Disagreement and Consensus in Science

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1. Introduction

Consensus and disagreement play important roles in the practice, development, and dissemination of science. The presence of a consensus on a scientific theory is often taken to indicate that the theory is at least substantially correct, which in turn prompts scientists to rely on the theory as a background or auxiliary hypothesis (e.g. for the purposes of testing other theories) and to present the theory as true in testimonial interactions with laypeople. Contrariwise, the presence of a sufficiently widespread scientific disagreement on a theory is frequently taken to indicate that it is not yet settled fact, which in turn makes it problematic to rely on the theory as a background or auxiliary hypothesis, and arguably disingenuous to present it as true in public scientific testimony.[1]

These uses of scientific consensus and disagreement raise important philosophical questions. Some of these issues are conceptual: When, exactly, does a scientific agreement count as a consensus? And in what sense, if any, is disagreement the opposite of consensus? (These questions are addressed in section 2.) Other questions concern the role of consensus and disagreement in the development of science. For example, is consensus on central methodological issues and assumptions necessary for scientific work to proceed normally? (See section 3.) Yet other questions are epistemological. From a layperson's perspective, does the presence of a scientific consensus ever indicate that the relevant theory is probably correct? If so, what are the conditions under which it does so? (See section 4.) Relatedly, should scientists themselves also defer to the consensus position among their peers whenever such a consensus

[1] The idea that scientific consensus strongly indicates that something is true is defended by, among others, Oreskes (2019) and Vickers (2022). The idea that science reporting to laypeople should focus on consensus theories, and indeed on reporting the consensus itself, is most prominently defended by Lewandowsky et al. (2013) and van der Linden et al. (2015).

exists? Or should they instead evaluate consensus theories for themselves, or even actively aim to dissent against such theories? (See section 5.)

2. Conceptual Issues Concerning Disagreement and Consensus

Let us start with some conceptual issues surrounding disagreement and consensus in science. While disagreement and consensus are clearly in some sense opposites of one another, the relation between them may not be as simple as one being the negation of the other. After all, it seems possible for a scientist to disagree with the scientific consensus on some theory, in which case scientific consensus and disagreement can co-exist. Furthermore, while it is perfectly natural to claim that two (or a few) scientists have a scientific disagreement about something, it is at best awkward to say that two (or a few) scientists could reach a scientific consensus. This indicates that the concept of a scientific consensus applies primarily to larger groups of agents, whereas the concept of scientific disagreement applies more widely. To be precise, then, we should say that the opposite of a scientific consensus is a specific type of disagreement, viz. widespread disagreement within a sufficiently large collection of scientists. We may call that a *scientific dissensus.*

Although consensus seems compatible with *some* disagreement, there are clearly limits to this compatibility. If too many of the agents in a given group disagree with the most popular position, e.g. in that 49% of them oppose the remaining 51%, then we seem to have a mere majority or plurality rather than a consensus. On the other hand, requiring 100% agreement for consensus seems excessive, as it would rule out many apparently legitimate applications of the term, such as the reported 97% consensus on anthropogenic global warming (Cook et al. 2013). There is arguably no single correct way to draw a line, in numerical terms, between consensus and dissensus. Rather, it presumably depends on the context how much agreement is required for consensus in the case at hand.

Regardless of where we draw the line between consensus and dissensus, it may be helpful to distinguish each of these terms from other related concepts with which they are sometimes confused. Although 100% agreement is arguably not necessary for consensus, it would constitute an especially strong form of consensus that may be of special interest (see, e.g., Tucker 2003, Dellsén 2021). Let us call it *unanimity*. In cases of non-unanimous consensus, it will be helpful to have a term for the state of disagreeing with the consensus position. Let us call that *dissent*. Dissent is thus incompatible with unanimity but compatible with consensus. Note that by this definition, there is a point at which increasing disagreement with the majority position turns dissent (against a consensus) into an opinion distribution in which there is no dissent, since there is no longer a consensus against which the dissent would be directed.

Thus far we have focused on how much agreement (or disagreement) is required for consensus (or dissensus). But is consensus (dissensus) merely a matter of numerical (dis-)agreement? Suppose that a given theory is endorsed by a slight majority, or even a minority, of the scientists in a given field, but that this group contains all of the most scientists that are most influential in their field. And suppose further that the remaining scientists, i.e. those who oppose the theory or take no stand on it, have little or no influence on what is investigated and published, and are thus largely irrelevant to the major discoveries and developments of the field. In such a case, it seems in some ways natural to describe the relevant scientific community as having reached a consensus on the theory, even though the would-be dissenters constitute a large minority or even a majority of the relevant scientists. This may suggest that the presence of a consensus on some theory depends, at least in part, on the functional role of those who endorse it – or, put differently, on the extent to which the theory is 'entrenched' or 'institutionalized'. In this way, attributing a consensus to a collection of individuals may be similar to attributing propositional attitudes to group agents (Gilbert 1989, Lackey 2020).

A final set of conceptual issues concern what type of attitudes the relevant individuals must have in order for these attitudes to jointly constitute a consensus. The most straightforward answer is that a sufficiently high proportion of the individuals must *believe* the relevant claim. However, for reasons that are familiar from discussions of disagreement in epistemology, this suggestion might need to be refined in two ways.

First, most epistemologists acknowledge that at least one variety of belief comes in degrees, i.e. that agents have *credences* as opposed to, or in addition to, having *outright beliefs*. Rowbottom (2018) argues that we should understand disagreement at least partly in terms of having non-identical credences, and develops various credence-based conceptions of disagreement. Similarly, one legitimate sense of the term 'consensus' arguably refers to cases in which a sufficiently large proportion of individuals have identically or similarly valued credences in some claim. By way of a more precise definition, we may to a first approximation say that a *credence-consensus* regarding a claim exists just in case, for some reasonably small margin ε, there is a number c such that the credences of a sufficiently high proportion of the population fall between $c - \varepsilon$ and $c + \varepsilon$. Note that such a credence-consensus might exist even if the agents in question do not also have outright beliefs in similar proportions, so as to create a consensus in terms of outright beliefs.[2]

A second way in which the idea that consensus is determined by outright beliefs may need to be refined concerns various other types of propositional attitudes that, when held by a sufficiently high proportion, arguably may constitute a type of consensus. Consider, for example, the notion of *acceptance* as developed by Cohen (1992), which arguably plays an important role in scientific practice (Elgin 2017) and our understanding of at least some disagreements therein (Elgin 2010). As Cohen and Elgin use the term, acceptance is a voluntary attitude in which one has a policy of treating something as true in a given context; belief, by contrast, is an involuntary attitude in which one is involuntarily disposed to regard something as true.[3] For example, a scientist may treat a theory as true for the purposes of making predictions, providing explanations, and so on, even while feeling that the theory is extremely speculative and thus unlikely to be true. Now, if a sufficiently large proportion of agents in a population accept some theory in this sense, regardless of whether they also believe it, then it seems appropriate to attribute to the population a type of consensus. We may call that an *acceptance-consensus*.

[2] Of course, the general notion of (dis)agreement may similarly be analyzed not only in terms of differing outright beliefs but also in terms of credences that diverge to a sufficient degree (see Rowbottom 2018).

[3] A related notion of acceptance is famously employed in van Fraassen's (1980) *constructive empiricism*. However, van Fraassen's notion of acceptance does involve a belief in a restricted part of a the content of a theory, i.e. the belief that the theory is empirically adequate (for discussion, see Dellsén 2017).

3. Explaining Disagreement and Consensus

A well-known tradition within philosophy of science, associated most prominently with the work of Kuhn (1970) and to a lesser extent that of Lakatos (1968), is centrally concerned with explaining the development of science, i.e. how science changes across time. Scholars within this Kuhnian tradition present historiographical explanations and then draw from them various philosophical lessons about, for example, scientific truth and rationality. Some of the central pieces of evidence for and against these historiographical explanations concern patterns of disagreement and consensus during various historical episodes, such as the initial disagreement between proponents of Ptolemaic and Copernican astronomy and the eventual consensus on the latter. In this section, we will briefly examine the Kuhnian tradition in philosophy of science with a particular focus on the role of consensus and disagreement.

Following Laudan (1984), let us start by considering a rather naïve, pre-Kuhnian view of science.[4] On this naïve view, scientific change is a strictly rational process in which each scientist comes to accept a theory if and only if it meets some universal, timeless standards of scientific rationality. On this view, scientific change would seem to be largely cumulative, i.e. such that each new theory is an addition to, rather than a replacement of, previous theories. After all, the universal, timeless standards of rationality would presumably not often license one inference today and a contradictory inference tomorrow, except perhaps in the unlikely event that the earlier inference was based on evidence that was later discovered to be misleading. Moreover, and importantly for our purposes, this view implies that decisions about whether to accept a given theory would be more-or-less consensual, since every rational scientist would be following the same standards for theory acceptance. Widespread and persistent scientific disagreements should be rare or non-existent.

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[4] Laudan calls this view 'the Leibnizian ideal' and attributes it to "most logicians and philosophers of science from the 1930s through the 1950s" (Laudan 1984: 5). I am not sure Laudan is right to imply that this view was so popular among actual philosophers (as opposed to conveniently constructed straw men thereof). However, the view does serve as a useful foil with which to contrast Kuhn‘s account of the development of science, so I‘ll follow Laudan in prefacing Kuhn‘s account with it.

This naïve view of the development of science came to be heavily criticized towards the latter half of the 20th century, as historical studies of scientific change began to show how frequently scientists have persistently disagreed with one another, even on theories on which there was a great deal of empirical evidence. Kuhn's (1957) early work on the Copernican revolution in astronomy, for example, emphasized the extent to which the disagreement between competing theories was persistent and was not simply settled by rational evaluation of an increasing body of empirical evidence, as the naïve view would seem to imply. With that said, Kuhn did not claim that scientists were normally in a constant state of disagreement about all theories. Rather, Kuhn came to see the history of science as characterized by long periods of stability, in which consensus is the typical state of affairs, which are interspersed with briefer periods of upheaval, in which disagreement reigns.

It was to explain this historical pattern that Kuhn proposed the notion of a *paradigm* (or, as he later called it, a *disciplinary matrix*). Very roughly, a Kuhnian paradigm can be thought of as consisting of a set of factors that determine how science within a particular discipline is normally carried out. These factors include central theories and assumptions that are taken for granted within the field, definitions of important terms, norms and methodologies that guide work within the discipline, and model examples, i.e. 'exemplars', of how to approach a scientific problem. For example, 18th and 19th century physics was conducted within a Newtonian paradigm consisting of, among other things, Newton's laws of motion and gravitation, definitions of 'mass' and 'motion', the Newtonian methodology of attempting to derive theories directly from empirical phenomena in conjunction with theoretical assumptions, and exemplars such as Newton's remarkably successful explanations of Kepler's laws of planetary motion.

Equipped with this idea of a paradigm, Kuhn was able to offer a promising explanation of both periods of consensus and of dissensus. When consensus is the norm in a given discipline, it is because the scientists in the discipline are all working within the same paradigm; Kuhn refers to this as *normal science*. The existence of a shared paradigm in periods of normal science explains consensus in two distinct ways. First, since the paradigm itself *includes* certain central theories, anyone who disagrees with those theories would thereby be rejecting an important part of the paradigm itself. In the absence of a viable alternative paradigm for the discipline, this would amount to giving up on doing science within the discipline altogether. Second, the attitudes that scientists take towards other non-central theories (i.e. to theories that do not themselves partly constitute the paradigm) is normally determined by the combination of central theories, definitions, methodologies, and exemplars of which the paradigm consists. In short, the paradigm tells scientists which new theories to accept. Thus the paradigm does not normally make room for disagreement amongst scientists on non-central theories either, except perhaps on minor or tangential details. In sum, then, the shared paradigm ensures that consensus becomes the norm within the discipline.

Kuhn can also invoke the notion of a paradigm to explain the briefer periods of upheaval in which disagreement reigns as stemming from the sudden absence of a shared paradigm. Every once in a while, some scientists will become disillusioned with the current paradigm's ability to solve what they consider its central problems, or *anomalies*. Consequently, these scientists will start to look for a new paradigm that does better in this regard. If such a paradigm is found, scientists may convert from one paradigm to the other; in addition, budding scientists, who are deciding where to place their allegiance, may opt for the new paradigm as opposed to the earlier one. For example, Kuhn might say that the Newtonian paradigm mentioned above was replaced in the 20th century with a relativistic paradigm due to Einstein, not just because various progressive physicists converted from one to the other, but also because younger physicists were significantly more inclined to work within the new and exciting relativistic paradigm than the old Newtonian one. This type of transition from one paradigm to another is what Kuhn called *revolutionary science*. For our purposes, the important thing about revolutionary science is that it characterizes periods in which there is no single paradigm shared by all of the discipline's scientists. Accordingly, disagreement is to be expected during revolutionary science, since the different paradigms in play during such periods will often differ in their verdicts on which theories to accept. These disagreements will persist as long as the discipline in question has not settled on a single shared paradigm to adjudicate such disagreements.

So Kuhn offers some intriguing potential explanations of the patterns of consensus and disagreement that many historical studies of science describe. But are Kuhn's explanations convincing? Although in many ways sympathetic to Kuhn's account, Laudan (1984: 16-19) criticizes Kuhn for failing to offer a compelling account of how a consensus emerges out of a dissensus. Why should the scientists who are on one side of a dispute between proponents of different paradigms during revolutionary science ever replace their paradigm with that of their opponents? Presumably, this can only happen if the relevant scientists are able to recognize that the alternative paradigm is superior. However, seeing another paradigm as superior to one's own seems in tension with Kuhn's notorious claim that different paradigms are *incommensurable*, in the sense that scientists who adopt different paradigms lack any shared standards on the basis of which to adjudicate disputes. In short, Laudan's point is that Kuhn cannot both claim that competing paradigms are incommensurable and that the replacement of one paradigm with another is explicable on his account. The problem is especially serious because consensus often emerges very quickly out of a revolutionary period, whereas Kuhn's account seems to imply that consensuses should emerge very slowly or not at all.

A potential post-Kuhnian solution to the problem of consensus formation in science would be inspired by work in the social epistemology of science. It is well-known that scientists do not invariably, or even usually, evaluate individual theories by assessing the scientific evidence for and against such theories. Instead, much of the time, scientists rely on information provided by other scientists both regarding the existence and character of scientific evidence and regarding the extent to which that evidence supports a given theory (Hardwig 1985; Fricker 2002). In short, scientists often place a great deal of trust in each other's judgment, so that even when only a relatively small minority of scientists have come to a conclusion about some theory, this conclusion can quickly be transmitted to other scientists, e.g. via testimony. On this view, then, the quick emergence of consensus, even on very complicated or delicate scientific issues, can be explained by social interactions in the presence of trust between scientists (see, e.g., Zollman 2012; Grim et al. 2013).

Interestingly, this testimony-based explanation of how consensus emerges in science breaks down in certain readily-identifiable cases, which in turn explains why scientists sometimes persistently disagree with one another: First, the explanation clearly does not apply when scientists don't communicate their results and opinions to one another. In such cases, consensus formation should be a considerably slower process. Relatedly, this explanation breaks down if there is little or no trust between scientists, such that the information provided by one of them is not taken in by her peers. Finally, in cases where the small minority of scientists who have considered the scientific evidence itself and evaluated a theory accordingly do not themselves agree, other scientists will receive mixed signals on whether the evidence supports the theory, e.g. with some scientists speaking in its favor and others against it. In that case, it seems plausible that the recipients of this information, i.e. the remaining scientists, should at least initially either become agnostic or themselves be similarly split between accepting and rejecting the theory.

4. Learning from Disagreement and Consensus

Let us turn now to the epistemological question of what can be learned from the presence of a scientific consensus – or indeed from its absence. As intimated in the introduction, consulting a scientific consensus is often the most reliable way, and sometimes the only realistic way, for laypeople to assess whether a scientific claim is correct (Anderson 2011). This idea can be supported by various formal results, such as the Condorcet Jury Theorem, which seem to demonstrate that under certain conditions a majority opinion among experts is a very reliable indicator of the truth (Estlund 1994, Odenbaugh 2012). Although these results typically make non-trivial assumptions about a certain kind of independence among experts and their individual reliability, there are there are various ways of relaxing these assumptions so as to broaden the applicability of the results (e.g., List and Goodin 2001), and some empirical reasons to think that these (weakened) assumptions are often satisfied (Surowiecki 2004).

With that said, it is of course logically possible for a consensus to form on a false theory. The history of science indicates that a scientific consensus is sometimes quite radically mistaken, even regarding the core posits of the consensus theory (Laudan 1981). Moreover, the reliability of results obtained in some scientific subdisciplines, e.g. social psychology and cancer research, have been questioned on the grounds that many such results are not replicable (Begley and Ellis 2012, Open Science Collaboration 2015). Finally, there are documented cases in which a scientific consensus has been reached for non-epistemic reasons (Beatty 2006). These concerns may not apply to all consensus theories, but they certainly apply to some. Thus, it would be good to have some way of estimating the reliability of consensus-to-truth inferences in different circumstances so as to be able to discriminate between those consensus theories that are more and less likely to be correct.

One approach to this problem is to identify other features of theories (i.e., features other than the fact that there is a consensus on them) which indicate that the theory is even more likely to be true than the presence of the consensus would do by itself. Some factors of this kind might refer to the relationship between the theory itself and the evidence that is taken to support it. Following Miller (2019), we may refer to these as *cognitive factors*. For example, Oreskes (2018: 45-55) argues that among the reasons to trust the consensus on anthropogenic climate change is the fact it has withstood repeated falsification attempts, that it is supported by data that the theory was not specifically constructed to accommodate, and that there are many different types of evidence in support of the theory. The last of these features, which was dubbed *consilience* by William Whewell (1858), appears also on Miller's (2013) list of requirements for a consensus to be 'knowledge-based'.

A potential problem with using cognitive factors such as consilience as a guide in deciding whether to place one's confidence in a given consensus theory is that it is doubtful that laypeople are generally in a position to evaluate the extent to which such cognitive factors are present. For example, evaluating the extent to which anthropogenic climate change is supported by diverse lines of evidence is hardly within the capabilities of most laypeople, in part because most laypeople may have no idea what those lines of evidence are, what counts as 'diverse' lines of evidence, how much diversity is required or desirable, and how to weigh this particular factor against other cognitive factors. In short, requiring laypeople to evaluate the consilience of scientific theories (or other cognitive factors of that kind) comes awfully close to requiring laypeople to become amateur scientists. That seems neither realistic nor desirable.[5]

For this reason, it would arguably be more helpful to find ways for laypeople to evaluate the reliability of a consensus by considering features of the community in which the consensus has emerged. Again following Miller (2019), we may call these *social factors*. For example, Miller (2013) suggests that two requirements for a consensus to be knowledge-based are, first, that those who form the consensus are committed to using the same evidential standards, formalisms and ontological frameworks; and, second, that they are nevertheless socially diverse in the sense of coming from different social backgrounds (see also Tucker 2003). However, one might still worry that some social factors, e.g. Miller's condition of using the same standards, formalisms and ontological frameworks, would be rather difficult for laypeople to evaluate in so far as they have little insight into the social makeup of scientific communities. In short, this approach to the problem seems to require laypeople to become amateur sociologists of science.

This suggests that the most accessible (and in some cases perhaps the only) type of factor to which laypeople can reliably appeal concerns the opinion distributions themselves, i.e. the extent to which there is or isn't a consensus on different theories. It may seem as if this makes it in principle impossible to evaluate the likelihood that a given consensus is correct. However, it may be that these opinion distributions contain within them certain indications as to whether they are themselves reliable. This idea has recently been developed in two different ways by Dellsén (2018, 2021).

On the one hand, Dellsén (2018) argues that if there is disagreement on *other* theories T1, T2, …, Tn within the same domain of expertise as some particular target theory T, then the emergence of a consensus on T is an especially reliable indicator that T is correct. For example, the fact that climate scientists disagree on a number of issues concerning the precise

[5] In effect, what would be required is massive science education for laypeople in such a way as to bridge the gap between the skills and knowledge of scientists and laypeople. This sort of idea, often labelled 'the information deficit model of science communication' has been widely discredited on various other grounds as well (see, e.g., Ziman 1991; Brown 2009).

mechanisms by which global warming is occurring is further reason to believe that their consensus position on anthropogenic global warming is correct. The reason is, roughly, that the surrounding disagreement suggest that climate scientists are in fact using varied lines of evidence (consilience), which in turn indicates that the theory is well supported by empirical evidence and thus likely true. Importantly, however, laypeople would not themselves be required to estimate the extent to which the theory is consilient; rather, they need only recognize that there is scientific disagreement on other nearby theories to evaluate the target theory as probably true.

On the other hand, Dellsén (2021) also argues that the presence of *some* scientific disagreement against a consensus theory is normally a stronger indicator of the theory's truth than if there had been no such disagreement. Put differently, a combination of consensus and some dissent on a theory is normally a stronger indication that the theory is true than strict unanimity (with no dissent). This is roughly because the presence of unanimity on any reasonably complex scientific issue suggests that the individual scientists did not form their opinions independently of each other, but are instead more likely to have engaged in some form of 'groupthink'. Given that groupthinking communities are unlikely to arrive at the truth, this in turn suggest that a unanimous community is less trustworthy than a community in which there is consensus combined with some dissent.

5. (Dis)agreeing with the Consensus

Let us turn finally to the issue of how scientists themselves should approach the presence or absence of a consensus on some matter that is within their own domain of expertise. Should they *defer* to the consensus position, or *dissent* against it, or perhaps do something else entirely? Although there is clearly no single correct answer to this question that applies universally, there are interesting epistemological and ethical issues that are relevant for how we should approach such questions in particular cases.

First of all, it is worth noting that there is a rather interesting connection to be made between this issue and the much-discussed issue of disagreements with peers or experts (e.g., Lackey 2008, Christensen 2009). The so-called *problem of peer disagreement* arises in situations where one encounters someone who disagrees with one's opinion on some proposition P, where one is aware that this someone is (roughly) equally well informed on about P and (roughly) equally competent to evaluate P. In such situations, it seems that one should become less confident in one's prior opinion being correct, e.g. because the disagreement suggests that one may have made a mistake in one's reasoning towards that opinion. If this line of reasoning is appropriate in the case of encountering a single such 'epistemic peer', then it seems that one should revise one's opinion even more strongly if one encounters an entire community of epistemic peers in which the consensus goes against that opinion. This in turn implies that scientists who dissent against a consensus position within their domain of expertise would be irrational in maintaining that position. After all, their colleagues in the field, most or all of whom would presumably count as their epistemic peers, would be disagreeing with the dissenting scientists' rejection of the consensus position.

Similar considerations suggest that those scientists who already accept the consensus position ought to increase their level of confidence in the consensus claim as they discover that they are part of the consensus (Easwaran et al. 2016). This might initially seem somewhat problematic, e.g. because it seems to involving a form of double-counting of the same evidence. On reflection, however, it is hard to see how it could be any other way – at least if and to the extent that peer disagreement should undermine one's conviction in otherwise identical cases. After all, to discover that there is a consensus on some claim just is to discover that there is little or no disagreement on that claim. So by discovering a consensus, one is effectively ruling out a potential reason to disbelieve the claim (i.e. the potential peer disagreement), which should in turn make one more confident that the consensus claim is true.

In sum, then, it seems that there is epistemic pressure for individual scientists to not only give up on dissenting opinions, but also to strengthen their conviction with respect to consensus claims. Thus the upshot appears to be that individual scientists would be epistemically rational in revising their opinions in ways that lead to a kind of conformity towards the already-established consensus.

However, there may be other reasons for individual scientists to resist conforming to a consensus position. Some of these reasons may be 'epistemic' in a broader sense that incorporates epistemic norms that are *socially* as opposed to *individually* truth-conducive (see Mayo-Wilson et al. 2011). For example, Dellsén (2020) argues that communities of experts who do not automatically defer to the majority position are more reliable in the consensus positions they reach. Thus, in so far as laypeople form their beliefs by appealing to consensus positions among scientific experts, it is socially beneficial for scientists not to defer to their fellow experts but instead to exercise a form of epistemic autonomy on matters that fall within their domain of expertise. Another type of reason for individual scientists to resist deferring to the consensus concerns the apparent long-term benefits of dissent for the scientific community in question. In particular, De Cruz and De Smedt (2013: 176) argue that scientific disagreement is valuable because it leads scientists to gather more evidence, to re-evaluate existing evidence and relevant assumptions, and to consider whether their conclusion may be due to confirmation bias. Somewhat similarly, Feyerabend (1963; see also Lloyd 1996) argued that disagreement in which a plurality of incompatible theories are endorsed by different scientists is beneficial to the progress of science, because the most reliable way to test a given theory is to compare it with as many well-developed alternatives as possible.

6. Conclusion & Related Work

The aim of this chapter has been to provide an overview of philosophical questions raised by consensus and disagreement in science. The focus has been on conceptual, historiographical, and epistemic issues. There is, however, a great deal of important work on related questions concerning consensus and disagreement that deserve mention as well. For example, there is an ongoing debate about whether, and the extent to which, scientists should be encouraged or discouraged from publicly expressing their dissent against consensus theories, e.g. on anthropogenic climate change, especially when this is liable to be misinterpreted or exploited for propaganda purposes (see, e.g., Biddle and Leuschner 2015, de Melo-Martín and Intemann 2018, Miller 2021). Another important question concerns what role non-epistemic values should and do play in the formation of a scientific consensus (see, e.g., Solomon 2001, Longino 2002). Finally, there are interesting questions about how a community of researchers who are aiming to create a consensus on some issue should go about doing so (see, e.g, Lehrer and Wagner 1981, Stegenga 2016).[6]

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