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CONSENSUS IN SCIENCE

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Because the idea of consensus in contemporary philosophy of science is typically seen as the locus of progress, rationality, and, often, truth, Mill's views on the undesirability of consensus have been largely dismissed. The historical data, however, shows that there are many examples of scientific progress without consensus, thus refuting the notion that consensus in science has any special epistemic status for rationality, scientific progress (success) or truth. What needs to be developed instead is an epistemology of dissent. I suggest that normative accounts of dissent be used as *prototypes* for theories of scientific rationality that can also be applied to episodes of consensus. Consensus in this case is to be treated as a special case of dissent, when the amount of dissent approaches zero. My main goal in this paper is to sketch how a normative account of dissent that aims to capture the idea of epistemic fairness can apply to situations of consensus.

I. BACKGROUND

John Stuart Mill thought that consensus, in science as well as other forms of inquiry, is an obstacle to progress, rationality and truth.[1] He had at least three general epistemic reasons for resisting consensus. First, since humans are fallible, a dissenting opinion might be the correct one. Second, opposed views might each be partly true and partly false, so consensus on one view would result in the loss of truth. And third, Mill claimed that even when there is consensus on a true theory, and no truth is lost by the rejection of competing theories, there is epistemic loss of "the clearer perception and livelier impression of truth, produced by its collision with error."[2] A better understanding of the meaning of a true theory is reached if one knows the pro and con arguments.[3]

Mill's views about dissent and consensus have never achieved much popularity in philosophy of science. Realists and anti-realists alike view consensus on a particular theory, method or practice as the rational endpoint of episodes of scientific change. Kitcher's 1993 statement is typical and contemporary:

> When disputes are resolved, when all the variants but one in some part of some component of individual practice are effectively eliminated, there is a change in *consensus practice*. If we are to understand the progress of science, we need to be able to articulate the relations among successive consensus practices.[4]

Dissent is seen as a necessary period when a thousand theories can bloom, yet as a *temporary* stage on the way to consensus.[5] Realists associate consensus with truth; antirealists with greater success or rationality.

The situation is similar in the wider science studies field, and in science policy. Collins and Pinch express a typical view when they write: "Science works by producing agreement among experts."[6] The recent proliferation of medical consensus conferences, in this country under the auspices of NIH and AHCPR, suggests the importance given to consensus formation in medicine.

Probably, in this audience, Paul Feyerabend immediately comes to mind as the major exception to the consensus on consensus. As Lisa Lloyd has argued, Feyerabend saw his own work as an exemplar of Mill's epistemic proposals.[7] Interestingly, for all the influence of Feyerabend on contemporary history and sociology of science, the production of narratives ending in consensus continues in those fields as well as in philosophy of science.[8] Another notable exception to the consensus on consensus is Helen Longino who argues for scientific pluralism as a goal in some fields of science, not just as a means to finding "the best theory," but as an appropriate end of inquiry.[9]

Consensus is even more important in philosophy of science than it was in the past, before the Kuhnian revolution. Kuhn convinced many philosophers of science that there are episodes of serious dissent in science, and that ideological and psychological differences among scientists cause them to take different sides in a debate. Philosophers of science found a way to absorb and counter this apparent challenge to scientific rationality.[10] They view the period of dissent as a period of investigation of alternatives, and ideological and psychological factors as ways of distributing cognitive labor among the alternatives. Then, consensus takes place on the most successful theory, which scientists choose on rational grounds such as most empirical success, most explanatory coherence, and so forth. Frequently, there are also claims about the association of consensus with truth (or, partial truth, resemblance of the theory to the world, and so forth).

The similarity to the old context of discovery/context of justification distinction is strong: dissent is the contemporary "context of discovery" where "anything goes" and consensus is the contemporary "context of justification" in this post-Kuhnian philosophy of science. Frequently (e.g., Giere, Hull, Kitcher), this is termed an "evolutionary" account of scientific change, where dissent is seen as due to "random variation" and consensus due to "survival of the fittest" theory.[11]

Thus, Mill's views on the undesirability of consensus have been largely dismissed in contemporary philosophy of science consensus in science is typically seen as the locus of progress, rationality, and, often, truth.

II. CONTRARY HISTORICAL CASES

The contemporary philosophical account of consensus makes a nice narrative, and a nice defense of scientific rationality, but it is not supported by historical data. In fact, historical data tell another story. There are many examples of scientific progress without consensus. There is no evidence that psychological and ideological factors have any lesser role at consensus than they do during dissent. And, frequently, there is consensus without truth (or approximate truth, partial truth, and so forth).

Presenting an ample number of contrary historical cases to prove these points would take a while, and it is not my goal in this paper. I am just going to indicate a range of examples (I detail them elsewhere[12]), and then move on to my main project, which is *to construct an account of scientific rationality that does not depend on granting consensus a privileged or even a positive epistemic status.*

Examples of scientific progress without consensus include theories in evolutionary biology during the "eclipse of Darwin" period at the end of the nineteenth century, the early years of the continental drift debate (1920–1950), competing theories of inheritance before the "central dogma" of Watson and Crick, viral versus genetic theories of cancer in mid-century, and, on the contemporary scene, particle physics and artificial intelligence.[13] In all these cases, there are scientific successes and insights without consensus in the participating scientific community.

Psychological and ideological factors are often necessary for coming to consensus. For example, Jim Cushing has documented the ideological conditions in Europe that favored the general acceptance of Copenhagen interpretations of quantum mechanics.[14] Evelyn Fox Keller and others have shown the ideological causes of consensus on the "central dogma" in genetics.[15] I have shown that consensus in plate tectonics depended on relations of authority, peer pressure, and cognitive biases.[16] At least one of these cases (plate tectonics), and perhaps more, are cases where the consensus was normatively appropriate.

Larry Laudan has a famous list of scientific theories which had considerable success, yet these theories turned out to be false.[17] Many of these theories were ones on which there was also consensus: e.g., the crystalline spheres of ancient and medieval astronomy, the humoral theory of medicine, catastrophist geology, the phlogiston theory of chemistry, and electromagnetic ether. I can add to this list with further examples: Newtonian mechanics, the "central dogma" account of the role of DNA, Skinnerian behaviorism and classical (linear, formal) AI in cognitive science. Consensus and truth do not go together, even when there is accompanying scientific success.

The conclusion of this kind of historical work is that there is no evidence that consensus in science has any special epistemic status for rationality, scientific progress (success) or truth.

III. Proposal

Let us turn back to the epistemology of dissent. I suggest that normative accounts of dissent be used as *prototypes* for theories of scientific rationality that can also be applied to episodes of consensus. Since the borderline between dissent and consensus is in any case unspecified—there are always dissenters, even when there is consensus—there is nothing inherently implausible in this approach. Consensus is treated as a special case of dissent, when the amount of dissent approaches zero.

The current state of the epistemology of dissent is somewhat better than "let a thousand theories bloom" or "anything goes."[18] In fact, both Philip Kitcher and Alvin Goldman have used models from economic theory to argue that dissent created by psychological factors such as desires for reward and credit can lead to a beneficial division of cognitive labor, one that will efficiently explore the plausible alternatives, each in proportion to its plausibility.[19]

I have criticized these accounts elsewhere. In particular, I have argued that they are versions of invisible hand arguments that involve unrealistic idealizations of scientific decision making.[20] However, I agree with Kitcher and Goldman that dissent should not be a time of epistemological anarchy. Rather, dissent is a time when the normative goal is to achieve a good distribution of cognitive labor. This is a *social* epistemic goal, rather than an individual one: cognitive labor is distributed across a community often without the efforts of any individual to accomplish this. It is at this point that epistemologists of dissent have become social epistemologists. They acknowledge that norms of rationality for individuals will not be an adequate epistemology for science.

I will set out my normative account of dissent without much argument here.[21] My main goal in this paper is to show how a normative account of dissent—mine in particular, but also others—can also apply to situations of consensus. One piece of terminology must be presented first: the concept of decision vectors, both empirical and non-empirical.

IV. Decision Vectors

I devised the terminology of decision vectors because I find the widespread use of the term "biasing factors" to be epistemically misleading.[22] Scientists's choices are, indeed, influenced by a variety of causes: social, psychological, ideological, heuristic, motivational, political, and so forth.

To call such choices "biased" is to suggest that more neutral thinking is both possible and desirable. But this is precisely what is in question. So, "decision vector" is chosen as an epistemically neutral term, signifying that the departure from some ideal of neutrality is not, in itself, either epistemically good or bad. Decision vectors are so called because they influence the outcome (direction) of a decision. The terminology is deliberately material (physical) rather than abstract (logical), indicating my commitment to naturalistic philosophy of science.

There are two kinds of decision vectors: empirical decision vectors and non-empirical decision vectors. Empirical decision vectors are associated with empirical success. So, for example, if a particular piece of evidence is cognitively salient (because it is, e.g., seen rather than read about) to a scientist, and that piece of evidence is associated with a particular theory, the scientist has an empirical decision vector in favor of that theory. Non-empirical decision vectors have no association with empirical success: for example, conservatism, peer pressure, ideological preferences, elegance, coherence and so forth are all non-empirical decision vectors.

V. Social Empiricism: A Normative Account of Dissent and Consensus

I will present the theory first, and some illustrative examples afterwards—although the presentation could be the other around.

The normative account aims to capture the idea of epistemic fairness: that each scientific theory receive attention in proportion to its merit. Here is my suggestion for the epistemology of *dissent*:

> (1) Theories on which there is dissent should each have associated empirical success.
>
> (2) Empirical decision vectors should be equitably distributed (in proportion to the empirical successes).
>
> (3) Non-empirical decision vectors should be equally distributed (the same number for each theory).

Some brief clarificatory comments at this point. Obviously, I equate scientific merit with empirical success—which I do not define or discuss here.[23] Also, I aggregate decision vectors without attention to their magnitude or possible interactions with one another. This is a recognized approximation which has quite good results—at least, much better than narrative approaches.[24]

Now, if this normative account of dissent is applied to the special case of consensus, where the amount of dissent becomes vanishingly small, it yields suggestions for both the formation of consensus and the dissolution of consensus.

It suggests that forming consensus is only appropriate if one theory has all the available empirical successes (otherwise other theories have merit and should be pursued) and all scientists are productively working on that theory (hence their empirical decision vectors will all be in favor of that theory—recall that that theory has all the empirical successes). So, in the special case of coming to consensus:

> (1) One theory comes to have all the empirical successes available in a domain of inquiry.
>
> (2) One theory comes to have all of the empirical decision vectors (all scientists working productively are working within the one theory).
>
> (3) Any distribution of non-empirical decision vectors is OK, but typically more will develop, over time, on the consensus theory, as the old theories fade away.

Dissolution of consensus is appropriate when the situation should revert to dissent:

> (1) A new theory has empirical success that cannot be associated with the consensus theory.
>
> (2) Empirical decision vectors come to be equitably distributed.

(3) Non-empirical decision vectors come to be equally distributed.

I have presented this theory, not because I think you will be immediately convinced of its overall plausibility, but because I hope it will at least make plausible the more general idea that normative theories of dissent can be extended to consensus.

VI. ILLUSTRATIVE EXAMPLES

A. Of Consensus Formation (less dissent)

(i) *Plate Tectonics.* Consensus took place in the mid-1960s on plate tectonics. The process of coming to consensus was gradual and distributed. As many historians of the period have observed, geologists became convinced of the plate tectonics version of continental drift as evidence for the theory was produced in their own specialty, thus salient to them: first paleomagnetists, then oceanographers, then seismologists and stratigraphers and finally continental geologists. By the time geologists came to consensus on plate tectonics, that theory had all the empirical successes. Even though geologists did not *individually* choose plate tectonics because it had all the empirical successes, they made that choice as a community.

Consensus took place on the theory with all empirical successes, and it came to have all the empirical decision vectors in its favor. At the same time, more non-empirical decision vectors in favor of plate tectonics developed: for example, the prestige of institutions such as Cambridge, Columbia and Scripps influenced geologists to take plate tectonics seriously, as did the increasing peer pressure.

(ii) *The "Central Dogma of Molecular Genetics.* Watson and Crick's discovery of the structure of DNA ushered in a consensus on the functioning of genetic material that was deeper and more widespread than the prior weight of opinion in favor of Mendelism. In the 1950s and 1960s, it was generally believed that DNA is the "master molecule" that directs cellular operations, and that it contains all the information necessary for protein synthesis. The information in DNA is translated into messenger RNA, which acts as a transcript for protein synthesis in the cytoplasm. Crick referred to this as the "central dogma."

As many historians and critics have noted the "central dogma" did not include all experimental successes available at that time in genetics.[25] Omitted or downplayed were the mechanisms of nuclear regulation, the phenomenon of genetic transposition through nuclear interactions, cases of cytoplasmic inheritance, and the means of inheritance of supramolecular structures. Results of Sonneborn, Ephrussi, McClintock, Sager and others were dismissed, ignored, and misunderstood. The consensus was thus not normative (at least, according to social empiricism's requirement that consensus occurs only when one theory has all the empirical successes). The cause of the inappropriate consensus was prior imbalance of non-empirical decision vectors: ideology institutional factors had for a long time been on the side of the nuclear monopoly, especially in the United States and Britain.

(iii) *Consensus on the Use of the EC-IC Surgical Technique.* The extracranial-intercranial bypass operation (EC-IC) was first performed in 1967 by Gazi Yasargil. It was reasoned that the bypass would increase blood flow to the brain and thus prevent stroke. Right away, the technique became popular, and when Yasargil published retrospective data on 84 bypasses ten years later, consensus solidified. Yet the technique was not shown to have empirical success: controlled studies of stroke patients who had the same up-to-date care except for the EC-IC surgery were not done. After those studies were done, in the early 1980s, the EC-IC was discovered to be ineffective.[26]

This is a case where consensus took place in the absence of empirical success. One of the causes of consensus was the faulty retrospective data. Non-empirical decision vectors also overwhelmingly favored the EC-IC: the surgery was very lucrative, it seemed based on "reasonable" thinking, and it demanded special microsurgical techniques that neurosurgeons could justifiably be proud of.

Consensus can thus be normatively appropriate or normatively inappropriate. Plate tectonics is an example of normatively appropriate consensus, and the others are examples of more or less inappropriate consensus.

B. Of Consensus Breakdown (More Dissent)

(i) *Causes and Treatment of Peptic Ulcer.* By the 1970s, there was consensus that peptic ulcer is caused by excess acid and should be treated with a combination of acid blockers and antacids. In the late 1980s and early 1990s, this consensus unraveled, and eventually a new consensus on a bacterial theory of ulcers formed. Here, I take a look at how consensus broke down. Barry Marshall and Robin Warren discovered *Helicobacter pylori* in the early 1980s. By the mid-1980s, they had strong evidence that they were associated with peptic ulcer disease, and by 1987, they had shown that antibiotics—but not acid blockers—both cured and prevented relapse in peptic ulcer disease. They had difficulty persuading gastroenterologists to take their results seriously, although infectious disease specialists were impressed. Gastroenterologists had established procedures for ulcer treatment, which they were reluctant to change. Surgery for ulcers was also extremely lucrative. Pharmaceutical companies were apathetic. Patents on acid blockers were not due to expire for a few more years

The situation changed after 1990. More data in favor of the Helicobacter theory appeared, spreading from work in infectious diseases to work in gastroenterology. A few prominent gastroenterologists supported the Helicobacter theory. Pharmaceutical companies began to have an interest in the debate. They secured their profits, in the USA, by marketing acid blockers over the counter as a treatment for heartburn and indigestion, while they developed antibiotic/antacid combinations targeted for peptic ulcer.

So the consensus on excess acid as the cause of peptic ulcer broke down as a new theory produced new empirical successes. Distribution of non-empirical decision vectors was important for producing an adequate level of dissent. The different investments of gastroenterologists and infectious disease specialists, the authority of some gastroenterologists and the change in marketing and development by pharmaceutical companies all played a role in distributing the research effort. Non-empirical decision vectors started out all in favor of the acid theory, but by the early 1990s were much more equally distributed. And the distribution of non-empirical decision vectors was important for further research on ulcers, which eventually resulted in a new consensus.

(ii) *The Debate over Cold Fusion.* In March 1989, Pons and Fleischman announced that they could produce significant excess energy in electrolytic cells, and that the explanation of this is cold fusion. The announcement caused a breakdown in the consensus that cold fusion was not a viable energy source. Electrochemists in particular were delighted that techniques from their field were overshadowing physicists, efforts to produce fusion. It looked like a triumph of "little science" over "big science," which attracted many. The reputation of Pons and Fleischman made many take their results seriously. Moreover there was immediate attention and funding from industry and from government. On the other side, physicists were conservative and skeptical. Some even argued that a significant amount of cold fusion was physically impossible. They favored results from institutions more prestigious than Utah, and they did not want their expensive and large scale work on hot fusion to get less support. Non-empirical decision vectors were, pretty quickly, equally balanced on both sides, and thus quickly produced dissent.

After a year or two, it became clear that there was no evidence for cold fusion. According to social empiricism, dissent on cold fusion was thus inappropriate: it deserved no scientific attention at all, since it had no empirical success. In March 1989, though, no one could have known that. Could scientists have done better? Several authors of books on cold fusion claim that cold fusion was "bad science" and the "scientific fiasco of the century" because of the role of media attention, funding politics, secrecy, etc.[27] According to social empiricism, it was not a "fiasco." The presence of non-empirical decision vectors such as media attention, industry funding, etc., were not a problem in themselves. It was a mistake. Perhaps such mistakes could be lessened by making sure (or, at least, surer that there is empirical success before disseminating results. Pons and Fleischman's results were released earlier than intended because of a dispute over priority with Steven Jones.

VII. Conclusion

As Mill, I have found that consensus is progressive for science far less often than it occurs. I agree with him that normative benefits of dissent are undervalued. I disagree with him, however, on the question of who benefits from dissent. Mill thought that the normative benefits of dissent—greater likelihood of knowing and understanding the truth—are reaped by the "disinterested bystander" who calmly surveys the epistemic scene.[28] Naturally, he thought that he could be such an individual. Social empiricism finds that scientific rationality depends on socially emergent dynamics and does not depend—thank goodness—on the individual rationality of any individual scientist, let alone any individual philosopher.

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Notes

1. J. S. Mill, "On Liberty," *Utilitarianism*, ed. Mary Warnock (Glasgow: William Collins Sons and Co., Ltd., 1962), 163, explicitly states that his views in "Of the Liberty of Thought and Discussion" apply to "natural philosophy," i.e., the sciences.

2. Ibid., 143.

3. Ibid., 170.

4. Philip Kitcher, *The Advancement of Science* (Oxford: Oxford University Press, 87).

5. See, e.g., D. Hull, *Science as a Process* (Chicago: University of Chicago Press, 1988), 521.

6. H. Collins and T. Pinch, *The Golem: What Everyone Should Know About Science* (Cambridge: Cambridge University Press, 1993), 148.

7. Lisa Lloyd, "Feyerabend, Mill and Pluralism," *Philosophy of Science* (Supplement) 64 (1997): S396–S407.

8. See M. Solomon, "Happily Ever After With Consensus?" *Fenomenlogica et societa* 21.1 (1998): 58–65. Here, there is not time to explore the reasons for this.

9. Helen Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Princeton: Princeton University Press, 1990).

10. See, e.g., Hull, op. cit.; Kitcher, op. cit.; Thagard, ~~*How Scientists Explain Disease* (Princeton: Princeton University Press), forthcoming~~.

11. This is a dubious view of evolutionary change as well as of scientific change.

12. Manuscript of *Social Empiricism.*

13. See, e.g., Margaret Boden, "AI's Half-Century," *AI Magazine* (Winter 1995): 96–99.

14. Jim Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* (Chicago: University of Chicago Press, 1994).

15. Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven/London: Yale University Press, 1985).

16. M. Solomon, "Social Empiricism," *Nous* 28.3 (1994): 325–43.

17. Larry Laudan, "A Confutation of Convergent Realism," *Philosophy of Science* 48 (1981): 19–48.

18. Hull, op. cit.; P. Feyerabend, *Against Method* (London: Verso, 1975)

19. Philip Kitcher, op. cit.; Alvin Goldman, *Liaisons: Philosophy Meet the Cognitive and Social Sciences* (Cambridge, Mass.: MIT Press, 1992).

20. See M. Solomon (typescript) "Is there an Invisible Hand of Reason?"

21. There is much argument in my book. manuscript, Social Empiricism

22. Some philosophers call them "social factors" which is descriptively misleading—relatively few of them are "social."

23. There is more extensive discussion in Chapter 1 of my book manuscript, *Social Empiricism.*

24. See R. Dawes, *Rational Choice in an Uncertain World* (Orlando: Harcourt Brace Jovanovich, 1988).

25. E.g., R. Burian, J. Gayon and B. Zallen, "The Singular Fate of Genetics in the History of French Biology, 1900–1940," *Journal of the History of Biology* (1988); Keller, op. cit.; Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York/Oxford: Oxford University Press, 1987).

26. F. Vertosick, "First, Do No Harm," *Discover* (July 1998): 106–11.

27. T. Huizenga, *Cold Fusion: The Scientific Fiasco of the Century* (Rochester, N.Y.: University of Rochester Press, 1992); G. Taubes, *Bad Science: The Short Life and Weird Times of Cold Fusion* (New York: Random House, 1993).

28. Mill, op. cit., 180.