Science & Speculation

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Abstract

Despite wide recognition that speculation is critical for successful science, philosophers have attended little to it. When they have, speculation has been characterized in narrowly epistemic terms: a hypothesis is speculative due to its (lack of) evidential support. These 'evidence-first' accounts provide little guidance for what makes speculation productive or egregious, nor how to foster the former while avoiding the latter. I examine how scientists discuss speculation and identify various functions speculations play. On this basis, I develop a 'function-first' account of speculation. This analysis grounds a richer discussion of when speculation is egregious and when it is productive, based in both fine-grained analysis of the speculation's purpose, and what I call the 'epistemic situation' scientists face.

1. Introduction

Scientists often navigate a tension between epistemic security—conservative, well-supported assertions—and productive speculation: reaching into uncertain, fragile territory. Oldschool philosophers of science accommodated this tension via a divide-and-conquer strategy. Scientific assertions belong to the context of justification, speculation in the context of discovery: the latter involves the generation of evidence and hypotheses, the former

epistemically linking hypotheses with evidence (Popper 1959/2001)[1]. However, philosophers now doubt that scientific practice can be so neatly delineated, that justification and discovery can be easily decoupled (e.g., Soo & Chang 2015). An understanding of the success of science (where it succeeds) requires accommodating science's economic, material and social conditions, its multitudinous aims, skills and processes, and the multitudinous audiences science is for; factors shot through with rich, complex values. In this paper I'll make some progress on understanding how speculative practices feed into scientific success, providing both a novel account of speculation and discussing how and when speculation might be justified.

I'll start with 'thin' accounts of speculation, which define speculation in narrowly-understood epistemic terms. In a nutshell, these consider speculation to be such in virtue of lacking evidential support. I'll provide some reasons to reject this approach, thus motivating an alternative: rather than evidence-first, a definition of speculation should be function-first. In developing such an account, we'll identify the varied epistemic functions speculation plays via an incomplete tour of how scientists discuss speculation, in both derogatory and vindicatory terms.

By my account, a hypothesis is speculative insofar as its warrant lies in 'productivity', where productivity is understood as the wide range of epistemic benefits a hypothesis might bring beyond its being well-supported. These benefits differ: some function towards developing a better-supported hypothesis downstream, others open new areas of investigation, others link together existing pieces of knowledge. These differing benefits require differing warrants. Taking speculation in these terms, then, requires clarity on how those various warrants go, and I hope to make some initial steps towards this. Crucially, evidential support plays a role in some, but not all, speculative functions. My suggestion is, then, fairly radical: speculation by definition has nothing directly to do with evidential support (although evidential support does matter for some speculative functions).

[1] The distinction originated with Reichenbach, but meant something different to him and he repudiated Popper's account (see Reichanbach 1935).

My account is prescriptive. It variously departs from how scientists use the term 'speculation', but this is a feature not a bug. First, as I'll explain, the productive function of hypotheses sometimes includes evidential support and sometimes not: a function-first definition includes evidential support where relevant. Second, some scientific appeals to, and accusations of, speculation serve more to obscure a hypothesis' warrant; my account both defends from unproductive gatekeeping and demands a finer-grained specification of a hypothesis' function.

I'll not spend time discussing the long philosophical tradition tackling 'the context of discovery' (see Schikore 2018 for an excellent summary). This has focused primarily on whether there is a rational logic, or perhaps a set of heuristics, for the generation of ideas, and on the connection between what William Whewell called 'happy thoughts' and their articulation into testable theories (Whewell 1840/1996). Insofar as this work discusses the power of speculation, if often focuses on the psychological properties of scientists (e.g., Peirce's suggestions that good ideas require a mind "in as passive and receptive a state" as possible, 1929, 285), or on forms of reasoning (e.g., Mary Hesse on analogy, 1996). More recently, Paul Thagard has developed neural-network-based analyses of the generation of ideas (Thagard 2012), and other philosophers have followed up on the 'context of pursuit' from Laudan's work (Šešelja & Straßer 2014, Šešelja & Weber 2012, Cabrera 2018, Currie 2019a, Nyrup forthcoming), as well as questioning whether scientific institutions and incentive structures support creative science (Stanford 2019, Currie 2019c, Bedessem forthcoming, Schneider forthcoming, Kidd forthcoming, Anscomb forthcoming). I see my task as complementary: no doubt there is further work to be done exploring connections between speculation and discovery, but I'll focus here on explicit discussion of speculation in science.

2. Thin Speculation

Philosophers considering scientific speculation often adopt *thin* accounts, that is, a hypothesis' being speculative turns merely on its epistemic properties, where we restrict 'epistemology' to a narrow range of concerns.

Distinguish between a narrow and expansive conception of the epistemic. A narrow conception takes the realm of knowledge to be restricted to the propositional, semantic properties of beliefs or hypotheses and their evidential support, where we understand evidential support in terms of some kind of argument-schema. For instance, a Bayesian conception of knowledge is narrow: at least for subjectivist Bayesians, we behave epistemically responsibly just when we update our priors on incoming information in accordance with Bayes' theorem. By contrast, expansive conceptions of epistemology include social, material and other contextual factors in the realm of knowledge. Here, in understanding scientific justification, we ask after the scientists' goals, material and social circumstance, and so forth. By narrow conceptions, if we're asking after the epistemic success of a hypothesis, we simply consider its relationship to its evidence; by broad conceptions, such questions are answered very differently: we ask how the investigative practices at hand are productive in that prevailing 'epistemic situation' (I'll cash this out in section 6).

A 'thin' conception of speculation, then, defines speculation in terms of narrow epistemic properties. Popper provides an influential example.

Popper argues that scientists should generate *bold hypotheses* (Popper 1963/2014). A hypothesis' boldness turns on two features. First, how falsifiable it is. Hypotheses vary regarding the number of possible circumstances by which observations could be made that would clash with their predictions. A hypothesis with more such possible observations is more falsifiable than one with less. For instance, the hypothesis that planets in our solar system follow the inverse square rule is less falsifiable than the hypothesis that all heavenly bodies follow it. By the former, observations of bodies in our solar system are a potential source of falsifying observations; by

the latter, observations of any heavenly body are a potential source. Second, a bold hypothesis goes against current background knowledge. Claiming that, say, a small number of heavenly bodies do not obey the inverse square rule is less bold than introducing a theory proposing a different set of dynamics altogether.

So, for Popper, bold hypotheses (1) have many possible falsifying instances and (2) depart from received wisdom. His account is thin insofar as what makes the hypothesis speculative turns on its semantic properties—which circumstances would falsify it—and on its relationship with surrounding knowledge. In judging whether a hypothesis is speculative, I only have to ask after a narrow range of epistemic properties; historical, material or social context are irrelevant.

In more recent philosophy of science we have two accounts on the market; both depart from Popper dramatically but are nonetheless thin. First, let's consider Peter Achinstein (2018). At base, he understands speculation as the practice of "introducing assumptions without knowing that there is evidence for those assumptions" (Ibid, 1). Achinstein defines speculation in contrast with idealizing assumptions or abstractions. In the latter cases, scientists introduce assumptions (say, that a population is infinite in a population genetics model) without thinking for a second that the hypothesis is true; in speculation, scientists think the assumption might be, or could be true. Idealizations are *truth-irrelevant*, while speculations are not[2].

Here's Achinstein's account of speculation:

> If P introduces *h* in a way that satisfies the "theorizing" conditions (a) and (b), we might say that
>
> > (Spec): *h* is a (truth-relevant) speculation for P if and only if P does not know that there is evidence that *h*. (Ibid, 20)

[2] Achinstein does call idealizations speculations, but *truth-irrelevant* speculations. For ease of discussion I've departed from him in this regard.

Let's unpack[3]. Achinstein's theorizing conditions are that the hypothesis is taken as a candidate for truth (that is, it isn't a truth-irrelevant idealization) and that it is introduced in the process of "explaining, unifying, predicting or calculating" (6). A hypothesis generated under such conditions is speculative for some agent, P, just when they are unsure of the evidential status of the hypothesis.

With his narrowly epistemic gloss on speculation in place, Achinstein turns to a pragmatic account of when to speculate. He argues there are no general rules of how and when one should speculate, but the goodness or otherwise of a speculation turns on context: "there are legitimate ways to evaluate a speculation—as a speculation—independent of standards of evidence. And different perspectives are possible for such an evaluation: ones pertaining to the speculator, with [their] knowledge and interests; and ones pertaining to an evaluator with different knowledge and interests" (Ibid, 62-63). Little guidance is given on how such evaluations ought to be made, and Achinstein shifts from discussion of speculation within science to philosophical speculation about science—beyond our scope here. As we'll see, once we've a better grasp on the epistemic function of speculation, we can say something more concrete regarding the goodness or otherwise of speculation.

In earlier work, I've argued that a subset of speculation—*productive* or *empirically grounded* speculation—is crucial for understanding the practice and success of historical sciences such as paleontology or archaeology (Currie 2008, 287-289; 2019b; Currie & Sterelny 2017). I defined speculation in terms of what a 'rational conservative epistemic agent' would commit to. Such an agent's credence in a hypothesis matches what is minimally demanded from the available evidence. So, a hypothesis is speculative insofar as one's interest in pursuing it goes beyond the conservative epistemic agent's credence.

[3] Later, Achinstein further develops this account in accordance with his theory of evidence, but this simpler account is suitable for our purposes.

I further distinguished between 'productive' or 'empirically-grounded' speculation which is contrasted with *idle* speculation:

> … speculation is a vice—is idle—when it is pointless: when it cannot or does not productively direct further inquiry; when it is not used to construct alternative scenarios to guide a search for evidence which would favour one at the expense of the other. (Currie & Sterelny 2017, 16)

So, I provided both a thin definition of speculation and an account of when speculation is justified. A hypothesis' speculativeness is due to our pursuit of it outrunning our credence in it; it is justified based on its pursuitworthiness. As with Achinstein, however, little grasp is gained on just what makes a speculation good or bad other than an appeal to somewhat amorphous 'pragmatic' factors: my aim in this paper is to do better.

So, here's a common feature of accounts of speculation: speculation outruns our evidence, and is not justified based on our credence in the hypothesis per se, but in its expected fruits. In both Achinstein and my previous work we see a thin account of speculation followed by an appeal to pragmatic considerations to distinguish between legitimate and illegitimate instances of speculation. Thin accounts are *evidence-first*: the domain of speculative hypotheses are delineated by whether they are well-supported, we then ask whether they are good or bad speculations based on pragmatic concerns.

Achinstein and my past self differ. For instance, Acheinstein provides an agent-relative notion, while I depended on an abstract, idealized knower. Acheinstein is focused on a hypothesis' evidential support (or, rather, an agent's ignorance of that support), while I compared evidential support with pursuit. Regardless, both reach for similar ideas. As opposed to Popper, both capture speculation in inductive terms; a hypothesis is speculative insofar as it is, let's say, truth-directed (we to some extent care about the truth or otherwise of the hypothesis,

compared at least with idealizations) and it outruns our evidence. But they're united with Popper insofar as their account of what it is to speculate is thin. In telling whether a hypothesis is speculative, or how speculative it is, we need only attend to its narrow epistemic properties: whether the agent has evidence for the hypothesis in hand (Achinstein); whether our pursuit of the hypothesis outruns a conservative agent's credence (Currie); how falsifiable the hypothesis is and the extent to which it departs from accepted knowledge (Popper).

Rather than quibbling over differences between thin accounts, I'll now put some pressure on them by way of motivating my proposal.

3. Against Thin Accounts

Against thin accounts I'll sketch three related arguments: first, as thin accounts lean on narrow conceptions of the epistemic, insofar as narrow conceptions fail, so too do thin accounts; second, these accounts provide little guidance as to how we should identify or understand speculative statements; third, thin accounts do not capture the role of speculation in scientific discourse. As we'll see, although this puts pressure on such accounts, it wouldn't put off a determined defender. We'll see why speculation is better understood function-first than evidence-first in section 5.

Narrow accounts of the epistemic tie its domain to matters of propositional belief or the semantic content of hypotheses, and their relationship with evidence as understood according to some schema. This allows analyses of knowledge abstracted from their conditions of existence and generation. But there's good reason to think that knowledge cannot be understood so abstracted. Here's not the place to argue for this in detail, but we can nonetheless point to several strategies philosophers have developed towards that conclusion.

One approach argues that narrow conceptions, particularly their reliance on schema, cannot capture the nature of scientific justification. John Norton develops this most explicitly: for him, induction is not justified based on, say, how it fits within the mechanics of Bayesianism, or any other narrow conception. Instead, it is the material facts on the ground that underwrite justification (Norton 2003, 2014, manuscript). As Norton puts it, "Particular facts in each domain license the inductive inferences admissible in that domain—hence the slogan: "All induction is local" (Norton 2003, 648).

On a material view, we're not justified in, say, inferring from a fossil to an extinct critter because that inference follows abstract reasoning rules; rather, we're justified because of our rich background knowledge about processes of fossilization. To use Alison Wylie's metaphor, to understand scientific reasoning we cannot simply consider a hypothesis 'on-stage', regarding only the explicit evidence it has garnered, but also at the sets of practices and background knowledge occurring 'off-stage':

> Much of the action is off-stage. It is at least as crucial to establish the security and relevance of a robust body of background knowledge-on the source side of the equation-as it is to work in the foreground, recovering and recording the material record that survives of an archaeological subject. (Wylie 2011, 389)

The off-stage features of the material basis of induction includes properties beyond narrow conceptions of epistemology: practices of data generation, storage and retrieval, the social and institutional structures underwriting those practices, and so on.

Here's how our determined defender of thin views can respond. No doubt, narrow conceptions of the epistemic are an abstraction, but pointing this out is no objection. As in science, philosophical abstractions serve to isolate relevant features for explication and understanding; and it is this isolation that facilitates successful philosophical analysis. However, I

think misses the lesson we should take from such objections. Evidence-schema often abstract us from the explanatory action in scientific justification. A philosophical explanation of scientific reasoning, the thought goes, must incorporate what goes on off-stage. By missing this, narrow conceptions of epistemology fail to provide philosophical explanations (or at least their explanatory power is limited).

Another route to undermining narrow conceptions of epistemology in scientific contexts is to deny that scientific reasoning can be reasonably decoupled from what are often considered extra-epistemic factors, various values in particular (Douglas 2000, Steel 2010, Brown 2013). Because decision making within science—what standards of evidence to accept, how to design and regulate data generation and storage, how to carve up our object of study—make a difference to the final result, insofar as those final results have consequences beyond the lab, they cannot be insulated from those 'extra-epistemic' factors. The strategy of claiming that scientists *qua* scientists only report their findings—laying out the probabilities, say—leaving others to decide what to do or believe on that basis, fails because there are a variety of ways of generating findings, and which ought to be adopted often cannot be answered with the toolset provisioned by epistemology narrowly understood.

So, we should reject narrow epistemology as it fails to illuminate how knowledge is generated and cannot be insulated from broader practices. Insofar as thin notions of speculation rely on narrow conceptions of epistemology, they also should be rejected. However, even if we find narrow epistemology defensible, there are further challenges to thin accounts.

Most obviously on Achinstein's account and my previous approach, by thin notions of speculation a hypothesis is a speculation depending on its evidential support. To put the point briefly, at base both accounts equate speculation with inductions made in particular contexts. But when are scientists not making inductions? The most apparently secure induction counts as speculative, as does the wildest, boldest of hypotheses. There is nothing wrong with this *per se*;

all thin accounts allow us to say that some inductions are more speculative than others: we can ask how many possible falsifying observations there are, or how far our pursuit outruns our reasons-for-believing, or how ignorant we are of a hypothesis' evidence. Thus, we can switch from worrying about what a speculation is to the perhaps more useful question of under what conditions more speculative hypotheses are warranted. However, there are two problems here: the first concerns providing guidance on what to do with scientific results, the second concerns the role speculation plays in science.

The vast majority of scientific results involve some measure of uncertainty; induction is rife. As such, on thin accounts most scientific results are speculative *to some extent*. Insofar as we are supposed to take scientific results seriously, that is, use them as the basis for further scientific work and guide policies across social domains, highlighting that some hypotheses have more inductive risk than others provides little guidance. It encourages us to consider hypotheses in terms of their evidential standing, and although this is critically important in some contexts, it also serves to obscure the variety of epistemic purposes to which hypotheses are put. As we'll see below, some objections to speculation follow directly from this: because speculation involves large inductive risk, and science is supposed to provide a firm empirical grounding for policy decisions, speculation should be avoided. I think a function-first account better places us to understand such objections to speculation.

As I'll expand on and illustrate in the next section, speculation-attributions play at least two roles, the first typically said of another scientist's work, the second typically said of one's own work. A hypothesis' being speculative can be *derogatory*: something can be dismissed as 'mere' speculation, or considered dangerously unscientific. Speculations are not to be countenanced; they denote a lack of scientific seriousness. A scientist who speculates has failed to properly negotiate the tension between epistemic productivity and security. But a hypothesis can also be called speculative in a *justificatory* vein. Here, the scientist is signaling the epistemic function of

the hypothesis. It is not being forwarded to be judged on its plausibility (or at least not entirely), but on something like its fruitfulness. Thin accounts of speculation have nothing directly to say about this.

Our determined defender might provide a similar reply to this objection as the first: thin accounts provide the groundwork for us to then examine the function of speculation in science. This is why Achinstein takes a pragmatic turn in the justification of speculation: whether a hypothesis is speculative is one thing—and thin accounts tell us this—but whether they are justified is another question, and for this we turn to pragmatic context. I think this response is fine as far as it goes, but suspect it does not go far; why should become clear as I turn to discuss the epistemic function of speculation in the next section.

Again, this discussion is not intended to provide knock-down objections: a determined defender of a thin account can likely accommodate them, and indeed I've sketched how they might go about this. As we'll see, however, fundamentally thin accounts are undesirable because they imply that what makes speculations speculations is their evidential support; but examination of how scientists discuss and use speculations suggests that epistemic function matters more than evidence (and, as I'll show, a function-first account can accommodate the importance of evidence). Let's turn to that now.

4. Derogatory & Justificatory Speculation

Labelling your work 'speculation' tells your reader something: the ideas are not to be judged on their evidential support *per se*; a speculative hypothesis might be true, but its truth or otherwise is not the point. This is what I'll call *justificatory* speculation appeals. As we'll see, justificatory appeals point to a multitude of epistemic functions: opening new areas of debate, laying groundwork for testing a hypothesis, linking together our knowledge, and guiding theory-development. By contrast, in *derogatory* contexts, being speculative often signals that the

hypothesis breaks some standard or rule of good scientific practice or presentation. My positive account, developed in section 5, will both unify these aspects and take us some way towards critiquing them.

4.1 Speculation is bad

Speculation is bad; unscientific and unserious—so imply derogatory appraisals of scientific work. Let's consider some examples.

One common derogatory use of speculation contrasts it with proper science: this often takes the form of asking whether some investigations are speculation or science. A quick search reveals a bunch of examples. In medicine ("Dravet syndrome and vaccination: when science prevails over speculation" Wiznitzer 2010), cosmology ("Cosmology: Science or Speculation?" Weidmann 1982), evolutionary biology ("Engulfed by speculation" Poole & Penny 2007), economic approaches to environmental science ("Valuing individual animals through tourism: Science or speculation?" Caitlin et al 2013) and, of course, various debacles surrounding string theory ("Theory in particle physics: Theological speculation versus practical knowledge", Richter 2006). In such cases speculation is set against science; speculators somehow break science's rules. Sticking with particle physics for a moment, consider the following:

> To me, some of what passes for the most advanced theory in particle physics these days is not really science. When I found myself on a panel recently with three distinguished theorists, I could not resist the opportunity to discuss what I see as major problems in the philosophy behind theory, which seems to have gone off into a kind of metaphysical wonderland. Simply put, much of what currently passes as the most advanced theory looks to be more theological speculation, the development of models with no testable consequences, than it is the development of practical knowledge, the development of

> models with testable and falsifiable consequences (Karl Popper's definition of science). (Richter 2006, 8).

Richter makes a distinction between 'practical' knowledge, the sort that can be put to work, especially in the development of new experimental tests, and 'speculative' knowledge which does not lend itself to such work. He identifies theoretical developments in string theory as falling into the latter, thus being unscientific. Similarly, Ellis and Silk worry that "theoretical physics risks becoming a no-man's-land between mathematics, physics and philosophy that does not truly meet the requirements of any" (2014, 321; see Richard Dawid's work for sophisticated responses to this kind of claim, 2006, 2013). In evolutionary biology, some of Gould & Lewontin's objections to adaptationism also take this kind of tone: adaptationists construct 'just-so' stories which provide a kind of 'how possibly' explanation for how some morphology could evolve. However, such stories, they argue, are not amenable to testability:

> First, the rejection of one adaptive story usually leads to its replacement by another, rather than to a suspicion that a different kind of explanation might be required… Secondly, the criteria for acceptance of a story are so loose that many pass without proper confirmation. (153-154)

One feature of egregious speculation, then, is a lack of testability or, more generally, failing to meet the standards of the discipline at hand. A similar way of contrasting speculation and science, seen particularly in mid-20th-Century discussions in archaeology and anthropology, is the idea that a science develops from a speculative stage to a proper scientific one; a speculative 'science' is at best immature. To take one example, consider Schulyer's 1970 discussion.

> If American archaeology, as a sub-branch of anthropology, is similar to other sciences, then [its development] will in general reduce to a triad consisting of a speculative period, followed

by a descriptive period, followed in turn by a "scientific" explanatory period. (Schulyer 1970, 383).

On such a conception an area of study begins unsystematized and uncareful: speculative. A reaction against this leads to a period of careful data-gathering which, finally, culminates in that data being put to scientific use in explanation. On this view speculation does have a kind of role in science, but it is merely preparatory, immature, something to be abandoned downstream[4]. This echoes the distinction between the contexts of discovery and justification we touched on in the introduction: scientists must first generate ideas and data before the proper epistemic business of linking them.

Another use of derogatory speculation concerns science's role in society more generally: scientific statements are supposed to be taken seriously, believed by the public and form the basis of policy. We can find clear examples in the much-discussed link (or lack thereof) between autism and vaccinations. Vaccines do at times have adverse side effects or results, and it is important to understand these. However, as Wiznitzer 2010 has complained, "Public concern over [adverse effects of vaccinations], based on scientific evidence or on conjecture heightened by some physicians' comments and media actions, has at times resulted in opposition to vaccination and a resurgence of natural infections" (abstract). The thought goes that scientists have, or should have, an epistemically privileged role in society. Taking speculation seriously might lead to either non-scientists taking the results too seriously, or the degradation of public trust in science.

A further derogatory use of speculation worries about our capacity to speculate fruitfully, given that speculation is often driven by, and reinforces, pre-existing ideas. Consider Derek Turner's recent discussion of a display in the Smithsonian of a *Tyrannosaurs rex* locked in combat

[4] This idea has echoes in more recent consideration of creativity and the generation of ideas, which are often explored using computational tools.

with a *Triceratops* on the basis of a highly speculative hypothesis that had been presented in conferences but not published in a peer-reviewed venue:

> the Smithsonian's new exhibit is showing us something that's all too familiar: There is a long history of representing T. rex in one-on-one combat with Triceratops, in paleoart and museum exhibits. However, there is no evidence that such combat ever occurred… Rather than challenging assumptions, it's giving us a speculative picture that reflects our own biases—we like to watch one-on-one combat (from lightsaber fights, to western duels, to dinosaur fights). (Turner 2019, 3-4)

Here, the speculative hypothesis fails to challenge, but instead unjustifiably reinforces, pre-existing ideas about the subject at hand. This worry is also present in Gould & Lewontin's arguments against adaptationism in evolutionary biology. Part of their concern is the tendency for adaptationists to *assume* adaptationist explanations—that explanatory framework—in investigating biological form. After providing a cascade of non-biological cases meant by analogy to illustrate the error, they ask: "if these had been biological systems, would we not, by force of habit, have regarded the epiphenomenal adaptation as primary and tried to build the whole structural system from it?" (Gould & Lewontin, 150).

The worry that speculation typically reinforces received wisdom underwrites ongoing—and long-standing—debates surrounding method in archaeology (see Chapman & Wylie 2016, Bell 2014, Gero 2007). Archaeologists are keenly aware of the fragmented nature of the material record, and the difficulty of inferring from that basis to their ultimate target: the cultural ways of past humans. As Alison Wylie has characterized it, this has led to various 'crisis debates' where archaeologists oscillate between a conservative, hard-nose empiricism and a much more speculative approach (Wylie 1985). Concerning the former, she says:

> If they are committed to epistemically responsible (scientific) practice, [archaeologists] must confine themselves to the pursuit of narrowly descriptive goals. A variant on this theme is the recurrent claim that archaeologists qua archaeologists should be primarily concerned with documenting the contents of the archaeological record as completely and systematically as they can, deferring any more expansive goals to later stages of enquiry. (Wylie 2011, 377)

This ramshackle dash through derogatory appeals to speculation reveals some patterns. We can see three related threads of argument:

(1) Speculation doesn't meet scientific standards;

(2) Speculation is publicly dangerous;

(3) Speculation can only reinforce pre-existing ideas.

Let's turn to justificatory appeals.

4.2 Speculation is good

Scientists sometimes label their own hypotheses as 'speculative'. Like in the derogatory case, a quick search gives us a bunch of examples: from paleogeography ("Contrasting morphological trends of islands in Central Philippines: Speculation on their origin", Graciano et al 2008), to plant science ("Speculation: Polyamines are important in abiotic stress signaling", Pál, Szalai & Janda 2015), to astrophysics ("Some Speculations About Black Hole Entropy in String Theory", Susskind 1998) and cosmology ("Before the big bang: an outrageous new perspective and its implications for particle physics", Penrose 2006).

The role of such attributions is to guide the reader in identifying the value and purpose of the work. Graciano et al, for instance, integrate varying evidence regarding biodiversity and

geographical patterns across the Philippines to generate a model of dispersal across the islands, explicitly linking this to further studies:

> A more systematic paleomagnetic study of this part of the Philippine island arc system will determine whether the model presented here is feasible or not. (635)

Here, a justificatory attribution of 'speculation' signals that the work is not to be considered valuable in terms of evidential support alone, but to be considered in terms of their fruitfulness.

Speculation is often considered important, of course, for the generation of ideas: without some hypotheses on the table, where might our investigations lead[5]? Paul Feyarabend's 'epistemic anarchism' is often taken as an example of taking this position to an unhelpful extreme (see, for instance, Bunge 1983, Godfrey-Smith 2003, 116, Achinstein 2018). The basic complaint is that Feyarabend's view doesn't provide any constraints on speculation, reinforced by his mantra that 'anything goes'. Such readings of Feyarebend attend more to his rhetoric than his actual views, which do examine the pragmatics and purpose of speculation (although Feyarabend exegesis is beyond our scope here, Kidd 2011). Let's turn to other appeals to the value of speculation.

One example of an appeal to speculation's role in driving science is Simon Fitzpatrick's arguments against Morgan's Canon in comparative psychology (2008, 2017). Approximately, Morgan's Canon tells us which psychological explanations to prefer for complex animal behaviors, particularly those demonstrated in experimental contexts. The worry is that we might over-attribute sophisticated or anthropomorphic capacities to animals due to projecting our own fancy cognition onto them. To counter this bias, the Canon tells us to consider the simplest (Dacey 2016), least functionally sophisticated (Fitzpatrick 2007), or phylogenetically ancestral (Sober 2009, Currie forthcoming) capacities. Fitzpatrick's worry is that this conservatism blocks

[5] It is worth noting in passing that the 'new experimentalists' of the 1980s might respond that experimental traditions can be productive irrespective of theorizing (Franklin 1989).

productive speculation. It is in exploring alternative hypotheses, which sometimes go far beyond our evidence, that surprising new discoveries might be generated.

> In any instances unless one is prepared to speculate far beyond the current data, to seriously entertain hypotheses involving more sophisticated cognitive processes when a less sophisticated one seems sufficient, it is unlikely that one will ever uncover such subtle and surprising capacities… (238).

This idea is writ large in Kyle Stanford's argument from unconceived alternatives (Stanford 2006, 2019). By his lights, the success of scientific theories can only be taken as reason for their truth if we've a good survey of the other options in hand. But, he argues, we typically don't. Scientists do not spend much time considering radical alternatives; they rather work within the constraints of the prevailing theory—and indeed the social structure of science further encourages this due to professionalization, the narrowing effects of peer-review, and so on. Fitzpatrick and Stanford, then, associate speculation with the generation of new, challenging ideas—something which is necessary both for surprising discoveries and for providing a contrast with which to judge the success of accepted theories.

Justificatory speculation is also taken seriously in the application of machine learning algorithms and similar techniques to scientific corpuses. A large set of texts can be explored for particular phrases, the aim in this context being identifying speculative hypotheses which might themselves be the basis of further tests, or for further linking bodies of knowledge. For instance, the increasing digitization of hospital records have the potential to underwrite more systematic studies, but "physicians need to have efficient tools to access this information" (Cruz Diaz et al 2012, 1398). Identifying speculative hypotheses in both those records and in the vast literature emerging from them, then, would be a great boon. Corpus searches typically identify speculation via 'hedging' statements (Argwal & Yu 2010), which include terms like 'might', or 'could be', or

'seems to be'. Malhotra et al (2013) apply these techniques to Alzheimer's research, here's one of their illustrative examples:

> *"Early cognitive deficit characteristic of early Alzheimer's disease seems to be produced by the soluble forms of beta-Amyloid protein."*

As they say, "in case of complex and mostly idiopathic diseases like [Alzheimer's] where the etiology of the disease is still unclear, neuroscientists are frequently introducing working hypotheses or speculations" (1). On the one hand, this recognises the link between speculation and inductive risk—it is in the face of a lack of information that speculation occurs. But critically the function of the speculation is clear: they recognise speculation as a way of generating new work; in this case identifying plausible directions for further understanding Alzheimer's.

How do speculative practices generate new ideas? One sophisticated discussion of how speculation generates ideas, perhaps surprisingly, arises from analysis of Isaac Newton's use of hypotheses.

Newton is often associated with a rather extreme view on speculation: that it is outside the realm of science and shouldn't be done (e.g., Achinstein 2018). Kirsten Walsh argues that hypotheses play various instrumental roles for Newton: they're not the goal of enquiry, but play critically important roles in developing theories. She suggests three roles for hypotheses, two explicitly in Newton's own reflections and a tacit third (Walsh 2019).

The first role for hypotheses involves explication: a metaphysical hypothesis can serve to explain a theory by concretizing it, also providing a kind of 'possibility proof'. Here, we needn't commit to the truth of the hypothesis, it rather shows that the abstract theory could in fact be concretely instantiated. This has two functions: aiding understanding and intelligibility, and responding to worries that the theory is physically impossible or fails to cohere with background knowledge. This latter function is similar to adaptationist responses to the posited 'irreducible

complexity' of some biological machinery. Such arguments go: there is no incremental way of building some trait, and so the trait cannot have evolved (and thus, *sotto voce,* must have been designed). The adaptationist responds by generating one such evolutionary story. In discharging the challenge they need not commit to the story being true, simply possible (Draper 2002).

The second two uses of hypotheses Walsh describes in terms of 'scaffolding': "hypotheses provide a temporary platform from which further experimental work and/or theorising can be carried out" (126). To continue the metaphor, hypotheses give us somewhere to stand whilst developing theories, concepts and experiments[6].

The second use of hypotheses focuses on the development of experiments: "Here one draws empirical consequences from the hypothesis… and uses these to make predictions which can be tested" (Walsh 132, references removed). That is, we ask what observational consequences there would be if some hypothesis were true, and use this to develop experiments which capture those observations. Irrelevant of whether the experiments vindicate or undermine the hypothesis, they regardless provide a way of further probing various properties of the system of interest. Here, hypotheses scaffold the development of experiments. Walsh's final use is more explicitly for theorizing.

Speculation can provide "scaffolding for the purpose of theory-building" (134). The basic idea here is that the development of a hypothesis can be a first step in formalizing a theory, the speculative aspects being later abstracted from the final product. As drawing the empirical consequences of a hypothesis can aid in developing experiments, so too can following the theoretical consequences of a hypothesis aid in developing theory. Walsh illustrates this through a discussion of how Newton developed his theory of intervals to explain various experimentally generated light phenomena—a topic beyond us here.

[6] For other instances of scaffolding metaphors applied to science see Wimsatt 2014, Currie 2015.

We've thus far toured defences of speculation where the epistemic function of the speculation is not to provide evidential support—they don't give us reason to believe, or at least believing in the hypothesis isn't the central point—but rather are routes to better experiments, hypotheses, and so on. It isn't the hypothesis' evidential support that matters, but how it helps us develop better science downstream. However, the generation of ideas is not the only function of speculation. Let's briefly return to Malhotra et al's corpus-searching approach to identifying speculation. Considering the benefits of their techniques, they say:

> …[speculation-identifying techniques] could be used to understand initial speculated mechanisms or modes of action that led to the success or failure of drugs. Systematic collection of hypotheses allows for rationalization of discussions about possible interpretations of data. Since speculations represent the gray zone of scientific knowledge, they can provide incremental support to the main hypothesis underlying the research. Conversely, if the speculations are contradictory then they could shift the direction of the research towards new and rewarding avenues. (9)

Malhotra et al don't only consider speculation in terms of generating further tests; speculations are opportunities to link together our bodies of knowledge, thus providing incremental support for hypotheses. This more confirmatory role for speculation has not been much discussed. We see it most explicitly in recent defences of 'story-telling' in historical reconstruction. A major source of worry for the production of narratives is that they are merely constrained by coherence, and coherence is of limited value. Here's Gould and Lewontin on this point:

> Often, evolutionists use consistency with natural selection as the sole criterion and consider their work done when they concoct a plausible story. But plausible stories can always be told. (Gould & Lewontin 1979, 588)

Responding to this species of worry, Sterelny and I (2017) have argued that coherence is significantly more difficult to be had, and has stronger epistemic benefits, than often thought. This consists in two claims. First, the generation of narratives is often highly constrained by surrounding knowledge: "The rich and varied evidential streams historical scientists exploit constrain the space of plausibility. Generating good narratives under such circumstances is a significant epistemic achievement" (Ibid, 19). Second, hypothesized links between past events are equally conjectural as those between current traces in the past, and a similar epistemic story can be told about both:

> … typically, when people consider the evidence underlying reconstruction, they think of it as a causal web linking some past event to the present. Processes of mineralization and taphonomy link a fossilized bone, a current trace, to an extinct animal; and our understanding of fossil formation, site transformation, and biology gives us some understanding of that animal. But these causal webs also link events in the past to one another. (Currie & Sterelny 2017, 19).

So, in addition to generating new ideas, speculation can serve an evidential function. New hypotheses, even with minimal support, can be knit into our existing knowledge in epistemically beneficial ways. Let's provide a simplified paleontological example. Say we're interested in an apparent extinction event as evidenced by the fossil record. We've fairly good reason to think that the taxa in question's disappearance from the record is a biological rather than geological signal, that is, is a trace of the lineage's extinction. Now imagine that we've evidence of some kind of ecological change at approximately the same time and place; let's say that we've shifted from forests to grasslands. An initial speculative hypothesis might be that that the lineage was unable to adapt to, or was less competitive in, that new environment, and this played a role in their extinction. This speculation links our knowledge of the lineage's extinction to our knowledge of the ecological change. And more than this, it links our knowledge of the lineage's

properties—what kinds of environments they likely thrived in, for instance—with our knowledge of how ecological changes, the spread of grasses, for instance, affect biotic turnover. It might turn out that the lineage in question is particularly sensitive to spreading grasslands according to our ecological theories (being a browser rather than a grazer, say). Potentially, by linking these two bodies of knowledge, the speculative hypothesis puts us in a stronger epistemic position—this is in addition to the benefits of suggesting further empirical studies.

So, we can identify a set of epistemic functions associated with speculation, broadly falling into two camps:

(1) Speculation opens new areas of research, can be a scaffold for further experiments or theories, pursuing them can generate surprise, and so forth.
(2) Speculation can knit together existing bodies of knowledge in ways which can increase evidential power.

It is critical to note that across these various uses, the evidential standing of the hypothesis matters for the success of some functions but not others. If the point of a hypothesis is simply to open a new area of research, or provide explication or a 'possibility proof', then the evidential standing of the hypothesis is a moot point. However, if the point of the hypothesis is to, say, develop a better method of testing that hypothesis in particular, or if the point is to conjoin existing knowledge, then its evidential standing does matter. As we'll see, that evidential support matters for the function of some speculation and not others is critical for my argument in favor of a function-first account.

5. An Account of Speculation

Thin accounts of speculation emphasize their epistemic status, that is, their falsifiability, the relevant agent's ignorance of their confirmation, or their outrunning of current evidence. But

examining scientific claims about speculation, we've come to consider their epistemic function. Derogatory appeals to speculation do not simply complain that speculative hypotheses lack epistemic support, but that they do so in an egregious way: are untestable, or could lead to miscommunication, or can only reinforce pre-existing ideas. By contrast, justificatory appeals to speculation signal a set of functions beyond whether the hypothesis is well-supported.

To get to a positive account, let's set up a contrast: *non-speculative* hypotheses. A non-speculative hypothesis is one that ought to be judged in terms of its evidential support, perhaps analogous to an assertion. By contrast, speculative hypotheses are not to be judged in terms of evidential support, but what I'll call their 'productivity'. Productivity includes, but is not limited to, fruitfulness as understood as generating new ideas. It also includes epistemic goods in the form of linking together other aspects of knowledge as we saw in the last section. So:

> A hypothesis is *speculative* when it aims to be 'productive': its function is to provision epistemic goods through opening new research, or scaffolding the development of theories or experiments, or generating possibility proofs, or providing epistemic links to further knowledge.

'Productivity', in a sense, acts as a kind of catch-all for epistemic benefits not directly related to the evidential standing of the hypothesis, hence its disjunctive (and potentially incomplete!) form. The definition says nothing about the epistemic standing or support of a hypothesis: in principle, an extremely well-supported hypothesis might nonetheless be speculative insofar as that is its function in the relevant context. Against Achinstein's account, the agent might very well know how well the evidenced the hypothesis is, but use it speculatively regardless. Against Popper's, hypotheses can fulfil speculative functions irrelevant of how falsifiable they are. Against my previous account, a hypothesis need not significantly outrun our evidence to play a speculative role. Further, non-speculative and speculative hypotheses are not mutually-exclusive. As opposed to thin accounts, which allow us to say how speculative a hypothesis is, on my

account hypotheses can play both functions simultaneously. Drawing on our toy case from the end of the last section, a hypothesis that an extinct lineage was driven extinct by changing grasslands might both be judged on its evidential support and its productivity. Let's consider initial objections before exploring the consequences of the view.

You might balk at an account of speculation that ignores evidence. We've seen that scientists often complain about the evidential standing of 'speculative' hypotheses: do I really want to disjoin speculation and evidential support? Another option would be a conjunctive account. Say, a hypothesis is speculative just in case it (1) counts as thin speculation and (2) aims to be productive[7]. Such an account would capture both the thought that speculations are evidentially unsupported and that they are justified in ways beyond that support. One problem with such an account, as with thin accounts, is that it focuses us on the evidential support a hypothesis garners. But as we've seen, evidential support only matters for some speculative functions and not others. A function-first account can accommodate this feature. In speculative contexts, the evidential support of a hypothesis matters when, say, we're using it to link together bodies of knowledge. If that is the function, then evidential standing *is* something we're interested in. Thus, function-first accounts do not simply ignore evidence, they rather bring it in when relevant. There's no need to define speculation in terms of (a lack of) evidence for that feature to never come up in consideration of speculation.

You might further complain that speculative hypotheses are typically weakly evidentially supported, so why not adopt something like a conjunctive account to accommodate this? I've a few responses. First, as we've just seen, when the relevant function is in play, my account does attend to weak support. Second, it is no surprise that weakly evidentially supported hypotheses abound in speculation if such standing often comes along with fulfilling that function. A 'bold' hypothesis aiming to open up some new area of research is likely to be evidentially weak

[7] Many thanks to an anonymous referee for suggesting this.

precisely because it is reaching beyond our current knowledge. But to say it is speculative because it is evidentially unsupported gets cart and horse backwards. It is speculative because it tries to open up new areas of research, and because those areas haven't been researched, the hypothesis hasn't yet much evidence for or against it. Finally, demanding that speculative functions only attach to hypotheses of low-support leads us to miss how well-supported hypotheses can nonetheless be fruitful in various ways: they can play speculative functions too.

Another upshot of the function-first view is that a hypothesis which is known to be false could be nonetheless turn out to be legitimate if it fulfils a speculative function[8]. In practice I doubt that known-to-be-false hypotheses really do often fulfil such functions. In some cases this is straightforward. If a speculation is intended to show that a hypothesis could be consistent with background knowledge—providing a possibility-proof—then its very falseness involves it failing to cohere, thus failing in its function. In other contexts, explication or scaffolding for instance, things are less clear. On the one hand, there seems to me nothing wrong with using a false claim to help explain a theory, or towards developing a new theory, measurement or experimental intervention. But on the other hand, the known falseness of the hypothesis could undermine those functions: scientists might reasonably be resistant to the explication, for instance. Further, a hypothesis is often only 'known' to be false against the backdrop of commonly accepted background knowledge. If we take arguments like Stanford's at least somewhat seriously, then considering radical alternatives is critical, even if just for establishing that we have in fact the best story about the phenomena that interest us. Denying a role for hypotheses which are 'known' to be false, then, potentially blocks revolutionary science (Schneider forthcoming).

Overall, a function-first account of speculation leads us to consider the evidential standing of a hypothesis only when that is relevant to it fulfilling the specific functions intended, and this requires fine-grained examination of those functions and relevant contexts (I'll turn to this in the

[8] Thanks to an anonymous referee for pointing this out.

next section). Given this, it would be a mistake to deny outright that false hypotheses cannot play speculative functions—we shouldn't build this into our account of speculation—rather, the account allows for such roles to be played, but demands they be carefully defended within the relevant functional context.

But, you might still insist, doesn't the account simply fail to line up with scientific appeals to speculation? Sometimes, yes, but my account isn't descriptive—I'm only partly systematizing scientific appeals to speculation—it is prescriptive. I think function-first accounts of speculation give us better grip on when derogatory and justificatory appeals to speculation are justified. Let's turn to this now.

6. Judging Speculation

Recall derogatory attributions of speculation. These fell into three categories: first, speculation fails on some criteria of 'good science'; second, speculation undermines science's role in society; third, speculation can only reinforce pre-existing ideas. Although some of these uses of 'speculation' do not track function-first accounts, the approach enables us to both make sense of these complaints and identify when they are legitimate. Let's take a quick tour.

To argue that a hypothesis should be rejected because it is speculative *per* se is clearly a mistake; but we can nonetheless identify legitimate versions of such objections. A legitimate version of the first complaint might be that the hypothesis has been put forwards *in the wrong functional context*, that is, it is being presented or used as if it ought to be judged on its evidential basis (or by mischaracterizing its evidential basis) when in fact it should be understood in terms of productivity. That is, it is better viewed as impermanent scaffolding than one of the permanent building bricks of knowledge. Illegitimate versions of the complaint, at their most extreme, erroneously imply that productivity is not a scientifically relevant property; others mistakenly judge a hypothesis on its evidential support rather than its productivity.

The second complaint links science's role in society with provisioning well-evidenced claims which feed into policy guidance. This is no doubt extremely complex; how and to what extent scientific results can be transported into policy contexts is a vexed issue (e.g., John 2015, Cartwright 2007). It is a mistake to view scientific results as unquestionable datum declared from on-high, but so also is it a mistake to dismiss them. Generally, making room for productive speculation likely requires a combination of repositioning how science is treated in public—informed, critical engagement is required, as is clarity from those reporting the results—and in, in some way, insulating scientific speculation from being taken by non-scientists as functioning as non-speculative hypotheses rather than speculative hypotheses (Currie 2019c).

The third species of worry takes speculative hypotheses at their proper function, but claims they are *failing* in that function. Instead of opening new indirect benefits, they illegitimately reinforce or reflect our pre-existing knowledge. Considering direct epistemic benefits from linking our pre-existing knowledge, telling legitimate from illegitimate uses is no doubt a subtle issue. I'll close by returning to justificatory appeals to speculation to make some further claims about speculation.

At base, judging the legitimacy of speculation requires setting it against a set of rich contextual factors.

First, judging a speculation requires attending to the specifics of its epistemic function. Speculation may be guided towards developing new experimental studies, conceptual machinery (operationalized concepts, theories) or new ideas and hypotheses; they can serve to highlight and link existing bodies of knowledge; they can explicate theories and provide possibility proofs. These are varied, and a speculation's appropriateness depends on which are at play: a speculation aiming to generate a new theory would be quite different to one attempting to explicate an existing one. Asking just what a speculation is attempting to do is required to tell whether it is well-crafted for that purpose—and recall that hypotheses often play multiple roles,

both within the categories of speculative functions and across speculation and more traditionally understood confirmatory roles.

Second, the admissibility of a speculation will turn in part on the 'epistemic situation' at hand (see Leonelli 2016, Currie 2018). We can characterize an *epistemic situation* in terms of the aims of an investigation, and the affordances and challenges in pursuing those aims. These are extremely diverse, to illustrate, let's consider two experimental situations. Synthetic organic chemists are often in the business of generating new, useful compounds. In doing so, they often exploit naturally occurring compounds with interesting properties (the antitumour properties of Yew trees, for instance). They seek to isolate the reactive aspects via complex laboratory techniques. That there are natural compounds and the capacities of their technological apparatus afford a rich epistemic strategy, and raises challenges related to identifying natural compounds, the expense of the apparatus, and the complexity of both the compounds and the techniques. By contrast, some particle physicists hunt for as-yet unobserved particles posited in otherwise well-confirmed physical theory. Because such posits can only be detected in artificial high-energy contexts, often vast particle accelerators are required, as well as sophisticated statistical techniques to infer whether the particles actually occurred in the collisions. Precise physical theories, in combination with experimental and statistical know-how afford the experimenters, while the intractability of the posits themselves and sheer expense raise challenges.

In different epistemic situations, scientists leverage different epistemic, technical, and socio-economic powers towards their goals. Scientists in different epistemic situations often adopt different standards for non-speculative hypotheses: sometimes statistical significance is required for publication, sometimes low-N results are tolerated, and so on. Similarly, different speculative strategies will be more appropriate in different situations.

Compared to non-speculative contexts, there are significantly fewer explicit, formalized standards for speculation. This is a shame. If speculation in its varied forms is so important for

science, then this should be reflected in how it is practiced, taught, and institutionally structured. If speculation might be misread or misused, we should develop ways of insulating it from such misreading. If speculation breaks accepted scientific standards, we should develop parallel standards. If speculation sometimes only reflects our preconceptions, we should develop strategies to combat this. Concrete proposals are far beyond my remit here, but my hunch is that many scientists already, more-or-less tacitly, have answers to many of these challenges. The trick is to identify them, and further identify features of their epistemic situations which make them appropriate and successful.

7. Conclusion

I began by putting pressure on 'thin' accounts which define speculation in terms of evidential support. Although a determined defender might well accommodate such objections, that discussion motivated a closer look at scientific appeals to speculation. We saw that scientists appeal to speculation in both derogatory and justificatory ways, and that (particularly in the latter) these appeal to the varieties of functions speculation might play, functions which are often decoupled from evidential support. I then introduced a function-first account of speculation, arguing that this better situates us to understand the various kinds of productivity speculative hypotheses generate. We further saw how evidential support sometimes does matter, and sometimes does not, for a speculation to fulfil its function well. This led to a quick sketch of how to determine the legitimacy of a piece of speculation: we should both attend to epistemic function and to epistemic situation.

Beyond the task of identifying just what a speculative hypothesis is, there is much work to do in analyzing speculative strategies within science and figuring out how to make scientific culture amenable to them. But adopting a functional account of speculation, as I've done here, well-positions us to that task.

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