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COLLABORATIVE RESEARCH, DELIBERATION, AND INNOVATION

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ABSTRACT

I evaluate the extent to which we could learn something about how we should be conducting collaborative research in science from the research on groupthink. I argue that Solomon has set us in the wrong direction, failing to recognize that the consensus in scientific specialties is not the result of deliberation. But the attention to the structure of problem-solving that has emerged in the groupthink research conducted by psychologists can help us see when deliberation could lead to problems for a research team. I argue that whenever we need to generate alternative solutions or proposals, groupthink is a genuine threat, and research teams would be wise to allow individuals opportunities to work alone. But the benefits of team work emerge when scientists seek to evaluate the various proposals generated, and determine a course of action. Then the group is less prone is groupthink, and the interaction of group members can be an epistemic asset.

Philosophers working in the social epistemology of science draw on a variety of conceptual and theoretical resources, including the resources of Game Theory (see Zollman 2007), mathematical modeling (see Kitcher 1993, De Langhe 2010), and research in social psychology and organizational behavior (see Rolin 2010). Indeed, the broad range of resources they appeal to ensures that the sub-field is dynamic and innovative. Miriam Solomon (2006) has recently appealed to the literature on groupthink and the Wisdom of Crowds in an effort to raise concerns about the power of deliberation as a means to reach a rational consensus in science.[1] Solomon is concerned that deliberation may lead to groupthink, where those involved in the deliberation reach a consensus prematurely, and are influenced by distorting factors like peer pressure, rather than evidence. She thinks that judgment aggregation is a more effective means to tap into the wisdom of the crowd. Judgment aggregation takes the determination of a group's view out of the hands of individuals and draws together the whole range of data that the various individual members of the group have. In this way, the group is more apt to be responsive to the full range of data than they would be without aggregation.

My aim in this paper is to make some advances in the social epistemology of science, specifically with respect to our understanding of collaborative research. Solomon's critical discussions of deliberation and the threat of groupthink provide a useful starting point for clarifying some important issues in the social epistemology of science. I will focus on four

1 For Solomon, the locus of rationality is the community as a whole, not the individual scientists. Consequently, as far as she is concerned, in principle, a community could reach a rational consensus even if no single individual scientist behaved according to the standard individualist canons of rationality (see Solomon 2001).

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issues. First, I want to draw attention to the complexity of the social structure of science, in particular, the need to recognize that there are social groups imbedded within social groups, and that these various groups operate and need to operate according to different norms. Most importantly, I distinguish between research teams who co-author articles, and scientific specialties, the broader social groups with which scientists often identify as a result of their education and training. Second, I aim to develop a better understanding of deliberation and its effectiveness as a means to reaching a consensus in science. I am concerned that Solomon has been too hasty and dismissive in her criticism of deliberation and rational discourse as means to achieving our goals in science. I have concerns similar to those raised by Deborah Tollefsen (2006: 38) and Alison Wylie (2006). Third, I aim to make some specific recommendations for organizing and managing collaborative research teams. Most importantly, I aim to show that there are specific tasks individual scientists should perform on their own, and other tasks that benefit from the constructive interaction of the group. An appropriately organized research team is in a better position to exploit the power of working in a group, and yet avoid the drawbacks, like the threat of groupthink. Fourth, I will make some methodological recommendations for future work in social epistemology.

In Section 1, I outline Solomon's concerns about the threat of groupthink. In Section 2, I explain why I take issue with her application of groupthink to the context of consensus formation in scientific *research communities*. I argue that research communities are not prone to groupthink. Then, in Sections 3 and 4, I examine a scientific context where groupthink may have some relevance, specifically collaborative research teams. Unlike research communities, *research teams* need to deliberate in order to reach a consensus about what view will stand as the view of the group. By examining some of the more recent critical literature on groupthink and group problem-solving, I aim to develop a better understanding of how collaborative research should be done. In Section 5, I briefly examine the relevance of judgment aggregation for collaborative research.

1. SOLOMON'S CONCERN: DYSFUNCTIONAL GROUPS

Solomon notes that deliberation and critical discussion have long been celebrated as the most effective means to realizing our epistemic goals. A number of influential philosophers, including John Stuart Mill (see Mill 1956), and philosophers of science, notably Karl Popper and Helen Longino (see Solomon 2006: 28), argue that critical discussion provides the most promising path to the truth.[2] These philosophers claim critical discussion leads people to refine and strengthen their views, altering them as needed in response to the criticisms of others (see Solomon 2006: 38).

Solomon, though, believes that "there is room for skepticism about the results of criticism, since sometimes deliberation makes things worse rather than better" (Solomon 2006: 38). She notes that studies in organizational research, often done by scholars at business schools, suggest that group deliberation is remarkably ineffective, often leading to suboptimal outcomes (see Solomon 2006: 31). For example, Solomon notes that

2 Philip Kitcher (1997) and Paul Feyerabend (1988) are also influenced by Mill's analysis and defense of free inquiry. Kitcher, though, is concerned that some topics should not be investigated, despite Mill's defense of free inquiry.

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"group deliberation often produces worse decisions than can be obtained without deliberation" and that "a group of nonexperts often produces better decisions on a topic than does an expert about that topic" (Solomon 2006: 31).[3]

Most importantly, Solomon suggests that when groups deliberate "pressure to reach a consensus can ... lead (1) to phenomena such as groupthink and (2) to the suppression of relevant data" (Solomon 2006: 28; numerals added). These concerns are related. When a group deliberates in their efforts to solve a problem, individual members of the group may be led to suppress information they have that may threaten to undermine a consensus that is emerging in the group. They may be compelled by peer pressure or intimidated by a forceful group leader to withhold information that would stall the group in reaching a consensus. But the specific information that they are suppressing may be just the sort of information that would have led the group to reach a different decision than the one they reached.

Solomon thinks that a more promising path to reaching a rational consensus is through judgment aggregation (see Solomon 2006: 31). When we aggregate judgments, we solicit each individual's judgment *independently*, thus preventing some individuals from adversely influencing the judgments of others in the group. Individuals are thus less likely to suppress the information they have, information that conflicts with the views of the most vocal members of the group. In this way, valuable information is less likely to be lost. Indeed, there is a growing body of literature on the wisdom of crowds that cites impressive examples where aggregation leads to effective solutions. For example, Francis Galton famously noted that the mean of the estimates of the weight of an ox of "approximately 800 people visiting a fair in Plymouth" was within one pound of the real weight of the ox (see Solomon 2006: 34; also Surowiecki 2004: xi–xiii). Solomon notes that unlike deliberation which can lead to groupthink, "*aggregation without deliberation preserves information*" (Solomon 2006: 36; emphasis in original). Hence, the resulting view is reached on the basis of a consideration of all the available data.

Solomon also argues that philosophers have exaggerated the value of consensus (see Solomon 2001). As far as she is concerned, it can be an impediment to reaching the truth. Dissent in a community helps preserve information. As long as dissenting views are heard, then valuable information is less likely to be lost. Solomon's defense of dissent is similar to Mill's defense of the protection of free thinking and free speech in *On Liberty*. Mill argues that when people are permitted to express dissenting opinions, the views we accept are kept alive, rather than being accepted uncritically and becoming dead dogmas (see Mill 1956: Ch. 2). When dissenting views are suppressed, on the other hand, people quickly forget why the accepted view is a rational view. Unlike Solomon, though, Mill does believe that deliberation is an effective means to getting at the truth.[4]

3 Unfortunately, Solomon does not provide sources for these "findings." Also unfortunate is the fact that the first claim is described as occurring "often," and parenthetically as "often enough for epistemic concern" (see Solomon 2006: 31). This is a bit too vague to warrant much concern. It is common now to note that the consequences of our actions are relevant to determining how much epistemic concern we should have (see Douglas 2009). So the frequency of mistakes alone cannot determine the worth of an epistemic practice. The frequency of mistakes has to be weighed along with consequences of mistakes.

4 Paul Feyerabend (1988) has developed a similar argument, explicitly drawing on Mill, but aimed at understanding the importance of developing alternative scientific theories. According to Feyerabend, some data can only be seen and appreciated if we consider matters from alternative theoretical perspectives. Theories, after all, limit what we see and how we conceptualize what we see.

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2. WHICH GROUPS?

The precise target of Solomon's analysis is far from clear. At times she seems to be concerned with social epistemology in general, and any sort of situation in which we might encounter a group deliberating about a topic with the aim of determining a course of action. At other times, though, she seems to have science and scientific inquiry specifically in mind. She does cite Longino (1990) and explicitly refers to Popper, and distinguishes her own view from their views. In what follows, I will assume her analysis was intended as a contribution to the social epistemology of *science*. I assume her aim is to demonstrate the relevance of both groupthink and judgment aggregation to the epistemology of science.

There is a further ambiguity about the target of Solomon's analysis. It is not exactly clear what types of groups of scientists she is concerned about. Given the authors she explicitly addresses, and her own past writings, I assume that Solomon is concerned that a *research community* as a whole may be prone to groupthink.[5] Specifically, it seems that her concern is that a research community or scientific specialty may settle on a theory prematurely because individual scientists in the community may withhold information that would challenge the theory about which a consensus is emerging either because of peer pressure or unwarranted pressure from community leaders, those in the research field who command power over resources.[6]

In the remainder of this section, I want to explain why the threat of groupthink in this context is unfounded, and also why judgment aggregation is equally irrelevant to understanding this dimension of the culture of science. Solomon, I argue, is focusing on the wrong sorts of groups.

Scientific specialties are not the sorts of groups that *deliberate*. This is a point I have discussed at length elsewhere (see Wray 2007). The scientists working in a specialty do not gather together to determine when a theory should be accepted, or whether in fact a long-accepted theory should be replaced. Instead, when there is a consensus in a scientific research community it is a consequence of many individual scientists accepting the same theory. Different scientists will be compelled by different evidence, given their training and past experiences, and no individual scientist will have access to all the relevant evidence. Some scientists may even be persuaded to accept a new theory by the leaders in their field, rather than as a result of their own assessment of the evidence (see Wray 2011: Ch. 11). But as a matter of fact sometimes scientific communities do reach a consensus. Geologists, for example, came to accept the theory of plate tectonics in the mid-1960s and early 1970s. But geologists did not need to deliberate as a group in

5 A similar concern is raised by Margaret Gilbert (2000). She thinks that theory change can be stalled when a research community collectively believes a theory. The belief of the community may not change even after most of the individual members of the community change their personal beliefs about the long-accepted theory.

6 It is widely recognized that scientists sometimes withhold information relevant to scientific investigations willfully. But in these cases generally the concern seems to be that scientists are withholding information, not out of fear, but rather to retain the advantage of having data that others do not have. Zuckerman and Merton (1971/1973) suggest that the scientific journal is an institution designed to discourage this sort of behavior. By publishing what one knows in a scientific journal, one gets the recognition from one's peers for the discovery. But one also makes available to one's peers information that may help their research projects, which in turn may help the research community as a whole (see Zuckerman and Merton 1971/1973: 464, 467).

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order to reach a consensus. Groupthink, though, only affects groups that reach a consensus by means of *deliberation*. Groups prone to groupthink must aim to settle on a view that will stand as the group's views. Hence, given that scientific specialties are not the sorts of groups that aim to let a view stand as the view of the group, they are not prone to groupthink.

There is another reason for thinking that Solomon is mistaken in claiming that scientific specialty communities could be prone to groupthink. It seems that such groups do not have key features that the sorts of groups that have been identified as prone to groupthink have.

There is now a vast body of literature on groupthink, much of it concerned with understanding when and what types of groups are prone to the phenomenon (see, for example, McCauley 1989; Aldag and Fuller 1993; Esser 1998). Irving Janis, the psychologist who first introduced the concept, originally appealed to it as a means to explain decision-making fiascos in international policy contexts (see Janis 1971; also Aldag and Fuller 1993). For example, Janis examined the decision making that led to the United States' support for the Bay of Pigs invasion in the early 1960s, a key event in the cold war. He also examined the decision making that led to the United States' decision to engage in a war in Vietnam. Other researchers have suggested that groupthink may occur in other contexts. Indeed, psychologists have conducted numerous laboratory studies aimed at determining what sorts of contexts and social groups give rise to the threat of groupthink. The various laboratory studies of groupthink assign tasks to groups of research subjects, tasks that are far removed from the sorts of situations that initially inspired Janis to invoke the concept. In principle any sort of deliberating group could be prone to groupthink and determining which groups are in fact prone to it requires empirical investigation.

Janis is quite explicit about the sorts of features that characterize the groups that are prone to groupthink.

(1) Such groups stick "with the policies to which the group has already committed itself, even when those policies are obviously working out badly";
(2) They make unrealistic appraisals "of alternative courses of action";
(3) The "members of decision-making groups . . . avoid being too harsh in their judgments of their leaders' or their colleagues' ideas" (Janis 1971: 43).

It seems clear that scientific specialties lack these features. They do not make policies. They do not make appraisals of courses of action. And it is not clear that the members of specialties avoid being harsh in their judgments of colleagues' ideas. Further, Solomon provides no evidence that specialties are similar in the relevant respects to the sorts of groups that Janis identified as prone to groupthink. In fact, she does not discuss the features that allegedly characterize groups prone to groupthink. And, to repeat what I have argued above, scientific specialties are not decision-making bodies. The consensus that does emerge in such communities is not a consequence of deliberation.[7] The exchange of ideas that plays out in the relevant scientific journals is not deliberation in the relevant

7 Some scientific groups, though, do have the task of determining a consensus view. Medical consensus conferences, for example, are responsible for making policy recommendations on medical treatments (see Thagard 1999), and other scientific groups have been brought together to give policy advice (see Beatty 2006 for a vivid example).

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sense. The various publications that are part of this exchange are not directed toward the same end. Deliberation, though, is directed toward an end, a plan of action. It is in the efforts to realize a shared goal that groups of individuals are led to act in ways that result in the fiascos associated with groupthink.

Judgment aggregation seems equally inapplicable to scientific specialties. There is no designated group or individual assigned the job of determining what the consensus or view of a research community should be. Philip Kitcher, in his modeling of the division of cognitive labor in science, has invoked the idea of a powerful science planner, a philosopher-monarch, with the power and responsibility to determine who works on what research programs (Kitcher 1993: 305). But even Kitcher knows no such figure exists. He invokes the idea as a heuristic. He was concerned to evaluate the effectiveness of the current division of labor in science, a division of labor that tends to lead to an excessive concentration of scientists on some research problems and strategies at the cost of neglecting other research problems and strategies.

Indeed, there is a deeper problem with Solomon's appeal to judgment aggregation. Solomon seems to assume that we all know what is involved in aggregating judgments. Her appeal to Galton's ox example makes it look like a straightforward procedure. In that example, one merely calculates the mean of the various individual judgments of the ox's weight. But judgment aggregation is a far more complicated affair, as is evident from the vast technical literature addressing the issue. Indeed, there are lively debates about how we should aggregate the judgments of a group. And things can be complicated further when we consider aggregating judgments that may involve chains of reasoning. For example, there is a famous paradox that arises when we consider a group's judgments of a set of premises and the group's judgment of a conclusion that is deductively entailed by the premises. Though the majority of the members of the group may accept a set of premises, the majority may not accept a conclusion that is deductively entailed by the premises (see Dietrich 2013: 512–13). And there is no universally accepted way to resolve such problems. Hence, judgment aggregation is not, at least not always, the straightforward procedure that Galton's ox example makes it out to be.

It would seem that the sort of judgment aggregation that would be relevant in the context of science would be the complex sort, and not merely the sort that takes the mean of various individual judgments on an issue. In scientific contexts where groups may be tempted to aggregate their judgments the groups are dealing with complex chains of reasoning.

There is a general methodological point to which I want to draw attention. Philosophers are encouraged to appeal to whatever conceptual and theoretical resources they find useful in their efforts to understand the epistemic culture of science. The science of science, including the philosophy of science, is no different than other areas of science. Methodological pluralism can be beneficial. But the onus is on the philosopher to show that the concepts and theoretical frameworks that were developed in one context, for example, international policy making, are applicable to the study of the culture of science. In this instance I do not think Solomon has made the case that the concept "groupthink" or the theoretical framework of judgment aggregation illuminates the issue at hand. Neither shed light on how or when a consensus is or should be reached in a scientific specialty.

Let me be clear about what I am not claiming. I am not objecting to Solomon's appeal to psychological research as a means to shed light on the culture of science. Indeed, there is

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a long tradition of naturalizing the epistemology of science by drawing on insights from research in psychology. Thomas Kuhn (1962/2012) did it to great effect, drawing on the anomalous playing card experiments of Bruner and Postman, the experiments involving inverted visual images, and other studies as well. The objection I raise is with respect to the specifics of Solomon's appeal to groupthink. So far it seems that the notion of groupthink is irrelevant to science. Certainly when examining the dynamics of scientific specialty communities and the erosion and emergence of consensus in them, it seems that groupthink is not a relevant concept. James Esser (1998) has rightly noted that "the risk of casually characterizing poor group decisions as groupthink without careful and thorough analysis, is to make groupthink a convenient label with little explanatory or predictive value" (Esser 1998: 126). If the concept is to be useful in illuminating group dynamics, then it must be used with precision, and applied with restraint. Concepts drawn from psychology, especially those that have been popularized, like "the tipping point" and "wisdom of crowds," risk becoming facile clichés.

3. COLLABORATION AND GROUPTHINK

There is another social context in science to which the concept of groupthink *may* be relevant. Specifically, I am thinking of collaborative research teams. Research teams, unlike scientific specialties, do *aim* to reach a consensus on an issue. Some sort of consensus seems necessary if the group is to publish the results of their research in a co-authored article. The group must, for example, resolve the issues of how to interpret their data, or which methods to use in their experiments. And deliberation plays a crucial role in the determination of the group's view. Hence, such groups are at least in principle vulnerable to groupthink.

It is worth noting that the types of groups that engage in collaborative research in science are a heterogeneous lot. There are collaborations involving two scientists, collaborations involving a team working in a laboratory together, and enormous collaborations that are tied to permanent and costly research facilities, like the research teams that work at CERN or Fermilab. So we need to be mindful of these differences in determining which sorts of scientific groups are prone to groupthink, if in fact any sorts of groups are. In the remainder of the paper, I will be concerned with groups of scientists that produce coauthored articles, articles that present the view of the group as a whole.

Let us first consider what insights we might gain from the scholarly literature on groupthink that has developed since the publication of Janis' original paper on the topic, specifically the laboratory studies of groupthink. Aldag and Fuller (1993) and Esser (1998) provide useful summaries of the findings from the laboratory studies of groupthink. The various laboratory studies involve bringing together experimental subjects to work as groups on solving a problem. A number of variables have been manipulated in these experiments. Some have included compatriots of the experimenters, planted in the group to determine the effects of a forceful leader (Esser 1998: 131). Other studies have involved time constraints, for it appears that when a group must make a decision in a timely fashion they are prone to a variety of dysfunctional behaviors, including groupthink. In some studies, experimental groups were given misinformation about the cohesiveness of the group (see Esser 1998: 127). Roughly, cohesiveness is a measure of the extent to

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which the group has "esprit de corps and they work together on a continuing basis" (Flowers 1977: 888).[8]

Let me briefly summarize some of the key findings of these laboratory studies.

(1) One study found that "open leadership style produced more suggested solutions" to the problem the group faced and that such groups use more of the "available facts than did [groups with a] closed leadership style" (see Aldag and Fuller 1993: 536).
(2) Another study found that "contrary to the groupthink prediction, members of noncohesive groups exhibited more self-censorship than did members of cohesive groups" (see Esser 1998: 127).
(3) Another study, though, found that "cohesive groups were more discouraging of dissent than noncohesive groups. However … cohesive groups reported less self-censorship and generated more alternatives than noncohesive groups" (Esser 1998: 130).
(4) Another study found that "groups with directive leaders used less available information, suggested fewer solutions, and rated their leader as more influential in the decision process than groups with nondirective leaders" (Esser 1998, 131).

Some of these findings might have some bearing on how we organize or manage research teams in science. But the findings are rather general and provide no concrete recommendations for organizing and managing a research team. Indeed, in order to draw specific recommendations one would need to compare the social structure of the collaborative research team and the constraints on their decision making with the social structure and constraints on the specific groups in the laboratory studies. Some studies may thus be relevant or insightful to some collaborative research teams, but not relevant or insightful to others. So far we are not yet in a position to make specific recommendations to scientists working collaboratively.

8 Tollefsen raises concerns about the ambiguity of the term "group cohesion" (see Tollefsen 2006: 40). As a matter of fact, the term has been defined and operationalized in a number of different ways in the literature on groupthink. Matie Flowers (1977) treats group cohesion as an independent variable, and defines it as "attractiveness of a group to its members" (889). Flowers claims that "two levels of this variable were employed [in her study] – groups of acquaintances (high cohesive) and groups of strangers (low cohesive)" (Flowers 1977: 889). Fodor and Smith (1982) also treat group cohesiveness as an independent variable. Unlike Flowers, though, who started with groups that were cohesive, because the group members were acquaintances, Fodor and Smith tried to "foster cohesiveness … by telling some groups that the group that performed the best would obtain a reward" (see Fodor and Smith 1982: 180). The groups promised a reward for excellent performance were classified as highly cohesive. In contrast, the subjects in the control group, who were offered no reward, were classified as low in cohesiveness (see Fodor and Smith 1982: 180). Callaway and Esser (1984) also treated group cohesion as an independent variable. The subjects in their study were administered a "bogus test" which they were told would be used to assign them to groups. "Subjects in the high cohesiveness (HC) conditions were led to believe that they were members of a select, congenial, and effective group whose members had been matched on personality and demographic information obtained in the initial phase of the study," that is, the bogus test (Callaway and Esser 1984: 159). In contrast, "subjects in the low cohesiveness conditions were led to believe that such a match had been impossible for the group" (Callaway and Esser 1984: 159). They were thus led to believe that their group had a low level of cohesion.

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4. PROBLEM-SOLVING AND COLLABORATIVE RESEARCH

There may be a more fruitful way to draw on the research on groupthink to make advances in the social epistemology of collaborative research. Aldag and Fuller provide a general model of *group problem-solving*, a framework that may be useful for understanding the challenges that collaborative research teams are apt to face.

Aldag and Fuller note that problem-solving involves a number of distinct stages:

(1) problem identification,
(2) alternative generation,
(3) alternative evaluation and choice,
(4) decision implementation, and
(5) decision control. (Aldag and Fuller 1993: 541; numerals added)

They argue "that groupthink defects focus primarily on the first three stages of the problem-solving process" (Aldag and Fuller 1993: 541). That is, groupthink threatens (1) the problem identification stage, (2) the alternative generation stage, and (3) the alternative evaluation and choice stage. Importantly, Aldag and Fuller also argue that

> the *various stages of the problem-solving process may require different social arrangements*, and those appropriate at one stage may be troublesome at another. For instance, although interaction may be facilitative at the alternative evaluation and choice stage, it is often dysfunctional at the alternative generation stage. (Aldag and Fuller 1993: 542; emphasis added)[9]

Aldag and Fuller were not concerned with research teams. Rather, they were concerned with developing a general model of group problem-solving and decision-making. But let us apply their insights to the context of scientific research teams. It seems that when a laboratory team is seeking to *generate* alternative solutions to a problem, interaction may lead members of the team to suppress their ideas, and thus lead to groupthink. That is, at the stage of generating alternative solutions, team members may be inclined to suppress their ideas, especially if they sense any sort of resistance from others in the group. But once the various individual members of the team have generated a number of alternative solutions, an interactive discussion with the others is crucial to effectively *evaluate* the various proposed solutions. That is, the group becomes an asset once alternative solutions are generated. The group becomes a valuable source of criticism, and the differences between the individual members ensure that the various alternatives will be scrutinized from multiple perspectives.

It may be tempting to think that we can rehabilitate the distinction between the context of discovery and the content of justification as a means to make sense of the findings on groupthink. In this framework the lessons learned from the groupthink research could be summarized as follows. In the context of discovery when alternative hypotheses are needed deliberation can be an impediment. But in the context of justification matters are quite different. There, we are choosing between competing alternatives, and deliberation is a useful resource. Deliberation is thus restricted to the context of

9 Similarly, James Surowiecki notes that "groups are better at deciding between possible solutions to a problem than they are at coming up with [possible solutions]" (Surowiecki 2004: 60).

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justification. In the context of discovery deliberation appears to be unnecessary, even counterproductive.

But this analysis is a bit too simplistic. It fails to take into account the fact that there are many stages in a research project where we need to encourage the development of *alternatives*, and where premature deliberation may stifle creativity and the advancement of science. At the stages in the research project where the team needs to generate multiple possible interpretations of the data, or multiple plausible explanatory hypotheses, group members may need a chance to work alone, shielded from the critical scrutiny of the other team members. Indeed, we may also want to encourage the generation of numerous alternatives when scientists are trying to determine what methods or instruments to employ. Premature deliberation about such matters could stifle the creativity of scientists and suppress interesting and novel ideas. But in order to glean the full benefits of working with a group the various competing interpretations or explanations should be scrutinized by different people. Hence, at the hypothesis evaluation stage deliberation seems to be a valuable resource afforded by teamwork. Similarly, after the individual members of the research team have generated ideas about which methods to employ, deliberation would aid the group in determining which proposal is apt to be most effective.

Clearly, this sort of information about group problem-solving seems relevant to the way we should structure and manage research teams in science.[10]

5. JUDGMENT AGGREGATION

It is worth briefly reflecting on the relevance of judgment aggregation to scientific research teams. I doubt that the sort of work that scientists do when they work in research teams would benefit from the straightforward sort of judgment aggregation that Galton discusses in the determination of the weight of the ox at the country fair. That is, I doubt that the interests of a research team can be served by merely calculating the mean of a series of individual judgments of some value. Science is far more complex than that.

Where judgment aggregation is relevant to the work of a research team, it would be at Stage 3 in the process described by Aldag and Fuller, the evaluation of alternatives. But scientists are apt to be reluctant to relinquish control over what views they accept as a research team to a mechanical aggregation process. But given Solomon's conception of judgment aggregation, that is what judgment aggregation requires. For judgment aggregation to work effectively, we need to "preserve" the information that the various individuals in a group have. And this requires that each individual member's judgment is independent of the other members' judgments. But this would amount to having each scientist work alone, and merely gather together to aggregate their judgments. Such an approach misses the point of collaborative research. Scientists collaborate in order to access resources, skills and knowledge they do not individually have.

Perhaps one scientific context where this straightforward sort of judgment aggregation might be useful and appropriate is with respect to committees of scientists commissioned

10 It may seem that my analysis, which draws on the literature on group decision-making, has some bearing on the role of values in science, aiding us in determining at which stages of inquiry values can play a legitimate role. Though I am not prepared to develop the argument here, I think the issues of values in science and this concern about groupthink run orthogonal to each other.

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to issue policy recommendations. Such an example is discussed at length by John Beatty (2006). In the 1950s in the United States a number of biologists were asked to make recommendations about safe levels of radiation exposure for humans. Some of those involved questioned whether scientists had adequate knowledge about radiation and its effects to make a warranted assessment and recommendation. But the biologists involved also knew that if they did not provide a recommendation someone else would be asked to provide one, and that someone else may have been even less qualified than the biologists to do so. Given that the biologists involved in this process disagreed quite extensively, judgment aggregation *may* have provided an expedient and reasonable solution to the problem at hand. Importantly, the situation these biologists faced is similar in some important respects to the situation that the political advisors faced in the examples discussed by Janis. The biologists had a pressing problem on their hands, and there were significant time constraints, as the government was determined to issue a policy statement about the safe levels of radiation exposure. The government was not going to wait until more research on the topic was completed. Consequently, we should not be surprised if groupthink is a genuine threat in contexts in which scientists are pressed to make policy recommendations. The conditions are very similar to those that led to the fiascos in international policy-making studied by Janis.

A more complex form of judgment aggregation probably does have a role to play in scientific research teams. Research teams, after all, must reach agreement on what views are expressed in their co-authored publications. Where agreement cannot be easily reached, it would make sense to rely on some mechanism or other to enable the various members of the group to transcendent their differences and settle on a view that will stand as the view of the group. But in this context, all the problems that arise in the technical literature on judgment aggregation are apt to arise, for there is no indisputably correct way to resolve the differences of opinion of a group. Resolving the larger question about what the proper means is to aggregate judgments is beyond the scope of this paper.

One proposal that has a prima facie plausibility is that it is probably best if different scientific research teams use different methods of judgment aggregation. In this way, collectively the scientific research community benefits from a diverse portfolio of methods, and in the long run scientists can study the success rates of different methods of aggregation. This proposal is in the spirit of naturalized epistemologies, for it recognizes that many questions in epistemology are empirical questions, and can only be adequately answered by appeal to empirical data.

6. CONCLUDING REMARKS

My aim has been to evaluate the extent to which we could learn something about how we should be conducting collaborative research in science from the research on groupthink. Solomon seems to have set us in the wrong direction, failing to recognize that the consensus in scientific specialties is not the result of deliberation, at least not in the sense relevant to the phenomenon of groupthink. Nonetheless, the attention to the structure of problem-solving that has emerged in the groupthink research conducted by psychologists can help us see when deliberation could lead to problems for a research team. The analysis of problem-solving developed by Aldag and Fuller provides a useful framework for understanding at which stages in the research process a research team is most prone to

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groupthink. Whenever we need to generate alternative solutions or proposals, groupthink is a genuine threat, and research teams would be wise to allow individuals opportunities to work alone. The benefits of team work emerge when scientists seek to evaluate the various proposals generated, and determine a course of action. Then the group is less prone to groupthink, and the interaction of group members can be an epistemic asset.

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