The Epistemic Significance of Collaborative Research*

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I examine the epistemic import of collaborative research in science. I develop and defend a functional explanation for its growing importance. Collaborative research is becoming more popular in the natural sciences, and to a lesser degree in the social sciences, because contemporary research in these fields frequently requires access to abundant resources, for which there is great competition. Scientists involved in collaborative research have been very successful in accessing these resources, which has in turn enabled them to realize the epistemic goals of science more effectively than other scientists, thus creating a research environment in which collaboration is now the norm.

1. Introduction. In "A Plea for Science Studies" Philip Kitcher (1998) calls for a new approach to the study of science, one that addresses traditional rationalist concerns, like accounting for progress in science, and yet acknowledges the social dimensions of scientific inquiry. David Hull has been a pioneer in this approach to the study of science, exposing ways in which the social structure of science contributes to the success of science. Following Hull's (1988) example, my aim in this paper is to examine the epistemic significance of collaborative research in science—research that culminates in coauthored articles. Initially, the phenomenon of collabo-

*Received February 2001; revised June 2001.

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‡I thank David Hull, Harold Kincaid, Robert K. Merton, Lori Nash, Jean-François Auger, Allan Walstad, Jonathan Cohen, Les Burkholder, and Keith Douglas for comments on earlier drafts. I also thank the referees for *Philosophy of Science* for their insightful reports. Finally, I thank my audience at the annual meeting of the Canadian Society for the History and Philosophy of Science, and the philosophy departments at the University of Saskatchewan, University of Mississippi, Trent University and the University of Guelph, to whom I presented earlier versions of the paper.

Philosophy of Science, 69 (March 2002) pp. 150–168. 0031-8248/2002/6901-0008$0

rative research might appear to be of minor epistemic significance. As we will see, if the explanation offered by Beaver and Rosen (1978, 1979a, 1979b) is adequate, that would indeed be the case, for they offer a strictly sociological explanation of collaborative research in science, and regard the trend toward more collaborative research as merely a change in the practice of science. However, I will argue that the data suggest that collaboration plays a much more interesting role. Collaborative research is becoming more popular in the natural sciences, and to a lesser degree in the social sciences, because contemporary research in these fields frequently requires access to abundant resources, for which there is great competition. Scientists involved in collaborative research have been very successful in accessing these resources, which has in turn enabled them to realize the epistemic goals of science more effectively than other scientists, thus creating a research environment in which collaboration is now the norm.

In developing my explanation, I will draw on Harold Kincaid's (1996) account of functional explanation because it seems the most straightforwardly applicable to the case at hand. By offering a functional explanation of the phenomenon in terms of its epistemic merits, and showing that it is the best explanation for the phenomena that needs to be explained, I will make a case for the epistemic relevance of collaboration. Specifically, I argue that collaboration plays a causal role in advancing scientists' epistemic goals, and that its growing popularity is a consequence of its effectiveness in aiding communities of scientists to realize their epistemic goals.

In Section 2, I provide some background on the changing trends of collaboration in science. In Section 3, I give an account of functional explanations. In Section 4, I present and defend a functional explanation for collaborative research in science. In Section 5, I consider a number of alternative explanations for the growing importance of collaborative research in science, highlighting the strengths of my functional account. Finally, in Section 6, I consider some of the epistemic costs of collaborative research.

2. Background. Collaboration plays a significant role in modern science. According to Paul Thagard (1997, 244), by the 1950s, "83% of papers in selected journals in the physical and biological sciences were collaborative efforts." In the social sciences, "the number was 32%" (244). By 1992, "in *Physical Review Letters,* 88% of papers were multiauthored, and in *Cognitive Psychology* 75% of papers involved collaboration" (244).[1] My own search on *The Web of Science* indicates that from a sample of 300 of the

1. Zuckerman and Merton (1973a, 476–477) found that of the 14,512 manuscripts submitted to *The Physical Review* between 1948 and 1956, slightly fewer than half were co-authored.

3105 articles published in 1998 in *Physical Review Letters,* 88% were multiauthored. And of the articles published in 1998 in *Review of Modern Physics, Journal of Physical and Chemical Reference Data,* and *Reports on Progress in Physics,* three of the most important journals in physics, 62% were multiauthored. In a sample of 365 articles drawn from articles published in 1998 in three of the most important journals in genetics, *Genes and Development, Annual Review of Genetics,* and *Trends in Genetics,* 82% of the articles were multiauthored. And, in samples drawn from the leading journals in both zoology and psychology, about 75% of the articles were multiauthored.

Even the scientific ultra-elite, the Nobel prize winners, collaborate. In fact, as Harriet Zuckerman (1977, 176) notes, "the majority of investigations honored by Nobel awards have involved collaboration." Further, as Zuckerman explains, "the laureates were trend setters and were more assiduously engaged in [collaborative] work than were other authors of journal articles in the same sciences published at the same time" (176). Even while still in their twenties, laureates-to-be jointly authored 7.9 articles on average, whereas a comparable sample of non-laureate scientists jointly authored only 2.9 articles on average (146). Zuckerman's matched sample of non-laureates consisted of American scientists who later became members of the National Academy of Science. Interestingly, it is the collaborative research record of laureates-to-be that distinguish them from these other promising young scientists, for "there was no great difference between the average number of single-authored papers published by future laureates in their twenties (5.2) and those published by the matched sample of productive scientists (4.3)" (147).

Collaborative research has not always been so prevalent in the sciences. Though much early modern scientific research in physics and chemistry was the result of the *collective* efforts of many people, it was generally not collaborative research. For example, as Steven Shapin (1994, 356) notes, *Continuation of New Experiments Physico-mechanical, Touching the Spring and Weight of the Air* is authored by Robert Boyle alone, though Boyle acknowledges in the preface that others, including Denis Pepin, a remunerated technician, contributed to the design of some of the experimental projects and the execution of many of the experiments. As Shapin explains, what entitled Boyle to the authorship of the text was the fact that he "took overall responsibility for what was claimed as a matter of fact and its legitimate interpretation in [the] text" (358). Boyle "was, and he insisted that he was, in charge of the scene and the operations that took place in his laboratory" (358). In collective but non-collaborative research, like the research in Boyle's laboratory, credit and responsibility rest with one person. In contrast, in collaborative research credit and responsibility are shared.[2]

2. A variety of types of social groups are operative in science, and I have only distin-

The fact that scientists collaborate to the extent that they do requires an explanation. And I want to emphasize that an explanation strictly in terms of individuals' motives is unsatisfactory, for, as I will explain in Section 5, scientists often resist collaborating with their peers, and there is evidence that scientists are often unaware of the epistemic benefits that collaboration affords. Further, what needs to be explained are social trends. An adequate explanation must explain both (a) why there has been a shift from collective research to collaborative research, and (b) why collaborative research is more popular in some sciences than in others. My aim in this paper is to offer an explanation.

3. Functional Explanations. Before I lay out the details of my account, I want to briefly discuss the structure of functional explanations, for the explanation I offer is a functional explanation. Providing a defense of functional explanations of human behavior in general is beyond the scope of this paper, and a thorough and compelling defense of such explanations has been developed by Kincaid (1996). Consequently, I will assume that functional explanations are, in principle, acceptable.

Functional explanations "identify specific causal effects of a practice or institution and then argue that the practice exists *in order to* promote those effects" (Kincaid 1996, 103). For example, Marvin Harris argues that the function of extended lactation among hunter-gatherer societies is to depress fertility and population growth (see Little 1991, 96). This explanation suggests that (I) when a hunter-gatherer society engages in extended lactation there is, as a consequence, reduced fertility and population growth, and (II) the resulting reduced fertility and population growth, in turn, causes the group to continue extended lactation. In such an explanation, it is assumed that the group did not initially engage in extended lactation in an effort to reduce fertility and population growth. Rather, the decrease in fertility and population growth is an unintended effect of extended lactation. But the resulting reduced fertility and population growth causes the group to persist in extended lactation. Hence, the cause is not consciously adapted as a means to achieving the desirable effect, at least not initially.[3] Further, in such explanations, the initial order of the events is

guished between collaborative researchers and collective research groups. As Hull (1988) notes, there are informal research groups, like those operative in the early days of numerical taxonomy.

3. Daniel Little is suspicious of functional explanations. Little (1991, 93–94) accepts George Williams's explanation, that certain deer have conspicuous markings on their tails which they display when they are in flight from a predator in order "to enhance the survivability of offspring." But, he insists that the features of social systems that can be explained functionally must be selected for their benefits "by intentional actors," because he does not believe that there is a process of social selection comparable to

important. In this example, extended lactation is the initial cause. Hence, the explanation does not imply that were there to be depressed fertility and population growth in a hunter-gatherer society that did not engage in extended lactation, their reduced fertility and population growth would lead them to engage in extended lactation. Rather, what is implied is that once the group has engaged in extended lactation and experienced the resulting depressed fertility and population growth, this will incline them to continue the practice.

We can now consider a schemata for understanding the general structure of functional explanations. Kincaid (1996, 105–114) suggests that functional explanations are complex causal explanations consisting of the following three claims:

(1) *A* causes *B*.
(2) *A* persists because it causes *B*.
(3) *A* is causally prior to *B*.

Given the first two claims, one would expect to see a causal chain of the following sort:

$$\begin{array}{cccc} t1 & t2 & t3 & t4 \\ A \rightarrow & B \rightarrow & A \rightarrow & B \end{array}$$

The third claim, Kincaid notes, serves to (i) "explicitly *rule out* inverting the explanation," thus treating *B* as the initial cause, and (ii) rule out situations of positive mutual reinforcement, where it does not matters whether *A* or *B* occurs first (112). Consider the example above. Even though depressed fertility and population growth may cause the persistence of extended lactation, it does so only if the reduced fertility and population growth is first brought about by extended lactation. Functional explanations thus aim to identify the initial cause in a causal chain of the sort illustrated above, which, once started, may be perpetuated indefinitely.

It is Kincaid's insistence on the third claim that distinguishes his account of functional explanations from other influential accounts. Larry Wright's (1973) and Robert Cummins's (1975) accounts of functional explanations do not demand the third condition. Kincaid argues that such accounts are too broad, and would lead us to attribute functions to far

natural selection (100–101). I disagree, for I believe that functional explanations of social phenomena invoke a process of social selection similar to the process of natural selection. I argue, later, that in some fields of science the proportion of scientists not inclined to collaborate is decreasing because those who do not collaborate are less productive, and are subsequently driven out of competitive research environments.

more things than he believes have functions in the relevant sense. According to these broader accounts, "to cite a function is to assert that an entity has certain persistent capacities in an overall system" (Kincaid 1996, 106 and 110). But, a function, in the sense relevant to Kincaid's analysis, exists because it serves the purpose it does. A bookshelf placed against a wall can *persist* in keeping a house free of mice. But if the bookshelf only inadvertently keeps mice out, then its *purpose* is not to keep the house free of mice. This distinction between these two types of functions is now widely recognized by others. Paul Griffiths (1993), for example, calls the type of entity or feature that merely has a persistent capacity a Cummins-function, whereas he calls the type of entity or feature that has a purpose a *proper* function. The sort of functional explanation that interests both Kincaid and myself is an explanation of a proper function.

Kincaid also believes that a plausible functional explanation requires a specification of the context in which the causal mechanism is efficacious. In fact, as Kincaid notes, this has been one of the principal weaknesses of functional explanations in the social sciences. Too frequently, social scientists have failed to specify the sort of environment in which the causal mechanism plays its functional role, with the result that such explanations seldom seem well confirmed. For example, consider a functional explanation for the Hindu practice of beef aversion. One might appeal to the often "ignored economic and ecological benefits." But, if such an explanation is to be compelling, one must explain what it is about the environments of those who eat beef that leads them to forego the benefits that Hindus enjoy (Kincaid 1996, 124–126). When the environmental conditions are adequately specified, many alleged anomalies can be set aside as irrelevant and beyond the scope of the explanation.

4. Collaboration Explained. With these preliminaries aside, I am now in a position to present my explanation for the prevalence of collaborative research in science.

1. Collaborative research plays a significant causal role in ensuring that scientific communities are able to realize their epistemic goals.
2. Collaborative research persists because it is so effective at enabling scientific communities to realize their epistemic goals.
3. Collaborative research is initially causally prior to the resulting success that it characteristically gives rise to.

The context in which the causal mechanism plays its functional role is as follows: (i) research in the relevant fields generally requires substantial resources for which there is fierce competition; and, (ii) the community of researchers are epistemic equals in a specific sense to be described below.

Only in such contexts can we expect that collaborative research will play the functional role I attribute to it.[4]

Now I want to turn to the task of defending this explanation. Again, I will appeal to Kincaid's analysis of functional explanations in an effort to identify what sort of evidence is needed to support such explanations. Kincaid (1996, 115) claims that, ideally, we should try to show that the three conditions obtain, one by one. This is how I will proceed.

In an effort to support the first claim in my functional explanation, I want to show how collaborative research enables scientists to realize their epistemic goals in five ways. First, collaboration seems to increase the quality of research. This is evident from the fact that "collaborative papers are more likely to be cited" (Frank Fox 1991, 198). Thus, the results of collaborative research generally play a more significant role in the subsequent development of scientific knowledge. This is further supported by the following data. Duncan Lindsey (1978, 82–83) found that in all six of the fields he examined in his recent study of the publication system in the social sciences—biochemistry, economics, psychiatry, psychology, social work and sociology—generally multi-authored articles are more frequently cited than single-author articles. And, of the articles published in *Systematic Zoology* in 1953, 1963, and 1973, less than 21% were coauthored, whereas of the articles published in this same journal that were cited in other journals at least 50 times between 1961 and 1983, 39% were coauthored (see Hull 1988, 525).

Second, collaborative research has made possible types of inquiries that would not otherwise be feasible. For example, Thagard (1997, 255) argues that "collaboration was essential for the development of the bacterial theory of ulcers because of the involvement of several different medical specialities." No single scientist was apt to have the required background knowledge. John Hardwig (1985) cites another such example. In an effort to measure the life-span of charmed particles, legions of scientists and technicians had to work together. The research culminated in an article coauthored by 99 scientists representing a variety of specializations in particle physics. In such large scale research projects scientists are deeply dependent on each other and on others. As Hardwig explains, none of the physicists who authored the article were "competent to design, build, and maintain the equipment without which the experiment could not be run at all" (348).

In addition, Thagard believes that collaboration plays an integral role in aiding scientists in developing unifying theories. As Thagard (1997, 255) explains, collaboration "can increase explanatory coherence," producing

4. In offering this functional explanation, I only purport to be identifying one of the many factors that play a causal role in ensuring success in science.

"conceptual combinations that establish new theoretical frameworks," especially when it involves scientists trained in different disciplines. Thus, some types of knowledge can only be had if scientists are prepared to collaborate with each other. Just as the European voyages of discovery and exploitation substantially increased the range of knowable things, so too does collaborative research.

Third, collaborative research ensures that we are less apt to forget or lose findings already discovered. Robert Merton (1973a) cites a number of examples of nearly lost discoveries involving famous scientists. But, as Merton explains, "when research is organized in teams, it would be less likely . . . that earlier ideas and findings would be altogether forgotten. For if some members of the team forget them, others will not. Moreover, repeated interaction between collaborators will tend to fix these ideas and findings in memory" (408).

Fourth, the high degree of collaborative research that characterizes modern science is partially responsible for the rapid growth of scientific knowledge. This is supported by a variety of studies. Diana Crane (1972), for example, found a correlation between collaboration and productivity. In her detailed study of two particular research areas, she found that each research area was composed of two types of subgroups: (I) collaborators, scientists who coauthor research papers together (34); and, (II) invisible colleges, or communication networks, which link the various groups of collaborators (35). Invisible colleges are held together by a few leaders, highly productive researchers actively involved in collaborative projects with many others, "who communicate with each other and transmit information informally across the whole field" (35). Crane's data suggest that "the most productive members of both areas had more relationships with other members of their area . . . than did the less productive members of these areas" (50). Moreover, the most productive members in the areas tended to belong to *larger* groups of collaborators (52). In addition, Crane found that research areas that exhibit the type of social organization described above have a period of exponential growth. In contrast, she found that "in two [other] fields in which the level of interpersonal communication and influence was low, the cumulative growth of publication was approximately linear" (25).[5]

Moreover, Ben-David and Aran (1991, 85), in their study of medical researchers in Israel who did postdoctoral training abroad, found that

5. Crane's study focuses on the research areas of mathematics of finite groups, and rural sociology. The research areas that exhibited only linear growth are invariant theory and reading research. Crane notes that her results are supported by data in Price and Beaver (1966), McGrath and Altman (1966), and a variety of unpublished dissertations and papers.

those who communicated more frequently after their return to Israel with the scientists they met abroad were more productive. And, Beaver and Rosen (1979a, 144), in their study of collaboration amongst the French scientific elite between 1799 and 1830, found that scientists who collaborated were generally more productive. Their data also suggest that "a scientist whose first paper was jointly authored stood a higher probability of producing another paper than did one who wrote his first paper alone."

Thagard (1997) offers additional evidence supporting the claim that collaboration is partially responsible for the rapid growth of scientific knowledge. According to Thagard, "computer simulations have shown that . . . for groups of complex agents working on tasks that require some degree of intelligence," we can expect a superlinear, as opposed to a sublinear, rate of improvement (251). As Thagard explains, "the cause of superlinear improvement seems to be that hints effectively reduce the size of search space: having agents start off at different locations increases the likelihood that some will find hints worth communicating to other agents to reduce their subsequent search" (251). This is precisely the type of advantage that connection with an invisible college would afford.[6]

Fifth, collaboration plays an important role in training young scientists (Thagard 1997). In fact, in a study of biochemistry graduates between 1956 and 1963, J. Scott Long (1992, 167) found that 55% collaborated with a mentor. Master-apprentice collaborations assist apprentices in gaining valuable experience and internalizing the values of science, which subsequently make them better scientists. In light of the considerations outlined above, I will take the first claim constitutive of my functional explanation to be adequately confirmed.[7]

Consider the second claim, that collaborative research persists because it enables scientific communities to realize their epistemic goals. As Kincaid explains, in order to support such a claim, we need to "establish that when *A* causes *B* then *A* continues to persist—and that it does so because it causes *B*" (115). Kincaid recommends that "we look for cases where *A* brings about *B*. We then ask if *A* tends to persist thereafter and try to ascertain why it does so" (115).

Ideally, it would be useful to have information on specific research groups, showing increased productivity after collaboration, followed by greater funding, which in turn would be followed by continued collaboration.[8] Unfortunately, at present, such data are not available. Nonethe-

6. Thagard (1997, 248–249) also argues that collaboration can result in gains in power, speed, and efficiency in generating results.

7. Indeed, there are also other, nonepistemic benefits that result from collaborative research. For example, some scientists enjoy collaborative research.

8. I thank Harold Kincaid for drawing this point to my attention.

less, there is some evidence that suggests that collaboration persists because of the function it fulfils. Specifically, collaborative research not only persists, it is becoming more and more popular in both the natural and social sciences. As Zuckerman and Merton (1973b, 547) note, whereas only 25% of the papers published in the natural sciences between 1900 and 1909 were multiauthored, the figure rises to 31% in the next decade, 49% in the next, then 56%, and then 66% in the 1940s. By the 1950s, 83% of the papers published in the natural sciences were multiauthored. There is a similar trend in the social sciences, though collaborative research is still less common there than in the natural sciences. Whereas only 6% of the papers published in the social sciences in the 1920s were multiauthored, the figure rises to 11% in the 1930s, then to 16% in the 1940s, finally reaching 32% in the 1950s (547). Further, Merton (1973a, 408–409) notes that in the *Journal of Abnormal and Social Psychology,* "taken by consecutive five-year periods, single-author papers decline from 80 percent of all in 1936 to 75 percent to 69 percent to 54 percent finally to 49 percent," and in the *American Sociological Review* single-author papers declined "from 92 percent to 87 percent to 76 percent . . . to 65 percent." Further, collaboration persists even though the groups themselves split, merge, and disappear. Indeed, there is evidence suggesting that collaborative groups are often quite short-lived. Crane (1972, 58), for example, found that "sixty-one percent of the groups of collaborators in [one area she studied] had lasted less than five years."[9]

Also noteworthy is the fact that collaboration is only popular in those research environments characterized by the conditions outlined above, environments in which (i) substantial resources are required for which there is competition, and (ii) the community of researchers are epistemic equals. The following three considerations illustrate this. First, we can note the lack of collaborative research in the humanities, research fields where extensive resources are not required in order to research effectively. As Zuckerman and Merton (1973b, 547) note, while by the 1950s 83% of the papers published in selected journals in the natural sciences were collaborative efforts, in the humanities the figure was between one and two percent. Research in the social sciences frequently requires more extensive resources than research in the humanities, but less than research in the natural sciences. And, as we would expect, collaborative research is more popular in the social sciences than in the humanities, but less popular in the social sciences than in the natural sciences.

Second, when those researching together are not epistemic equals we see far less collaboration. Research in the physical sciences in early modern Europe was often collective, enlisting the efforts of numerous people, but

9. I thank David Hull for drawing this point to my attention.

it was seldom collaborative. The people working in early modern scientific laboratories typically were not epistemic equals. This point requires some clarification to avoid misunderstandings. Shapin (1994) argues that the early modern English scientific community adopted their norms of decorum from the upper classes. In particular, it was widely recognized that the growth of science depended upon trust, and that only noblemen, people who were both socially and economically independent, could be trusted to speak frankly. Women and servants might lie for gain as a consequence of their constrained social conditions. In such a social environment it is no wonder that Boyle had to authorize the findings he reported occurred in his laboratory, even if he was not present when they occurred.

In contemporary science, on the other hand, some degree of epistemic authority is attributed to any scientist who has earned a Ph.D.[10] Indeed, not all scientists' opinions count equally. There are differences of authority that arise as a result of scientists' previous performance as researchers. Nonetheless, even though a particular scientist may have a greater reputation than her fellow collaborators, she must still acknowledge the contributions of others. As Zuckerman and Merton (1973b, 552) explain, "in the matter of deciding on authorship and name-order . . . it is generally the senior investigator who has the authority. [But] the exercise of that authority is hedged by norms and by constraints of maintaining a degree of cooperation in the research group." In light of the epistemic leveling that has occurred since Boyle's time, scientists with Ph.D.s who are working together have *some* basis for a claim to coauthorship. And, their careers as research scientists depend upon their getting recognition for their contributions.

Third, it seems that many scientists are inclined not to collaborate when given an alternative. In fact, Zuckerman and Merton (1973b, 549) found that, even in fields where collaborative research is the norm, there is a drop off in scientists' involvement in such research later in their careers due, in part, to the fact that it is then that "scientists often turn to broader 'philosophical' or 'sociological' subjects of the kind that have little place for collaboration." In some fields it would be very difficult, if not impossible, to build a career on such work. Only after a scientist is well established as a researcher can she indulge in such investigations. And, significantly, such research can be done without extensive funding.

In light of the above-mentioned considerations, I believe the second

10. As Kitcher (1993, 316) notes, "the authority structure of a [scientific] community is that of an inverted pyramid: almost all those who have survived their novitiate, have fairly high authority (with respect to the topics about which they make pronouncements). Nevertheless, the same communities can be sharply pyramidal in terms of credit, with a tiny fraction of the members aspiring to the highest levels of reputation (and concomitant resources)."

claim constitutive of my proposed functional explanation is adequately supported.

Let us now consider the third claim, that collaborative research is causally prior to the resulting success that it facilitates. Here, I want to emphasize, my interest is in determining the initial cause, for, given the structure of the causal chains that functional explanations explain, once the initial cause has been brought about there will be feedback between the cause and the effect, and the effect will ensure that the cause persists.

As Kincaid (1996, 115) notes, in an adequate functional explanation, "generally, background information will make [the third condition] fairly obvious," for it is often implausible to construe the alleged effect as the *initial* cause. Consider the following example. An anthropologist might argue that the function of a rain dance is to reduce the escalating social tensions caused by the hardships experienced during a drought, and that the practice of engaging in rain dances persists because it reduces social tensions during droughts. Here, the cause is the rain dance and the effect is the reduced social tensions. Clearly, it would be implausible to argue that reduced social tensions during a drought *initially* caused the groups' rain dancing, even though the rain dancing may persist because it reduces social tension.

The functional explanation developed here is similar. I have claimed that collaborative research causes certain types of scientific communities to produce research effectively. It seems implausible to construe scientists' effectiveness at research as the *initial* cause of their collaborating. After all, scientists have been productive researchers without collaborating. And, indeed, in some research areas scientists continue to effectively realize their epistemic goals and yet are still not led to collaborate.

In addition, evidence suggests that collaborative research only became the norm when the requisite environmental conditions obtain. As Joseph Ben-David (1991, 327) notes, "originally [the scientific academies] were designed as institutions for cooperation in research, and in the case of the Paris Academy of Sciences, even for cooperative research." But, as Ben-David notes, this initiative on the part of the early academies was insufficient to cause scientists to collaborate, for much early modern scientific research could be effectively executed without collaborating. As Ben-David explains, "the state of the art, in which experiments could be performed in a workshop far simpler than a modern kitchen, called for little cooperative research" (327). Collaboration was not to be forced upon scientists prematurely. Not until the environment was such that collaborative research was indispensable was it going to become the norm. Now, in those fields of science where the environmental conditions are satisfied, collaborators are out-surviving non-collaborators, thus increasing their proportion in each succeeding generation.

Once scientists begin to collaborate and experience the epistemic benefits, this will induce some to collaborate subsequently. In fact, their awareness of the epistemic benefits is apt to play *some* role in creating a research environment in which collaboration is the norm. But, as I will explain in the next section, there are reasons to believe that a direct explanation strictly in terms of scientists' motives cannot provide an adequate account of the various trends that need to be explained.

5. Alternative Explanations Considered. In this section, I want to briefly consider some alternative explanations for the increase in collaborative research in science. Two of these alternative explanations are strictly sociological, and regard the increase in collaborative research as having no epistemic significance. An examination of these explanations will provide an opportunity for me to highlight some of the advantages of my proposed explanation, and thus provide additional support for it. The most comprehensive study of collaboration in science culminated in the publication of three articles by Beaver and Rosen (1978; 1979a; 1979b). In these articles, Beaver and Rosen criticize what they take to be the most popular explanation for the increase in collaborative research in science, and they then present and defend their own explanation.

According to Beaver and Rosen (1978, 69), "the most frequently advanced explanation for teamwork attributes it to the specialization of science." As "scientific vision becomes ever more specialized and narrow, . . . collaboration . . . becomes necessary when scientists deal with problems which cross disciplinary bounds" (69). Beaver and Rosen argue that this explanation is implausible. As they explain, such an explanation "leaves unsolved . . . the large variation in the incidence of collaboration by field" (70). For example, they claim that "using specialization to explain collaboration . . . requires a belief that, compared with chemistry, mathematics is relatively unspecialized," which is not the case (70).

Beaver and Rosen (1978, 65) believe that "scientific collaboration represents a response to the professionalization of science." As they explain, " 'professionalization' refers to a dynamic organizational process which led to a revolutionary restructuring of what had been a loose group of amateurs and full-time scientists into a scientific community" (66). They argue that " 'professionalization' redefined how science was done, who did it, where it was done, what paid for it, what its practitioners wanted, and how they became scientists" (66). Part of this new way of doing science involved working collaboratively.

Their evidence for this includes the following. First, they note that in the seventeenth and eighteenth centuries when scientists began to engage in collaborative research, much of it was in astronomy, the field that was "closest to being a professionalized science in the 18th century" (74). Second, they note that "collaboration during the first decades of the nineteenth century is mainly limited to the French scientific community," which was the first national scientific community to be professionalized (76).

I believe that the principal shortcoming of Beaver's and Rosen's explanation is that, like the explanation it sought to replace, it fails to explain the variation in fields. For example, both the social sciences and the humanities are professionalized in the relevant sense, yet there is substantially less collaborative research done in either area. And, professionalization cannot explain the variation in the various sub-fields of the natural sciences. For example, as Beaver and Rosen note, "experimentalists tend to collaborate more than theoreticians" (70), a disparity that extends back to the seventeenth and eighteenth centuries, when the first collaborative research was published (73). Given their explanation, they seem committed to claiming that experimentalists are more professionalized than theoreticians. This, though, just doesn't seem to be the case. Consequently, their appeal to professionalization does not resolve the problem it sets out to resolve.

My functional explanation, which appeals to the competition for extensive funding in some fields, provides a means to explain the variation in collaborative research in different fields. The variation in available funding leads to variation in the proportion of research that is done collaboratively. In fields where extensive funding is required in order to research effectively, those who collaborate are more productive epistemically, which enhances their chances of getting funding in the future.

Further, there are two other things that my explanation can account for that Beaver's and Rosen's cannot. First, there is the increasing trend toward papers authored by more than two scientists (Beaver and Rosen 1979b). Professionalization offers little by way of explanation here. On my account, such a trend is to be expected, given that projects requiring larger amounts of funding will also generally require greater numbers of scientists.

Second, given my account we can provide an explanation for fluctuations in the frequency of collaborative research. As Beaver and Rosen (1979b, 241) note, "during the Depression, the frequency of collaboration in relation to individual work decreased." Science, though, did not undergo a process of deprofessionalization at this time. Rather, as they note, "financial support for basic research decreased" (241). Hence, the fate of collaborative research in science is intimately tied to the financial resources available. Extensive resources make possible elaborate projects which require collaboration. And, when funding decreases substantially, scientists no longer have a strong incentive to collaborate.

This covariance between funding and collaboration is the most telling evidence against what some would regard as the most obvious explanation for collaboration in science. Some might think that the reason why collaboration is so prevalent in science is because it is in scientists' interest to collaborate. That is, scientists are rational, and either (a) they know that collaborative research yields epistemic benefits, or (b) they know it yields career benefits, given that the epistemic benefits it yields translate into career benefits. This direct explanation, however, is at odds with the changes that occurred during the Depression. Despite the fact that funding opportunities changed, the epistemic advantages of collaboration persisted. But these epistemic advantages were not sufficient to induce scientists to collaborate to the same extent as they did before the Depression. This suggests that many scientists, even many who collaborate frequently, may be unaware of the epistemic advantages of collaborative research.

There is one question that still cries out for an answer: why did collaborative research acquire the functional role it did at the particular time in history that it did? I think that Beaver and Rosen offer us insight into answering this question. The period in history when science professionalized marks the beginning of the process by which those working in science formed a community of epistemic equals in the sense discussed above. Prior to the professionalization of science, very competent and able men of science often had to work for others, not as equals, but as servants, as Denis Pepin did in Boyle's laboratory.[11] But, once a person like Pepin could rely on a salary working as a scientist, he was in a better position to ensure that his contributions to other's research projects translated into scientific credit, expressed in the form of coauthorship. Thus, the professionalization of science played an important role in creating the sort of research environment where collaborative research could play the functional role outlined above.

6. The Epistemic Costs of Collaboration. In this section, I want to consider the risks of negative epistemic effects resulting from collaboration in science. There are four such risks. First, there is the risk that in collaborative research there will be a diffusion of epistemic responsibility. As Zuckerman and Merton (1973b, 552–553) explain, with "the growth of multiauthorship . . . it becomes increasingly difficult and sometimes impossible to gauge the contributions of individual scientists to the collective product of ever larger group projects." When an article authored by one scientist is subsequently found to rely on questionable data, it is clear whose credibility is affected. But when a coauthored article is found to have a similar problem, it is far less clear whose credibility should be affected. And, the blame is more easily diffused. This diffusion of responsibility and credit

11. For a discussion of the range of people at work in early modern laboratories, see Shapin 1994, Ch. 8.

could have a negative impact on science. As David Hull (1988) notes, the success of science depends, to some extent, upon scientists getting credit where credit is deserved, and losing credibility when blame is deserved.[12] And, there is evidence that scientists themselves are increasingly less sure of their contributions and entitlements in collaborative projects. For example, Linda Wilcox (1998, 216) found that at Harvard Medical School, Dental School, School of Public Health, and affiliated hospitals, "from the 1991–1992 through the 1996–1997 academic years, queries related to authorship have grown as a percentage of the total complaints to the Ombuds Office from 8 (2.3%) to 59 (10.7%)."

Second, the increase in collaborative research might erode the motivation of scientists, which is apt, in turn, to have an impact on their productivity. As Zuckerman and Merton (1973b, 548) explain, in collaborative research "the distinctive contributions of the individual get lost in the crowd of scientists putting their names to the paper . . . [which may be] especially damaging for young scientists who have not published independent work that testifies to their abilities."

Third, given the potential benefits that collaborative research affords, we may find that certain types of researchers, merely because the relevant research communities are not inclined to collaborate, are less able to get the resources required. If funding agencies develop a tendency to fund collaborative research, certain types of inquiry may suffer, thus diminishing the growth rate of scientific knowledge in some areas. There is some evidence to suggest that some funding agencies do prefer larger, collaborative projects. In their study of the peer review system in the National Science Foundation, Cole, Rubin and Cole (1978, 139) found that "in algebra, meteorology, and solid-state physics . . . larger projects are more apt than smaller ones to be given awards." On the assumption that projects that require greater amounts of funding are more apt to result in collaborative research, there does seem to be a preference to fund collaborative work.

Kincaid (1996) attributes the poor track record of research in some social sciences to the lack of cooperation among researchers. The lack of cooperation, he claims, "makes rigorous testing of hypotheses about complex phenomena harder" (264). And this lack of rigorous testing, which might be curbed if more collaborative research projects were funded, probably perpetuates the enormous discrepancy between funding for research in the social and natural sciences. As Kincaid notes, "in 1991 . . . the total U.S. government spending for the social sciences was $189 million. Spend-

12. Indeed, it was Merton (1973b) who first noted the significance of institutional recognition for originality in science.

ing for the natural sciences was approximately $12 billion or sixty-three times what was spent on the social sciences" (264).

Fourth, once collaborative networks are established, these groups may become powerful lobbying groups. Derek de Solla Price (1963, 108) argues that groups of collaborators and invisible colleges "seem to exercise pressure to keep scientific advance directed toward those ends for which the group or project has been created." As he explains, "such a group develops into an integrated body, increasing its efficiency and ability to coordinate the activities of a large number of men and their projects, so the power of the group seems to increase even more rapidly than its size" (105). Further, as Steve Fuller (2000, 79) explains, "once enough material and human capital have been invested in a line of inquiry, it becomes difficult to justify its discontinuation, especially if it reaps reasonable benefits for those pursuing it." And, Jane Maienschein (1993, 167) suggests that, given that collaborative research groups "may be eligible for resources that individual researchers could not obtain," such groups may often be created to serve political, rather than epistemic, ends. These factors further influence the direction of funding and research at the expense of other areas of research. The impact of the social organization of research groups in affecting change in science should not be underestimated. As Hull (1988) notes, a key difference between early Darwinians and their adversaries is that the former, but not the latter, "formed a social network." As a result, the early critics of Darwinism "attacked . . . in isolation [but were] met with an organized response" (114).

7. Concluding Remarks. In summary, I have developed and defended a functional explanation for the persistence of collaborative research in science. In certain fields, those in which scientists must compete for access to resources in order to engage in research effectively, collaborative research has become the norm, playing an important causal role in enabling scientific communities to realize their epistemic goals. In such a research environment, those scientists who are unwilling or not inclined to work collaboratively are apt to find it difficult to access the resources necessary to be effective researchers. As a result, they are apt to be less productive than their peers who choose to collaborate.

One might wonder about the trend toward greater and greater collaboration, and ask: will it ultimately completely replace single-author publication in some disciplines? Indeed, in 1979 Beaver and Rosen (1979b, 237) forecasted "the virtual demise of the lone researcher." I suspect that they are mistaken. Just as collaborative research has an important functional role to play, so, too, do single-author papers. In particular, single-author papers provide young scientists with opportunities to prove themselves, both to themselves and other scientists. Unless they are given the opportunities to prove themselves, young scientists are unlikely to sustain the interest and motivation required to be productive research scientists. Consequently, each research area is apt to reach a saturation level at which point a greater proportion of collaborative research would no longer fulfill its function, ensuring that the scientific research community works effectively.

Even in *Philosophy of Science* collaborative research is on the rise. Of the 143 articles published in the first five years of the journal's publication (1934–1938), only one was coauthored. Between 1950 and 1954, 3% (5 out of 154) of the articles were coauthored. Between 1970 and 1974, 5% (11 out of 213) of the articles were coauthored. And between 1990 and 1994, 11% (22 out of 194) of the articles were coauthored. Further, whereas in the samples from the 1950s and 1970s all 16 coauthored papers had only two authors, in the sample from the 1990s two papers had three authors and one had 11. This change, anomalous for the humanities in general, may reflect the fact that research in philosophy of science is becoming more responsive to empirical research about science. As those working in the field have become less satisfied with rational reconstruction they find themselves needing the assistance of researchers in other disciplines. This paper exemplifies the trend toward a more empirically minded approach to philosophy of science, but, despite the conclusions drawn here about the benefits of collaborative research, this paper has only one author.

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