Collective Belief, Kuhn, and the String Theory Community

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Abstract

One of us [Gilbert, M. (2000). “Collective Belief and Scientific Change.” *Sociality and Responsibility*. Lanham, MD: Rowman & Littlefield. 37-49.] has proposed that ascriptions of beliefs to scientific communities generally involve a common notion of *collective belief* described by her in numerous places. A given collective belief involves a joint commitment of the parties, who thereby constitute what Gilbert refers to as a plural subject. Assuming that this interpretive hypothesis is correct, and that some of the belief ascriptions in question are true, then the members of some scientific communities have obligations that may act as barriers both to the generation and, hence, the fair evaluation of new ideas and to changes in their community’s beliefs. We argue that this may help to explain Thomas Kuhn’s observations on “normal science”, and go on to develop the relationship between Gilbert's proposal and several features of a group of physicists working on a fundamental physical theory called “string theory”, as described by physicist Lee Smolin [Smolin, L. (2006). *The Trouble with Physics*. Mariner Books: New York.]. We argue that the features of the string theory community that Smolin cites are well explained by the hypothesis that the community is a plural subject of belief.

1. Introduction

Authoritative scientific assertions often take the form of an ascription of belief to a particular population of scientists. For instance, one regularly hears or reads statements such as, “physicists believe that elementary particles obey the laws of quantum mechanics,” or “biologists think the chimpanzee and bonobo share a recent common ancestor.” A chemist might say, speaking of his colleagues, “We believe that there may be additional undiscovered elements.” Let us refer to these statements as assertions of *scientific consensus*.

Many philosophers take understanding the development of and (especially) changes in scientific

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consensus to be essential to understanding science. Despite their ubiquity, however, statements of the above form may appear puzzling. For it is natural to construe them as ascribing beliefs not to individual scientists, *seriatim,* but rather to collections or communities of scientists---to physicists, for instance, as a group. One may wonder what a *group's* belief can amount to.

Evidently, if one wants to understand the nature of scientific consensus, and by extension, change in scientific consensus, one would do well initially to explore what is intended when one ascribes a belief to a group. Several proposals have been made on this score in the literature.

According to one immediate and widely accepted suggestion a group of scientists, for instance, is rightly said to hold a given belief about a relevant scientific topic just in case all or nearly all members of the group holds the belief (cf. Quinton [1975/76, pg. 17]). And perhaps in some cases, this or something very like it *is* what is meant when one says that a group has a belief. But there is also a radically distinct possibility.

In her writings on the subject one of us (Gilbert) has pointed out that in standard cases of ordinary usage, when one ascribes a belief to a group, one seems not to be asserting that all or most of the members of the group hold the belief. Indeed, it often seems that group beliefs are not a matter of the beliefs of the members of the group at all—that is, it is neither necessary nor sufficient for a group to a hold a belief that all or most of its members hold that belief. The account of group belief that Gilbert has developed over the last several decades (see e.g. Gilbert [1987], [1989], and [2002]) accords with this observation.

It is now standard to call the first sort of account of group beliefs a "summative" account. Gilbert's, in contrast, is a "collective" account.

Gilbert's account was intended to capture a central intuitive meaning of assertions of the general form "G believes that p", where G is some group of people. On this account, members of groups that hold beliefs in this collective sense have certain obligations regarding the belief. And it is the ground of these obligations and not necessarily the beliefs of the members of the group, that determine whether a group has a belief.[1]

Suppose now that assertions of scientific consensus are accurate ascriptions of group beliefs to scientific communities, in the collective sense of group belief described by Gilbert. In that case members of a scientific community holding a consensus have the obligations associated with being party to a group belief.[2] In her (2000a), Gilbert suggests that this would have consequences for the conditions under which scientific change can occur. In particular, she argued that the obligations associated with a group belief on her account can act as barriers both to the introduction of new ideas by members of the group and to the fair evaluation of ideas proposed by experts outside the group.[3] This suggests a way of explaining some of historian of science Thomas Kuhn's best known observations on scientific change, in particular his focus on the way "normal science" proceeds, a point we develop in what follows.[4]

This paper focuses on some commentary, by contemporary physicist Lee Smolin, on certain "sociological" features of a particular high-profile branch of contemporary physics known as string theory, and the way in which his observations can be explained in Gilbertian terms, which are in turn supported by these observations. It thus presents a particular case-study in relation both to Kuhn's and

1 The ground of the obligations is what Gilbert refers to as "joint commitment". See the next section.

2 Wray (2001) and others have argued that one should not think of the phenomenon Gilbert describes as group "belief", but rather as group "acceptance", because the phenomenon differs in certain ways from belief as traditionally understood. We will not address the belief/acceptance worry here (for more on this, see Gilbert [2002] and Gilbert and Pilchman [2014]). The important point for the present paper will be that members of certain scientific communities act as though there is a *joint commitment* with a particular content in place. Whether this joint commitment is better referred to as constituting a case of belief or, rather, acceptance has no direct bearing; for convenience, we will continue to refer to the phenomenon in question as group belief.

3 Much of the literature on collective epistemology has focused on group *knowledge*, rather than group *belief*. (For examples, see Longino [1990], Kitcher [1994], Knorr-Cetina [1999], Giere and Moffatt [2003], Mathiesen [2006], Wray [2007], Rolin [2008, 2010], and Fagan [2011, 201xa, 201xb].) See also Schmitt (1994) This debate, however, is orthogonal to the present discussion, as the question we are addressing here is not whether a scientific community has knowledge, but rather whether certain features of one such community are well explained by the hypothesis that the community has a group belief, with its attendant obligations. Such group belief may, of course, be a central component in group knowledge, at least on a collective construal; that is a topic on which we do not attempt to pronounce here.

4 Kuhn (1962). Gilbert had privately noted this connection without planning to write about it. The present discussion was prompted by Miranda Fricker, commenting on a draft of this paper. For another brief discussion of Gilbert's ideas in relation to Kuhnian normal science, see Bird (2010).

Gilbert's work---with an emphasis on the latter.

The commentary we discuss arises in connection with Smolin's (2006) criticism of string theory. In particular, Smolin argues that string theory has failed to provide an adequate or compelling account of nature, and yet it continues to have a dominant role in theoretical physics (or at least, in high energy physics, which is concerned with elementary particles). As a result, high energy physics has failed to make significant progress in the last 30 years. In part, Smolin claims, the continued interest in string theory despite the problems he identifies can be explained by appealing to sociological factors. He goes on to list "seven unusual aspects of the string theory community" (284) that, on his view, have contributed to the recent failure of high energy theory.

For our purposes here, we take no stand on whether Smolin's broader critique has merit, or whether string theory is a fruitful or promising research program. Rather, we focus on the sociological considerations Smolin raises. Our purpose is to explore the relationship between the features one would expect of a group holding a belief on Gilbert's account and the features of the string theory community as described by Smolin.[5]

We argue that the features that Smolin ascribes to the string theory community are precisely what one would expect if Gilbert's suggestion that such communities hold group beliefs on her account is correct.[6] In other words, *if* Smolin's picture of the sociology of string theory is accurate—something we assume for the sake of our discussion here—then the string theory community has properties that

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5 As Gilbert points out in her (2000a), whether and to what extent scientific communities hold group beliefs is ultimately an empirical question. In this regard, the present paper can be understood as a study in the vein of Beatty (2006) and, especially, Bouvier (2004), which seek to evaluate whether real scientific communities are well-described as groups with group beliefs by studying these communities.

6 Wray (2007) has argued that *research teams* can have group beliefs (or rather, group acceptances—see fn. 2), but that *communities of scientists associated with research fields* (such as the community of astrophysicists or of biologists) and *the community of scientists* as a whole cannot. (For a compelling reply, see Rolin [2008].) It is not clear where string theorists fit into Wray's categorization of groups of scientists—surely string theorists are not systematically organized as a research team, and yet they are only a sub-group of high-energy physicists. In any case, the present claim is that one does well to understand string theorists as the kind of community that not only *can*, but *does* have group beliefs. One may well take this as an argument against Wray's view that only research teams can have group beliefs. We shall not, however, attempt to engage with the specifics of Wray's discussion here.

are very well explained on Gilbert's account of group belief, applied to the string theory community with the assumption that the community holds relevant group beliefs.

Gilbert's account offers, in particular, an effective explanation of why the string theory community may appear to act *irrationally* with regard to certain countervailing evidence.[7, 8] It is here that the connection to Kuhn arises. We will suggest that the apparent irrationality of the string theory community, as described by Smolin, is symptomatic of the conservatism Kuhn attributes to normal science.

At the end of the paper, we will address an apparent conflict that arises between our argument here and Smolin's characterization of his seven features, related to the fact that he explicitly characterizes the features as *unusual*, whereas if Gilbert's account is correct, one should expect such features to be exhibited by *any* group holding a belief—including any scientific community holding a consensus view. We will argue that these features seem unusual to Smolin not because they are actually unusual, but because he occupies an unusual position from which to observe them.

We should note that the goal of this paper is not to evaluate or dispute Smolin's picture of the string theory community. Likewise, we will not directly evaluate Gilbert's account of group belief. Any evaluative content with regard to Gilbert will be implicit in how effectively her theory accounts for the string theory community, at least as described by Smolin. Similarly, insofar as Smolin's

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7 Wray (2001) and Mathiesen (2006) have argued that groups holding group beliefs on Gilbert's account may not be epistemically rational, in the sense that group beliefs may not be updated appropriately in light of new evidence. This is taken to be a criticism of Gilbert's view. We will not address this criticism in any detail in the present work, though we will note that if, in fact, groups holding group beliefs *do* fail to account for evidence in an epistemically responsible way, then it is a *virtue* of Gilbert's account that it appears to predict this behavior. (For another response to Wray and Mathiesen, see Rolin [2010].)

8 Gilbert (2000a) argues that postulating that certain scientific communities hold group beliefs explains certain features of scientific communities and theory change related to resistance to heterodoxy. This argument has been criticized by a number of authors, including Wray (2003, 2007), Rolin (2008), and Fagan (2011), on the grounds that it is not clear how generic the features Gilbert mentions are, and thus that it is not clear that such features stand in need of explanation. For present purposes, one can set this general discussion aside and focus on the *specific* explanatory question presented by Smolin's work, namely, does Gilbert's account provide a compelling explanation of the striking features of the string theory community? If so, then it seems that there is a potentially interesting question to be explored concerning the role of group belief in at least some scientific communities.

perceptions of the string theory community accord with Gilbert's collective account of group belief, which was arrived at independently by reflection on everyday thought, talk and behavior in multiple domains, the latter has some tendency to confirm the accuracy of the former.

The remainder of this paper is organized as follows. We begin with a brief presentation of Gilbert's theory of social groups and group belief, along with its connection to scientific consensus and change, as described in her (2000a). We then link her theories to Kuhn's observations on scientific change. Next, we give a brief history and description of string theory, explaining how it has now come to have a dominant role in the broader high energy physics community. Finally, we discuss the seven points of Smolin's sociological critique in light of Gilbert's theory of group belief, arguing that the striking features of string theory Smolin describes are well explained by the hypotheses that group beliefs on Gilbert's account are at issue and that the string theory community holds relevant group beliefs.

2. Gilbert on Collective Belief and Scientific Change

Gilbert's account of group belief offers an interpretation of such statements as, "The seminar believes that the second article is less convincing than the first"; "The European Union believes that the Euro will rebound in light of the economic indicators released today"; or "Ben and AJ believe Jim is coming, even though he's already a few minutes late." In each of these sentences, a belief is ascribed to the subject. These subjects are not individual human beings, but rather something that is made up of human beings. One might call them "groups of people," or just "groups," to capture that they refer to something of which several (or many) people are members. The problem, then, is to understand what such "group belief" amounts to.

Gilbert's account of group belief can be expressed as follows.

A group G believes that p if and only if the members of G are *jointly committed* to believing that p as a body.[9]

When this condition obtains (and only then), each member of G may truly say "We believe that p," where "we" is intended with respect to G.

It is worth clarifying first that the phrase "as a body" is not meant to be sacrosanct. "As a unit" or "as one" would do just as well. The idea is that the object of the commitment, the thing being committed to, is to emulate as far as possible a single believer of the proposition in question, by virtue of the combined actions and utterances of the parties.

This analysis relies on the technical concept of "joint commitment." We will not define this term outright; instead, we will explain it by describing some central features of a joint commitment.[10] When people are jointly committed in some way, they are in a particular normative situation---each is committed to act in a certain way---as a result of a specific process that involves them all. As to that process, it is common knowledge[11] between the people in question that each of them has expressed his or her personal willingness together with the others to commit them all in the way in question. Thus, in the case of a group belief that p, each member of G has expressed his or her willingness together with the others to commit them all to emulating a single believer of p by virtue of their combined utterances and actions, and it is common knowledge among the members of G that this is the case.

It is important to emphasize that in order for the parties to be jointly committed, it is neither necessary nor sufficient for the members of G to each make what Gilbert refers to as a *personal* commitment to do his or her part in emulating, together with the others, a single believer of some proposition. A personal commitment in Gilbert's sense would be engaged by Jim's decision to work late tonight, for instance. A joint commitment is not composed of personal commitments, which are the

9 For more on Gilbert's account, see e. g., Gilbert (1987, 1989), her earliest discussions, and Gilbert (2000a), (2004).

10 For a longer but still compact general introduction to the notion see Gilbert (2013: Introduction) and for further details see Gilbert (2013: ch. 2).

11 "Common knowledge" is usually construed as a technical term in philosophy and the technical sense we have in mind is akin to Lewis (1969). However, for the current purposes, the intuitive concept is sufficient. We will not later make any use of a specific formulation of common knowledge.

unilateral product of the committed person's will and unilaterally rescindable by that person.

In order that a joint commitment be rescinded, the parties must concur in its rescission. If an individual fails to conform to a joint commitment, then, absent certain background understandings, she offends against all of the other parties to the joint commitment.

Amplification of this last point is important for a full understanding of the concepts of joint commitment and *collective belief*, that is, group belief on Gilbert's account of it. [12]The offense just referred to is a matter of violating an obligation to the other parties, who have rights to one's conformity to the joint commitment. This is a function of the joint commitment itself: a joint commitment obligates each party *to the others* to conform to it, at the same time endowing them with rights to such conformity.[13]

Once party to a joint commitment, a person is required to act in accordance with the commitment under penalty of rebuke from the other members of the group. Should someone indicate that he is about to fail to accord with the commitment, the other parties have the standing to demand that he conform after all.[14]

The notions of "rebuke'' and "demand" here are strong ones. As just indicated, in order to rebuke someone or demand some action of him, in the relevant senses, one must have a certain standing or authority. Thus it is not the case that everyone who finds one's action distasteful is in a position to rebuke one for it, or that everyone who thinks one ought to perform some action can demand that action of one.

Further, there is a sharp distinction between having the standing to demand a certain action of another and being justified in making that demand. The same thing holds for rebukes. Sometimes one

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[12] Henceforth we use the phrase "collective belief" as short for "group belief on Gilbert's account of it".

[13] For discussion of the relationship of joint commitment and directed obligation see Gilbert (2012) and elsewhere.

14 Gilbert sees the following as "equivalents" in roughly the sense of the rights theorist Wesley Hohfeld: (1) A has a right against B to B's doing φ, (2) B is obligated to A to do φ; (2) A has the standing to rebuke B for not φ-ing; (3) A has the standing to demand that B φ (before the time for A's appropriately φ-ing has passed). See Gilbert (2012).

will have the standing to make a demand, but will be unjustified, all things considered, in making it. Perhaps the psychological make-up of the potential addressee makes that inadvisable, for instance. Or perhaps the otherwise acceptable action one has the standing to demand is not justified, all things considered, in the circumstances. In that case, presumably, one will not be justified in demanding that action. Again, one might lack the standing to make a certain demand, though, if one had that standing, one would be fully justified in exercising it. Finally, one may be justified in pressuring someone to do something or in letting them know that one thinks poorly of their doing it, without having the standing to demand that they do it or the standing to rebuke them for doing it.

In the particular case of a collective belief, these consequences of joint commitment mean that if an individual is party to a collective belief and if she speaks in a way that contradicts the belief (without significantly qualifying her statement), then she has offended against the other members of the group.[15] In other terms, she has failed to fulfill obligations she has to them, and they have the standing to rebuke her for this failure. To see how this works in a particular case, imagine that the parents of a teenager disagree about when the teen's curfew should be. The mother believes it should be 9pm, whereas the father says 11pm. They compromise, and later tell the teen, “We think you should be home by 10.” If the father then says, “Actually, 11 is fine,” the mother would rightly feel affronted. In addition to any other reactions she may have when or after their son is present, she may well rebuke the father for speaking as he did, and he will understand that she has the standing to do so.

According to Gilbert, *any* group of people holding a collective belief counts as a social group. A group of people is a *social group* for Gilbert just in case the members are jointly committed in some way. In that case, she argues, each member appropriately thinks of the group as “we” or “us”. She refers to social groups of this sort as “plural subjects”. Two or more people constitute a plural subject of

[15] The kind of qualification at issue includes in particular one’s using such a preliminary as “Personally, I….” that makes it clear that one is about to express one’s personal belief as opposed to the collective’s belief. See e.g. Gilbert (2000a).

φ-ing, where φ-ing is the activity/belief/etc. about which they have a joint commitment.

Even though “social group” is a technical term here, it is intended to capture the same content as would the words interpreted informally, at least in a certain central sense.[16] Intuitively, the idea is that a collection of people becomes a social group when they openly decide to “join forces” in a coordinated action (say). Thus suppose that Wolfgang comes across Alice struggling up a hill with her groceries. Wolfgang stops and offers to help; Alice accepts. Wolfgang takes a few of Alice's bags, and then the pair walks up the hill together. Now Alice and Wolfgang are jointly committed to espousing as a body the goal of carrying Alice's groceries up the hill. Suppose a few minutes pass and Alice sees her friend Jacques. Jacques stops and asks what is going on, to which Alice can justly reply, “*We* are carrying my groceries home.” They are sharing the action in a way that they would not be if they both happened to be carrying groceries up the same hill at the same time. It is for this reason they count as a social group. Collective belief, or rather, the joint commitment to believe that p as a body, is just one example of the kind of joint commitment necessary and sufficient for social group formation.

As noted above, it is helpful to distinguish Gilbert's position from another possible view that is common in the philosophical literature. Gilbert's account of group belief is not “summative,” in that Gilbert does not take statements of the form “G believes that X,” where G is a group, to mean “all or most of the members of G believe that X,” or any of the possible variations on that theme. Indeed, it is neither necessary nor sufficient on Gilbert's view for most (or even any—recall the curfew example) of the people in G to *personally* believe that X. A group can believe a proposition even if few or none of its members believe it, so long as the members are jointly committed in the right way.

It is of particular importance for what will follow that this joint commitment to believe that p as a body need not entail a commitment on behalf of each member personally to believe that p, or even personally to act as though he believes that p. Rather, the members of the group are to act as separate

16 See Gilbert (1989: ch. 4) and elsewhere.

mouthpieces of the group, expressing the belief in that role in any setting in which they are acting in their capacity as group members. Conversely, if every member of a group happens to believe something, it does not follow that they believe it as a group, as it is possible that they have not yet jointly committed to believe it as a body.

As observed above, if Gilbert's analysis of sentences of the general type in question is correct, it should apply equally well to assertions of scientific consensus understood as ascriptions of belief to the relevant group. One can thus construe scientific consensus as a matter of collective belief . Scientific change, then, which amounts to moving from one consensus to another (at least according to a prominent vein in the history and philosophy of science), can be thought of as collective belief revision.

Before asking whether the features Smolin attributes to the string theory community are well explained by the suggestion that the community has one or more collective belief, it is worth pointing out some consequences that a consensus qua collective belief would have for a scientific community, on Gilbert's account. The most important of these all follow from the fact that the existence of a scientific consensus would imply that a scientific community is a social group with a joint commitment, which in turn implies that members of the community have obligations to behave in certain ways. In particular, this means that the members of a scientific community are obligated to act as mouthpieces of the group with respect to the consensus, or risk rebuke from other members of the community.

Expressing a contrary view—bucking the consensus—is an offense against the other members of the community, and threatens to put the contrarian outside the bounds of the social group.[17] So irrespective of their personal beliefs, there are pressures on individual scientists to speak in certain ways. Moreover, insofar as individuals are psychologically disposed to avoid cognitive dissonance, the

17 A qualified expression such as “Personally, I have my doubts about the theory” may avoid outright default on an obligation, but it is likely to make one stand out as a potentially unreliable group member. See Gilbert (1987; 2000).

obligation to speak in certain ways can affect one's personal beliefs so as to bring them into line with the consensus , further suppressing dissent from within the group.

Finally, if scientific consensus is a set of collective beliefs, then effecting scientific change is not necessarily a matter of convincing a majority of individual scientists that a new view is correct. Instead, it is necessary to get the members of the community to jointly commit to believing, as a body, a new proposition. That is a particularly arduous undertaking in that it requires one or more individual scientists at least temporarily to risk rebuke from their colleagues as they attempt to build public support for the new view, expressing support for that view in the face of a contrary consensus.

Considerations of cognitive dissonance and potentially conflicting commitments may have another consequence as well, regarding how scientists, both individually and collectively, deal with evidence for propositions that conflict with their consensus. Bringing up such evidence will itself have costs, in the form of possible professional rebuke, akin to those associated with outright denial of the consensus.

In some cases, these costs may prevent new results from being submitted by individual scientists or research teams to scientific peers for consideration, or from being selected by a relevant individuals or committees for presentation to scientific peers.[18] In others, individual scientists or research teams may avoid pursuing potentially transformative research in the first place.

Even when evidence against a consensus is found by an individual scientist, one might expect it to be ignored, suppressed, or explained away by its discoverer, since such evidence would force a psychologically unsustainable conflict between the scientist's commitment to act in a certain way and his (or her) beliefs concerning the epistemic warrant for those actions. In the case of evidence contrary

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[18] As may have happened in the case of the initial proposal of a bacterial theory of ulcers---now well entrenched. This was one of very few papers rejected for a gastroenterology conference when the received view was that ulcers were caused by other, including dietary, factors. See Gilbert (2000a) citing Thagard (1999), then in ms. form, which to some extent inspired it.

to a consensus that is made public, the scientific community, also, may ignore, suppress, or explain it away. For instance, it may be collectively affirmed that a crucial experiment cannot have been properly done, or it may be assumed that facts that are not at odds with the consensus can explain it, however implausible such an assumption really is. For these reasons, scientific communities should be expected to hold certain beliefs in the face of considerable conflicting evidence, to the point of being, or at least appearing, irrationally dogmatic or epistemically irresponsible.

3. Kuhnian Paradigms and Gilbertian Collective Beliefs

Gilbert's account of scientific consensus and change can be brought to bear on what are sometimes called "two-process" views of scientific change, including the view developed and defended by Kuhn (1964).[19] On Kuhn's picture, scientific change (at least in "mature" sciences) occurs in two distinct modes.

One mode, which Kuhn calls "normal science", is characterized by the broad acceptance of a "paradigm". A paradigm, meanwhile, consists in (at least) a collection of theoretical commitments, acceptable research methods, and recognized problems of pressing interest. When working within a paradigm, scientists may apply their accepted methods to make incremental progress on the problems recognized by the community. This is change *within* a paradigm.

The other mode, meanwhile, which Kuhn calls "revolutionary science", consists in change *between* paradigms. That is, during revolutionary science, scientists reject a previously accepted paradigm and adopt a new collection of theoretical commitments, research methods, and important problems.

[19] The terminology of "one-process" and "two-process" views is due to Godfrey-Smith (2003). Aside from Kuhn, one might recognize Carnap (1956), Lakatos (1970), Laudan (1977), and Friedman (2001) as defending two-process views. Much of what is said here relating Kuhn and Gilbert could be extended to relate Gilbert's proposal with these other views.

The connection to Gilbert's proposal can be seen by observing that the theoretical and methodological commitments associated with a paradigm may best be understood as a set of foundational collective beliefs of the community of scientists working within the paradigm.[20] In other terms, the collective beliefs in question create the overarching framework within which the work of this community is conducted. Revolutionary change, then, may best be conceived as a variety of collective belief revision, consisting in the rejection of a prior joint commitment with respect to one or more core propositions associated with a paradigm, and the institution of at least one new joint commitment that conflicts with these core propositions.

This way of thinking about Kuhn's views offers an explanation of one of the most striking and controversial features of normal science. Specifically, Kuhn argues that during periods of normal science, "anomalies", which are theoretical or experimental discoveries that are apparently incompatible with the central tenets of a paradigm, are either not recognized or ignored. In other words, during normal science, researchers seem to focus on evidence that appears to confirm the beliefs associated with the paradigm, and to disregard contrary evidence. Given that on many views of evidence, high-quality contrary evidence should be taken to be of especially high value, this tendency to ignore contrary evidence may seem (at least) strange, and perhaps irrational.

Note, however, that if a paradigm is a collection of foundational collective beliefs of a scientific community, this attitude towards conflicting evidence should be expected. As discussed earlier, some degree of resistance to contrary evidence is predictable given the nature of the joint commitment constitutive of any collective belief whatsoever. That is so both for individual members or sub-communities of a given scientific community and for the community as a whole.

[20] In a more nuanced treatment one would most likely bring in phenomena other than group beliefs as Gilbert construes these, phenomena such as the joint acceptance of certain methodological rules or conventions, each of which can be construed in terms of joint commitment. (On the latter see Gilbert (1989: ch. 6; 2013, ch. 9).) Doubtless, however, beliefs of one kind or another play a central role in any Kuhnian paradigm and, for present purposes, we write as if all of its elements are matters of belief.

Given the foundational nature of the beliefs constitutive of a paradigm, one would expect an even greater tendency to resist contrary evidence than is present in every case of collective belief. Such resistance would be evident in, for example, the particular harshness of the rebukes meted out should one challenge these core collective beliefs. At the extreme, a given member of the community may be judged to have removed himself from the community---to have ex-communicated himself. After all, acceptance of a challenge to a given foundational belief is apt to bring down the whole edifice of beliefs within which this community has been working---perhaps for a very long time.

In sum, the apparent conservatism that Kuhn attributes to scientific communities may be explained by the general nature of scientific consensus understood as collective belief. This conservatism would be amplified given the centrality of the beliefs in question.

One might push this idea still further. Kuhn seems to suggest that despite the apparent failures of rationality associated with ignoring or suppressing contrary evidence, the epistemological features of normal science provide a partial explanation of the *success* of science.[21] The idea, here, is that science is successful in part because of a distinctive kind of focused, collaborative research. And this sort of collaborative research is enabled by the existence of a paradigm insofar as a paradigm provides a stable shared agenda and collection of methods for realizing that agenda. The resistance of a scientific community to accepting changes in the paradigm thus provides a mechanism for preserving this collaboration.

Gilbert's account of group belief suggests a way to understand this mechanism. Indeed, it suggests a way of understanding any relatively long-term collaborative process.

[21] Here we are largely setting aside the question of what it means to say that science is successful—that is, whether the success of science should be measured by the empirical adequacy of scientific theories, the truth of those theories, their explanatory power, etc. Kuhn's own notion of the success of science was deeply entwined with his notion of normal science as "puzzle solving". Roughly, Kuhn argued that science is very successful at solving the puzzles deemed to be important within a paradigm, and that the features of normal science described explained this sort of success. Our point here is merely that Kuhn gave reasons to believe that the conservatism of normal science may not be the impediment to the success of science that it would appear to be.

Suppose one accepts that science, at least as understood in the modern period, is an essentially collaborative activity of relatively long duration, and that any such collaboration requires a framework of beliefs, concerning, at least, the nature and viability of its goals and the best way to achieve them. If these beliefs are conceived as group beliefs in Gilbert's sense, then they will be constituted by an appropriate set of joint commitments. As we have discussed, such joint commitments, once established, are apt to provoke resistance against anyone who is inclined to push for their rescission. Thus, the resistance to change on which Kuhn focused need not be conceived as a special feature of scientific communities. Instead, these may be seen as characteristic features of *any* long-term collaborative activity. The participants can be expected to resist change with respect to the framework of collective beliefs that help to define its goals and the means to be adopted to achieve them---its methods, if you will.

In the case of a long-standing scientific discipline, of course, there will be a special corpus of collective beliefs that represent not only the approved aims and methods of the enterprise, but an increasingly sophisticated body of theory, supported where possible with accredited empirical results, involving a host of linkages between foundational propositions and others. The collaborative enterprise that is science in its various branches is, then, of particular interest to the collective epistemologist. The collective beliefs of a mature science go far beyond those that concern a set of goals and methods, or arise incidentally in the course of pursuit of a collective goal. They involve a multiplicity of highly articulated, closely interwoven and mutually sustaining collective beliefs about the world.

Indeed, the point of the enterprise, prescinding from various epistemic cautions, is that those beliefs be true. Hence conservatism with respect to scientific paradigms, in particular, has its problematic side. Though it helps to provide a climate in which fledging theories can grow and flourish, it also helps to hide from view alternative theories that may in fact be better.[22]

3. The Rise of Strings

We now turn to string theory, and offer, first, a brief history of this theory. Today, string theory is a would-be “final theory,” that is, a theory with foundational aspirations. It originated in the late 1960s, however, in a role rather different from its current one.[23] Initially, string theory was a phenomenological attempt to understand one of the four fundamental forces, known as the strong nuclear force.[24] The strong force acts on particles known as “hadrons,” among which are the relatively familiar proton and neutron that form the nuclei of atoms. By the late 1960s, it was believed that these hadrons were composed of smaller particles, called “quarks,” but their properties were not well understood. All that was known was that quarks must be, in some sense, “confined” to hadrons, since no one had ever observed any free quarks. String theory was supposed to explain this confinement via very small elastic bands (the strings) that bound the quarks together. Although the theory was fairly successful, its progress halted abruptly in 1973 with the remarkable experimental success of a competing theory of the strong force, known as quantum chromodynamics (QCD). As Galison points out, however, string theory had flaws of its own: in its capacity as model of the strong force, string theory predicted a new particle with no counterpart in the phenomena to be explained.

As it became clear that string theory was a dead end with regard to hadronic physics, committed

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[22] Here there is a connection to recent work by Stanford (2006) on the problem of “unconceived alternatives”. Stanford argues that scientists' systematic failure to identify alternative theories that can deal with available evidence as well as or better than current theory is the strongest threat to scientific realism. If the conservatism of normal science as we have described it here contributes to that failure to recognize alternative theories, then it bears severe epistemic costs. Of course, Kuhn was not a realist in the sense Stanford attacks.

23 As mentioned in the introduction, the material here is derived from Smolin (2006) and Galison (1995); for additional perspectives, see Cappelli et al (2012) and Dawid (2013). This section is intended as background and not argument. If anything of the history presented here is contentious—aside, of course, from Smolin's claim that string theory has not accomplished its stated goals, and should now be viewed as a failure—it is unintentionally so.

24 As a matter of vocabulary, physicists use “phenomenological” to refer to models/theories intended to describe specific phenomena. The contrast class would be “fundamental” theories, which claim to be generally valid and universally applicable (at least in principle).

devotees, convinced of the power of the theory's mathematical structure, looked for new applications. In 1974, John Schwarz and Joël Scherk, and independently T. Yoneya, proposed a reinterpretation of string theory. This new theory was essentially identical to the old one, except now the strings were 10^{20} times smaller. Instead of binding the constituents of hadrons together, strings were now proposed as the fundamental building blocks of both elementary particles and spacetime itself. In this role, the unobserved particle that threatened to derail the hadronic theory could be interpreted as a carrier of the gravitational force. (The problem of finding an adequate quantum mechanical theory of gravitation had proved remarkably stubborn, and any candidate for a particle corresponding to gravitation was considered a promising one.) In this new role, however, the theory had some worrisome and undesirable properties: for one, it predicted that the universe had at least 6 additional, unseen spatial dimensions. Whether for this reason or some other, over the next decade string theory was largely ignored by the physics community, aside from a small group of researchers.

All this changed in 1984, with such dramatic suddenness that the period is often referred to as the first superstring revolution.[25] The tipping point was a calculation by Schwarz and a young collaborator named Michael Green that appeared in August of that year. They showed that string theory lacked an inconsistency that had plagued other so-called unified theories then under consideration. The response was surprise, celebration, and a massive movement of physicists into the field. In 1984, there were about 150 articles published on string theory in total, about three times the average annual output in the previous decade. In 1985, the number was well over 400, and then over a thousand in 1986.

This explosive growth can be attributed to a variety of factors. One was that the then dominant theory—called the Standard Model, which included QCD as one of its two subparts—had been around

25 “String theory” is now common shorthand for a theory that was known as “superstring theory” when it was first developed in its modern form, during the 1980s.

for a decade and was doing too well. None of its predictions had been falsified and a good number had been confirmed to a high degree of accuracy. Wonderful as this sounds, it was widely believed that the Standard Model could not be a fundamental theory, because it left too many of its own parameters unexplained; yet, without any experimental disagreement, there was little to point the way towards more fundamental theories.

After the Schwarz-Green calculation, string theory was a promising possibility in a landscape where all other options seemed exhausted. It was in this period, between 1984 and 1989, that string theory first rose to be the leading candidate for a theory of everything. Smolin also describes the mid to late 1980s as the time when the string theory community began to exhibit the sociological features he highlights. (We will state and discuss these in detail below.) He writes of string theory, “It was the hottest game in town. ... Very quickly there developed a cultlike atmosphere. You were either a string theorist or you were not” (Smolin 2006, 116). One reason for this appearance of division, according to Smolin, was that string theory required new technical tools that most physicists would not have learned in graduate school. The investment of time and energy were risky, and were taken as evidence of one's commitment to the new project. Theorists who did not take the time to learn the new tools were viewed as either incapable of understanding the new developments (a stance common among younger physicists towards their elders, when the elders began to question the unconventional new research). And it was easy to tell who had devoted themselves to the new theory, because the research methods were distinct enough to distinguish “string theorists” from others based solely on the sorts of papers they published.

It was also during this period that the first outspoken dissenters appeared. Among these critics numbered many of the most prominent theorists of the previous generations. Nobel laureate Richard Feynman, for instance, wrote in 1988, “[string theory] doesn't produce anything; it has to be excused most of the time. It doesn't look right.” Another Nobelist, Sheldon Glashow, who was largely responsible for a big chunk of the Standard Model, wrote in the same year that string theorists "cannot demonstrate that the standard theory is a logical outcome of string theory. They cannot even be sure that their formalism includes a description of such things as protons and electrons. And they have not yet made even one teeny-tiny experimental prediction" (both of these are quoted in Smolin [2006, 125]). Howard Georgi, who with Glashow proposed the first Grand Unified Theory (and thus started the path of which string theory was supposed to be the end), wrote in 1989, "I feel about the present state of GUTs as I imagine Richard Nixon's parents might have felt had they been around during the final days of the Nixon administration" (quoted in Galison [1995, 392]).

Interest in string theory calmed down somewhat during the early 1990s. Approximately 600 papers were published each year between 1990 and 1993—half the peak of 1200 in 1987. Although many still found the theory to be promising, the wide interest of the late 1980s had revealed several new undesirable features. Most prominent was that there appeared to be a handful of *different* theories, all justly going by the name string theory and between which there was no way to adjudicate. This was in conflict with one of the principal virtues many physicists hoped and expected string theory to have, namely that its mathematical structure would lead to an essentially unique theory. Physicists felt that a highly constrained theory of this sort was desirable because, if confirmed, it would carry a sense of necessity with it. The world is the way it is, one might then say, because small changes (in the true theory) would lead to a mathematical inconsistency.

Soon physicists were able to classify five possible string theories. This number might have seemed manageable, except that to make any of these five theories physically acceptable, it was necessary to "compactify" (literally, roll up and hide from view) the six extra dimensions. By the late 1980s, it had been observed that there were millions of consistent ways to do this, and picking the one that corresponded to the world appeared arbitrary and *ad hoc*. In other words, string theory appeared incapable of making substantive predictions at all, since the geometry of the theory was so radically underdetermined by known mathematical constraints that almost any possible experimental data was compatible with the theory. Twenty years later, these features continue to be among the ones that string theory's critics cite.

The field accelerated once again in 1995, however, when Edward Witten made a rather striking proposal. He said that although it seemed there were five different string theories, he believed it was possible that all of these were examples of a single underlying theory, with seven extra dimensions (instead of six). This new theory did not have strings in it, *per se*. Now, it had two dimensional surfaces. One of the dimensions of these surfaces was tightly wound in the new extra dimension, so that the objects would appear to be one dimensional strings in a 10 dimensional space, much as in the older theory. Witten conjectured, and later proved, that the five string theories that were discovered in the late 1980s corresponded to different ways of winding the two dimensional objects around in this 11th dimension. He named the new theory M-theory. Once it was understood that the five theories were actually different parts of one theory, physicists' interests were reignited. (The additional problem of compactification, however, remained.)

This period, following Witten's proof and a handful of developments that followed quickly from it, is often called the second superstring revolution. Since then, string theory and its descendant, M-theory, remain hegemonic, despite the fact that some of the earliest concerns—about the prediction of unseen particles without any details of their properties; about the non-uniqueness of the scheme for hiding the theory's extra dimensions; and about the lack of connection with experiment—remain unresolved.[26] The mid 1990s saw a slew of popular books, by such authors as Michio Kaku and Brian

26 This is not to say that nothing has changed in the last 15-20 years. In particular, the interests of string theorists have shifted to new topics, including questions concerning the role of strings in cosmology and the early universe and the so-called AdS/CFT correspondence, which attempts to draw a connection between string theory and more traditional approaches to high energy particle physics. Curiously, some string theorists have also had success in applying the methods characteristic of their discipline to problems in other, radically different areas of physics, including atomic and nuclear physics. (This latter work has led some string theorists to claim that string theory *has* made testable predictions. But they are not predictions concerning fundamental physics, the supposed domain of the theory. At best, predictions of

Greene, that spread the word of the superstring revolutions to non-scientists, and helped create the sense that despite its problems, string theory was already established science.[27] Now, however, many (perhaps most) outsiders to the theory, in both the public press and in other areas of physics, tend to be more critical. Despite the efforts of some string theorists, such as Leonard Susskind of Stanford and Lisa Randall of Harvard, to present new developments of the theory and sustain a feeling of hopefulness, an increasing number of physicists outside of the string theory community have come to believe that the grace period during which the theory's problems could be excused has ended. And yet, it continues to be dominant within its sub-field of physics. In other words, from Smolin and others' perspectives as outside experts evaluating string theory, given the current state of the field string theorists appear to be epistemically irrational in their continued confidence that string theory is the best available proposal for approaching questions in quantum gravity.[28] This is where Smolin suggests that sociology plays a role.

4. Smolin's Sociological Critique: String Theory and Collective Belief

In his (2006), Smolin describes seven features of the contemporary string theory community that are intended to explain why it continues to be the dominant candidate for a fundamental theory, despite the widely held view of experts outside the fold that the theory is no longer as promising as it once seemed.[29] We first state them in their entirety, and then we relate them to Gilbert's description of

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this other sort provide evidence that some of the mathematical and physical reasoning used by string theorists is not inherently fallacious; it emphatically does *not* provide evidence that string theory is the correct fundamental theory of nature.) Importantly for present purposes, the *sociological* features of string theory that Smolin emphasizes were already well-established by the mid-90s and, he claims, had not changed significantly by the time he wrote his book.

27 Greene (1999, pp. 213-4) in particular provides quotes from some of the same critics Smolin and Galison cite—including Georgi and Glashow—that are more conciliatory.

28 String theory has not been without "outside" defenders—for instance, the particle physicist-turned-philosopher Richard Dawid (2013) has recently argued that not only is string theory not a failure, but its success should force a change in how we understand science.

29 We should emphasize that the claim is not that string theory has failed—insofar as it has—because of these sociological features. Instead, string theory has failed because it is not an adequate theory of nature. It continues to enjoy a privileged place in the physics community, however, for these sociological reasons; this continued dominance,

a scientific community holding one or more collective beliefs. Smolin writes that string theory has qualities of,

> *1. Tremendous self confidence*, leading to a sense of entitlement and of belonging to an elite community of experts.
> 2. *An unusually monolithic community*, with a strong sense of consensus, whether driven by the evidence or not, and an unusual uniformity of views on open questions. These views seem related to the existence of a hierarchical structure in which the ideas of a few leaders dictate the viewpoint, strategy, and direction of the field.
> 3. In some cases, a *sense of identification with the group*, akin to identification with a religious faith or political platform.
> 4. A strong sense of the *boundary between the group and other experts*.
> 5. A *disregard for and disinterest in* the ideas, opinions, and work of experts who are not part of the group, and a preference for talking only with other members of the community.
> 6. A tendency to *interpret evidence optimistically*, to believe exaggerated or incorrect statements of results, and to disregard the possibility that the theory might be wrong. This is coupled with a tendency to *believe results are true because they are "widely believed,"* even if one has not checked (or even seen) the proof oneself.
> 7. A lack of appreciation for the extent to which a research program *ought to involve risk*. (Smolin 2006, 284, emphasis in original)

In what follows, we work with the hypothesis that string theorists are a group of people with collective beliefs concerning the fundamental nature of the world. (Given the connection we have noted with Kuhn's work, one might rephrase this as "a group of people working within a Kuhnian paradigm".) We will use the proposition "string theory is true" as shorthand for whatever those beliefs are supposed to be, setting aside the worry that "string theory" may not refer to a single, well-defined set of propositions. The actual views of string theorists are more subtle, and would involve a number of technical propositions and beliefs about the relation between these propositions and the world, plus opinions about the ontology of the world and the prospects for string theory's ultimate success. It is not important precisely what statements are part of the core set of collective beliefs of the community, so long as there *is* such a core set shared among all string theorists.

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meanwhile, has prevented other possible theories from receiving much attention, which, Smolin claims, explains why high energy physics has stalled. Thanks to Gerald Cantu for pointing out this possible ambiguity.

We will treat the features that Smolin describes in turn, grouped according to their explanation in terms of collective belief. We will argue that all of these features have natural explanations given the assumptions made thus far concerning the string theory community and the nature and implications of collective belief. The central assumption regarding the string theory community for present purposes is its possession of a core set of collective beliefs---group beliefs in Gilbert's joint commitment sense.. Then, in section 4.iv, we will turn to the question of why, if the features Smolin describes are precisely what one should expect of a scientific community, Smolin describes them as "unusual".

4.i Features 2, 3, & 4: identification with the group and a boundary with other experts

Features 2, 3, and 4 of Smolin's description are direct consequences of collective belief. Holding a collective belief is a sufficient condition to constitute a social group, which means that string theorists can justly refer to themselves as "we" with regard to the consensus of the community. So it is unsurprising that the parties to the collective belief tend to have a sense of identification with the group holding the belief. After all, they are *members* of that group. The point will presumably pertain even more strongly to a population of scientists with many interconnected collective beliefs.[30]

Likewise, the sense of a boundary between the string theory community and individuals and other groups of a different persuasion follows from the string theory community's status as a social group. Insofar as the people who collectively believe that string theory is true can refer to themselves collectively as "we," there is an available distinction between "us" and "them". And so features 3 and 4 can be expected of any social group.

Similarly, feature 2 should be expected of any social group holding one or more collective beliefs. A monolithic community with a strong sense of consensus is precisely what would characterize

30 Can a given group have inconsistent beliefs? If so it could indeed be described as a house divided. Supposing that this can happen within a given scientific community, it is clearly a special though interesting case that we shall set aside here.

a group with multiple joint commitments to speak and act in ways expressive of particular beliefs—particularly if there is a large number of interconnected beliefs with a central core.

Indeed, Smolin's description of the early history of string theory suggests the process by which an initial joint commitment, out of which the social group arose, was formed. Early string theorists needed to devote significant amounts of time to learning new theoretical methods—different enough from other methods that Witten once described the theory as “a piece of 21st-century physics that fell by chance into the 20th century” (Cole 1987)--and it was clear who among the broader physics community had chosen to do so. As Smolin explains, physicists who *did* choose to learn string theory quickly came to view themselves in opposition to those physicists who did not.

Some physicists used the plural subject to refer to those committed to string theory: as Harvard string theorist Andrew Strominger put it later, reflecting on the early days, “We were once considered semi-crackpots working on some bizarre idea” (Johnson 1998). Their expenditure of time and energy, with consequences that could be easily observed by their peers, amounted to a public commitment on the part of those who made the investment. By the mid to late 1980s, at least, it seems likely that through their interactions in the course of their work the relevant group of physicists had thus openly expressed their readiness *jointly* to commit to work as a body on string theory, in conditions of common knowledge, so that the conditions for a Gilbertian social group obtained. As described above, it was during the same period that a consensus concerning the basic propositions of string theory was first arrived at by those physicists jointly committed to working on string theory, and that Smolin’s feature 4 became apparent.[31]

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31 As indicated in footnote 2 above, the distinction or rather distinctions between belief and some form of “acceptance” made by various philosophers is not strictly germane here. Mutatis mutandis, the points made here about collective belief can be made about collective acceptance, on whatever construal. The question whether it is better to characterize string theorists as collectively believing as opposed to collectively accepting the basic propositions of string theory is an interesting question that we shall not pursue. For some it will hang on whether they are collectively agnostic as to the truth of these propositions, as opposed to the desirability of supposing their truth for the purposes of scientific inquiry.

4.ii Features 5, 6, & 7: disinterest in other ideas, tendency to interpret evidence optimistically, and poor appreciation of risk

Features 5, 6, and 7 follow only slightly less obviously from Gilbert's account. There are two things to say here: the first is to explain why these features might *appear* to be true of the string theory community to an outside expert like Smolin, and the second is to explain why they might in fact come to be true of the members of the community.

As parties to a joint commitment, members of the string theory community are obligated to act as mouthpieces of their collective belief. This means that if they speak in a way that contradicts the consensus, without significant qualification, they risk offending other parties to the collective belief. As it is difficult (and professionally unwise) to offend against other members of one's community, one expects parties to any collective belief to refrain from speaking approvingly about ideas that contradict or challenge the consensus.[32]

Moreover, no individual can rescind the commitment alone, even in the face of overwhelming evidence against it—at most, she can choose to violate the commitment and risk rebuke. Thus the collective belief entails that the members of the group have an associated obligation to dismiss evidence or viable alternative views that might instill doubts about the consensus. Likewise, any evidence that can be taken in favor of the collective belief is embraced. Someone who is obligated to speak as (for short) the mouthpiece of a group is naturally inclined to emphasize evidence in its favor—i.e. to take a rhetorical stance in evaluating and communicating the evidence. So features 5 and 6 seem to follow on Gilbert's account. Or at least, it is to be expected that an outsider would characterize a group with a collective belief as having features 5 and 6.

As Gilbert argues in her (2000a) and as we briefly noted above, features such as these may run deeper still. When a person is obligated to speak or act in certain ways, it can affect his private

[32] This is most likely true even if the approval is qualified as in "Personally, I approve…."See e.g. Gilbert (2000a).

thoughts, inhibiting him from pursuing spontaneous doubts about the group view or from fully accounting for available evidence. Even though being party to a joint commitment to believe that p as a body does not require one to in fact believe that p individually, it is awkward and often difficult to believe one thing and say another with conviction. Whether consciously or not, this difficulty can impede a party to a collective belief from exploring possibilities that seem likely to lead to a contradiction between one's personal beliefs and the belief referenced in the pertinent joint commitment. An individual's personal beliefs, then, are liable to be strongly influenced by considerations directly arising from the collective belief. And so the collective belief can in fact change how an individual will react to and interpret new evidence in such a way that features 5 and 6 become true of the members of a group with a collective belief.[33]

To explain how the appreciation of risk in a research program relates to these considerations, and to the obligations arising from joint commitments in particular, it is important to say just what Smolin means by risk. Risk-taking scientists "invent their own directions" and "tend to provoke strongly polarized reactions" (342). Risk-taking within a research program amounts to exploring ideas that oppose the "entrenched approaches" on one's own initiative (294). That is, risk, according to Smolin, amounts to systematically and sustainedly bucking the consensus—precisely what one cannot do as a party to a collective belief.

On this account of riskiness in research, an obligation to endorse string theory makes risk-taking even more unlikely, since if one succeeds in taking risks, one violates the applicable joint commitment and may well be excluded from the social group by one's fellows, who take one to have indicated one's own readiness to be so excluded. And so it is unsurprising that Smolin finds few risk takers within the string theory community. One cannot take risks as Smolin understands them and yet

33 This phenomenon of avoiding cognitive dissonance is not pure speculation. For instance, see Festinger and Carlsmith (1959), and the subsequent literature on forced compliance.

still be sure of retaining one’s membership of the social group.

Smolin's biography is telling here. He worked successfully as a string theorist for many years, before deciding to explore new possibilities; now, by his own lights, he is no longer a member of the string theory community.

4.iii Feature 1: tremendous self confidence and sense of eliteness

Tremendous self-confidence or the sense that the group holding a belief is somehow elite are not features that Gilbert has previously discussed in relation to her account of collective belief. But there are two considerations that are in the spirit of Gilbert's account that lead one to expect string theorists to exhibit this feature.

The first consideration is an explanation of the appearance to an outside expert of tremendous (or irrational) confidence in the beliefs collectively held. Take Smolin as an example. He understands string theory, its consequences, and the evidence for and against it as well as any string theorist. On the basis of this knowledge, he has determined that the status of string theory is at best uncertain. And yet members of this community speak as though string theory is true. We may suppose that, for the reasons described above, they inure themselves against evidence and ideas that might conflict with their collective belief. An outside expert, however, might reasonably assume that they are accounting for all of the evidence he is (especially evidence he might present in argument). In other words, the apparent irrationality of the string theory community—its unwillingness to update its beliefs in an epistemically responsible way, according to some outside experts—is naturally interpreted by outsiders as certainty bordering on dogmatism—or even as hubris.

Once again, as with features 5 and 6, this effect may also run deeper.[34] When a member of a social group with a collective belief speaks as a mouthpiece of the group, she is acting on an authority

34 Thank you to Cailin O'Connor for pointing out this consequence of Gilbert's view.

partially independent of and likely more significant than her own. She speaks for the group, and the views that she espouses have the blessing of the group's other members. If we add to this that there are distinguished intellectuals and academics for whom the speaker has great esteem among the members of the group, that would seem to make the effect only more prominent. When a string theorist says that string theory is true---or unassailable---she can do so with confidence derived from the understanding that she speaks with and for these distinguished members of the group. Moreover, members of the group see other string theorists behaving similarly. When they express the views of the group amongst themselves, they do so assuredly and with the gravitas that comes from believing they will not be contradicted. Seeing this confidence can be impressive: the members of the group appear to have authority and expertise. Each member of the group, seeing the other members behave thus, can easily come to believe that the group is populated with elite experts on the subjects that the collective belief concerns.

4.iv Seven *unusual* features?

As we have just seen, all seven of Smolin's features appear consistent with Gilbert's account. But there is still one aspect of Smolin's description of these features that does not appear to mesh. Specifically, if, as Smolin says, string theory is *unusual* in having these sociological features, then Smolin's view is in tension with the idea that joint commitments of the kind that constitute collective beliefs are likely to be present in *all* scientific communitie. . So why does Smolin claim that the features he describes are unusual, if in fact they obtain for any group holding a collective belief?

One way to resolve the tension is simply to suggest that what makes string theory unusual in this regard is a matter of degree—that is, perhaps the manifestations of the string theory community's joint commitment are particularly emphatic, for reasons independent of their group belief. On this suggestion, scientific communities may often have just the sociological features Smolin describes, and may even be monolithic in general. But for various reasons, string theory has turned out to be an especially striking case. (Indeed, Smolin describes string theory as “unusually monolithic, with a strong sense of consensus”. One might take this as an acknowledgement that *all* scientific communities are monolithic to some degree , and all communities have some sense of consensus; string theory is merely an extreme example.)

A second possibility—compatible with the first—is that Smolin considers these features striking (or rather, they *appear* unusual to him) not because they are in fact unusual, but rather because Smolin and his collaborators stand in a different relationship with string theory than they do to other areas of physics. One might even think that Smolin’s position with regard to string theory is, historically speaking, an unusual one for *any* prominent scientist to occupy.

The idea is that it is uncommon for the physics community to fracture into such clearly defined sub-groups with conflicting collective beliefs . This means that it is unusual for a scientist to be a leader in his or her field—a fully competent expert—who nonetheless stands outside of a given collective belief within the field. But if and when this does happen, experts in the disagreeing communities would have a perspective that would make the features of the other communities—features such as those Smolin describes—more transparent, since such experts are well-equipped to evaluate the evidence for and against a particular theory without being party to the consensus. If this is right, then string theory would seem quite different to Smolin than other areas of physics.

One can connect this point with the discussion of Kuhnian paradigms above. There we suggested that paradigms might be thought of as collections of fundamental collective beliefs, that is, beliefs to which the members of a given community of scientists are jointly committed. The apparently irrational features of normal science, then, are just consequences of the existence of core joint commitments obligating scientists to speak and act in certain ways on pain of severe rebuke or even excommunication. In these terms, Smolin’s perspective on string theory is that of an expert in an area of science that is divided into groups with different paradigms, who stands outside of one of these paradigms, peering in.[35] From this perspective—not so different, perhaps, from Kuhn's own perspective in *The Structure of Scientific Revolutions*—certain epistemic pathologies that most scientists never notice are cast into stark relief.

Of course, on Kuhn's view, there is nothing unusual about science within a paradigm. It is *normal science*, after all. But the fact that normal science has certain features does not mean that those features are obvious to working scientists. Quite the contrary. If anything, what is unusual is the ability to recognize these features at all, since to do so, one needs to be able to step outside of one's own particular corner of normal science and its associated paradigms and examine it from this external perspective. We suggest that this, in effect, is what Smolin has done. He has stepped outside the string theory community and is able to view it form an uncommon perspective, that of an informed.

5. Conclusion

We have argued here that, given (1) Gilbert's joint commitment account of group belief and (2) the hypothesis that the consensus within the string theory community can be properly construed as an example of group belief in Gilbert's sense, the seven sociological features that Smolin attributes to string theory are precisely what one should expect of a group holding relevant group beliefs. In particular, we have observed that, apparently contra Smolin, and consistently with a Kuhnian approach to normal science, these features may not be *unusual* after all.

What *may* be unusual, however, is that such a circumscribed social group has come to exist as a

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[35] Kuhn believed there could be at most one paradigm in a field at a time—a view famously challenged by Lakatos (1970) and Laudan (1977). So there is something un-Kuhnian about the suggestion that there are multiple competing paradigms in quantum gravity. Thus, one might take the fact that Smolin does seem to stand outside of string theory when he criticizes it as evidence that Kuhn was mistaken, or perhaps as evidence that quantum gravity is in a period of crisis, during which paradigms are permitted to fracture on Kuhn's account. One way or the other, however, it is fully consistent with Kuhn's views that Smolin's perspective is *unusual*, insofar as he has the expertise to evaluate string theory without being committed to the paradigm.

sub-group within the physics community. Particularly, it may be uncommon for there to be practitioners in a given field with full accreditation and expertise who are not party to the joint commitments of the relevant scientific community. Then it would only be because Smolin occupies this "outside expert" vantage point that the features of string theory appear unusual to him. These considerations suggest that at least in some cases, the social features of scientific communities are well explained by supposing that scientific consensus, both with respect to paradigms and otherwise, can be understood as a variety of group belief in Gilbert's sense, with its attendant obligations.

Before concluding we would like to emphasize the following. As a matter of logic, that a given scientific community believes, in Gilbert's sense, that p, implies neither that p is true nor that p is false. Again, it does not imply that the community's belief that p is well-grounded empirically, or that it is not. The most that can be said along these lines is that *if* the core beliefs of a scientific community are false, or poorly grounded, there are the noted barriers to that community's coming to reject these beliefs, *in spite of* their falsity or poor grounding. That is all that this paper means to imply with respect to the string theory community, or any other.

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