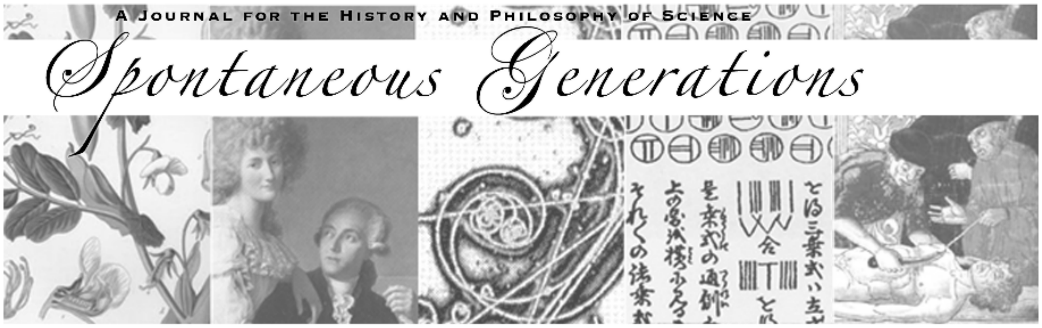

Why the Realism Debate Matters for Science Policy: The Case of the Human Brain Project

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Why the Realism Debate Matters for Science Policy: The Case of the Human Brain Project[*,†]

Jamie Shaw[‡]

I. Abstract

There has been a great deal of skepticism about the value of the realism/anti-realism debate. More specifically, many have argued that plausible formulations of realism and anti-realism do not differ substantially in any way (Fine 1986; Stein 1989; Blackburn 2002). In this paper, I argue against this trend by demonstrating how a hypothetical resolution of the debate, through deeper engagement with the historical record, has important implications for our criteria of theory pursuit and science policy. I do this by revisiting Arthur Fine's "small handful" argument for realism and by showing how the debate centers on whether continuity (either ontological or structural) should be an indicator for the future fruitfulness of a theory. I then demonstrate how these debates work in practice by considering the case of the Human Brain Project. I close by considering some potential practical considerations of formulating meta-inductions. By doing this, I contribute three insights to the current debate: 1) I demonstrate how the realism/anti-realism debate is a substantive debate, 2) I connect debates about realism/anti-realism to debates about theory choice and pursuit, and 3) I show the practical significance of meta-inductions.

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II. Preamble

David Hume, remarking on the enduring stand-off between compatibilists and determinists, states that if "a controversy [remains] long kept on foot, and remains still undecided, we may presume that there is some ambiguity in the expression, and that the disputants affix different ideas to the terms employed in the controversy" (Hume 1902, 81). This sentiment has been echoed by many in relation to the realism/anti-realism debate. Hasok Chang, for instance, writes that "[d]ebates about realism can be frustrating. It is easy to get a sense that one does not know what all the fuss is about" (Chang 2001, 5). Given this frustration, it can seem quite tempting to give up on the debate altogether. In this paper, I argue that the resolution of the realism/anti-realism debate has significant implications for our criteria for theory-choice and thus for science policy.[1] More specifically, I argue that continuity or discontinuity can be used to inductively justify the pursuit (or non-pursuit) of scientific theories.[2] By doing this I hope to contribute three insights to the discussion: I provide a novel defense of the realism/anti-realism debate from skeptical or deflationary arguments, I connect the hypothetical resolution of the debate to discussions about theory choice and theory pursuit, and I demonstrate the practical importance of meta-inductions.

The structure of this paper is as follows. After providing a brief overview of the debate and skeptical arguments which seek to deflate the debate, I motivate our continued engagement in the realism/anti-realism debate by revisiting Arthur Fine's "small handful" argument for realism and relate it to discussions of theory-choice. I then provide a survey of a variety of realist and anti-realist positions and relate them back to the small handful argument. This demonstrates how our adoption of a realist or anti-realist position has normative implications for scientific practices. Following this, I provide the case study of the Human Brain Project in which scientists used meta-inductions to justify their attitudes toward the future prospects of the project. This shows the practical significance of meta-inductions. I conclude by briefly positing a few considerations for future meta-inductions.

III. Some Background on the Debate

Since Putnam (1978) and Boyd (1983), many have conceived of realism as an empirical thesis about the history of science. More specifically, they provide an abductive argument from the success of science to the (approximate) truth of our best current scientific theories. Putnam's original

[1] For this paper, "realism" means the explanationist defense of realism.

[2] For the remainder of this paper, "continuity" and "discontinuity" are meant as neutral terms (i.e., they do not refer to ontological continuity, structural, etc. specifically).

thesis argues that the history of science is *cumulative* in that earlier successful scientific theories were retained as limiting cases of our current best theories. Many since have followed suit and argued that assuming realism is a coherent position (i.e., that abductive reasoning isn't problematically circular),[3] it requires the historical continuity between successful theories to be an accurate description of the history of science. Laudan's (1981) pessimistic meta-induction (PMI) and his anti-realist followers (most notably, Kyle Stanford [2003, 2006]) provide historical counterexamples to the continuity thesis to demonstrate that realism is either false or should be qualified.[4] Given that the correctness of these theses can be provided, if we assume that interpreting the history of science can be done objectively,[5] by the historical record, we should have a straightforward, albeit complicated, answer to our inquiry.

A part of what is difficult about assessing the realism/anti-realism debate is the breadth of the subject matter. Realism, in its earliest formulations, was a *global* thesis about the *entire* history of science. Similarly, Laudan makes the rather bold claim that "for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring" (Laudan 1981, 35). As Lewis notes:

> Given that past theories are not automatically successful, the only way to ascertain whether the history of science supports convergent realism or undermines it would be to conduct a thorough survey of past theories, true and false, successful and unsuccessful. A moment's reflection on the difficulties of such a survey perhaps indicates why nothing like it has been attempted. (Lewis 2001, 379)

Because of this, contemporary realists tend to make more qualified claims.[6] However, they have qualified this global thesis in two distinct

[3] See Psillos (1999, ch. 10) for a defence of abductive reasoning. I also will not consider the objections that realists commit the "base rate fallacy" (Magnus and Callender 2004) or the "turnover fallacy" (Lange 2002). I think both of these objections have been answered definitively by Saatsi (2005).

[4] Stanford's use of the PMI is unique in that it combines historical meta-inductions with underdetermination. I will restrict myself to traditional conceptions of the PMI in this paper.

[5] This is an extremely under-discussed assumption of the debate. See Lakatos (1970) for a criticism of this.

[6] Some, such as Hardin and Rosenberg (1982) and Leplin (1997), retain an extremely "thin" conception of realism that allows for substantive discontinuities. I will not consider these positions further since I accept Stanford's claim that this strategy dilutes realism into a trivial position (Stanford 2003, 566).

ways. Psillos (1996, 1999), for example, argues that realists shouldn't commit themselves to an "all-or-nothing" view in which a single example of discontinuity would undermine the realist thesis, but should instead limit realism to particular cases. This qualification makes realism a claim about particular historical episodes. Other formulations of realism, however, retain a global thesis while restricting the domain of realism to particular aspects of scientific theories (e.g., structures) (see Chakravartty 2011). I will return to these accounts in section 6; for now, I want to calm the worry that the debate about realism is of no significance whatsoever.

IV. Motivating the Realism/Anti-Realism Debate

Regardless of the historical exegesis in which a philosopher of science engages, many commentators on this debate suspect that there is nothing worth debating about. The most famous instance of this is Arthur Fine's "Natural Ontological Attitude" (NOA) which offers a deflationary alternative to realism and anti-realism: science provides "homely truths" grounded in the evidence available to scientists at the time and those truths are potentially defeasible (Fine 1986, 153).[7] Since anti-realists such as Laudan, Stanford, and van Fraassen do not deny the existence of the external world (i.e., the metaphysical thesis of realism), accept some weak version of the semantic thesis which states that theoretical terms refer to *something* in the world, and realists do not deny some weak version of fallibilism, proponents of both positions can subscribe to NOA.[8] What realism seeks to add to NOA, Fine argues, is some superfluous notion of "approximate truth" that exists in addition to instrumental success that amounts to mere "foot stomping" that theoretical entities are "really real." Since there is no substantive difference between "approximate truth" and "empirical success," realism and anti-realism dissolve into NOA.

Proponents of the NOA are not the only philosophers who seek to dissolve the debate. Kukla, for example, writes, "there are no scientific practice arguments on the table that support either side of the debate" (Kukla 1994, 955). Blackburn claims that realism wavers between being "indisputably true" and "obviously false" depending on its interpretation (Blackburn 2002, 112). Howard Stein claims that there is no notable difference between plausible formulations of instrumentalism and realism (Stein 1989, 47). Finally, Maddy argues that we shouldn't have to "add extra-scientific standards of justification to our repertoire" (Maddy 2001, 47-48). This

[7] Further proponents of the NOA include Rouse (1988), Brandon (1997), and Sismondo and Chrisman (2001).

[8] I am implicitly using Psillos' (2000) characterization of realism as the joint holding of the sematic, metaphysical, and epistemic theses.

increasing push for a deflationary take on the debate has left many to search for other means of justifying realism (e.g., Chang 2012) or simply abandoning the attempt to justify realism abductively altogether. I want to resist this deflationary movement and demonstrate how the resolution of the debate has implications for science policy and funding distribution.

V. The Small Handful Argument and Theory-Choice

An underappreciated consequence of realism that Fine considers is what he calls the "small handful argument." He puts the argument like this:

> Suppose that the already existing theories are themselves approximately true descriptions of the domain under consideration. Then surely it is reasonable to restrict one's search for successor theories to those whose ontologies and laws resemble what we already have especially where what we already have is well-confirmed. And if these earlier theories were approximately true, then so will be such conservative successors. Hence, such successors will be good predictive instruments; that is, they will be a successor in their own right. (Fine 1984, 87)

This can be put another way: since realists argue that approximate truth can be inferred from empirical success, they are committed to the view that approximately true theories will be successful in the future. This means that theories explaining some class of phenomena should be continuous with previous theories within that domain (though, see the caveats below). Therefore, continuity becomes a sufficient condition for pursuing a theory.[9] This provides a crucial difference between approximate truth and empirical success: approximate truth entails the *continued* success of the theory whereas mere empirical success does not.

If some explanatory defense of realism is true, then, we should add "continuity" to the list of factors considered in choosing between rival theories. Historically, there has been a great deal of emphasis on synchronic virtues such as simplicity, accuracy, explanatory scope and depth, and consistency (see Mackonis 2013). However, diachronic virtues such as fruitfulness are rarely discussed.[10] If realism is correct, then continuity

[9] I am using the term "pursuing" in Laudan's (1978) sense in which pursuing a theory means we develop its theoretical apparatus, applying it to new phenomena, and so forth. However, Laudan does not go into any detail about what the rational conditions of pursuit are. Whitt (1990, 1992), filling in this blank, argues that mathematical analogies to previous theories are a criterion for theory pursuit.

[10] Kuhn (1977) lists fruitfulness as a criterion of theory choice. However, he provides no analysis of it and simply states that it "deserves more emphasis than it has yet [or since]

provides a key indicator that theories pursued are likely to continue to be successful. By contraposition, we are also able to infer that we should not pursue theories that *contradict* or are *incommensurable* with those established and well-confirmed theories. This conflicts with notable views such as that of Feyerabend who argues that the proliferation of incommensurable alternatives is an essential feature of scientific progress (Feyerabend 1975, 28). However, if Laudan's anti-realism is true, continuity is either irrelevant or negligible for our criteria of theory choice.[11] Revolutions, in Kuhn's sense, are allowable or even desirable events in scientific development on the anti-realist account. Our conditional acceptance of realism or anti-realism, then, has important implications for theory choice and pursuit.

It is important to clarify a few things here. First, the resolution of the realism/anti-realism debate does not univocally determine the ontological conditions for pursuit for two primary reasons. If some kind of realism is correct, ontologies that are discontinuous with previous ontologies (within the same domain) may still be pursued for *heuristic reasons* (e.g., they aid in the formulation of more acute criticisms,[12] they suggest new experimental methods, they provide a useful pedagogical contrast, etc.). Second, the ontological commitments of *idealizations* may be incompatible but still jointly explanatory. Take, for example, Sandra Mitchell's example of studying social insect colonies: models make incompatible ontological assumptions about genetic diversity and yet can, in specific cases, jointly provide a unified true explanation of individual colonies (Mitchell 2002). Having incompatible ontological commitments within the broader domain of social insect colonization is necessary for true *individual* explanations. In both of these cases, we pursue ontologically diverse theories within a domain *even if realism (of some kind) is correct.* Therefore, even if realism is true, ontological pluralism is not refuted. Finally, as will be pointed out later (see n. 20 below), methodological decisions impact theory pursuit as well. In order for a meta-induction to be useful, past instances must be *of the same kind* as current ones. It is quite possible that two theories are ontologically continuous but new evidence suggests adopting some ontologically incompatible theory.

received" (Kuhn 1977, 322). An exception to this oversight is found in the aforementioned literature on theory pursuit.

[11]If Laudan is correct that 6/7 theories are discontinuous across theory change ("I daresay that for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find a half dozen once successful theories which we now regard as substantially non-referring" [Laudan 1981, 35]) then ontological continuity is not even a *reliable* indicator of theory promise.

[12]See Feyerabend (1962) on this issue. See also section 3.2 of Shaw (2016) for a brief list of pragmatic factors that often partially determine funding and an argument for pluralism in funding distribution.

Meta-inductions *alone* do not determine theory pursuit, but they can, *ceteris paribus*, be a sufficient (but not necessary) condition for theory pursuit.

VI. Realisms and Inductive Bases

Before illustrating how meta-inductions are important in practice, I want to make some brief remarks about the importance of the different formulations of realism and their resultant claims about continuity. Here, I will survey three prominent positions: entity realism, structural realism, and traditional realism.

Entity realism, as outlined and defended by Hacking (1983) Cartwright (1983) and Giere (1999), commits itself to the existence of entities in contrast to the truth of theories (or laws). If we can consistently and reliably intervene and interact with the entities handled in laboratories, we can be realists about those entities. This is because experimentation, Hacking argues, operates largely independently of theoretical considerations. This, when combined with an adherence to a Kripke/Putnam causal theory of reference, supports the thesis that we are referring to the same entities across theory change.[13] In contrast to this, structural realism, as defended by Worrall (1989, 1994), Ladyman (1998) and Zahar (2007), holds that realism applies only to the *structures*, understood as the formalisms of a theory,[14] of scientific theories. Worrall's structural realism grounds its optimism in the case study of the transitions between Fresnel's luminiferous ether and Maxwell's electrodynamics. Since his landmark paper, more case studies have been recruited to the structural realist cause.[15] Finally, traditional realism, as defended by Psillos (1994, 1996, 1999, 2000), Kitcher (1993), and Chakravartty (2007), provides a more general defense of realism. Here, traditional realists argue that those posits that were essential to the success of a past theory are preserved in subsequent theories.[16] I will not articulate or defend any of these positions here, but it is worth pausing to consider the implications that adopting one of these positions would have.

Clearly the kind of continuity we are seeking will differ depending on which position is adopted. If we are structural realists, we will pursue theories

[13]See Hacking (1983) and Devitt (1984) for examples of the entity realist thesis and see Busch (2006) for a counterexample.

[14]Structure is a notoriously difficult term to explicate precisely. See Frigg and Votsis (2011) for an overview of the different formulations.

[15]See Ladyman and French (2003) for an example of structural continuity from quantum theory. For counterexamples to structural realism, see Newmann (2005) and Rivadulla (2010).

[16]Psillos makes a distinction between "idle" and "essential" posits of a theory which is largely analogous to Kitcher's distinction between "presuppositional" and "working" posits.

that contain the structures (however conceived) that previous theories had. If we are traditional realists, we will seek theories that are consistent with the essential posits of previous theories. For entity realists, future knowledge of how to manipulate or intervene with entities mustn't conflict with previous experimental knowledge. However, there is more to say about what the proper inductive base of these positions is (i.e., which kinds of historical case studies could confirm or disconfirm the view in question). If they are intended as global views, like Worrall's or Hacking's, then the inductive base is the whole of science.[17] However, the inductive base could be localized to a particular discipline or domain, in which case the inductive base would be qualified appropriately. In addition to what the inductive base is, one must determine a threshold that must be passed and the probability method being employed. Clearly a single counterexample, if there are dozens of genuine examples, should not discredit a view from contention as a tool for theory choice. What the exact threshold is, therefore, must be explicitly stated.[18]

The historical record will be the ultimate arbiter of which position is correct, under what conditions, and to what extent. Which threshold and which probability method we use depend on the goals and contingencies of a given case. In the following section, I will illustrate the importance of meta-inductions in practice with the case study of the Human Brain Project.

VII. Case Study: The Human Brain Project

Meta-inductions are used quite frequently to justify the potential value of scientific proposals (though they often are done quite informally). In this section, I provide one such instance in which meta-inductions have actively been employed. By doing this, I will illustrate the importance of meta-inductions in practice and how they may be formulated.

The Human Brain Project (HBP) is a large collaborative project that aims to model approximately 1% of the human brain using supercomputers. After being announced in early 2013, the ten-year program, which incorporates over eighty partner institutions and 256 researchers (Kenall 2014), has

[17] It is worth mentioning that realists often restrict their scope to "mature" scientific theories though they are quite vague about what counts as "mature." Worrall (1989), for example, dismisses phlogiston as an immature theory. However, phlogiston theory had been pursued for over a hundred years and had made numerous predictions such as the production of water, that metals can be created by heating combinations of phlogiston-producing substances and some calxes, and many others (see Chang 2010). This suggests that phlogiston theory should be identified as a mature theory by any reasonable standard.

[18] I will leave it open as to whether this probability is determined by a Bayesian or non-Bayesian methodology.

accrued approximately 1.2 billion Euros[19] in funding and seeks to "gain profound insights into what makes us human, develop new treatments for brain disease and build revolutionary new computing technologies" (Horrigan 2013). Additionally, the HPB aims to systematize peer-reviewed articles in a uniform manner in order to, as project figurehead Henry Markram has put it, to create a new paradigm about the brain. This would integrate all knowledge about the brain, from the structures of ion channels in neural cell membranes to the cognitive mechanisms underlying conscious decision-making, into a single computational framework (Waldrop 2012). However, this project has met a great deal of resistance, with over 280 neuroscientists boycotting HBP funds (Curtis 2014). Many neuroscientists have argued that this project will be unable to attain the goals it has set out to achieve and that the mind-brain sciences should not be systematized at this early point in their development. This has resulted in a strong disagreement about whether the HBP will be successful or not.[20]

The computational model of the brain has steadily accumulated the ability to represent and to model neuron behaviour effectively.[21] Most of Markram's own work, for example, exemplified successful computational representations of neurotransmitter release probabilities (Senn et al. 2001), generic neural microcircuits (Maass et al. 2002), short-term synaptic plasticity (Richardson et al. 2005), and many other phenomena. More acutely, the HBP explicitly aims to build upon the Blue Brain Project, which successfully modeled approximately thirty thousand neurons of the neocortical column in mice. The challenges the HBP expects to meet involve creating larger and more powerful supercomputers to model approximately four million neurons and one billion synapses.[22] While this would require supercomputers with the ability to run 10^{18} operations per second and the invention of new virtual technologies (e.g., virtual fMRIs), Markram is

[19] There are also private revenue streams coming from companies such as Hewlett-Packard, GlaxoSmithKline, Olympus, among others.

[20] It is worth noting the disagreement between scientists is not purely a result of analyzing historical trajectories. Methodological debates play a key role as well. For example, Robert Epstein argues that computational approaches cannot explain behaviour in neonates or pre-linguistic children, or synaptic plasticity (Epstein 2016). Others argue that computational approaches are superior to neurophysiological approaches since they can provide clinically useful biomarkers for neurodegenerative diseases (see Rose 2014). Meta-inductions *alone* do not justify one approach over the other, but they still provide meaningful arguments to be optimistic or skeptical about the HBP.

[21] While there are many formulations of different computational approaches in the mind-brain sciences, the computational approach I am referencing in this paper is the specific one endorsed by Markram.

[22] This would be a modest first step towards modeling the human brain which contains approximately one hundred billion neurons and one hundred trillion synapses.

confident that this increase in computing power will be possible by 2023, given the consistent trend of computing power doubling every eighteen months (Waldrop 2012).

Markram is optimistic in two respects. First, he is optimistic that computing power will continue to increase as it has done historically (Markram 2006). Here, there is no ontological or structural continuity but the growth of technological capacities. Second, he is optimistic that computational *theories* of the brain will be successful *because they have been so in the past*. Markram's strategy of reverse-engineering (i.e., using supercomputers to run statistical simulations to predict how neuron clusters will combine, and to test these simulations against "real data" from biology) involves various assumptions about the nature of the brain.[23] Markram uses past successes of his version of computationalism, including his own aforementioned studies, the Blue Brain wet labs, Steve Furber's work on asynchronous circuit designs (Furber & Jens 2002), and Karlheinz Meier's work on the physical grounding of information processing, to justify a belief in its continued success on larger scales.[24] This claim can be reconstructed in a straightforward manner: because the ontology presupposed by computationalism has been empirically successful, it must be an approximately true representation of the underlying principles governing neural morphology and architecture. This historical series of successes suggests to Markram, and to proponents of the HBP, that "there is no fundamental obstacle to modelling the brain [computationally] and it is therefore likely that we will have detailed models of mammalian brains, including that of man, in the near future" (Markram 2006, 159).

Many neuroscientists, on the other hand, do not share in this enthusiasm. Rodney Douglas, co-director of the Institute for Neuroinformatics, argues, as recounted by Waldrop, that neuroscience is not ready to be unified under one set of paradigmatic assumptions. Douglas argues that contemporary neuroscience *requires* variance—huge amounts of disagreement and multiple competing accounts of particular domains. At least tentatively, the mind-brain sciences should proceed in a pluralistic fashion, with many different ontologies, since there is not enough knowledge to justify one paradigm over others. For example, brain imaging studies require the assumption that brain areas (or clusters of voxels) are real and non-reducible

[23] See Maass, Markram, and Natschlager (2002) for a more detailed description of this.

[24] Markram writes, for instance, "We know that [computational] rules exist because we discovered some of them while laying the groundwork for the HBP" (Markram 2012, 52). He also implies that the fact that computational models of individual neurons used to be three-year PhD projects, and now larger brain circuits can be done more easily, suggests that "[i]t was clear that more ambitious goals would soon become achievable" (Markram 2012, 52).

to neurons and synapses; modelling in cognitive science assumes neural networks similarly exist; and even within computational approaches there are disagreement between connectionist and classical notions of information (i.e., the latter sees information as discrete whereas the former sees information as emergent from node activations), and so on. Put another way, many incompatible ontologies have provided explanations and predictions, and have aided in developing useful technologies, and there is no reason (at this point in history) to expect one ontology to ultimately be "correct." Given the immaturity of the mind-brain sciences, Douglas argues that "we need as many different people expressing as many different ideas as possible" (Waldrop 2012) and that this becomes increasingly difficult when funds and intellectual efforts become diverted from methodologically and ontologically isolated efforts to explore sporadic and remote corners of the brain. Douglas's claims are implicitly grounded on the history of contemporary mind-brain sciences. Many resist Markram's reductionist approach to understanding the brain (i.e., beginning at ion channels and having the gaps in knowledge filled in by incoming data from the HBP) and instead use more approximate and abstract models of basic biological functions to explore higher-level brain functions (Waldrop 2012).[25] Realism, therefore, is unwarranted given this history of ontological fragmentation. This makes attempts at constructing a unified theory of the brain premature and unwarranted for the time being.[26]

Both computer scientists and neuroscientists use the history of their own subfields to ground their realist optimism and anti-realist pessimism, respectively. The growth of computer modeling has been cumulative, and is therefore likely to continue being cumulative,[27] while the growth of knowledge in neuroscience has been ontologically non-convergent. While I do not want to side with either position of the debate, I will conclude by remarking on these uses of historical inductions to justify their attitudes towards the potential

[25] This is not the only method. Some computational neuroscientists use models of individual neurons to explain higher-level brain functions as well (Waldrop 2012).

[26] It is important to underline that Douglas (and others) are not criticising the HBP on the *mere fact* that it is unificationist. It is due to the fact that it is unificationist *and* that is detracts resources and downsizes alternative approaches. Furthermore, while they are not cited by the detractors of the HBP, there may be further inductive reasons to be skeptical of the HBP's promise to find the physiological basis of some brain diseases (specifically Alzheimer's) given the failures of many previous attempts (e.g., understanding illnesses through disruption in blood circulation, focal sepsis, overstimulation of nerves, and genetic markers). As Owen Whooley puts it, "the history of the search for ... the underlying mechanisms of mental disorders is one of cyclical patterns of enthusiastic optimism and fatalistic pessimism, but ultimately a story of failure" (Whooley 2014, 14). See also Grob 1998.

[27] Though it has been cumulative, it has not been convergent since the growth of knowledge in computer modeling has not "withstood" any substantial theory-change.

fruitfulness of the HBP.

VIII. Reflections on Meta-Inductions and Science Policy

The computer scientists' meta-induction is optimistic. It expects the HBP to be successful given the cumulative growth in knowledge in computational models of the brain. The neuroscientific meta-induction, on the other hand, is pessimistic given the ontologically fragmented growth of knowledge in neuroscience. What is interesting to note here is that different *disciplines* studying the same *domain* (i.e., human and mammalian brains), can exhibit both realist and anti-realist trajectories. If we only analyzed the growth of knowledge in computational approaches, we would be realists about their ontology. This would make the pursuit of conflicting approaches in the mind/brain sciences, which are currently making novel predictions and are successful in their own right, unwarranted. This is clearly problematic. A lesson, then, for general meta-inductions is that disciplines studying the same domain must not have differing ontological or structural commitments if we are to be fully confident in the theories potential success.

Another thing to note is what kind of realism/anti-realism is *relevant* to the case at hand. Entity realists or traditional realists can make sense of the HBP case in their own ways. However, structural realists would have little to say about many of the mind/brain studies, which make fewer structural commitments than mathematical physics.[28] There are simply very few instances of continuity that the structural realist can point to, making a structural realist analysis of the HBP case relatively uninformative.[29] Unless one wants to state that current neuroscience is too "immature" to warrant a structural realist analysis, we may generalize this conclusion to state that certain theses are going to be more applicable in particular theoretical contexts.

Finally, both meta-inductions use very short periods of time: the computer science meta-induction begins after the late 1990s when substantive computer modelling of the brain began, and neuroscientists refer back as early as the 1960s when many contemporary neuroscientific institutions

[28] In low-level neurobiology, there are many chemical and electrical laws, but these are, strictly speaking, laws of chemistry and electrophysics. There are fewer mathematical formalisms used in cognitive science or psychology.

[29] It should be noted that this depends on what is meant by "structure." Here, I am using Worrall's notion of structure as a set of mathematical formalisms that make no reference to the kind of entities that constitute the relata. However, some argue that *all* properties are inherently relational making structural realism viable again for the HPB case (the so-called "upward path"). I am unconvinced that this watered-down structural realism is not simply a repetition of traditional realism, but I will not argue this here (see Psillos 2009 for an argument to this effect).

began.[30] Neither position refers back to Pascal's calculator or trepanning in ancient Egypt for support. If the timeframe of either meta-induction had been expanded to this extent, it would have diluted the force of both arguments. Since the HBP is a *ten-year* project, it may be pointless to speculate on what the state of the mind/brain sciences will be, say, hundreds of years from now. Perhaps, then, meta-inductions should be limited in historical scope, depending on what kind of projections we are attempting to make.

I don't think any of these suggestions are definitive. But they do suggest different ways that meta-inductions could be formulated. More practice-oriented analyses of meta-inductions can reveal greater details about what the history of science can contribute towards science policy.

IX. Concluding Remarks

While the division between computer scientists and neuroscientists is not a strict or rigid divide between realism and anti-realism, the two sides make polar-opposite predictions about the fruitfulness of the HBP. A hypothetical resolution, or even an approximate resolution, of the realism/anti-realism debate would have allowed for a more informed policy decision and could have improved those meta-inductions used by computer scientists and neuroscientists. I believe that this pragmatic value, in combination with our advanced knowledge about theory choice and theory pursuit, justifies our continued engagement with the historical record for meta-inductive purposes.

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