Where the interdisciplinary issue instead exists at a vertex of scientific and nonscientific issues, the scientist is again at an advantage. For example, many bioethicists (those academics that evaluate normative concerns surrounding biological issues) usually have a background in biological sciences which provides them with a better ability to understand and weigh-in on issues at the vertex of fields like biology, ethics, and theology. This improved ability exists because the insight bioethicists obtain from their biological understanding can give them a unique and relevant perspective that is distinct from a layperson’s. Therefore, Feyerabend’s interdisciplinary problem for scientists does not succeed.

The second argument against Feyerabend’s position involves rejecting an empirical claim that Feyerabend makes in support of his committee of laypeople. Feyerabend’s empirical claim is that “the errors of specialists can be discovered by ordinary people provided they are prepared to do some hard work is the basic assumption of any trial by jury” and is supported by his contention is that it is “confirmed trial after trial”.24 If this claim is proven wrong, much of the apparent faith Feyerabend has in a “hard-working” committee of laypeople appears unfounded.

There are a number of deficits in jurors that have been identified in studies on juror reasoning, such as their inability to properly employ cost-benefit analysis or memorize even small amounts of technical information. Further, a number of studies show laypeople’s inability to assess their own inability to understand science or effectively attain scientific knowledge even with guidance. These studies, unavailable to Feyerabend at the time of his publications, cast significant doubt on his layperson’s committee being as successful at assessing scientific issues as he claims.

I think these empirical studies also serve to point at a larger issue with a layperson committee being unrestrained by the SCVP: if a scientific consensus is attained, it is unclear from where the jury is intended to attain the evidence for contrary reasoning on scientific issues. As Scharrer et al., note “complex science-based issues usually cannot be understood fully without experts’ further advice,” and yet, if all scientific experts should not be trusted, as Feyerabend has argued, then the laypeople committee will be hopelessly without guidance.

Section VI: The Justified SCVP

While the bulk of Feyerabend’s critique is met by these replies, there is still significant room for Feyerabend’s criticisms to be

27 Rainer Bromme, Dorothe Kienhues and Torsten Porsch, “Who knows what and who can we believe? Epistemological beliefs are beliefs about knowledge (mostly) attained from others,” in Personal Epistemology in the Classroom: Theory, Research, and Implications for Practice, eds. Lisa D. Benixden and Florian C. Feucht (Cambridge: Cambridge University Press, 2010).
29 Bromme et al., “Who knows what”.
used to improve the use of the SCVP. By applying these criticisms, the modern scientific community of the SCVP can better emulate the spirit of 17th and 18th century science that Feyerabend was so enthralled with.\textsuperscript{31} I will now propose and discuss potential amendments to the scientific community by which a more justified SCVP can be created.

These amendments are targeted at removing two potential pollutants of the content of science, which will better ensure that the SCVP is being used only when there is a consensus among scientists that arises from the actual content of science. As already discussed, Feyerabend has a number of concerns about the scientific community itself polluting the content of science. I believe that ensuring a more critical review of individual scientists’s research can assuage these concerns, such that the scientific community is relied upon as more than just faith-based authority.\textsuperscript{32} There are a number of steps that can be taken to remove this pollutant, such as by ensuring that academic journals engage in more rigorous peer-reviews of studies before publication or that funding is provided for the replication of studies. Finally, to prevent situations like Eddington’s solar eclipse experiment, the scientific community should be called upon to more assertively identify when knowledge about a field is being provided by the leading experts of a field instead of peer-reviewed studies.\textsuperscript{33}

Brown himself raises the second source of pollution that can affect the content of science. Throughout his research, Brown is concerned with the influence of large corporations over the content of science, who he argues have the power to distort research through a variety of techniques, such as by only providing funding to researchers that continue to support the

\textsuperscript{31} Feyerabend, \textit{Science in a Free Society}, 75.
\textsuperscript{32} Almassi, “Trust in expert testimony”, 66.
\textsuperscript{33} Ibid.
company’s product. The influence of these corporations undermines the content of science of that field to such a degree that it is “science” in name only, hardly capable of being relied on in the application of the SCVP. It is therefore only after the influences from corporations have been purged from a sub-discipline that it can be appealed to for the use of the SCVP.

Therefore, while Feyerabend’s criticisms of both the existence of the scientific community and its role in societal decision-making serves to refute its unconditional acceptance, I believe the SCVP should remain. By highlighting the weaknesses of the layperson committee, I showed that Feyerabend’s argument does not effectively prove why it is the case that laypeople are capable of deciding on issues of science, without becoming experts themselves. Further, by touching on various policy implementations that could be used to reduce the risk of values and unwarranted authorities distorting the scientific community, I assuaged a number of the fears that Feyerabend raises — it is not inherent to the scientific community that it is susceptible to Feyerabend’s criticisms. Therefore, I believe that, by looking at aspects of both Brown’s and Feyerabend’s designs for scientific decision-making on social issues, I have outlined what a justified SCVP would look like.

**Bibliography**


Isaiah Berlin’s
“The Concept of Scientific History”

Mahdi Chowdhury

In his 1960 essay, “The Concept of Scientific History”, Isaiah Berlin criticizes the vogue interest among his contemporaries to theorize the study of history as a kind of science. Berlin questions the project of scientific history by accounting for the irreconcilability of both fields. Berlin argues that history is unique, and likens the practice of historians to artists. I offer a critique of the assumptions latent in Berlin’s essay. Namely, Berlin assumes a definition of historiography that overdetermines and essentializes its difference from science, while downplaying the methodological overlaps between both fields.
In his 1960 essay “The Concept of Scientific History”, Isaiah Berlin identifies “scientific history” as the attempt to establish a “natural science of history” (6). Scientific history aims to systematize the conduct of historiography, and establish standard sets of laws and models for historical explanation. The project of scientific history, in the context of Berlin's time, was most notably espoused and formulated by logical positivists, like Carl Hempel in his 1942 essay “The function of general laws in history”.

Berlin disagrees with the fundamental concept of scientific history. He argues the “chimera” (31) that is scientific history is both a misapprehension and misappreciation of history, science, and their essential methodological differences. Berlin’s conception of these differences is the topic of this paper. This paper is parsed into two sections. Section I explicates (a) what Berlin argues the methodological differences between history and the natural sciences (hereafter, science) are, and (b) what Berlin believes the distinctive features of history and historiography are. Thereafter in Section II, I offer a brief critique of Berlin’s arguments from Section I. The focus of this critique is Berlin’s problematic characterization of history’s supposed methodological uniqueness from science.

**Section I: Summary and Explication**

Central to Berlin’s argument is the view that historiography is resistant to “deductive schemata” (11). By “deductive schemata”, Berlin refers to the abstract, general models or diagrams of natural phenomena that are used in the sciences; examples of deductive schemata include models like the gene or the electron. These models of natural phenomena are simplified, idealized, and generalized versions of their physical referents. In other words, these ideal models are deductive and schematic because they illustrate the behavioural characteristics of their referents in the form of a ideal model. The more a natural phenomena deviates
from the characteristics of its ideal model (deductive schemata), the less credible and useful that ideal model is as a means of explaining or deducing the laws of that natural phenomena. For this reason, Berlin argues that the employment of deductive schemata in explaining history is ineffective. Berlin’s conception of historiography as anti-schematic is the foundational argument for his paper’s other arguments. To unpack these later arguments, this paper will consider what Berlin believes to be scientific history’s (a) misapprehensions about history; and (b) how Berlin thinks scientific history misappreciates history.

(a) Misapprehensions: Differences Between History & Science

What are the mistaken assumptions that scientific history has about history? Likewise, what mistaken assumptions does scientific history make about science? What does Berlin believe to be the differences between these two disciplines? First, Berlin argues that history is essentially resistant to deductive schemata. Unlike the sciences, which can place confidence in general propositions, historical explanations give more credence to the particularity of facts (8) – that is, an emphasis on what is unique, not general, in certain human actions, events, and relations. Thus, scientific history is at odds with the basic epistemic methods of history. He later reinforces this argument by observing that historians in their work prefer different methods, and pursue different interests than simply emphasizing similarities and generalities; rather, many historians study what is special and particular about their personal fields of study. For example, many historians are more interested in characteristics unique to the French Revolution; they may not be interested in developing a general explanatory model of the concept of ‘Revolution’ (18).

Second, Berlin argues that for the historian, there is “no Archimedean point outside history to survey the whole of [history] and pronounce upon it” (10). In other words, the
historian is already embedded within history and what is paradigmatically commonsensical to say about historical events. In other words, the historian is endowed with her own historicity; the historian can only study history with one foot metaphorically already planted on the ground. Berlin contrasts this with the sciences, in which one is seemingly more able to step outside the world, to some extent, by the use of ideal models and abstract and general propositions.

These two points – history’s interest in the particularities of an event, and its epistemic/ontological footing within history itself – serve to contrast with the following characteristic of science that Berlin observes: the disciplines of science corroborate each other through an interconnected tissue of scientific generalizations and laws (10). This is the goal of scientific history – a standardized, schematic conduct of historiography. But what happens when these deductive schemata are applied to history? Berlin foresees two outcomes. First, there is a top-down model of scientific history, as formulated by thinkers like Hegel, Spengler, and Toynbee (14). They often produce aggrandized and general theories of history that often prove uninformative when applied to particular events (12). Second, there are models of scientific history constituted through the writing of monographs about selected histories, (i.e. the history of science in X place at Y time). Berlin believes these monographic histories satisfy some pretense of scientific history. However, these highly focused histories come at the expense of excluding greater parts of history from systematic study (13). Consequently, highly narrow monographs are harder to interconnect with other explanatory frameworks of history. Thus, quite ironically, these models then stray farther away from scientific history, because instead of forming a discourse based on interconnecting laws and models, these narrow histories further disjoint and scatter the possible interconnectedness of scientific history.

Berlin has thus far argued that historical explanation opts for particulars over general propositions, that it lacks an
Archimedean point of inquiry, and lastly, that schematic historical models are either too general and vague, or too particular and specialized. Now, Berlin attempts to critique the idea of scientific history in a slightly different way: not on the level of theory, but by focusing on the practicality and usefulness of applied scientific history. As mentioned before, the utility of deductive schemata as a way to explain phenomena is determined by how little its referent phenomena deviates from it. Berlin argues that the minimalist and general ideal models are the most effective; the simplicity and deductive scope of a model like “the atom” is more helpful than of the model of something like “constitutional democracy”, which cannot deduce the characteristics of its numerous referent as generally (14). In other words, the less an ideal model has to account for phenomena that diverges or varies from its explanatory framework, the more useful it is. Thus, Berlin argues that “theories festooned with ad hoc hypotheses would not be as useful” (14), and this is what the models employed by scientific history will look like. The explanatory models of scientific history will be bloated with ad hoc factums about the particularities of each and every event; these models, contrary to that of the atom or gene, will be shapeless and useless for their generalized explanatory power. In short, scientific models for history would have limited utility as explanatory devices.

It should be noted that this summary does not want to give the impression that Berlin wants to reproduce, or widen the gap between supposedly scientific and humanistic methods of study. On the contrary, Berlin states that scientific techniques can aid the historians (18). However, Berlin is persistent that though “[science] can aid, sharpen, criticize, [and] correct [it can] never replace” (18) the critical, imaginative agency of the historian. The historian always directs historical inquiry. Berlin shifts his tone to now consider, in sharper relief, what is distinctive about the occupation of the historian and the discipline of history at large.
Ideal models better enable scientists to see the world through the lens of an “external observer” (21). In other words, Berlin states that scientific models allow for more dispassionate inquiry by scientists. By contrast, history, in the absence of deductive schemata that may enable the historian to become an external observer, leads the historian to enter into history as a type of “actor” (22). By this Berlin means that the historian psychologically positions herself within history’s “interplay of men with one another” (22) to discern and explain their “feelings, thoughts, actions, [and] ideas” (22). Since the historian is human herself, this inward reconstitution of what historical persons felt, did, and thought, in an almost theatrical mode, is what Berlin envisions the historian does. Thus, the historian’s speciality is her ability to organize fragmentary records into rational and compelling exegeses. The historian’s talents, in Berlin’s account, make her more akin to the artist than to the scientist (22).

The historian as an artistic figure is an analogy that Berlin nurtures throughout his paper to posit the distinctive qualities of historiography. The historian is a kind of impressionist: a sensitive person who arranges facts, patterns, and traces into portraits of coherence and explanation. As previously mentioned, the historian is also comparable to an actor. Berlin makes a binary distinction, similar to Collingwood’s notion of the inner-outer dimensions of history, when he posits this actor analogy: on one level, the historian studies the outer motions and bodies of historical events, but on another level, the historian psychologically positions herself within the inner dispositions of her historical agents; that is, she charts the “inward” movement of their spirit” (25) and times.

The historian-as-artist analogy is used to emphasize the subjective traits of historiography. One example Berlin uses to substantiate this analogy is an analysis of the historian’s rhetoric.
Berlin lists several cliché phrases and strategies that historians use in their language to artificially emphasize the causality between two events. These phrases amount to saying 'thus' or 'therefore', but are craftily employed by historians to sublimate causal linkages between events in their writing. Berlin posits that, unlike an account of events subsumed under some deductive schemata, the historian's language of emphasized-causality is necessary because “the bald juxtaposition of events or facts” (12) would otherwise be difficult to follow. Berlin's implication is that historians have to construct the flow of historical explanation. By arguing that history-writing is subjective, he further characterizes historiography as something thoroughly non-scientific in its interplay between imaginative and creative constructions of meaning from historical traces. This is not an admonishment of historical studies – on the contrary, Berlin finds this interplay to be what makes history unique and interesting.

**Section II: Critique**

My brief critique of Berlin's paper is not an attempt to argue for, or against the concept of scientific history, but rather, to deconstruct certain assumptions inherent in Berlin's characterization of history. Namely, that history and historical methodology are problematically defined by Berlin through an argumentative strategy, similar to how Aristotle arrives at definitions like the function of Man, wherein qualities that are peculiar to X become equated with the essence of X. History is similarly defined by Berlin as that which is unique to historical methodology. In doing so, he de-emphasizes many principles of sameness and overlap between historical inquiry and scientific inquiry. In advancing history’s unique qualities, Berlin sometimes overstates the methodological gap between history and science. This critique offers two illustrative examples.

First, Berlin makes the argument that history lacks an Archimedean point of inquiry – and that this is something unique to history. Berlin simultaneously thinks of scientific disciplines as
being, in a sense, able to step 'outside-of-history'. This is a problematic binary between history and science. First, one must ask whether the historian's enfoldment within history is unique to her discipline; and second, whether the scientist's figurative ahistoricity is plausible? I argue that the answer is no and no. Berlin's binary generalization ignores the historicity of scientific disciplines, and their methodologies – which are a) by no means static and reified in a universal, ahistorical logic, and b) consequently, not unique to history. Two years after Berlin's paper, Thomas Kuhn published his seminal work, *The Structure of Scientific Revolutions*. Kuhn develops and reiterates a theme within 1950's history and philosophy of science: science always has a historical-social-political context, and the rigid disassociation between subjectivity and the sciences is not something that can be taken for granted. In this light, the issue is plainly visible that Berlin focuses on differences, and then emblematizes these differences as essential properties of history or science. The problem is that this undercuts the commonalities between both academic methods that try to account for causal, phenomenal events.

A second example of this tendency is evident in how Berlin distances the method of historical inquiry from that of scientific inquiry with the analogy of the historian-as-artist. He observes that historians attempt to grasp the inner dispositions of historical agents, similar to how actors attempt to get into the minds of their characters. Though this inward and theatrical study may be a device used by historians, Berlin problematically emblematizes this as historical methodology, while de-emphasizing the former, 'outer' and objective study that historians conduct. Second, this analogy homogenizes the object of historical inquiry to be rational human agents. There are two issues with this conception. One, the problematic assumption made is that the historian's mind, can retrace the contents of the mind of another human agent. Two, by defining the object of historical inquiry to be human agents, Berlin excludes notable other genres of history. Examples of non-human subjects of historical inquiry are numerous – histories of oceans, diseases,
micro-histories of salt or debt, etc. Berlin does not account for these historical inquiries, which are mostly absent of performative, inner dispositions. Many of these aforementioned topics overlap with the topics of natural sciences, and are often studied by historians through scientific models, notably the specialized monographic style of writing.

In short, Berlin is following this problematic definition in regards to historiography; latent in his argument is a type of modus tollen definition, which can syllogistically be expressed as follows:

\begin{enumerate}
  \item Historiography \((p)\) is principally that which is unique to the study of history \((q)\)
  \item \(X\) is a common, non-unique quality shared between science and the study of history \((\text{not-}q)\)
  \item Therefore, \(X\) \((\text{not-}q)\) are not what historiography is \((p)\)
\end{enumerate}

This hopefully clarifies how Berlin’s definition of historiography structurally distances historiography from scientific inquiry, and why Berlin thus frames his consideration of the possibility of scientific history through central premises like history that is anti-schemata, or other premises that deny the consequent. Overall, however, Berlin makes a compelling case for a primarily humanistic study of history and the chimeric issues that may occur from confusing the epistemic–ontological conducts of historical and scientific inquiries.

**Bibliography**


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Professor James Brown

Professor Brown teaches Philosophy of Mathematics and Philosophy of Physics with the Department of Philosophy, has published about the role of thought experiments, Realism in mathematics and science, and the Science Wars. This being his last year at the University of Toronto, we talked to Prof. Brown about what it took him to get into the field, his reflections on teaching students at UofT, and advice that he has for Philosophy and HPS undergraduates.
How to Become an HPS Professor

“I never explicitly thought about becoming a professor. It all happened one step at a time.”

Professor Brown told us that he never explicitly set out as an undergrad with the idea that he wanted to teach. “As a kid, I had a vague idea about philosophy and that sounded kinda cool.” He said that he was very interested in leftwing politics during his BA. “You naturally gravitate towards either philosophy or political science, and at that point it becomes an accident.”

Not that that’s a bad thing, he says! Above all else, Prof. Brown told us that it was vital to follow your interests, or at least, try to take an interest in new ideas. “At the time I had no particular interest in the sciences, but I was forced to take logic, and went ‘oh, this is too cool!’.” Coming upon his first teaching job was a bit of an accident as well. When we asked him about when he realized he wanted to teach, Prof. Brown told us “at no point. It happened very very gradually.” During his PhD, it seemed obvious there were no jobs. Instead of stressing, he figured that means he can take it easy! “I don’t have to be brilliant in grad school to get a job, there’s no job regardless!” But after teaching a summer course at Dalhousie, another professor up and disappeared right before the next semester started, and so they offered Prof. Brown the job without competition. “Incredible luck … And once you’ve had a job, you’ve passed a filter.” Sometimes, just taking it easy can work out. The job market isn’t any good nowadays, either, Prof. Brown says, so why stress?

But what about students nowadays? “Make sure you’re doing it the right reason, which is for fun.” Prof. Brown said that there are other options beyond being a professor, though! High schools are teaching Philosophy nowadays as well, so teaching younger students isn’t out of the question. In addition, “law firms will take a B Philosophy student over an A+ anything else,” Prof. Brown told us. “And did you
know Philosophers make more money than anybody else?” Though Prof. Brown admitted we don’t know which is cause and which is effect. Is it that Philosophy teaches you how to be successful and flourish in industry, or that people more inclined to flourish end up taking Philosophy? “If you’re a Philosophy student you know there’s an issue here, though if you’re not, we’ll lie to you and tell you Philosophy makes you smart!”

**Reflections on Teaching**

“I just tell bad jokes!”

Given that it’s Prof. Brown’s last year at UofT, we wanted to ask him what his favourite part of teaching was. “I’ve enjoyed almost every class enormously,” he said. “I benefit from having subject matter that my students like,” he said. “I like teaching Philosophy of Math or Philosophy of Physics because I get a mix of philosophy students and science students.” Prof. Brown wouldn’t tell us any of his trade secrets when we asked about how he creates an interesting course for both philosophy and science students. All he said was “I just tell bad jokes.”

We also talked about students with “math anxiety.” How do you get a student who has never taken university level math, and might even be totally afraid to even look at an equation, to get through a Philosophy of Math or a Logic course? “I have no clue! ... It’s hard though, even OISE struggles with teaching math.” In light of how he got into Philosophy though, Prof. Brown did tell us “what I do know is being forced to take logic, sometimes you find you do love it.” At the end of the day, you need to step a bit outside of your comfort zone sometimes.
Advice for Students

“I often listen to people pick their favourite book, and then laugh at them.”

We asked Prof. Brown what he thinks of interdisciplinary work based in philosophy and if it’s worth pursuing, and he admitted that he’s a bit skeptical of it. “There’s some people who set out to be interdisciplinary, I think that’s a mistake. It’s like, I want to develop a theory that starts with the letter W!” Instead, he encourages students to let their interests lead them first and foremost. He added, “It’s easy in philosophy to do anything you like,” more so than in the sciences. As a philosopher Prof. Brown suggested that “you should do what you like doing and let it go where it goes and if it crosses orthodox boundaries, that’s great.”

We also asked Prof. Brown if there was one book he would suggest all undergraduates read, but he just said “I honestly don’t know.” Prof. Brown was a bit skeptical of upholding one book as his very favourite. “I often listen to people pick their favourite book, and then laugh at them!” he said. “In American elections, they’ll often ask candidates their favourite book, and you know the inevitable answer: the Bible.” But when Al Gore ran for president, the question was asked and he answered “Kuhn’s Structure of Scientific Revolutions.” Prof. Brown was really impressed by it, and seems to think that it’s better to be well read all around.

That being said, Prof. Brown admitted “if I was running for election and somebody asked me that question, I might say ‘uh, uh, the Bible?’”
An enormous thank you to Prof. James Brown for sitting down to talk with us at the Almagest! When we asked Prof. Brown what’s in store next, he told us he’ll be “carrying on doing exactly as I am: puttering around the housing, doing my own stuff the way I always have. Except I’ll have more time for it.” Prof. Brown said that he’s wanted to give a TED talk, and hopes to keep in touch.

“Philosophers tend to flourish late, so maybe when I’m ninety I’ll do something worthwhile!”

Best of luck to Prof. Brown in all his future puttering! Prof. Brown’s list of publications, and more information about the courses he’s taught and work he’s done are available on his Department of Philosophy page here: http://homes.chass.utoronto.ca/~jrbrown/.
Rory Harder

Rory Harder is a PhD Candidate in Philosophy at UofT, after doing his undergraduate here in Philosophy and HPS as well. He primarily studies Philosophy of Language and the History of Analytic Philosophy, and considers Kierkegaard to be his favourite philosopher. He also thinks Carnap gets a really bad rap. Rory talked to us about what it was like transferring to UofT and what he wish he knew at the time, his research and current topics in Philosophy of Language, and gave us some advice on how to tackle grad school as a philosopher.
Coming to UofT

“When I got here, I found there was a strong kind of student communities ... you just had to be a lot more independently motivated.”

Rory got into philosophy when he was about 17. “I was a teenager and just trying to figure out what I was doing with my life.” Through a friend finishing up his own Bachelor’s book, Rory got an introduction into formal logic, and found Nietzsche, Kierkegaard, and Quine. “Because he was doing an undergrad degree, he knew the legitimate philosophy stuff for me to read, as opposed to maybe the pop stuff.” From that foundation Rory got interested in Carnap and other philosophers and science, and found the HPS department here.

Before Rory was at UofT, he first studied Philosophy at Trent University, where the academic climate was very different. Trent was a lot smaller, and as a result, you could get a more face time with your professors. UofT felt huge on the other hand, but that doesn’t necessarily mean it’s impossible to engage with the campus. “When I got here, I found there was a strong kind of student communities,” Rory told us. And even though you can be in classes of 200 or 300 people, it seemed like “hardly anyone goes to office hours, and if you have something to say about the reading you can do it.” At UofT, Rory said, “you just have to be more independently motivated, which I was, so I got a lot out of it.” Rory told us that he really enjoyed the fact that you could get more cohesive community of students that come out to PCU events, for example.

Contemporary Philosophy of Language

“You ought not to kill anyone.”
Rory told us about some of the work in Philosophy of Language that he’s focused on, lately. You can easily use language to describe what’s happening here and now, and formal logic is well adapted to this. But what’s more complicated is when you want to talk about potential occurrences beyond what’s immediately happening. “What it seems like is that we’re talking about how things are in other worlds in a sense.” If you want to analyze ‘it might be raining,’ what you’re saying is “there is some possible way things could be, consistent is what I know, in which it’s raining.” Why is this important? Rory tells us that this is often take as one of the fundamental features that distinguishes human language from other communication systems. Rory is also interested in the logic of morality, or “deontic logic.” What do we mean when we say that “you **ought** not to kill anyone?” “It’s not actually the case that you’re talking about this world. What you ought to do can be violated or obeyed.” It gets even more complicated, and there isn’t necessarily a clear answer. “It’s not just as simple as thinking about objects and properties.”

We asked Rory how much he thinks Philosophy of Language still bears on HPS. He still thinks there’s a very strong connection between the two. “One big question in Philosophy of Science today is what Quantum Mechanics says about the way the world is,” Rory says, because there’s plenty of very strange aspects to the formalism “that don’t have clear things in reality that they can correspond to.” Answering the question of what this tells us “is all about the interpretation of the language,” says Rory. Once you know the math, in a welldefined way, you can think about “how can I change this theory to still keep it true, and maybe if I can change aspects of this structure, then does that correspond to something in the world?” That methodology of dealing with complex theories, Rory says, is still hugely important for presenting what we know scientifically. So even if we don’t think that a Philosophy of Language is as foundational to science, like we might have done before, a lot of these methods are still shared between the fields.
After a Bachelor’s

“Getting a job teaching has the same structure now as if you wanted to be famous.”

To wrap up, we talked a bit with Rory about things he wish he knew as an undergrad, now that he’s in grad school. “I don’t think I realized as an undergrad how to have a good writing sample.” Rory said that’s it isn’t easy to exactly know where the contemporary field you’re interested is at. “If you want to be really engaging with what people are thinking now, you want your writing sample and the things you’re thinking about to reflect that,” he said, but even in 3rd and 4th year courses it’s hard to grasp what your professors are really doing. So how do you get around it? Rory told us that “it’s essential to read a lot in your own time,” if you want to continue with school. “That’ll only come out when you talk to your professors at a more casual level.” Like Rory said before, taking initiative can be incredibly vital.

As for his future goals, Rory wants to go on and find a job teaching. “I’m not optimistic, the odds are against you a lot,” he said. “Pursuing this is not rational in many ways. If your main goal is to teach, because that would be kinda cool, then you really shouldn’t be doing this.” Rory said, though, that graduate school can be a very very fun thing to do. As fun as the undergraduate community can be, there’s a whole other world “that you just won’t get in undergrad.” We asked if Rory agreed with Prof. Brown’s outlook — “go to have fun” — and Rory said he agreed. “You can relax if you don’t have the set goal of getting a job in academia.” Either way, if this is something that you love to do, it’s hard to go wrong. “You just have to be independently motivated.”
Thanks to Rory Harder for giving us a glimpse into what it’s like to be a graduate student, after studying HPS and Philosophy at UofT! We wish him all the best with his future plans. You can find more info about Rory, and read some of the work he does, on his page with the Department of Philosophy, here: http://www.philosophy.utoronto.ca/directory/roryharder/.
Professor Matthew Allen

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Professor Matthew Allen is an Assistant Professor at UofT’s Faculty of Architecture. He writes about the history of architecture, has studied Physics and some Sociology of Science in the past, and even also developed video games a long time ago. We sat down with Prof. Allen to talk about what it’s like moving back and forth between industry and academia, how philosophy plays into design, why the divide between arts and science is “bogus,” and about his advice for interdisciplinary students, having been a disciplinary butterfly, in a sense, himself.
Prof. Allen might be the living, breathing definition of changing career directions. He actually started out as the lead programmer at his own video game company, before pursuing a bachelor’s degree in Physics. After taking some architecture courses during his time as an undergrad, he did a Master’s in Architecture at Harvard, worked for some time in the architecture industry, before coming back to school to pursue a PhD, teach, and write about the history of architecture. Nowadays, he writes specifically about how a group of Cambridge architects in the 1960s invented their own subdiscipline, making use of early computers.

We asked Prof. Allen about why he’s moved around so much, and what it was like. He told us that working in industry, as opposed to academia, was disappointing in a way. “Building is a normative enterprise.” At the end of the day, you have to lease or sell a property: if you’re designed residential spaces, they have to be “normal,” more or less. On the other hand, public architecture is just treated like an art piece, more like a sculpture than a building. “What I didn’t like about architecture was having to work for other people” in a meaningless way, is what Prof. Allen told us.

Coming back to academia wasn’t necessarily a hard transition. “I never left academia, I worked full time but the architect I was working for was the chair of the arch department.” Prof. Allen said he was “constantly connected to things in the school.” In fact, Prof. Allen told us that he was hired for the department because of the fact that he could bring his game design experience into the architectural process. “Things that I did on my own, years later, inevitably come back to inform what I do,” he told us. Anything that you’re really interested in, and throw yourself into learning, inevitably
ends up being useful. “But you only realize that after a certain number of years of bouncing around in different fields.”

Starting off with a Bachelor’s degree in Physics, Prof. Allen took a course in Architectural Theory with no idea that he’d end up Mastering in Architecture. “I suppose I was uniquely lucky in that regard,” he admitted. Overall, considering that millennials are ending up changing jobs more than every generation before us, we thought that Prof. Allen’s experiences were really reassuring.

**Philosophy in Architecture**

“The reason I could never be a philosopher is that I don’t quite buy it.”

So where does HPS fit in? Prof. Allen primarily studied in the area of Science and Technology Studies, and is incredibly interested in the ways that people go about explaining things. When you deal with new technologies and ideas, the task quickly becomes figuring out how to convince people that the idea is real, or true. Prof. Allen recommended us Shapin’s *Pump and Circumstance*, as a great book for how realism is constructed in science.

Prof. Allen also wrote a paper recently, titled *The Screenshot Aesthetic*, about how the first architects to start using computers had to convince their community that screenshots of architectural models could stand in for the traditional drawings and models that architects use. “You suddenly have these machines that nobody understands, but you want to convince people that the interactive computer is real.” When we asked Prof. Allen about his favourite philosophers, he listed Foucault and DeLeuze, but admitted that “the reason I could never be a philosopher is that I don’t quite buy it.” Of course if you start with reasonablesounding premises, you can
get nice conclusions. Working in a technical discipline, like architecture, takes a bit more pragmatism than you might find in academia.

We also talked to Prof. Allen about the method employed in architecture, and whether that’s comparable to searching for a ‘scientific method.’ It turns out that, in the 1970s, a group of neo-positivists actually started mapping the design process in architecture, but, ultimately, in the exact same ways that a positivist view of science was challenged, their project fell apart too. “A static method of science and a static method of architectural design both break down in light of sociology,” Prof. Allen told us. More recently, “nonlinear determinism” has proven interesting to architects. In the early 1990s, architects started asking questions about how buildings influenced people, and tried to break the idea of strict determinism in design by looking as far as evolutionary biology and epigenetics. “Channeling and redirecting” social forces became a huge focus. So even in technical disciplines, metatheories about the way people interact and move, and beliefs about science, end up playing a huge role.

The Arts versus Science Divide

“I think the most interesting things happen when boundaries become permeable.”

When we asked about the grand ‘Arts vs. Science’ debate, Prof. Allen immediately told us that “I think that ‘art versus science’ thing is totally bogus.”

Despite architects usually trying to “defend the boundaries” of architecture against scientists, there seems to be plenty of room, as there is with any field, for crossdisciplinary communication. But Prof.
Allen admitted to us that “I think the most interesting things happen when those boundaries become permeable for whatever reason.” In his own career, moving back and forth between different roles is what made it fun, he told us.

That being said, for Prof. Allen, the divide is very real, especially in a technical design field. When making video games, there was “a big split in the way people think.” “The people I worked with were happy to work as artists 100% of the time, I was happy to work as a programmer 100% of the time.” Prof. Allen mentioned again that, when it comes to architecture, because it’s such a “normative enterprise,” you’re often torn between building something “normal” or something that’s more like a sculpture. But Prof. Allen thinks these aren’t exactly distinct things. “The historical work I’m looking at now is a bunch of people who are working as artists who sort of found themselves also working as scientists.” To Prof. Allen, any artistic movement or scientific discipline is just a kind of “aesthetic investigation.”

The process of designing a building is rarely divorced from the artistic side of things. “There’s a lot of people who go into architecture who go into it to make the world a better place,” Prof. Allen said. They’re usually interested in making architecture political, and there’s a few obvious ways that you can use buildings in political processes. “What’s less obvious is how a piece of architecture already embodies political ideals.” The many inconsequential choices you make along the way of envisioning a building end up drawing from the architect’s political will and cultural choices. Prof. Allen said that “seeing students get that is incredibly rewarding.” When we asked if that goes for scientists and philosophers too, Prof. Allen absolutely agreed. Metacognition and reflection are vital in any field.
Advice for Interdisciplinarians

“Do what you’re interested in, because it will be useful.”

Prof. Allen said that architecture and HPS share a lot in the way that they’re “generalist” fields. “There’s a place in the world for specialization,” but the point of education isn’t to give you a single kind of worldview. All of Prof. Allen’s experience, as he said before, ended up being useful years down the line. “This is why people say to do what you’re interested in,” Prof. Allen told us, “because it will be useful.”

Prof. Allen’s advice is not to restrict yourself in where life leads you. “I take it as a premise that the ‘art versus science’ divide doesn’t exist.” Seeing the world as a series of “aesthetic investigations” is a way more productive means of exploring passions and finding new links that haven’t been considered before.

Any experience you can bring forward can end up contributing down the line. The most important thing is a strong sense of conviction, and the willingness to be lead down different roads. UofT can be a great school for exactly this, Prof. Allen told us. You can easily step outside of your Faculty or Department to take courses in almost anything to produce amazing things. Bringing HPS to architecture, Prof. Allen said that he just wants to “give architects a piece of their history.”
A huge thank you to Professor Allen for sharing so much of his experience with us, and we wish him best in future projects! More about Prof. Allen can be found on his page with the Daniels Faculty of Architecture, here: https://www.daniels.utoronto.ca/people/allenmat. We also asked about his games, but unfortunately Prof. Allen’s games are probably too old to be played anymore.