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Fighting about frequency

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Abstract
Scientific disputes about how often different processes or patterns occur are *relative frequency controversies*. These controversies occur across the sciences. In some areas—especially biology—they are even the dominant mode of dispute. Yet they depart from the standard picture of what a scientific controversy is like. In fact, standard philosophical accounts of scientific controversies suggest that relative frequency controversies are irrational or lacking in epistemic value. This is because standard philosophical accounts of scientific controversies often assume that in order to be rational, a scientific controversy must (a) reach a resolution and (b) be about a scientifically interesting question. Relative frequency controversies rarely reach a resolution, however, and some scientists and philosophers are skeptical that these controversies center on scientifically interesting questions. In this paper, I provide a novel account of the epistemic contribution that relative frequency controversies make to science. I show that these controversies are rational in the sense of furthering the epistemic aims of the scientific communities in which they occur. They do this despite rarely reaching a resolution, and independent of whether the controversies are about scientifically interesting questions. This means that assumptions (a) and (b) about what is required for a controversy to be rational are wrong. Controversies do not need to reach a resolution in order to be rational. And they do not need to be about anything scientifically interesting in order to make valuable epistemic contributions to science.

Keywords Scientific controversies · Epistemic rationality · Relative significance controversies · Relative frequency controversies · Social structure of science · Independence thesis

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1 Introduction

The phrase “scientific controversy” calls to mind a clash of ideas. One side is right, the others are wrong, and debate is a means of identifying which is which. But this intuitive picture admits of many exceptions.

Consider Darwin’s theory of gradual speciation. It dominated the field of evolutionary biology for over a century, until Niles Eldredge and Stephen Jay Gould argued that the lifespan of a species is not characterized by slow and continuous change, but by long periods of stability interrupted by bursts of rapid evolution. Their theory, which they called *punctuated equilibrium*, challenged a central commitment in evolutionary biology. Gould (1980) claimed standard Darwinism was “effectively dead,” while skeptics responded that Gould and Eldredge had merely identified a “minor wrinkle on the surface of neo-Darwinian theory” (Dawkins, 1986, p. 251).

This dispute about punctuated equilibrium occupied biologists for decades (Ruse, 2000). But strangely, both sides agreed that speciation is sometimes gradual and sometimes punctuated. At stake was not which pattern was correct, but rather, which was more frequent. In other words, the dispute was about the relative frequency of the two patterns.

Scientific disputes about how often different processes or patterns occur are *relative frequency controversies*. These controversies occur across the sciences. In some areas—especially biology—they are even the dominant mode of dispute. Yet they depart from the standard picture of what a scientific controversy is like. In fact, standard philosophical accounts of scientific controversies suggest that relative frequency controversies are irrational or lacking in epistemic value. In this paper, I defend relative frequency controversies by providing a novel account of the epistemic contribution that they make to science. This account has two surprising consequences for the way we think about scientific controversies more generally. First, controversies do not need to reach a resolution in order to be epistemically rational. Second, controversies do not need to be about anything scientifically interesting in order to make valuable epistemic contributions to science.

2 What relative frequency controversies are

The concept of relative frequency controversies first appeared in the philosophical literature in a 1997 paper by John Beatty, though he uses the term “relative significance” rather than “relative frequency.” I use the latter term because arguments about significance are a distinct type of disagreement from arguments about frequency.[1] Despite this difference in terminology, I agree with Beatty about what relative frequency controversies are.

[1] This difference is nicely captured in Godfrey-Smith’s (2001) distinction between empirical adaptationism (an issue of frequency) and explanatory adaptationism (an issue of significance).

Scientists develop theories[2] to explain natural phenomena. Some phenomena, such as the extinction of dinosaurs, are one-time events. Others, like speciation or the melting point of lead, have many instances. Phenomena that have many instances fall into two further kinds: those for which a single theory explains all of their instances, and those for which more than one theory is required to explain all of their instances. An example of the first kind of phenomenon is the melting point of lead. The same theory explains every instance of lead melting at 327.5 °C. Speciation, by contrast, is a phenomenon that requires multiple theories to explain all of its instances. There are different speciation patterns, some gradual and some punctuated. Two different theories are needed to account for both of these patterns.

Relative frequency controversies do not arise for phenomena that are fully explained by one theory. There is simply no opportunity for them. Instead, scientific disagreements about such phenomena often focus on which competing theory is the *right* explanation. The situation is different for phenomena that do require more than one theory to explain all of their instances. In these cases, there is opportunity to disagree about how many of the total instances of a phenomenon different theories explain. Such disagreements are relative frequency controversies:

Relative frequency controversy a dispute about the (non-zero, non-total) proportion of the instances of a phenomenon that a theory explains.[3]

Not all debates involving multiple explanations or theories of a phenomenon are relative frequency controversies. Two classes of debate which do *not* qualify are (1) debates about multiple, distinct theories that explain the same instances of a phenomenon simultaneously (e.g. optimality modeling and genetic modeling, discussed in Potochnik, 2010), and (2) debates about multiple, mutually exclusive explanatory approaches to the same phenomenon (e.g. different approaches to atomic-molecular chemistry in the nineteenth century, discussed in Chang, 2012). Rather, the key features of a relative frequency controversy are disagreement about the fraction of a phenomenon that different theories explain, combined with agreement that these different theories all explain part of the phenomenon.[4] To get a better feel for relative frequency controversies, consider these two examples:

What causes microevolution? Sometimes, small evolutionary changes to a population are the result of natural selection. For example, the average length of the beaks in a population of Galápagos finches might increase because it is easier for finches with longer beaks to survive and reproduce (Grant & Grant, 2003). But changes in beak shape can also occur by chance. Even if longer beaks do not provide a fitness

[2] Here and throughout the paper, I use the term "theory" in an informal sense—often synonymously with "hypothesis" or "mechanism."

[3] Beatty (1997) defines the relative significance of a scientific theory as "roughly the proportion of phenomena within the domain that the theory correctly describes" (p. S432). He does not explicitly define the term "scientific controversy." My own understanding of "scientific controversy" follows Ernest McMullin's: "a publicly and persistently maintained dispute…concerned with a matter of belief…determinable by scientific means" (1987, p. 51).

[4] Though not the paradigmatic type of case, controversies about the relative roles of different causal factors in producing single-event phenomena (e.g. the extinction of dinosaurs) are, if not relative frequency controversies, quite similar to them in terms of their structure and dynamics.

advantage, random events can still favor the reproduction and survival of finches with longer beaks. When changes to a population occur by chance, rather than natural selection, biologists call it drift. Which of these two forces, selection or drift, is the more common and powerful source of evolutionary change? Biologists have fought over this question since the middle of the twentieth century, and the issue remains unsettled to this day (reviewed in Provine, 1992; see also Skipper, 2002). Further complicating this debate is the fact that a combination of selection and drift can produce evolutionary change in the same trait, which makes disentangling their relative contributions to particular instances exceptionally difficult.

What causes intra-plate volcanism? Most volcanoes form at the edges of tectonic plates as the plates collide or pull apart. But there are also hundreds of volcanoes that occur near the centers of tectonic plates, including those responsible for the Galápagos Islands, the Hawaiian Islands, and Yellowstone National Park. How do geologists explain the existence of these intra-plate volcanoes? There are two main contending explanations, the plume hypothesis and the plate hypothesis. According to the plume hypothesis, intra-plate volcanoes are caused by plumes of molten rock welling up from deep within the Earth's mantle. The locations of these mantle plumes are fixed, and as the earth's crust moves slowly over them, the plumes generate time-progressive chains of volcanoes such as the Hawaiian Islands. The competing hypothesis, the plate hypothesis, tries to account for intra-plate volcanoes by appealing to "shallow" processes in the earth's crust and upper mantle. Though some geologists are skeptical that mantle plumes exist at all, most agree that both plumes and tectonic processes produce intra-plate volcanoes. Their dispute, which they call "the great plume debate," focuses on the likely causes of particular volcanoes and the relative frequency of the mechanisms that produce them (reviewed in Foulger, 2010).

Beatty tells us that relative frequency controversies occur all the time and "at every level of investigation" (1997, p. S434) in biology. Elisabeth Lloyd and Stephen Jay Gould write, "Almost all major questions, and great debates, in natural history, revolve around the issue of relative frequency" (1993, p. 598). Additional examples of relative frequency controversies include debates about the frequencies of different inheritance mechanisms, debates about the frequencies of different mechanisms of sexual selection, debates about the frequencies of different speciation mechanisms, and debates about the status of different explanations for the maintenance of sex. Though most of the literature on relative frequency controversies focuses on biology, there are also many examples (such as the great plume debate) of relative frequency controversies outside of biology. These controversies are very common, and they take up research time, energy, and funds. If they are irrational, or if they make no epistemic contribution to science, or if the cost of that contribution is too high, we want to know. If these controversies do make an epistemic contribution to science, we want to explain how and why.

3 Evaluating scientific controversies

3.1 What makes a controversy rational?

The first step in understanding whether and how relative frequency controversies contribute to science is to clarify what it means for a controversy to be rational. Traditionally, philosophical work on scientific controversies has tried to reconcile the optimistic idea that science is a rational, systematic way of learning about the world with the undisputable fact that scientists in the heat of controversy often behave irrationally. Scientists are liable to let politics and personal ambition influence which side they take in a controversy. Cognitive biases can cause them to cling to positions that are not well-supported by evidence. Can a controversy be considered rational when such non-epistemic factors are at play?

Recent work on the social structure of science has made considerable progress on this issue by showing that individual and group rationality are somewhat independent. Rational individuals can form irrational groups, and vice versa (e.g. Kitcher, 1990; Solomon 1992; Weisberg & Muldoon, 2009; Mayo-Wilson et al. 2011). Much of the research in this area focuses on questions about the optimal division of labor within a scientific community (e.g. Strevens, 2003), but the insights apply to scientific controversies as well. For example, one upshot of this research is that it can be good for scientists to have extreme, even implausible, beliefs (e.g. Zollman, 2010). Sometimes, implausible theories turn out to be true, and stubborn—even irrational—defenders can keep a theory alive long enough for researchers to turn up evidence in its favor.

For our purposes, the lesson of this research is that we cannot evaluate the rationality of a controversy simply by asking whether the people engaging in it are behaving rationally (e.g. appropriately updating credences in response to evidence). So, how should we evaluate controversies? According to a popular[5] view of *individual* epistemic rationality, evaluating the epistemic rationality of an individual is a matter of asking if they form beliefs in ways that further their epistemic aims (Foley, 1987; Giere, 1989; Kitcher, 1992; Laudan, 1996). On this view, epistemic rationality is a subset of instrumental rationality. I propose an analogous strategy for evaluating the epistemic rationality of controversies: we should ask if they further the epistemic aims of the scientific communities in which they occur. That is, for a scientific controversy, to be epistemically rational and to further the epistemic aims of science are nearly the same thing.

My approach to the epistemic rationality of disagreement differs from the way most epistemologists view peer disagreement. The literature on peer disagreement asks under what conditions epistemic peers who disagree with one another are epistemically rational. There is wide agreement that in order for someone to be rational in such a case, they must respond appropriately to the higher-order evidence provided by the fact that their peers disagree with them—e.g. by splitting the difference

[5] Though not uncontroversial: see Kelly (2003).

(e.g. Matheson, 2015), holding fast (e.g. Inwagen, 1996), considering both first-order and higher-order evidence (e.g. Kelly, 2010), etc. By contrast, my understanding of the epistemic rationality of a controversy does not depend on how individuals respond to facts about peer disagreement. Instead, I am interested in exploring the possibility that the epistemic rationality of a scientific controversy is independent of the rationality of the individuals involved in it.

My basic proposal—that a scientific controversy is epistemically rational if it furthers the epistemic aims of science—needs some further development before we can put it to use. First, what are the epistemic aims of science? My assumption here is that this is an open question,[6] and it will not be my goal to provide a definitive list. Instead, throughout the paper, I will focus on two uncontroversial epistemic aims of science: explanation and prediction. If a controversy improves the explanations a community of scientists is able to give of or the predictions it is able to make about the phenomena it studies, then that controversy has furthered the epistemic aims of the scientific community. The same is true for science's other epistemic aims, whatever they are.

Second, my discussion of epistemic aims does not depend on the existence of a sharp divide between the epistemic and non-epistemic aims of science. In fact, the nature of the epistemic/non-epistemic distinction is another open question in philosophy of science, and I am only committed to the claim that there is some useful distinction to be made here (perhaps along the lines of Steel's (2010) proposal), though the best account of the distinction may be imprecise and context-specific.

Third, a controversy might further one of the epistemic aims of science but be such an inefficient way of doing so that it is not justified. Such a controversy is not epistemically rational, because the opportunity cost of engaging in it is too high. In practice it is difficult, if not impossible, to make determinations about opportunity cost. But in principle, engaging in a controversy should not be so costly that it sets back the epistemic aims of a community overall for the sake of making an advance in one area.

Fourth, when we evaluate the rationality of a controversy we can do so *prospectively* or *retrospectively*. Our evaluation is prospective if we are considering whether a current or potential controversy will further the epistemic aims of science. It is retrospective if we are asking this question about a controversy that has already ended. Unlike retrospective evaluations, prospective evaluations are probabilistic. They cannot say with certainty whether a controversy will further the epistemic aims of science, but they can say whether it is likely to.

3.2 Two common assumptions about rational controversies

The characterization I have just given of what makes a controversy epistemically rational does not specify any particular features that such controversies must or typically have. But it is possible that epistemically rational controversies do share

[6] There is a long history of philosophical debate on this point. I am particularly persuaded by Angela Potochnik's (2015) defense of the idea that science's epistemic aims are subject to change and expansion.

common features. In fact, much of the existing philosophical work on controversies either assumes or tries to identify such features. Here I draw attention to two features that epistemically rational controversies are often assumed to have.

The first feature rational controversies are often assumed to have is a *resolution*. A controversy is resolved if it (a) the question animating it is answered (b) due to reasoning and argument about the question. If scientists do not answer the question animating a controversy but rather decide that the question itself is confused or otherwise unanswerable, the controversy will end, but not be resolved. Following Ernest McMullin (1987), I will distinguish two other ways that controversies can end without being resolved: *abandonment* or *closure*. A controversy is abandoned if it ends because participants lose interest in it, even though the question animating the controversy is not answered. A controversy is closed if it ends because of the influence of a non-epistemic force such as societal pressure, governmental fiat, or even the say-so of powerful voices within the scientific community. According to many analyses, questions about rationality arise only for resolved controversies, not for ones that are abandoned or closed.

McMullin distinguishes these three ways in which controversies can end before arguing that scientific controversies typically end in resolution rather than abandonment or closure, and that epistemic factors are typically more important than sociopolitical factors in cases of resolution. His goal is to show that scientific controversies are rational more often than not. In order to reach this conclusion, he sets aside controversies that do not reach resolution. Abandoned and closed controversies are not part of the argument for the rationality of scientific controversies.

Philip Kitcher's (2000) analysis of rational controversies is similar. He concedes that controversies are often ignited and maintained for practical or irrational reasons, but goes on to argue that when it comes to judging a controversy as rational or not, what matters most "is that controversies are *closed* by reason and argument" (p. 27). Kitcher does not consider what it would mean to say that an unresolved controversy is rational. Implicitly, the space of potentially rational controversies is limited to controversies that reach a resolution.

A second common assumption about rational controversies is that they must be about *scientifically interesting questions*. I understand a scientifically interesting question to be one whose answer is directly relevant to advancing the aims—either epistemic or practical—of science. Questions whose answers help scientists explain and predict natural phenomena, for example, are scientifically interesting. But these are not the only scientifically interesting questions. Questions whose answers are not explanatory or predictive but still help scientists achieve practical aims, such as medical or ecological interventions, are scientifically interesting as well. Being about a scientifically interesting question is not sufficient for a controversy to be rational, because a controversy about an interesting question could be conducted in a way that does not further the aims of the relevant scientific community. Still, philosophers have (implicitly and explicitly) treated this as a necessary condition that a rational controversy must meet.

Not all scientific controversies are about scientifically interesting questions. Consider disputes about who deserves credit for an idea or discovery, such as the one surrounding Eric Lander's (2016) account of the development of CRISPR technology.

Such disputes can become genuine controversies. That is, they can become persistent, public disputes among scientists about a factual matter. But apportioning credit, while necessary for science to function, is not itself an aim of science. This means that while settling a question about credit may be indirectly relevant to advancing the aims of science, it is not directly relevant, and controversies about apportioning credit are not controversies about scientifically interesting questions.

The assumption that rational controversies must be about scientifically interesting questions is quite widespread. One way to see this is to look at the controversies that philosophers choose to study. These controversies are invariably the most scientifically interesting and exciting ones: heliocentrism versus geocentrism, different interpretations of quantum mechanics, continental drift theory, the specific mechanisms by which new species and traits evolve, etc. Sometimes, the assumption is even built into the way that philosophers define the term "scientific controversy." Gideon Freudenthal, for example, writes that a controversy must concern "a substantial scientific issue" (2000, p. 128). On his analysis, a dispute cannot even *be* a controversy unless it is about a scientifically interesting question. Rather than settle the issue by fiat, however, we should look more closely at the stock of examples science has to offer us in order to determine whether all genuine, epistemically rational controversies are about scientifically interesting questions.

4 Unresolved and uninteresting controversies

Now we return to relative frequency controversies. Do they typically reach resolution? Are they about scientifically interesting questions? Here I show that (a) relative frequency controversies are rarely resolved, and (b) there are doubts about whether they are about scientifically interesting questions. Therefore, either relative frequency controversies are often epistemically irrational, or at least one common assumption about rational controversies is mistaken.

4.1 Relative frequency controversies are often unresolved

Perhaps the most distinctive feature of relative frequency controversies is that they do not always end in resolution or closure. Instead, they are likely to fizzle out and be abandoned, sometimes after decades of active disagreement. This means scientists often do not get an answer to the question that animates relative frequency controversies: how many (or what proportion) of the particular instances of a general phenomenon are explained by a given theory or theories?

Scientists, philosophers, and historians have all commented on the lack of resolution in relative frequency controversies. As an example, we'll consider the neutralist-selectionist controversy in evolutionary biology. This controversy was similar to the controversy about adaptation and drift described in Sect. 2, except it focused on evolution at the molecular rather than phenotypic level. The central issue was whether the evolution of organisms' amino acid sequences is driven more by selection or by neutral, drift-like processes. The neutralist-selectionist controversy was a major controversy within evolutionary biology during the 1970s and 1980s. According to the geneticist Jody Hey:

> Imagine growing up a fan of a fierce rivalry, of two great teams persistently at loggerheads. And suppose that on the morning of the final test—the definitive encounter between the rivals—everyone involved lost interest and went home. That is partly what it seemed like to a graduate student reading avidly of the neutralist-selectionist debate in the early 1980s…the debate quietly withered and came to some indeterminate demise in the mid to late 1980s (1999, p. 35).

While describing the neutralist-selectionist controversy, along with several other prominent relative frequency controversies in evolutionary genetics, Michael Dietrich, a philosopher and historian of biology, observes[7]:

> The controversies of evolutionary genetics typically began as highly polarized disputes, but the positions in question developed, sometimes radically, sometimes more subtly. These transformations allowed the controversies to depolarize by enabling some participants to disengage, revise their opinions, or change their focus (2006, p. 12).

Dietrich's use of the term "depolarize" tracks the notion of abandonment from Sect. 3. A depolarized controversy is not one where the original question is answered to anyone's satisfaction. It is one that ends because participants disengage, revise their opinions, or change focus. Opponents in a relative frequency controversy often start out at opposite extremes, claiming, say, that molecular evolution is nearly always the result of selection, or nearly always the result of neutral processes. By the time the controversy is abandoned, however, most people have moved toward the center, though they still have strong disagreements with one another and maintain that the controversy has not been resolved, because it is still the case that no one knows the answer to the original relative frequency question. So, while a controversy will likely eliminate some possible answers to the original relative frequency question, enough uncertainty remains about the actual answer that members of the scientific community do not consider the controversy resolved.

Scientists sometimes express distress about relative frequency controversies that do not reach resolution. Surprisingly, philosophers do not. This is surprising for two reasons. First, this feature of relative frequency controversies means they run afoul of a common assumption about what is required for a controversy to be rational. Second, in some scientific communities it is widely accepted that relative frequency controversies rarely reach resolution. When this is the case, why engage in the controversy at all? If the outcome is known with high probability, and the question under discussion will not be answered by the end, how can the controversy be epistemically rational?

[7] Similar observations from other philosophers can be found in Millstein (2007), Skipper (2002, 2009) and Plutynski (2005). For an example of a relative frequency controversy that looks like it is in the process of being abandoned, see Bird et al.'s analysis of the controversy over sympatric speciation (2012, p. 176), as well as Via (2001) and Jiggins (2006).

These puzzles concern both the rationality of individual scientists who participate in relative frequency controversies and the rationality of the controversies themselves. With respect to individual scientists, it is hard to see how participating in controversies known to have high probabilities of abandonment furthers individuals' epistemic aims. It is also likely that the actual behavior of scientists in these controversies falls far short of the various evidentiary norms discussed in the peer disagreement literature. But luckily, the epistemic irrationality of individual scientists does not settle the question of the epistemic rationality of relative frequency controversies themselves.

In order to be rational, a controversy must further the epistemic aims of science. But if the question animating the controversy is not answered, what kind of epistemic contribution is made? Even if we identify an epistemic contribution, isn't there a less costly way of achieving the same epistemic end than a fierce controversy with a predictable outcome? Answering these questions, as I show in Sect. 5, requires us to locate the epistemic value of relative frequency controversies in something other than answering the questions that participants take these controversies to be about. This move separates the question of epistemic value, or rationality, from the question of resolution—whether a controversy is rational will turn out to be independent of whether it is resolved.[8]

4.2 Relative frequency controversies may not be about anything scientifically interesting

This brings us to another matter: are the questions at the center of relative frequency controversies scientifically interesting questions? Or, as Beatty puts it:

> Suppose that we had stated a relative significance position precisely, and that we had doggedly pursued it, and that we had tallied the hypothesized proportion of instances of the theory within its intended domain…Why would we consider this proportion to be telling? (1997, p. S440)

Beatty believes that when it comes to biology, any particular distribution of relative frequency—such as the relative frequencies of adaptation and drift—is itself a contingent outcome of evolution. He reasons that this contingency means we may not gain anything from determining relative frequency distributions. Whatever the actual ratio of adaptation to drift, or of punctuated to gradual evolution, that ratio could have easily been different. There is no scientifically interesting reason why we have one ratio and not another. Thus, we may not understand the world any better

[8] That relative frequency controversies often go unresolved is basically a consensus position in the scientific and philosophical literature on these controversies. Still, one might disagree with this claim, either because no one is in a position to report the actual proportion of resolved to unresolved controversies, or because eliminating some possible answers to the original relative frequency question seems like resolution of a sort. If you are not persuaded that these controversies are often unresolved, the important point for my argument is that their being resolved is independent of their being rational, as I discuss in Sect. 5.

once we have determined a particular relative frequency distribution than we did before we determined it.

Contrast the adaptation vs. drift case with our knowledge of the relative frequency of the outcomes of a coin flip or a dice roll. There is something telling about the 51% probability that when you flip a coin it will come up the same way it started (Diaconis et al. 2007). In the coin toss case, physical laws determine the relative frequency of heads to tails, but in the adaptation vs. drift case, there is no deeper principle that explains or determines relative frequency. This point is underscored by the fact that biological relative frequency distributions themselves can evolve and change, so having resolved a relative frequency controversy at one time does not mean that the answer was always or will continue to be correct.

As formulated by Beatty, this worry about relative frequency controversies is particular to biology and rests on a view of evolutionary contingency which is itself contentious.[9] But his worry generalizes.

A sure way for a relative frequency question to be a scientifically interesting question is if answering it makes some scientific explanations better. After all, science has multiple goals, but explanation is one of the foremost. Roughly, to give a scientific explanation is to answer a why-question about the world.[10] For example, the theory of particulate inheritance provides an answer to the question, “Why is there so much variation in the physical traits of closely related individuals?” Some facts about the world are directly relevant to developing such explanations. Famously, Gregor Mendel’s experiments with peas provided facts that helped him formulate his basic laws of inheritance. But other facts are not explanation-relevant. Mere enumeration, for example, such as collecting as many samples of a particular species as possible simply for the sake of having them in a museum is more like stamp collecting than science (Johnson, 2007).

The general form of Beatty’s worry, then, is as follows: Scientists often engage in relative frequency controversies because they believe the questions at the heart of these controversies are explanation-relevant. But in fact, these questions are not explanation-relevant, and pursuing a relative frequency controversy is little more than enumerative fact gathering. When scientists argue about how often microevolution is driven by adaptation or drift, they are merely tallying cases, nothing more. It is beyond the scope of this paper to settle whether Beatty is correct that relative frequency questions are scientifically uninteresting, and that controversies about them are not explanation-relevant. But, as I argue below, Beatty’s worry is plausible for at least some relative frequency controversies. Further, even if relative frequency controversies do turn out to be about scientifically interesting questions, engaging with Beatty’s worry provides the occasion to see that whether these controversies are rational is independent of whether they are about scientifically interesting questions.

Asking whether relative frequency controversies contribute to scientific explanations is much narrower than asking whether they are about scientifically interesting

[9] It’s also a relative frequency controversy in its own right! For a discussion, see (Turner 2015).

[10] I intend the following discussion to be neutral among competing characterizations of scientific explanation.

questions. Scientifically interesting questions are questions that are directly relevant to furthering the aims of science, where those aims may be epistemic or practical. And relative frequency questions are often scientifically interesting in this sense. Ecologists who study invasive species, for example, have determined that invasions within a continent are far more common than invasions across continents (Mueller & Hellmann, 2008). This knowledge about relative frequency guides conservationists as they allocate limited resources for combating invasions.

But we cannot dismiss Beatty's worry simply by pointing to cases where relative frequency questions turn out to be scientifically interesting because they further a practical aim of science. In many cases, determining a relative frequency distribution does not further science's practical aims. This is plausibly the case for determining how many intra-plate volcanoes that are produced by mantle plumes and how many are produced by shallow plate tectonic processes. As far as anyone knows, this is not a project with any practical applications. Evaluated prospectively, it is not a project that is likely to have practical applications. More importantly, even if it turned out that knowing the relative frequencies of volcanoes produced by these different processes did advance a practical aim of science, this practical benefit would only be a by-product of a controversy that the participants take to be about an epistemically important issue.

These, then, are the kinds of cases Beatty is worried about: relative frequency controversies that participants believe are epistemically important, but in fact are not. Such controversies are not *about* anything scientifically interesting, even in the cases where they lead to scientifically interesting results. If it is true that many relative frequency controversies are not about scientifically interesting questions, then either these controversies are not epistemically rational, or the common assumption that epistemically rational controversies must be about scientifically interesting questions is false.

This worry that relative frequency controversies may not be about scientifically interesting questions raises the issue of how one determines what a relative frequency controversy is actually about. So far, I have assumed that a scientific controversy is about whatever the participants in the controversy understand it to be about. But perhaps my assumption is wrong. Perhaps participants in a scientific controversy can be mistaken about what the controversy is about. If so, a controversy may appear to be about relative frequency because its participants believe it is about relative frequency, when in fact it is about something else.

It is true that people involved in a disagreement sometimes change their interpretation of the disagreement such that what they initially thought the disagreement was about is not what they think the disagreement is about at a later time. It is also true that people observing a disagreement often have a different view of what the disagreement is about than the participants in the disagreement do. But neither of these facts indicates that there is some participant-independent fact of the matter as to what controversies are *really* about, or that we have any more reliable way of determining what controversies are about than taking the word of participants at face-value. I am skeptical that there is a better method for identifying what a controversy is about, and at any rate, there is no developed alternative. Invoking the idea that relative frequency controversies might actually be about something other than relative frequency strikes me as an ad hoc attempt to explain their puzzling nature rather than a well-motivated proposal.

We can now return to Beatty's worry that relative frequency controversies are not about scientifically interesting questions. I will not argue that Beatty is correct, but rather that the contributions, both epistemic and practical, that relative frequency controversies make to science are independent of whether the questions they are about are scientifically interesting. So, while it is worthwhile to ask whether relative frequency controversies are about scientifically interesting questions, this consideration does not matter for determining whether these controversies are epistemically rational.

5 Epistemic contributions of relative frequency controversies

In this section I show that relative frequency controversies can make at least two kinds of epistemic contribution to science: they can improve scientific explanations of the phenomena they are about, and they can reveal previously unnoticed underdetermination problems. Other kinds of scientific controversy make these epistemic contributions as well, but in the case of relative frequency controversies, these contributions can occur when relative frequency controversies are not resolved, and even if they are not about scientifically interesting questions.

5.1 Improved explanations

Relative frequency controversies improve scientific explanations by clarifying the scope of the theories under dispute. Angela Potochnik (2015, 2017) has argued that specifying the scope of a causal relationship is a central task of explanation. A satisfying explanation should not merely identify a causal relationship that can produce the phenomenon of interest. It should also provide information about *scope*, that is, the conditions under which we expect the casual relationship to produce the phenomenon of interest. Specifying the scope of a causal relationship shows how particular events fits into the larger causal structure of the world.

For example, it is somewhat explanatory to say that lactose tolerance depends on inheriting a particular variant of the MCM6 gene from one parent (Liebert et al. 2017). When explaining the phenomenon of lactose tolerance, however, it is better to provide additional information about the scope of the relationship between the MCM6 gene and lactose tolerance. For example, it is relevant to point out that people who do have one of the relevant variants of MCM6 can still experience symptoms of lactose *intolerance* if their gut bacteria do not metabolize lactose efficiently (He et al. 2008), and that symptoms of lactose intolerance in people who do not have one of the relevant MCM6 variants can be relieved by changing the composition of their microbiomes (Forsgård, 2019). The relationship between lactose tolerance and possessing the MCM6 gene is not without exception, and noting this fact about the scope of the relationship improves the overall explanation of the phenomenon of lactose tolerance.

Such information about the scope of a causal relationship improves explanations in at least two ways. First, it provides a better understanding of the phenomenon we are interested in explaining by telling us (a) when we should we expect to observe the phenomenon of interest and (b) how it is or is not related to similar phenomena. Our understanding of the scope of the relationship between lactose tolerance and the MCM6 gene, for example, tells us that we should often but not always expect lactose tolerance in someone who possesses the relevant genetic variant. Second, information about scope also gives us a better understanding of the causes of the phenomenon of interest by telling us how general and robust those causes are. In the case of lactose tolerance, we know that the causal efficacy of the MCM6 gene can be interrupted by the presence of certain gut bacteria.

How does all of this relate to relative frequency controversies? Relative frequency controversies take place when scientists have identified more than one causal relationship that can produce the phenomenon they are interested in—speciation, the evolution of a new trait, intra-plate volcanoes, etc. What they do not yet know is the scope of these different causal relationships. They do not know enough about the conditions favoring punctuated versus gradual speciation to be able to classify particular speciation events as caused by one process or the other, and of course, they are also unable to say how frequent the different processes are.

The process of fighting about relative frequency often leads to a better understanding of the scope of the causal relationships at issue. An ongoing controversy in evolutionary biology about the relative frequency of genetic versus extra-genetic inheritance mechanisms has, for example, inspired a large volume of research into extra-genetic inheritance, which was a poorly understood phenomenon throughout the twentieth century. As a small group of biologists became more and more interested in extra-genetic inheritance, they raised the possibility that it could be important in evolution, challenging the orthodox view that evolutionary change is driven exclusively by genetic variation and transmission. This claim has sparked a fierce controversy, with some scientists claiming that extra-genetic inheritance "can have profound effects on adaptive evolution and speciation" (Jablonka, 2017, p. 4), while many others remain skeptical that the evolutionary effects of extra-genetic inheritance mechanisms are particularly interesting. Both sides have recognized that not enough is known about how extra-genetic inheritance works and have called for more research and more funding to address unanswered questions. This research has had (and continues to have) the effect of improving our overall understanding of the causal relationships between a variety of extra-genetic inheritance mechanisms and evolutionary change.

The contribution of the relative frequency controversy in this case is not merely to inspire research that ultimately contributes to the project of explanation. The connection between the controversy and an improved understanding of the scope of important casual relationships is more direct. The exact question at issue—how much evolutionary change is attributable to extra-genetic inheritance and how much to genetic inheritance—is a question about the scopes of these two different kinds of mechanisms. In order to make progress on the relative frequency question that is the subject of debate, scientists often investigate scope. The resulting insights are explanation-relevant and thus a genuine epistemic contribution.

This connection between relative frequency and explanatory scope offers a plausible explanation for why so many relative frequency controversies fizzle out. The better grasp we have of the scope of the different causes producing the phenomenon of interest, the less important determining a more precise distribution of relative frequency becomes. It may seem, in the early days of a controversy, that answering the relative frequency question really matters. But as scientists' understanding of the scope of the phenomena they are investigating becomes more complete, the relative frequency question fades into the background. Why? Because once the conditions under which you are likely to see selection vs. drift, or punctuated vs. gradual speciation are known, researchers are able to do much of what they could do with a precise relative frequency distribution. They can estimate roughly how important a given phenomenon (e.g. extra-genetic inheritance) is to producing another phenomenon (e.g. microevolutionary change). They can also identify or make educated guesses about the cause of a specific event, such as a particular instance of speciation, by asking whether the conditions leading up to the event favored one or another sort of cause.

None of this means that increasing our understanding of scope is the same thing as determining a relative frequency distribution, however. We might find a genetic signature of sympatric speciation (i.e. reproductive isolation without geographical separation) that allows us to determine with great accuracy whether any species evolved in sympatry. But this genetic signature wouldn't necessarily tell us what factors (to do with environments, population structures, etc.) tend favor sympatric speciation over allopatric speciation (speciation due to geographical isolation preventing gene flow). In such a case, we could estimate the relative frequency of allopatric speciation without understanding why it occurs when it does. The converse is also true. Though scientists have actually learned quite a bit about the conditions that favor sympatric speciation, those discoveries have not translated into a precise or accurate relative frequency distribution, though it has led to a declining interest in the debate itself (Bird et al. 2012, p. 176).

If this is the right explanation for why relative frequency controversies so often fizzle out, then fizzling out does not demonstrate the controversy is epistemically irrational; instead, it's a sign that the epistemic contribution the controversy has the potential to make has indeed been made. Still, a controversy that fizzles out has not been resolved, because the question participants take the controversy to be about has not been resolved. Instead, it has been abandoned, because the original question no longer seems interesting in light of these epistemic advances related to explanatory scope.

This connection between relative frequency and explanatory scope also shows how the epistemic value (and thus, rationality) of relative frequency controversies is independent of whether they are about scientifically interesting questions. Even if determining a relative frequency distribution is a boring, uninteresting pursuit along the lines of mere enumeration, trying to determine the distribution often involves finding information about the scope of the phenomena under investigation. And because this information about scope improves the quality of scientific explanations, these controversies further at least one of the epistemic aims of science.

Now, I haven't quite shown that relative frequency controversies are epistemically rational. There is still the issue of efficiency, or opportunity cost: is there not a better way to learn about the scope of causal relationships than by fighting over relative frequency? I allow that this is possible, and to the extent that relative frequency controversies are inefficient relative to an alternative way of learning about the scope of certain causal relationships, my claims about epistemic value and rationality are undermined. To really get into the issue of efficiency, however, is beyond the scope of this paper. My goal here is to show that a mismatch between the stated purpose of a controversy and the epistemic contribution of the controversy is not on its own a good reason to classify a controversy as epistemically irrational or a waste of time.

5.2 Uncovering underdetermination

A second kind of epistemic contribution relative frequency controversies can make is allowing scientists to realize when some data do not support the conclusion they have been taken to support. Relative frequency controversies involve fights about multiple mechanisms or theories, all of which are plausible explanations of a particular phenomenon. In the controversy about extra-genetic inheritance, for example, both genetic and extra-genetic mechanisms can explain the phenomenon of micro-evolutionary change. Since it used to be true that most scientists believed genetic inheritance explained all cases of micro-evolutionary change, a lot of data that are consistent with this hypothesis were taken as evidence for it. When extra-genetic inheritance emerged as a plausible alternative explanation for micro-evolutionary change, however, it became clear that much of the data are consistent with both types of mechanism. Rather than being evidence for the overwhelming frequency of genetic inheritance, the data fail to discriminate between cases where extra-genetic inheritance produces micro-evolutionary change and cases where genetic inheritance produces micro-evolutionary change (Pigliucci et al. 2006).

This situation in which the data fail to discriminate between two rival theories or mechanisms is a particular kind of underdetermination problem called contrast failure (Forber, 2009). Recognizing contrast failure and then devising new data collection methods to overcome it is an extremely important part of the scientific process, and in many cases, relative frequency controversies are the mechanism by which this kind of underdetermination is uncovered.[11] Once scientists realize that multiple causal factors produce a phenomenon of interest, the relative frequency debate tends to involve re-examining the evidence that was previously taken to speak in favor of only one causal factor and discovering when this evidence speaks in favor of Factor X *as much as* it does Factor Y. The reason that relative frequency controversies often play this role is turns out to be quite interesting.

[11] They can also be a mechanism by which new processes which can produce the phenomenon of interest are discovered. Mary Jane West-Eberhard (1984) did not propose the sensory bias model for sexual selection (in which mating preferences evolve from pre-existing sensory preferences) until 1984, years into the ongoing debate about good genes versus arbitrary choice, and peripatric and parapatric speciation were not recognized as mechanisms until the sympatric vs. allopatric controversy was well underway.

Relative frequency controversies are often a stage through which longer-term scientific disagreements progress. Consider the following example. There was a time when biologists thought all speciation was allopatric (Mayr, 1947). When John Maynard Smith (1966) pioneered the idea of sympatric speciation, people argued whether the sympatric speciation mechanism was even *possible*. Eventually, after the theoretical possibility of the idea was demonstrated, the fight shifted to whether sympatric speciation ever *actually* occurred in nature. Once careful empirical work produced plausible real-life examples (e.g. Feder et al. 1988; Savolainen et al. 2006), the focus of the controversy shifted again, to the relative frequency of allopatric, sympatric, and other speciation mechanisms.

This is a common pattern. Often, relative frequency becomes an issue only after a theory's possibility and actuality are established.[12] There are exceptions to this pattern: Gould, for example, claimed that punctuated equilibrium was the primary speciation pattern before there were any convincing examples of it. Even in such cases, however, the ensuing debate often focuses on developing actual examples before transitioning into a fight about frequency. So, although this logical ordering of stages doesn't always map on to the exact chronology of a controversy, it still captures a useful generalization.

The usefulness of this generalization is that it places fights about frequency into conversation with other fights that scientists have. It helps us see that these fights are not wholly independent forms of scientific controversy. Instead, they are linked to more paradigmatic forms of controversy. The lifetime of a controversy may include arguments about possibility, actuality, frequency, and significance. It may also include arguments about the appropriate methodologies or instruments for studying a process, or arguments about how to best characterize the questions at the center of scientific investigation.

Sometimes, this context helps to explain the bitterness of a particular controversy. Part of why the extra-genetic inheritance controversy is so heated is that in the middle of the twentieth century, the idea that *all* inheritance is genetic was viewed as central to the definitive statement of evolutionary theory known as the Modern Synthesis. Since then, biologists' interpretations of the core commitments of the Modern Synthesis have softened and evolved, but the debate about inheritance is still reckoning with evolutionary biology's history of absolutism about genetic inheritance (Pigliucci & Müller, 2010).[13]

This context also explains why it is so common for relative frequency controversies to uncover underdetermination problems. Relative frequency controversies often arise after a long history of assuming that a single theory or mechanism suffices to explain every instance of some phenomenon. This assumption leads investigators to classify data as supporting this single theory or mechanism without ever seriously asking whether the same data might not support some alternative theory or

[12] The tendency to start with what Adrian Currie (2019) calls simple, "one-shot" hypotheses is it not necessarily an epistemic failing; rather, it could be an epistemically valuable strategic move that put scientists in a better position to investigate complex combinations of causal factors farther down the line.

[13] See Jiggins (2006) for a similar analysis of the development of the sympatric speciation controversy.

mechanism as well. Once these alternatives arise and become the subject of relative frequency controversies, the controversies can provide an occasion for re-examining data and finally recognizing cases of contrast failure.

Discovering underdetermination problems certainly furthers the epistemic aims of science. That relative frequency controversies are so often a mechanism for doing so is a second way in which they make a valuable epistemic contribution to science. And, as in the case of improving scientific explanations, this contribution does not depend on relative frequency controversies reaching resolution or being about scientifically interesting questions. Thus, we see for a second time that, provided they do not violate the efficiency condition, relative frequency controversies are epistemically rational, despite the fact that they violate widespread assumptions about what epistemically rational controversies are like.

6 Peer disagreement and epistemically rational controversies

The primary claim I have defended in this paper is that, unlike many other kinds of scientific controversies, relative frequency controversies contribute to science even though they are rarely resolved, and independently of whether their animating questions are scientifically interesting. A consequence of this claim is that neither resolution nor being about a scientifically interesting question are necessary elements of an epistemically rational controversy.

To make my argument, I began with an insight from recent research on the social structure of science: that individual and group rationality are somewhat independent from one another. While this "independence thesis" has most often been used to better understand the optimal epistemic division of labor in scientific research, I have used it to clarify and develop the concept of an epistemically rational scientific controversy. This methodological choice means that in many ways the argument of this paper has been orthogonal to questions about the rationality of controversy that arise in the epistemological literature on peer disagreement.

Yet there is an important connection between the peer disagreement literature and the argument of this paper. In the peer disagreement literature, the default for thinking about rationality is in terms of whether individuals are behaving in ways that will lead them to have true beliefs and reject false ones. The primary question in this literature is how peers who disagree should respond to the higher-order evidence provided by the fact that they disagree with one another. In particular, philosophers have debated whether an individual is justified in holding fast to their belief or credence in a controversial proposition in the face of this kind of higher-order evidence.

Both Richard Feldman (2006, p. 214) and Jon Matheson (2015, p. 144–7) have recently argued that though we may not have epistemic reasons to hold fast to controversial propositions if we are solely concerned with epistemic *justification*, we may have epistemic reasons to hold fast to controversial propositions if we are also interested in creating the conditions for successful inquiry. Their argument is similar in many ways to the view I have defended here. In fact, my account of the epistemic value of relative frequency controversies identifies real-world examples of the epistemic benefits that can result from individuals holding fast to controversial propositions.[14]

What we have, then, are some particular cases where irrational individual responses to peer disagreement appear to generate epistemic rationality at the group level. These cases raise further research questions: What is the scope of the connection between (a) possible responses to the higher-order evidence provided by peer disagreement and (b) the epistemic contributions such responses to disagreement can make at group level? How systematic is the relationship between individual responses to peer disagreement and epistemic benefits to a community of inquirers? Researchers who study the social structure of science have used game theoretic models to explore systematic relationships between individual methodological choices and community-level epistemic benefits (Mayo-Wilson et al. 2011). They have also shown that individuals who are irrational in the sense of possessing extreme beliefs can epistemically benefit epistemic a research community, though only under particular conditions. (Zollman 2010).

This excellent research notwithstanding, the question whether there is a systematic connection between particular responses to peer disagreement and the kinds of epistemic goods that constitute epistemic rationality at the group level remains unexplored. While my primary goal has been to defend the epistemic rationality of relative frequency controversies, reflection on these controversies also suggests that there is much more to learn about interactions between individual and group (ir) rationality, particular in the context of long-standing controversies.

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[14] Though it would be a mistake to take any of my claims in this paper as establishing the rationality of other kinds of unresolved, polarizing controversies! Whether the considerations that establish the rationality of relative frequency controversies apply to other kinds of cases would have to be investigated separately.

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