Эпистемология и философия науки
2019. Т. 56. № 2. С. 77–94
УДК 101.2

Epistemology & Philosophy of Science
2019, vol. 56, no. 2, pp. 77–94
DOI: 10.5840/eps201956231

EPISTEMIC PROGRESS DESPITE SYSTEMATIC DISAGREEMENT

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A number of philosophers argue that because of its history of systematic disagreement, philosophy has made little to no epistemic progress – especially in comparison to the hard sciences. One argument for this conclusion contends that the best explanation for systematic disagreement in philosophy is that at least some, potentially all, philosophers are unreliable. Since we do not know who is reliable, we have reason to conclude that we ourselves are probably unreliable. Evidence of one's potential unreliability in a domain purportedly defeats any first-order support one has for any judgments in that domain. This paper defends philosophy. First, accepting that science is rightfully treated as the benchmark of epistemic progress, I contend that a proper conception of epistemic progress highlights that philosophy and science are relevantly similar in terms of such progress. Secondly, even granting that systematic disagreement is a mark of unreliability and that it does characterize philosophy, this paper further argues that evidence of unreliability is insufficient for meta-level, domain-wide, defeat of philosophical judgments more generally.

Keywords: disagreement, higher-order defeat, epistemic progress, metaphilosophy

ЭПИСТЕМИЧЕСКИЙ ПРОГРЕСС ВОПРЕКИ СИСТЕМАТИЧЕСКИМ РАЗНОГЛАСИЯМ

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Ряд философов утверждает, что из-за систематических разногласий прогресс в философии, особенно в сравнении с естественными науками, фактически отсутствует. Одним из следствий такой позиции является тезис о том, что некоторые – а возможно, и все – философы не заслуживают доверия. Поскольку мы не знаем, кому можно доверять, мы имеем основания полагать, что мы не можем доверять и себе. Свидетельство о чьей-либо потенциальной ненадежности якобы дезавуирует всякое обоснованное суждение. Автор этой статьи защищает философию. Во-первых, признавая, что наука по праву считается эталоном эпистемического прогресса, автор утверждает, что правильное понимание эпистемического прогресса указывает на то, что философия и наука относительно схожи. Во-вторых, автор утверждает, что, даже если допустить, что систематическое разногласие является признаком ненадежности и что оно действительно характеризует философию, свидетельства ненадежности недостаточно для метауровня – для дискредитации философских суждений в целом.

Ключевые слова: разногласие, обоснование высшего порядка, эпистемический прогресс, метафилософия

Introduction

There is much ado concerning the epistemic status of philosophical judgments and philosophy's epistemic progress in general. A troubling reality a propos theoretical inquiry, of which philosophy is a paradigm domain, is that consensus is difficult to come by, even amongst putative experts. This purported feature of philosophy has prompted a series of related theses. The first, which is notably moderate, suggests that due to a lack of convergence on answers to its biggest questions, philosophy has made little epistemic progress.[1] Building from this conclusion, some theorists contend that the lack of convergence in a domain is best explained by some, perhaps most or all, participants to that domain being epistemically unreliable.[2] Such evidence of general unreliability, as we have no means of adjudicating who is reliable, serves to defeat any first-order evidence one has supporting their philosophical views. It follows, then, that no philosophical judgments are justified. All philosophical judgments are subject to defeat.

We can characterize the above theses as progress pessimism and reliability pessimism respectively.[3] The former applies to the collective state of philosophy overall. Using convergence on correct responses to our biggest questions as the standard, philosophy has made little to no epistemic progress. The latter applies to the epistemic condition of individual philosophers, primarily based on disagreement-based, higher-order, defeat. I am suspicious of each of these pessimistic conclusions. My goal, then, is twofold. First, I want to pressure the collective convergence thesis in favour of philosophy's epistemic progress, specifically as compared to science, which is often offered as an epistemically progressive domain *par excellence*. Secondly, I argue that even if philosophy is epistemically inert due to a persistent lack of convergence, or systematic disagreement, it does not follow that such disagreement defeats all philosophical judgments. I consider each of these challenges in subsequent sections below.

I. Collective Convergence as Epistemic Progress?

To begin, let us formulate an argument consistent with the progress pessimist's contention that collective convergence is the mark of epistemic progress:

1 See especially [Chalmers, 2015].

2 See especially [Goldberg, 2009, 2013a, 2013b] and [Kornblith, 2010, 2013].

3 As discussed below, the reliability pessimist thesis is based on what [Beebee, 2018] refers to as *the disagreement challenge*.

PP

1. A domain D makes epistemic progress to the degree that it produces collective convergence to true or correct responses to its biggest questions.[4]
2. Philosophy has little to no collective convergence in response to its biggest questions.
3. Philosophy has made little to no epistemic progress.

Sympathizers of PP often appeal to the purported progress made in the hard sciences to support their thesis. David Chalmers, e.g., suggests that epistemic progress is measured by "collective convergence to the truth," and "the benchmark [used] is comparison to the hard sciences" [2015, p. 4]. Hillary Kornblith suggests likewise: "the field of philosophy in general…simply does not have anything like the epistemic standing of the empirical sciences" [2010, p. 44]; philosophy's problem is "not that [it] has failed to yield explanations or predictions of physical phenomena, let alone that it has failed to produce exciting technological innovations," because, of course, "philosophy does not aspire to such things." The problem, instead, is that despite philosophy's long history and brilliant practitioners, "we have made little if any progress in answering philosophical questions" [Kornblith, 2013, pp. 260–261]. By contrast, then, science is purported to have made progress in answering scientific questions in ways that philosophy has not been able to answer philosophical questions. A notable mark of this progress is the level of agreement amongst scientists not found amongst philosophers.[5] Scientific inquiry, then, is the paradigm and benchmark of collective epistemic progress.

Let us consider the conception of epistemic progress as collective convergence to truth in response to a domain's big questions. I suggest that this standard is stronger than progress pessimists intend, as it seems to deny that even the hard sciences make progress on questions where they clearly have. Consider two scientific research areas: life in evolutionary biology and gravity in theoretical physics. Biologists seek explanations for the diversity, complexity, and origins of life. Theoretical physicists seek to understand what gravity is, how it applies in different parts of the universe, and how we might be able to answer these questions. Let us suppose that the big questions for each field concern the ontology and explanations of their candidate topic. They ask: What is life/

[4] This premise echoes David Chalmers' [2015] measure of epistemic progress, discussed below.

[5] There are many points garnering suspicion here. That consensus indicates reliability has obvious counter examples. Consensus amongst Stalinist scientists under state influence should confer no increased epistemic confidence in the content of their agreement. Further, what degree of consensus is needed for reliability? And, it's unclear what *science* is? Addressing these concerns is beyond the purpose of this paper. These concerns and the argument to follow nevertheless seem to indicate a question-begging account of science as the ideal for epistemic progress.

gravity? What explains life/gravity? If the claim that a domain's epistemic progress is evidenced by converging judgments, or widely accepted theories, by the majority of practitioners in response to their fields biggest questions is accurate, then we might be concerned that evolutionary biology's and theoretical physics' epistemic progress are no better off than philosophy's.

In response to the question of what life is, "astrobiologists will tell you honestly that this question has no simple or generally accepted answer" [Tyson and Goldsmith, 2005, p. 235]. There is certainly convergence on evolution by natural selection and the role genetics play in life's evolution. Nevertheless, few (probably no) biologist would claim that there is a generally accepted answer on how life began on Earth. Indeed, there is virtual consensus that life had to have emerged in the first third of Earth's existence, pre-oxygen. However, even the time period at which life emerged has a hundreds of millions, possibly a billion, year margin of error. And this convergence says nothing of how life emerged here. Possibly life was transplanted from space dust traveling from other galaxies or solar-systems; perhaps life was sparked and diminished many time over due to cometary bombardment in Earth's early period; perhaps an electrical storm produced a chemical reaction sparking life. Nevertheless, there is no widely accepted answer to what we might think are some of biology's biggest questions. We might conclude with the progress pessimists that there has been no epistemic progress in this branch of biology.

Few (probably no) physicists would claim that there is an accepted answer to the question of gravity. There is certainly consensus over the success of Einstein's general theory of relativity (GTR). However, there is also consensus that GTR is presently incompatible with quantum mechanics, the accepted model of the subatomic. So the best theory of gravity we have to date, GTR, is inconsistent with our best theory of the subatomic – which some might argue is fundamental. A number of hypotheses seeking to account for quantum gravity that jibe with GTR are on offer.[6] None has gained anything close to consensus. The gravity question remains unanswered. We might conclude therefore that there has been no epistemic progress on this issue in this domain.

Of course, the above conclusions are absurd. To say that the genetic revolution has not progressed past basic Darwinian Theory, which was a monumental shift beyond teleological explanations of life, is simply false; to say that GTR has not progressed past Newtonian gravity, which itself progressed beyond Galilean gravity, is simply false. I suggest that the problem emerges when convergence is demanded of responses to a domain's *big* questions. We might think that the above examples of two

[6] See [Joshi, 2009], [Moffat, 2008], [Randall, 2005], and [Smolin, 2001] for general accounts of this challenge.

of the biggest questions in two of the most established hard sciences are offside. First, because I am highlighting the empirical and theoretical limits scientists face in those two fields, and secondly, because the examples ignore the significant convergence on the models that have allowed scientists to reach these limitations. If indeed these examples are offside for these reasons, then we might wonder what is meant by *biggest questions* in the context of the hard sciences. I suspect that convergence on GTR and the genetic explanation for life's evolution qualify as the type of consensus that progress pessimists have in mind when appealing to science's success. If this suspicion is correct, then we might cash out convergence in terms of theory or model acceptance.[7] Notice also, however, that if this suspicion is correct, then it becomes unclear what to do with competing, equally plausible, paradigms, and furthermore, why we would conclude that philosophy has not made similar progress.

One way to address the above puzzle is to remove the *biggest questions* condition, thereby reformulating *epistemic progress*.[8] Under such a reformulation, epistemic progress is also demonstrated when we have a better understanding of the target concept, its application, where further investigation is needed, and what no longer works or is no longer needed for one's theory. This latter conception of progress may not produce convergence on a single theory in response to our big questions, even while there is convergence on these additional considerations; it seems like epistemic progress nevertheless. We can accept, then, that both widespread theory acceptance and a refined understanding of the relevant issues concerning a target concept or theory seem to signify epistemic progress and are not mutually exclusive. Acknowledging this point accounts for why we think there is scientific progress on the problems of life and gravity, despite the current lack of consensus on some of the major points in these domains. We can also note that issues in epistemology, e.g., have advanced in relevantly similar ways, despite the remaining open, admittedly large, questions. Some examples: most epistemologists accept that there are important internal and external components to knowledge; few, if any, epistemologists defend the coherence theory of truth, truth tracking as a theory of justification, or the contextualist account of knowledge. While many of the questions remain, questions as old as the debate, it is wrong to conclude that no progress has been made in this domain – just as it is wrong to conclude that no epistemic progress has been made concerning gravity or life's origins.

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7 This interpretation of convergence muddies the *truth* or *correctness* condition, but for the sake of argument we can assume a realist thesis concerning scientific models approximating truth.

8 Chalmers [2015: §3] acknowledges that under various conceptions of *epistemic progress*, philosophy indeed has progressed.

We might think that even if progress is made in more piecemeal ways in philosophy, that disagreement remains ubiquitous is a sign of epistemic inertia. I propose that this disagreement, if not overstated, is at least ignoring that the philosophical scrutiny that produces disagreement often occurs within an (or even many) underlying agreement(s). *A fortiori*, there is significant agreement at work in the dialectic to begin with. The disagreement is in the details. Here is an example: Political philosophers John Rawls and Robert Nozick have very different views on distributive justice, the role of government, and a citizen's civic responsibility.[9] They have also garnered a number of defenders for their respective systems who continue to debate these issues nearly 50 years on – we will refer to these two camps as the Socs and Narcs respectively. If we were to focus solely on the differences between the Socs and Narcs, we might conclude that their disagreement is systematic – indeed, they are continuing in the millennia old debates concerning justice and centuries old debates concerning the social contract. Noteworthy, however, is the significant agreement between these two camps. Each endorses the tenets of the liberal democratic tradition, each champion the individual as a free and rational agent amongst equals, and each seeks to minimize the role of government within these parameters. There is therefore significant agreement between the Socs and Narcs and at least some evidence that a degree of convergence has occurred amidst or prior to the disagreements.

One may contend that this is still a case of the type of persistent disagreement characteristic of epistemic inertia. The Socs-Narcs debate concerns justice, which is a debate as old as philosophy itself. So the big questions on justice remain unanswered. There have not been any monumental or ubiquitous adoptions of any one theory of justice. The progress pessimist may contend, then, that the Socs-Narcs example actually brings into sharper contrast philosophy's lack of progress. Nevertheless, this contention fails to appreciate the significant agreement. So, we have an example of a disagreement that rests on a broader agreement. At the very least, then, we can conclude that not all propositions relevant to a theoretical domain like philosophy diverge. The democratic framework accepted by the majority of political theorists today serves as an example.[10] Moreover, if agreement is a sign of progress, then we also have an example of progress. What is more, *justice* and *democracy* also provide analogues to *gravity*. Justice and democracy are concepts as old as

[9] See [Nozick, 1974] and [Rawls, 1971].

[10] Indeed, a number of theorists working within different, at times opposing, traditions adopt the liberal-democratic framework. In the pragmatic tradition see [Dewey, 1986], out of the Frankfurt School see [Habermas, 1992], out of the social contract tradition see [Nussbaum, 2006], and, on some readings – [Wellmer, 2000], e.g. – in the classical tradition [Arendt, 1965] to name only a few.

philosophy. However, the *justice* at issue in the above dialectic is contextualized and updated to the dynamics of the current debate. The salient considerations, points of emphasis, and theoretical parameters are set by the agreed upon and updated framework – *viz*. the tenets of contemporary democracy. This phenomenon is again analogous to scientific agreement. As scientists are responsive to new information and adopt new models to address old problems, so too do philosophers adopt new models and modes of thinking to address old problems. It seems, then, that in important respects philosophy is epistemically similar to science, our benchmark of epistemic progress.

We also find an additional similarity between philosophy and science when discussing those issues that lack scientific consensus, like a complete theory of gravity. As science produces results on the basis of some model, the model itself will eventually run into theoretical or empirical limits. It seems that when we push a view to such limits, we uncover more, sometimes bigger, questions. At some point, the science-philosophy distinction seems to break down entirely. Consider, for example, work in the ontology and direction of time,[11] on fundamentality and the wave function,[12] and on biodiversity.[13] There is significant overlap, including collaborations, between philosophers and scientists working on these issues. Given that the science-philosophy distinction breaks down at a specific point in scientific research, we have reason to infer that philosophy is capable of making significant contributions to epistemic progress in the sciences.

In sum: This section pressures the acceptability of PP's first premise, specifically that collective convergence on true or correct responses to a domain's biggest questions is the mark of epistemic progress. Accepting that the hard sciences are the benchmark for epistemic progress, I have highlighted that there is perhaps less convergence on responses to the big questions in those domains than we think, similar to the lack of convergence we find in philosophy. Conversely, if we reformulate a slightly weaker conception of epistemic progress that accounts for why we justifiably conclude that the hard sciences have made significant epistemic progress, it becomes less clear that philosophy doesn't make similar progressions. I am not suggesting that philosophy has made the same degree of progress as the hard sciences. I am suggesting, however, that the epistemic benchmark provided by science is insufficient for showing that philosophy has made little or no epistemic progress – even on some of its biggest questions.

11 See, e.g., [Albert, 2000], [Carroll, 2010], and [Maudlin, 2007: Ch. 4].
12 See the various essays in [Albert and Ney, 2013].
13 See [Maclaurin and Sterelny, 2008].

II. Systematic Disagreement as Defeater?

A second, stronger, pessimistic view is offered in response to philosophy's purported lack of convergence, framed in terms of longstanding or *systematic* disagreement (hereafter SD).[14] On this view, the concern isn't simply that philosophy hasn't made epistemic progress in ways commensurate with domains like the hard sciences. Rather, because of the SD characteristic of philosophy, philosophers have few, if any, doxastically reasonable judgments. The support for this conclusion rests on the higher-order defeat resulting from recognized disagreement. Sanford Goldberg argues, e.g., that anyone working in philosophy for any length of time will recognize that SD abounds and that in such disagreements "(there is a serious chance that) at least one of the disputing parties is unreliable and I don't know that it's not me" [2013a: 180]. All philosophers, then, have reason to question their reliability. And if one has reason to question one's reliability with respect to domain D, then one has a higher-order defeater for any judgment within D. The result, then, is a Pyrrhonian skepticism (hereafter p-skepticism), wherein no philosophical judgments are justified in virtue of the state of the domain itself.

In the previous section I suggested that it might be an overstatement to conclude that philosophy is indeed subject to ubiquitous SD. Let us suppose, however, that SD does provide evidence that at least a good number of philosophers are unreliable.[15] Does it follow from the existence of this higher-order evidence that all of one's judgments relevant to a p-domain are *ipso facto* unreasonable – that all of one's relevant judgments in that domain are defeated? I argue that the existence of SD does not imply universal defeat. For the sake of argument, let us assume a total-evidence view of doxastic justification. On this view, one's judgments are reasonable to the degree that those judgments are on balance supported by and based on one's total evidence. What is more, on a total-evidence view we find that SD is simply another evidential consideration amidst numerous considerations. By parity, one's consideration that SD shows that one might be unreliable applies equally well to others; one might be unreliable and so might anyone else. This strikes me as evidence in favor of epistemic humility and more suitable for fallibilist arguments than for p-skepticism.[16] Further, SD

[14] Sanford Goldberg [2013a] characterizes SD as non-local, widespread, and entrenched, meaning that the disagreement crosses a range of issues, the contending positions have gained support from members of the broader community of inquirers, and the disagreement is not likely to end anytime soon.

[15] Philosophers in addition to Goldberg who accept this contention but not necessarily p-skepticism, include: [Beebee, 2018], [Brennan, 2010], [Christensen, 2014], [Fumerton, 2010], [Kornbnlith, 2010 and 2013] and [van Ingwagen, 1996].

[16] Worth noting is that fallibilism is a claim about knowledge. In its simplest form, we can characterize fallibilism thus: S can know that *p*, but might be mistaken that *p*.

does not seem to carry sufficient evidential weight to eclipse all other considerations. So, while there is some evidence that one might be unreliable given SD, it is plausible that one has additional evidential considerations that on balance justify one's judgments despite the potential that one is unreliable. To make this point, I begin with a discussion on higher-order evidence in the context of peer disagreement.

Peer Disagreement

Much of the current p-skeptical concerns are rooted in the recent epistemic *cause célèbre* surrounding *peer* disagreement. A motivating question in this domain concerns rational responses to the discovery of peer disagreement. Very roughly, the disagreement of interest concerns epistemic peers – people who have equal evidential access, epistemic virtue, and epistemic abilities – who disagree even after full disclosure. That is, the disagreement persists between peers even after each party to the disagreement has an opportunity to explain her reasoning and reasons for the diverging judgment. Thus the question: How should one rationally respond when one finds oneself in this situation? A popular view motivating higher-order evidence's potential to defeat first-order judgments, and thereby related to motivations for p-skepticism, is so-called conciliationism.[17] On this view, when faced with an epistemic peer who disagrees with oneself, one should conciliate in response. One way conciliationists make this point is that upon finding oneself in a peer disagreement under full disclosure, each peer should suspend judgment. If there is no obvious evidential or aptitudinal asymmetry between the peers, then the higher-order evidence produced by the disagreement is sufficient to defeat one's first-order evidence. In either case the point is that higher-order evidential considerations are sufficient defeaters for first-order considerations.

I am sympathetic with conciliationism. Nevertheless, I contend that there are circumstances under which one can remain reasonably steadfast even in the face of peer disagreement. First, consider the relevant type of disagreement we would dub unreasonable:

Or in weak logical terms: S could know that *p*, on the basis of non-entailing evidence. Or in weak epistemic terms: S could know that *p* even though that *p* is not maximally justified for S. I am not concerned here with knowledge claims, but rather doxastic reasonability. So while I suggest that SD seems more suitable for defenses of fallibilism, I am not making an argument about knowledge. I am arguing that one can hold a doxastically reasonable judgment even if one might be unreliable or might be wrong.

17 In addition to Kornblith [2010, 2013] and Goldberg [2013a], other notable conciliationists include David Christensen [2007, 2009], Adam Elga [2007], Richard Feldman [2006, 2007], Jon Mattheson [2009], and Roger White [2005].

> *Unreasonable Peer Disagreement*: If S1 and S2 remain steadfast in their respective opposing judgments over the correct conclusion to draw from a shared body of evidence at time *t*, even after they acknowledge each other as peers and have fully shared their respective reasons for their opposing judgments, then S1 and S2 are members of an unreasonable disagreement at *t*.

It is important to highlight that there is no epistemic asymmetry between S1 and S2 here. Another point to note is that it is strictly in virtue of the higher-order evidence that S1 and S2 would be unreasonably steadfast under such circumstances. Their respective total bodies of evidence – the combination of first and second-order evidence – supports suspension of judgment. If, however, there were an epistemic asymmetry between the two peers, then whoever was in the better epistemic situation could reasonably remain steadfast. This consideration suggests that were S1, say, to provide additional reasons in favor of her judgment that were not available during the initial disagreement, she could reasonably maintain her initial judgment despite recognizing S2's disagreement.

The above example might seem uninteresting. Despite its simplicity, however, I suggest that it sheds light on how one might respond to both challenges from peer and systematic disagreement. Consider a less straightforward example of unreasonable disagreement from modern physics:

> *Deep Reality*: Albert and Niels are widely recognized, and recognize each other, as world-leading physicists. Both have published iconoclastic research that lead to the quantum revolution in physics. Both are up to date with current research pertaining to quantum mechanics and its implications for our understanding of the universe at a deep level. There are no salient arguments or formulas that Albert has not considered that Niels has and vice versa. Indeed, they have even discussed their disagreement in person. From this evidence, Niels concludes that there is no deep observer-independent reality in the universe. Niels argues that his conclusion is a direct implication of quantum mechanical mathematics and our limitations as classically confined observers. Alternatively, after considering all of the same evidence, Albert responds to Niels's view in a research paper, showing that this view is paradoxical. What is more, both physicists agree with the mathematics and its implications. Albert says that it leads to a paradox, so the theory cannot be correct; Niels argues that the theory is correct, so much the worse for our conception of reality. There are roughly the same number of experts in this field endorsing Albert's view as there are experts endorsing Niels'.[18]

[18] Nick Herbert describes the story:

> Quantum theory is complete as it stands, said [Niels]... [Albert's] strategy was to confront [Niels] with a series of thought experiments which aimed to show that quantum theory had left something out....

According to conciliationism, both Niels and Albert should suspend judgment. This conclusion seems right. It is strange, however, that two world-leading experts could hold unreasonable judgments in their specializations.

Contrast the above circumstances with a scenario that represents when one might reasonably remain committed to a judgment in the face of disagreement. At the turn of the twentieth-century, physicists were concerned with puzzles about light. According to their best models, light had to travel through a medium known as ether. Just as the speed of a ball hurled out of a moving vehicle would travel at a speed commensurate with that vehicle's velocity, so too, it was thought, should light travel at a speed commensurate with the ether's motion. Experiments showed, however, that light travels uniformly no matter the direction it is moving – against or with the Earth's rotation on different axis, for example. After considering this problem, Einstein concluded that there was no ether because time and motion are relative to a frame of reference and that the speed of light was invariant across such frames. Einstein considered the existing evidence and physics against his judgments but could not render them coherent until he rejected views universally accepted by his peers.[19] Upon making this move and producing his results, Einstein's conclusions were widely rejected by the leaders in his field. Now, experts nearly universally accept Einstein's theory.

Per the concerns in Deep Reality, one might think that since participation in a peer disagreement renders one unreasonable, Einstein's judgments were epistemically unreasonable as he was surely cognizant of these disagreements. Fortunately, we can explain how Einstein can remain justified in his initial judgments despite facing peer opposition – a phenomenon that seems impossible if the p-skeptical argument is sound. This justification results from the evidential support Einstein

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> As the winners tell the story, [Niels] closed each of [Albert's] loopholes, but in the minds of each the debate was never settled. Long after their arguments had ended, on the day [Niels] died, his blackboard contained a drawing of each one of [Albert's] thought experiments. [Niels] struggled with [Albert] until the end.
>
> [Albert] too never gave up. In his autobiography he expresses his final thoughts on the quantum reality question: "I still believe in the possibility of a model of reality – that is, of a theory which represents things themselves and not merely the probability of their occurrence" [1985: 24].

19 There a number of excellent accounts of Einstein's process of discovery. See, e.g., [Carroll, 2010: Ch. 4], [Frank, 2011: Ch. 5], and especially [Norton, 2016]. Physicist Lisa Randall's commentary is particularly a propos: "Although by now [2005] relativity has been well tested and even gives rise to effects that need to be accounted for in practical devices, I do find it very remarkable that anyone listened to Einstein as first. He was completely unknown, working in a Bern patent office because he couldn't get a better job. From this unlikely location he proposed a theory that went against the beliefs of all other physicists of his time" [2005: 113–4].

acquires after considering alternatives to his view and uncovering new evidence in its favor. After publishing his results concerning STR at time t_1, I suggest that prior to anyone challenging his view, Einstein's judgments are reasonable. When at t_2 a recognized peer challenged Einstein's conclusion and Einstein knew of the reasons behind this challenge, Einstein would have a higher-order defeater against his judgments.[20] If, however, at t_3 Einstein offers additional evidence in favor of his view, or reasons why the alternative view is mistaken, or how the criticism has misunderstood its target, then he would defeat the defeaters, as it were. Einstein could thereby remain reasonably steadfast in maintaining his judgment given his evidence at t_3. If at t_4 a peer were to offer additional reasons against Einstein's conclusions, and Einstein could not provide additional support without bootstrapping his original evidence he could not reasonably maintain his initial judgment until such time that he could provide novel evidence.[21] Failing such responses, however, Einstein can remain reasonably steadfast with the contested judgment.

It is important to highlight that one's justification in response to an opposing judgment may be short lived. The interval between t_1 and t_2, e.g., could be quite short. Also worth noting is that during the intervals between discovering direct challenges to one's initial judgment and being able to provide an epistemically satisfactory response to those challenges, one should suspend judgment. Suppose, e.g., that there is a lengthy interval between t_2 and t_3, during which Einstein neither suspended judgment nor offered any novel reasons for his view or against the challenges. In this circumstance, he would be unreasonably steadfast. Conversely, there will also be circumstances where Einstein requires no substantial time to refute a putative peer's challenge and no suspension of judgment necessary. Upon considering Einstein's conclusions about STR, for example, Einstein's contemporaries were required to consider a new paradigm that Einstein grappled with for nearly a decade. Misunderstandings would be common under such circumstances. Einstein could swiftly dismiss challenges based on these types of misunderstandings without epistemic fault. We find, then, that in remaining adaptive to the evidence as it is presented, one will at times be required to suspend judgment but at other times will have means to reasonably maintain the initial judgment when in a peer disagreement.[22]

[20] Einstein surely found himself in such a position. Henri Poincare, for example, rejected Einstein's interpretation of spacetime and dismissal of ether despite himself helping to advance the special theory of relativity.

[21] We might suggest that a certain level of bootstrapping is permissible if the bootstrapping highlights a nuance to one's evidence missed by one's peer.

[22] The preceding section borrows substantially from [Olson, 2018: Ch. 4], where I provide a more thorough argument for a quasi-conciliatory response to peer disagreement.

Remaining Reasonably Steadfast Despite SD

Let us now connect the foregoing discussion concerning peer disagreement to our target concern, the potential defeat emerging from SD. If the above account for reasonable steadfastness is correct, then it seems that when faced with a *peer* disagreement under full disclosure, one has two options: suspend judgment or provide additional reasons/evidence in light of the shared extant reasons between one's peers and oneself. One of the problems with p-skepticism is that it nullifies the epistemic significance of this type of reasoning process, even though it seems equally as applicable to SD as it does to peer disagreement. Consider, for example, Bert's reasoning process concerning the traditional analysis of knowledge [TAK]:[23]

> *TAK holds that for one to know one must have a justified true belief. But think about a trustworthy clock that one has used for years. One could see this clock 12 hours after it stopped working and accurately judge the time on that basis. It doesn't seem as though one would know the time under such circumstances, however. So it seems like there is good reason to conclude that justified true belief is insufficient for knowledge even though most of my peers accept this account.*[24]

Suppose that Bert is the first person to notice this flaw in TAK. Upon this discovery, then, there are no strong counterarguments to his position. It appears as though Bert's reasons on balance support judgment.

The p-skeptical position seems to imply that even if one were able to provide novel arguments in light of new or extant evidence, as Bert has, one's judgment would remain unreasonable nonetheless. In virtue of the argument occurring within a p-domain, one's judgments are unreasonable given the higher-order evidence of unreliability. Bert should therefore include the following in his considerations:

> *Despite what seems to me a reasonable conclusion, I recognize that epistemology is subject to SD; so, I might be unreliable on this issue. It therefore would be unreasonable for me to hold this judgment.*

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[23] This reasoning is analogous to Einstein's in the previous section, adjusted *mutatis mutandis* for a domain subject to SD.

[24] This example is from [Russell, 1948, pp. 139–40] and is thus an anticipation of the famous Gettier problems. Russell anticipated such problems much earlier: [Russell, 1912, pp. 76–7]. It is noteworthy that contemporary epistemologists accept that Gettier problems create significant trouble for TAK – a notable exception here is [Bonjour, 2010] – and that Russell was, as far as I know, the first to bring real attention to this issue. Moreover, per above, I think that this provides a counterexample to SD as representative of philosophy for two reasons. First is the aforementioned acceptance of the Gettier problems by epistemologists, which seems much as the convergence denied as applying to p-domains. Secondly, novel arguments are insufficient for reasonable judgments in domains with SD. We discuss this second point more detail in what follows.

According to the p-skeptics, this is the only reasonable response available in the face of SD. Is Bert's total evidence indeed inconclusive though?

We have just noted one way that higher-order evidence can serve as defeating evidence in the context of a peer disagreement. If one discovers that one's peer has considered all of the same evidence, alternative judgments and reasons for them, and forms an alternative judgment, then one has defeating evidence against one's initial judgment. The p-skeptics adopt similar reasoning in support of the claim that SD provides defeating evidence. In this context, philosophers working on these issues are generally epistemic peers; at least some philosophers, perhaps many or most, are equally as intelligent, informed, and reliable in their specific sub-domains. Thus, according to the p-skeptic, we can infer that SD is a generalized form of peer disagreement, with the evidential significance that brings with it. If the appeal to one-to-one peer disagreement is apt, then there is reason to conclude that Bert's evidence is indeed inconclusive.

The above comparison may not be apt in the relevant ways, however. One minor point, a propos the comparison between peer and systematic disagreement, concerns full disclosure. Unlike one-to-one peer disagreement, SD is "entrenched, non-local, and persistent," from which we are to infer that at least some participants to the domain are unreliable. Goldberg: "to have evidence that the dispute in which one is engaged is a *systematic* disagreement is already to have evidence that puts rational pressure on one to call into question one's own reliability" [2013a: 177–8]. Conversely, in a peer disagreement, in the relevant sense of an *unreasonable* peer disagreement discussed above, there is full disclosure. That is, one has considered all of a peer's reasons and reasoning. This strikes me as an important distinction. The evidential weight purported to show that one and one's peers are unreliable in SD has little to do with the first-order reasons. The force of the higher-order evidence in a *peer* disagreement is that the participants have considered all of the same relevant evidence and their reasons for their judgments. Under such circumstances, if one cannot offer additional evidence that one's peer has misinterpreted the evidence, or missed some salient piece of evidence, or committed a fallacy, then the evidence does indeed seem inconclusive. This is the case in Deep Reality. It is not the case with Bert or Einstein. That SD does not require full disclosure is a significant evidential omission that diminishes the force of the higher-order evidence in SD.

That SD does not mandate full disclosure is a minor point.[25] A related and more substantial issue for the p-skeptical contention is that Bert's reasons are defeated simply in light of SD. Consider again the p-skeptical addendum to Bert's reasoning from above:

[25] One might think that theorists working in a field for a sufficient length of time will be familiar enough with the issues that their epistemic situation is virtually equivalent to what it would be under full disclosure.

> *Despite what seems to me a reasonable conclusion, I recognize that epistemology is subject to SD; so, I might be unreliable on this issue.*

I propose, rather than concluding that evidence of potential unreliability implies universal defeat, the more reasonable implication of SD in philosophy is that evidence of potential unreliability implies the potential for error. Acknowledging the potential for error seems like responsible epistemic practice and is much weaker than outright defeat. Consider how we might amend Bert's reasoning. Rather than suspending judgment, Bert might reason thus:

> *Despite what seems to me a reasonable conclusion, I recognize that epistemology is subject to SD; so, I might be unreliable on this issue. Granted, I have no reason to conclude that I am uniquely unreliable – perhaps we all are. So I might be unreliable, but so too might those who disagree with me. What is clear is that I seem to have good reason to conclude that TAK is insufficient. Given my potential unreliability it would be infelicitous for me to be dogmatic about this judgment. Nevertheless, without counterarguments or counterevidence against my reasoning, it is not infelicitous or unreasonable to maintain this judgment.*

There are two related points here. First, at the time of Bert's judgment, Bert is proposing a novel argument. It is unclear how p-skeptics would deal with this type of phenomena. Secondly, even accepting that SD provides evidence for one's unreliability, Bert's judgment remains reasonable given his total evidence, including the higher-order evidence from SD.

Let us consider the first point. It is stipulated that at the time of Bert's considerations, there is, as of yet, no clear SD surrounding the Gettier problem and our conception of knowledge in light of this problem. With hindsight, we can also note that Gettier problems, with very few exceptions, are accepted by most expert epistemologists as bonafide challenges to TAK. We have a case, then, where a judgment in a p-domain [a domain fraught with SD] sheds new evidence on an old question. What is more, this evidence is widely accepted by more theorists working in this domain than not. It is unclear, however, how p-skeptics could accept this as a reasonable judgment given its membership in a p-domain. This is a strange result. It suggests that even a judgment that has evidential support and as of yet no counterevidence can be deemed unreasonable simply in virtue of the domain. There would thus be no way to work towards forming reasonable judgments within a p-domain. If the p-skeptic allows judgments to become more reasonable – gain more evidential support – through converging judgments, then it seems that not all judgments in p-domains are unreasonable. Novel arguments and evidence have the potential to garner convergence, rendering a judgment reasonable by the p-skeptics own lights. The strange, I suggest incorrect, implication here is that those theorists who accept this judgment must initially do so unreasonably. Of course, we do not contend

that Einstein was unreasonable in remaining steadfast in his judgments concerning STR. Bert likewise does not seem unreasonable in holding his judgment concerning TAK.

The second point to note from Bert's amended reasoning is that it accepts the p-skeptical contention that SD is evidence for unreliability. Even accepting this contention, however, we at best have an argument for epistemic humility. Included in Bert's total evidence is that he and his peer's might be unreliable. This evidence is certainly grounds for epistemic humility (it could also support a fallibilist conception of knowledge). That the evidence motivates a quasi-conciliationism and serves to support the contention that we might be wrong or unreliable on a number of judgments, however, does not preclude that any, and most certainly not all, judgments in that domain are defeated. There are no grounds, therefore, to conclude that all p-domain judgments are *ipso facto* unreasonable.

III. Conclusion

I have attempted to pressure two pessimistic views concerning philosophy's epistemic progress. I refer to defenders of the first view as progress pessimists. These pessimists appeal to the purported failure of philosophy to match science's benchmark epistemic progress as reason to doubt philosophy's progress. I response to this challenge, I suggest that there is relevant overlap between these two domains, such that philosophy shares a number of positive epistemic credentials with science, and that science is plagued by a number of similar limitations that philosophy has. Defenders of the second and stronger pessimistic view, p-skeptics, hold that systematic disagreement provides evidence of a general unreliability amongst philosophers. This higher-order evidence purportedly defeats our first-order judgments. Appealing to available rational responses to higher-order defeat in peer disagreement, I propose that similar rational responses are available to philosophers faced with higher-order evidence of potential unreliability. Such evidence surely suggests a robust acceptance of epistemic humility. If I am right, however, it does not suggest that philosophers or philosophy itself cannot make rational progress.[26]

[26] For their helpful discussions or comments on various parts of this paper, I would like to thank Charles Bakker, Earl Conee, Richard Feldman, members of my Metaphilosophy in 20th Century Analytic Philosophy Winter 2018 seminar at McMaster University, and audience members at the Canadian Society for the History and Philosophy of Science 2018 annual meeting.

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