Why Collaborate?

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The recent escalation of concern about scientific integrity has provoked a larger discussion of many questions about why we do science the way we do, as well as about how we should do it. One of these questions concerns collaboration: who should count as a collaborator? This, in turn, raises the question why collaborators collaborate, and whether and when they should. Here, history offers insights that can illuminate the current debate.

Collaborations typically occur for one or more of three overlapping reasons. Sometimes individuals need help, and the division of labor will increase efficiency. This may simply be a matter of needing more hands doing the same kind of work, or it may involve bringing together specialists who provide different types of expertise. This is the more-heads-(and hands)-are-better-than-one factor.

Second, there is the credibility factor. Collaborations among different individuals may produce greater credibility because each brings to the project his or her own credentials and acceptability in a different research community. This motivation often lies behind the impulse to interdisciplinary work. Individuals from different disciplines, or from different authenticating groups, come together for a project which then is regarded by a larger group as both important and legitimate. Students are sometimes also brought into research in this way.

The third factor is more explicitly political. Collaborators may work together (or want to appear to be working together) to create a community (or the appearance of a community). Such a community may be eligible for resources that individual researchers could not obtain, or they may be better able to succeed in their chosen task than other groups or individuals without collaborators.

Of course, some collaborations have more than one of these factors in operation. Furthermore, the categories are not neatly or perfectly separable. Student-faculty relationships may or may not

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involve collaborating, for instance. Examples of each motivation for collaboration should help to reveal some of the problems, uncertainties, and advantages of each. This discussion should also show that the history and philosophy of biology community has begun to examine questions about collaboration in ways that can help to illuminate the current debates within the sciences.

WHAT IS COLLABORATION?

Since different people have meant such different things by collaboration, the category needs further analysis. Toward this clarification, James Griesemer and Elihu Gerson have suggested that collaboration should literally find its root in co-laboring: collaboration should minimally involve working together toward a common product.[1] The individuals should have come together in pursuit of a shared goal. This may be true even if they do not articulate their goal in precisely the same way, or indeed even if they do not articulate it at all. According to this definition, a leader might recruit specialists or laborers to work toward a common purpose, namely the leader's project, and the workers might not participate in defining or refining that purpose. Students early in their careers may fall into this group.

Museum collection provides a prime example here. At the most basic level, collectors can be co-laborers. Thus, German collector Amalie Dietrich could be said, in Griesemer's and Gerson's terms, to have co-labored with the various Kabinett collectors to whom she sent the botanical specimens she collected.[2] Yet, though she suffered tremendous hardships to make her collections, and though she was an exceptional collector, she did not participate in defining the projects for which she collected. She collected whatever she was told would sell – although when economic conditions warranted and she had a choice, she would choose to sell to the best and most respected collectors. She thrilled at the suggestion that her specimens made an important contribution to science and helped in the advancement of knowledge. Only later, in the 1860s and 1870s, did she begin to define the collecting herself, when she was hired to visit Australia and Polynesia to collect a variety of

1. James Griesemer and Elihu Gerson, various discussions relating to their respective studies of the development of the Museum of Vertebrate Zoology at Berkeley. See below for further consideration of this work. I am using the term "collaboration," even when the scientists in question did not, because the concept is the same and the action falls into the same category of behavior.

2. Charitas Bischoff, *The Hard Road: The Life Story of Amalie Dietrich, Naturalist, 1821–1891* (London: Martin Hopkinson, 1931).

interesting things; then she was the one who selected what was interesting and decided how to preserve and present it. Yet even then she gave up control of her materials as soon as she sent them back to Germany, and the Kabinett collector used them for his own purposes.

Similarly, and at about the same time, Charles Sternberg described the hardships of collecting large fossil specimens in the dustbowls and deserts of the American mid- and southwest. He persisted because at some level he enjoyed the work outdoors, and because he felt that he was contributing to science when he sent his specimens off to well-known scientists such as Edward Drinker Cope. Clearly, Sternberg saw himself as part of the scientific process, which he regarded as a process of unlocking his Creator's secrets. As he put it,

> Surely, "my cup runneth over; I have a goodly heritage." Greater than their obligations to me, are mine to the men of science who have described, published, but, above all, have prepared and exhibited the noble monuments of creative genius which I have been so fortunate as to discover and make known to the civilized world. My own body will crumble in the dust, my soul return to God who gave it, but the works of His hands, those animals of other days, will give joy and pleasures "to generations yet unborn."[3]

Certainly, Sternberg was co-laboring toward the larger project of discovering the secrets of creation. Yet he did not define what Cope and the other paleontologists for whom he collected did with the specimens he collected,

Thus, in the simplest sense of co-laboring, Dietrich and Sternberg were certainly collaborators. Yet they did not participate in defining their task. Nor did either of these rugged individuals carry the credentials or move in the social or professional circles necessary to validate their work as "scientific" in its own right. Without them – or someone like them – to carry out the dirty work, the work would not have been done. They did nothing theoretical in the usual sense, nor did they realize that they were making great discoveries. Instead, they found things, recognized them as valuable, and shipped them off to the certified scientists.

They were contributors and unquestionably deserve credit for their work. Indeed, they deserve far more than they have received.

3. Charles H. Sternberg, *The Life of a Fossil Hunter* (Bloomington: Indiana University Press, 1990), p. 281.

Yet they do not clearly deserve the status of coauthors on the publications that resulted from their collecting, and it is these which are usually counted as the legitimate scientific contributions. Thus, co-laboring toward a common goal is, in itself, not sufficient for all to be equal partners or full collaborators. Participation in articulating the goal – whether in the very first place as the project begins or later, during the development or presentation of results – will help to insure the contributor's role as a "true" or "full" collaborator. It is useful to distinguish along these lines between primary and secondary collaborators, where it is the former who participate fully in defining the project and its goals.

Some would say that only primary collaborators deserve to be called that.[4] The other co-laborers are contributors or participants in the project certainly, but they do not share the responsibility for the product in the same way. They are responsible for carrying out their part of the project according to the highest possible standards, but they do not have accountability for the entire project – thus they deserve credit for their part, but not the whole. This account would place technicians, collectors, "mere" data collectors, and other "lesser" co-laborers along the scale of contributors but off the subscale of collaborators. This schema recognizes a hierarchy of workers.

While it does seem correct that some contributors have more responsibility than others, and while it also seems correct that they deserve more credit than others, it is not obvious that the line between those who accept responsibility and those who do not is the same line as that between collaborators and noncollaborators. Instead, there is a substantial gray area. Contributors may feel that they deserve credit, ultimately even as coauthors, and accept responsibility even though they do not help in defining the whole project. Others may regard these coauthors as responsible for the entire project, but these contributors may not agree. Some faculty members might not feel fully responsible for a postdoctoral fellow's work, for example, even when it leads to a coauthored paper. Alternatively, a participant may feel that he or she is fully responsible and deserves an equal voice, even when others do not regard that person as such. A student may feel responsible for telling the laboratory director about a perceived error

4. For example, my colleague James Collins makes this point, and suggests that what I am considering as "collaboration" is much broader than he would accept. Yet he concedes the point that it is necessary to expand our recognition of those who contribute to the process of doing scientific work. As he sees it, collaboration is a "process-laden" term and must be unpacked as such.

and may act as a collaborator in calling the work into question, and yet the director may dismiss the charges and reject the claims of the co-laborer.

Therefore, some workers seem more clearly to qualify as collaborators than others. Levels of accepted responsibility and of credit play a central role in determining this. These levels are not decisive, however, and we are left with a range of contributors who co-labor on a project. I propose to call "primary collaborators" those who are clearly responsible because they have participated in defining the project and because they accept the project as valuable and as at least partially their responsibility. Those who contribute to the project but do not also fully participate in the project definition I call "secondary collaborators." These latter hold less responsibility for the project and also deserve fewer rewards. While not all scientists would call them "collaborators" at all, the term is useful in order to acknowledge the contributions of a complex variety of contributors.

The case of Joseph Grinnell, Annie Alexander, and the Museum of Vertebrate Zoology at Berkeley is instructive here. In this case, as Griesemer and Gerson explain, the wealthy patron Alexander hired the organizer Grinnell to develop a museum. Each had the goal of producing the museum, and they shared a conception of just what that museum should do: it should not just store specimens but should also capture the environments in which the specimens live, and should present them in ways that made them maximally useful for biological research.[5] Thus, Alexander and Grinnell shared their objectives, and together they articulated the project's purposes and procedures. They were full collaborators, and they both accepted responsibility for the project. In the final analysis, they settled on an effective division of labor to carry out the project: Alexander loved collecting and was good at managing the financial end, while Grinnell organized the collection and all aspects of the collecting process. In consultation with Alexander, Grinnell provided the conceptual framework with which to turn the collection into potential research material for a variety of present and future projects. As Griesemer and Gerson point out, Alexander, Grinnell, other collectors, the students, and all participants from the janitors to the display case builders to researchers from "outside" who used the collection were contributors and were responsible at some level. It is useful to include them as secondary collaborators, then, and to open up the recognition of what – and whom

5. James Griesemer and Elihu Gerson, "Collaboration in the Museum of Vertebrate Zoology," *J. Hist. Biol.*, this issue.

– it takes to do science. Yet, as Griesemer and Gerson also explain, science is not an "undifferentiated network of social relations" or in any way a seamless web; some researchers are more indispensable than others in carrying out the scientific project. Thus, Alexander and Grinnell were the primary collaborators, while the others – though no less essential – remained secondary.

The American Museum of Natural History provides a useful contrast. Here, as Ronald Rainger has demonstrated, many of the scientists were at least partially unwilling co-laborers, in effect serving as involuntary laborers under Henry Fairfield Osborn's dictatorship.[6] Osborn largely defined the projects, and the workers followed them. Sometimes they rebelled, and sometimes they quit, but working with Osborn generally meant working for Osborn.

At first, Louis Agassiz's students played a similar (though even more confining) role at Harvard's Museum of Comparative Zoology, as Mary P. Winsor explains. Some students began to use the collections – collections gathered under Agassiz's direction and generally with funds that he had acquired – for their own research purposes. Agassiz saw this as rebellion and instituted a set of strict rules governing museum property and ideas. The boundaries, however, were not always clear. As one student put it, "the rules were not 'in accordance with the broad and liberal spirit that we had been taught to consider as the one which would govern the Museum.'" Another student complained that "the Museum is guarded against any infringements of propriety on the part of the students, [but] the students are not guarded against any infringement of propriety on the part of their superiors . . . neither do I think the laws of intellectual property are either very carefully or justly laid down."[7] Though Agassiz acknowledged that authorship belonged to the writer of a research article, he felt that he had control over the specimens that allowed the research and he had rights and responsibilities as a result. In accepting those responsibilities and demanding those rights, he denied his students the status of primary collaborators. To a large extent, his son Alexander followed the same policy when he took over control of the Museum after his father's death.

In all these cases, then, primary collaboration should involve co-laboring on a project and toward a product that the collaborator has had a share in articulating. Minimally, the primary

6. Ronald Rainger, *An Agenda for Antiquity: Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935* (Tuscaloosa: University of Alabama Press, 1991).

7. Mary P. Winsor, *Reading the Shape of Nature* (Chicago: University of Chicago Press, 1991), p. 60.

collaborator should also accept some responsibility for the project and its results.

TYPES OF COLLABORATION

Helping Hands

Returning to the basic types of collaboration reminds us that not all cases fit precisely the same pattern. In cases of the first type, help of some sort is needed that the researcher cannot provide. This may be simply a straightforward need for more hands, as in many collecting trips. Such participants become primary collaborators only insofar as they share in articulating the goal as desirable and accepting the responsibility for the project.

In some cases, the researcher needs help of a particular, most often technical, sort. For example, cytologist Edmund Beecher Wilson enlisted the collaboration of Columbia University photographer Edward Leaming to help him produce his *Atlas of Fertilization* in 1895. Wilson recognized the importance of capturing the early stages of cell division exactly, which he felt could only be done with photographs, rather than with the usual camera lucida or freehand drawings or diagrams. As he said, "no drawing, however excellent, can convey an accurate mental picture of the real object."[8] Yet he did not have the skill to produce the requisite detailed photographs himself. He could not have simply hired any random photographer, who would presumably have never worked with microscopic material before. Instead, he needed to enlist someone to share the project and to understand the objectives. Leaming did not help to articulate those objectives nor did he have responsibility for the project, which grew directly out of Wilson's own study of the cell. Thus, appropriately, Wilson remained the author of his work, but he recognized the efforts of the secondary collaborator by assigning him secondary authorship. The book is identified as by Wilson "With the Cooperation of" Leaming. In addition, in the preface Wilson acknowledged the help of the man who had performed the mechanical reproduction of the photographic plates, recognizing that his contribution was also an essential part of the project. The project remained Wilson's, with the secondary collaboration of the photographer and the cooperation of the plate developer.

8. Edmund B. Wilson (With the Cooperation of Edward Leaming), *An Atlas of the Fertilization and Karyokinesis of the Ovum* (New York: Macmillan, 1895), p. v.

Expanding Credibility

In an earlier case, Wilson had worked with his friend William Sedgwick as equal partners and primary collaborators. After graduating with their Ph.D.'s from the Johns Hopkins University, Segwick and Wilson decided to produce an American textbook of *General Biology*. As a physiologist and a morphologist, respectively, Sedgwick and Wilson could each contribute to the project in a way that would presumably produce a whole greater than the sum of its parts. The product would be a coordinated volume rather than two separate pieces stuck together. This collaboration involved a combining of perspectives to work toward the shared and mutually articulated goal, for which each author was responsible and for which each deserved credit. It also involved gaining greater credibility for the project by expanding the range of expertise and credentials of the contributors. Wilson and Sedgwick were both primary collaborators and coauthors. In the preface they acknowledged the contributions of the illustrators as well, but these contributors remained cooperators who worked under the direction of the authors and hence were only, at most, secondary collaborators.[9]

Students and Professors

Student-professor work introduces another kind of co-laboring, which involves both getting help and sharing expertise to increase credibility. Many students start out in the professor's laboratory, doing the project prescribed for them. They gradually develop into independent researchers, but they often remain in the original laboratory until they finish their first project at least. The Ph.D. dissertation typically appears in the student's name but with acknowledgment of the professor's help. Responsibility and credit for publications resulting from the work are not always clear, and a variety of patterns have appeared.

Charles Otis Whitman gives us one extreme. In 1879–1881 he taught biology at the Imperial University in Tokyo. At the end of the two years, he presented the work of his four advanced students for publication in one of the university's journals. The editor responded that the journal's purpose was to publish faculty research, and any student publication would have to appear under the professor's name. Whitman was outraged and sent the papers for publication elsewhere, so that three of them appeared in the much more prestigious *Quarterly Journal for Microscopical Science*. Fortunately for the later development of American biology, the disagreement ultimately led to other problems and caused Whitman to resign and to return to the United States.[10] He never published papers jointly with students, always insisting that their work remained their own.

Another model is that followed by Charles Manning Child and his student Libbie Henrietta Hyman. Hyman completed her graduate work under Child's direction at the University of Chicago in 1915. After that, she remained at Chicago as Child's "Assistant" until 1931. During that time, she worked on projects that grew out of Child's theoretical approach, and that were always closely related to Child's. Yet with the exception of two joint papers with Child, she published under her own name and thereby accepted the responsibility – and credit – for her own work. Indeed, during her years at Chicago she published more than forty articles and two very successful laboratory manuals of her own. Was she merely an "assistant" to Child, or a secondary co-laborer on his larger projects, or a primary collaborator, or an independent researcher?

Hyman considered herself as not a particularly original researcher, and to some extent this is accurate. At first, her work did follow Child's fairly closely. She asked many of the same questions and used many of the same organisms – or they asked the same questions, but each used a different organism or slightly different experimental design to produce valuable comparative cases. She was an extremely careful worker, and she was an excellent observer. Thus, it is not surprising that one of her colleagues recalled, in discussing Child's work, that "in much of his work he had the able assistance of Dr. Libbie Hyman, generally regarded as the ablest American woman zoologist now living (some say the ablest of either sex)."[11]

On many parts of their research, Hyman and Child were primary collaborators even though they published independently, and even though Child remained generally more theoretically oriented and Hyman more oriented toward careful description and systematic identification. They worked together to articulate the goals and the details of how to carry out the project. It is to Child's credit that he insisted on Hyman's receiving her own recognition and supported her work for many years. It is to her credit that she

9. William T. Sedgwick and Edmund B. Wilson, *General Biology* (New York: Henry Holt, 1886).

10. Frank R. Lillie, "Charles Otis Whitman," *J. Morph.*, *22* (1911), xxiii.

11. H. H. Newman, "History of the Department of Zoology in the University of Chicago," *Bios*, *19* (1948), 232.

recognized the value of his theoretical insights and saw ways to explore them experimentally. Theirs was certainly an unusually productive working together. It ended only when Child reduced his research activities to travel internationally and then to serve as departmental chair, and when Hyman simultaneously resolved to move away from Chicago for personal reasons.[12]

Other student-professor relationships have proved less mutually beneficial or have raised questions later. Researchers have asked whether Nettie Maria Stevens deserved more credit than she received for her work on sex determination by accessory chromosomes, for example. The claim is that while a student at Bryn Mawr and afterwards, working with Thomas Hunt Morgan, she did not receive sufficient recognition for her work; instead, either Morgan or his friend and colleague at Columbia University, Wilson, has been accorded priority. The claim is not that Wilson stole Stevens's ideas – rather, that she was seen as a student or a less important researcher even when her work deserved greater credit for its originality.[13] She was seen as a follower rather than a leader, in other words, as if she were merely a secondary collaborator. Rosalind Franklin's role in identifying the double-helical structure of DNA provides a similar example.[14]

Critics suggest that this is the fate of many students: they remain in their major professor's shadow and never achieve the level of primary collaborator even when they deserve such a designation. This should serve as a reminder to faculty to define carefully what will count as independent research, as secondary but authorship-deserving collaboration, or as mere cooperation or laboring for the professor's purposes. Questions about collaboration clearly merge with questions about intellectual property rights as well. They become heated when the stakes are high, as they are when students or younger researchers seek to establish their own credentials.

12. On Hyman, see Rachel Fink's entry in the *Dictionary of Scientific Biography*, Supplement II, vol XVII, pp. 442–443. On Hyman at Chicago, see Jane Maienschein, "Hyman and Child at Chicago," paper presented at the American Society of Zoologists meeting, Atlanta, 1991, as part of a symposium on Libbie Hyman.

13. Stephen G. Brush, "Nettie M. Stevens and the Discovery of Sex Determination by Chromosomes," *Isis, 69* (1978), 163–172. Also see Marilyn Bailey Ogilvie and Clifford J. Choquette, "Nettie Maria Stevens (1861–1912), Her Life and Contributions of Cytogenetics," *Proc. Amer. Phil. Soc., 125* (1981), 292–311.

14. Compare, for example, the discussion of Franklin's work in James Watson, *The Double Helix* (New York: Signet Books, 1968), and Anne Sayer, *Rosalind Franklin and DNA* (New York: W. W. Norton, 1975).

Coerced Collaboration

In other cases one or more co-laborers may come to feel coerced and may seek to distance themselves from the project. This was the case with the work of the Atomic Bomb Casualty Commission (ABCC) following World War II, as John Beatty explains.[15] U.S. officials needed cooperation from the Japanese government, the medical community, and the population generally in order to study the genetic effects of the Hiroshima bombing. It was crucial to gather as much information as possible from women who had been pregnant at the time of the attack. Yet they could not force cooperation, or collaboration at any level. The United States had just attacked the Japanese, and word that American scientists now wanted to use the Japanese citizens as guinea pigs for research purposes did not fall on entirely receptive ears. The challenge was to enlist the Japanese as secondary, or ideally even as primary, collaborators in the project. At the very least it was essential to create the appearance that they were collaborators.

Cooperation at various levels resulted. The project's definition was guided by the American scientists, yet there was some effort to remain sensitive to Japanese needs. The assumption was that science is science, and that Japanese and American interests give way in the face of the objective internationalism of science. Nevertheless, the Americans still needed help and cooperation. Individual pregnant women had no reason to participate in any study. Especially if the pregnancy and offspring were not normal, the public exposure of that fact could prove embarrassing. Thus, the scientists recruited midwives to help get information for them. The midwives were paid and hence did have an interest in the project, as the individual women did not; they accepted responsibility for part of the project and became secondary collaborators.

The ABCC remained the primary researchers, and all these others really remained secondary. Yet, as Beatty explains, the Americans stressed the importance of this working together, and they endeavored to make it look to the Japanese as if the project were truly collaborative at all levels and in everyone's interest. Having a greater range of even secondary collaborators would bring increased credibility and a wider group of supporters for the project. The

15. John Beatty, "Genetics in the Atomic Age: The Atomic Bomb Casualty Commission, 1947–1956," in *The Expansion of American Biology*, ed. Keith Benson, Jane Maienschein, and Ronald Rainger, (New Brunswick, N.J.: Rutgers University Press, 1991), pp. 284–324; John Beatty, "Scientific Collaboration, Internationalism, and Diplomacy: The Case of the Atomic Bomb Casualty Commission," *J. Hist. Biol.*, this issue.

Japanese citizens would be more likely to participate if they felt this was at least partly a Japanese project. The Japanese government and scientists would be more supportive if they received some credit for the results. The American public would be more positive if they did not think that scientists were inflicting research on an already suffering population of victims. As the Japanese sought at various points to dissociate themselves from the project, the ABCC had to work to keep them involved in some ways. This represents, at least in part, a coerced collaboration. Coercion was necessary to achieve the goal of greater credibility.

Textbook Projects

Other efforts to gain greater credibility through collaboration have come in the form of textbook projects. One such effort began when a group of researchers in various aspects of cytology gathered every summer at the Marine Biological Laboratory (MBL) in Woods Hole. Each studied the cell in some way, using some approach, and asking some set of questions, but these individual researches remained quite diverse. Each contributed important understanding of some aspect of cell structure and function. Each went in his or her particular emphasis beyond the existing best textbook in cytology, Wilson's *Cell*.[16] No one of the individual researchers could produce a general textbook to cover it all; therefore, the group decided they needed to collaborate to produce a textbook. As they explained:

> We have briefly indicated how the earlier morphological cytology has broadened out into a many-sided *cellular biology* (to employ the phrase of [J. B.] Carnoy) in which observation and experiment, morphology and physiology, have entered into close affiliation with one another and with biophysics and biochemistry. The result has been to create a new cytology, a new cell physiology, a new cellular embryology, and a new genetics; and these various lines of inquiry have now become so closely interwoven that they can hardly be disentangled. This much-to-be-desired result has been made possible by an always growing co-operation between lines of attack widely different in method and seemingly in point of view. Such concerted effort in cell research long seemed an almost unattainable ideal; but

16. Edmund B. Wilson, *The Cell in Development and Inheritance* (New York: Macmillan, 1896; 2nd ed., 1900; 3rd ed., as *The Cell in Development and Heredity*, 1925).

> its realization now seems close at hand. The present book has been undertaken in hope of furthering this co-operation. In the nature of the case it is hardly possible to arrive at complete unity in a work produced by several collaborators representing widely diverse fields of research. Such a group, however, can at least bring to their task a broader and more critical knowledge of the subject than any single writer can at this day hope to command.[17]

They selected an editor (Edmund Cowdry), divided up the tasks, and set to work.

The result, as they realized, could not have been written by any one author. Indeed, probably no one contributor would even have agreed with all the conclusions in it. The approaches, assumptions, and conclusions varied widely. The product was impressive and wide-ranging, even though its separately authored chapters remained individualized and only secondarily collaborative. The collaboration resulted because of the commitment of all participants to defining the project and to working together on it, and it does seem appropriate to call the project collaborative. Again, however, only in the secondary sense.

A number of current textbooks carry on this tradition. Molecular biology may provide the most interesting recent example, for two teams of leading scientists have produced two competing collaborative texts.[18] Both are impressive, high-quality products. Both were produced by a team of experts working together and with the goal of producing a seamless encapsulation of the best of what we now know in molecular biology. The products are very different, with different emphases and graphic designs, yet they remain more similar than different in their approaches. The projects appear to have been truly collaborative and to have produced volumes that have gained more credibility, and from a much larger audience, than they would have gained without the collaborative effect of shared expertise.

The larger question remains for any such project, however, whether all collaborators participated fully: did they all value the

17. Edmund V. Cowdry, editor, *General Cytology: A Textbook of Cellular Structure and Function for Students of Biology and Medicine* (Chicago: University of Chicago Press, 1924), p. 11. For further discussion of this volume, see Jane Maienschein, "Cytology in 1924," in Benson et al., *Expansion* (above, n. 15), pp. 23–51.

18. Bruce Alberts et al., *Molecular Biology of the Cell* 2nd ed. (New York: Garland, 1989); James Darnell et al., *Molecular Cell Biology*, 2nd ed. (New York: Scientific American Books, 1990).

project, or were some hired just to "do a job" and contribute some small part to the volume? Have they all even read, and do they agree with, the entire product? Should they have done so in order to qualify as full primary collaborators? Here we enter gray areas of definition and of professional ethics. As recent highly publicized cases show, it can be unwise at best, and immoral in some cases, to lend one's name as coauthor to a project with which one does not agree in full. The advantages of sharing the credit through coauthorship and collaboration are clear – but so are the responsibilities, which bring potential problems. Collaboration is not always an unequivocally good thing.

The same impulse lies behind the urge to carry out interdisciplinary projects. These often slide off the scale of collaboration to become a conglomeration of specialized expertise. Each contributor wants a product and shares in the general goals, yet no one really even understands it all. Though they share in articulating the project, the individuals do not share in the parts. The whole truly becomes more than the sum of the parts – and takes on a life beyond that of the parts. It remains for an editor to make sure that the parts do, in fact, fit together legitimately and to their mutual benefit.

Creating Communities

One paper by Wilson and Morgan from 1920 illustrates what appears to be an incomplete or artificial co-laboring. Both wished to give responses to two short papers they had recently read. Normally, they would have submitted two separate papers – yet for some unstated reason these two colleagues stapled their independent contributions together and sent them off as parts I and II under the same title.[19] The resulting hybrid paper seems odd, but they probably gained credibility and notice for both positions by doing it this way. In part also, they may have been working to effect a sense of community. They were both at Columbia and shared many of the same interests and some of the same students. Wilson emphasized cytology and Morgan genetics and embryology, but they asked many overlapping questions and pursued them in similar ways, consistent with the training they both

19. E. B. Wilson and T. H. Morgan, "Chiasmatype and Crossing Over," *Amer. Nat.*, *54* (1920), 193–219. Part I, by Wilson, is "A Cytological View of the Chiasmatype Theory"; Part II, by Morgan, is "The Spiral Looping of the Chromosomes and the Theory of Crossing Over." I am grateful to Eli Gerson for pointing this odd paper out to me.

had received at Johns Hopkins. They often discussed research ideas and shared discoveries.

This desired sense of community also inspired those cytologists who participated in Cowdry's cytology project. Others at Woods Hole have also shared interests, cooperated with each other in sharing techniques or ideas, and collaborated as well in many cases.[20] And a number of husband-wife teams have collaborated on projects which have benefited from having more than one set of hands – or more than one head and expertise – in the laboratory, sharing responsibility in working toward a common goal.[21]

A century ago, Whitman tried to forge a community of biologists both at the MBL and as first chairman in biology at the University of Chicago. Instead of calling it "collaboration" explicitly, he called for "cooperation." Specialization and cooperation would go hand in hand, he felt: if individuals concentrated on their own specialized research, they would come to work together to their mutual benefit thereafter.[22] He set an ideal for biology, which some others adopted. Yet increasing specialization and a proliferation of biology programs in the early part of this century created divisions and lack of even basic cooperation, while the increase of money available for research created a climate for competition as well.

It was precisely the availability of new money that ultimately created a push to collaboration and cooperation again in the 1920s, however. As Robert Kohler has explained, research foundations began to develop partnerships with universities in the pursuit of science. The foundations, and the government agencies that began to emerge at about the same time, pushed away from individual research efforts and toward the development of a "collective, communitarian character" for science.[23] The foundations purposefully wished to promote cooperation. As both Kohler and Lily Kay have pointed out, the Rockefeller Foundation was particu-

20. See, for example, a compilation of examples in Jane Maienschein, "Collaboration," in Maienschein, *100 Years Exploring Life, 1888–1988* (Boston: Jones and Bartlett, 1989), pp. 120–122.

21. See examples in Pnina Abir-Am, ed., *Creative Couples in Science* (New Brunswick, N.J.: Rutgers University Press, forthcoming).

22. For discussion of this point see especially the introduction to Jane Maienschein, ed., *Defining Biology: Lectures from the 1890s* (Cambridge, Mass.: Harvard University Press, 1986), pp. 3–50. Also see Charles Otis Whitman, "Specialization and Organization: Companion Principles of All Progress – The Most Important Need of American Biology," *Biol. Lect. Marine Biol. Lab.*, (1890), 1–26.

23. Robert E. Kohler, "Science, Foundations, and American Universities in the 1920s," *Osiris*, *3* (1987), 140.

larly instrumental in pushing in this direction.[24] It was to the advantage of scientists to at least appear to be working cooperatively, and ideally even collaboratively, in teams. Sinclair Lewis captured this emphasis when he had the director of his shiny research factory, the McGurk Institute, say to the young Martin Arrowsmith: "The one thing for you to keep in view in all your work is the ideal of cooperation."[25]

The evolutionary synthesis provides another example. As Joe Cain argues, work on systematics within biology had become the underdog in funding and in priority by the 1940s. Biology had always had to fight for its place among the sciences, and recent successes in physics and chemistry by the 1930s had placed the biological sciences in a weak relative position. Thus, it was in the interest of biologists to band together, and in the interest of systematists and evolutionary biologists to insist on providing a unifying framework, in order to gain access to resources. It was in their interest to forge a community. Failing that, it was at least necessary to create the impression of community. Thus, the growing emphasis on *synthesis*, on *cooperation*, and on *collaboration* played a basic political function in gaining credibility and visibility for the participants. This sense of working together to create a community played an important role in defining and redefining biology.[26]

CONCLUSION

Clearly, researchers collaborate for a variety of intellectual and social reasons: to get help, to combine expertise, to gain credibility, or to create a community. No one model will hold for all cases. Similarly, no one simplistic set of rules to guide scientific conduct and guarantee scientific integrity will suffice either. Instead, those prescribing professional behavior must recognize the various types and levels of collaboration, the reasons for the collaboration, and the implications of each case.

24. Robert E. Kohler, *Partners in Science: Foundations and Natural Scientists 1900–1945* (Chicago: University of Chicago Press, 1991); Lily E. Kay, "Into the Lab: Creating a Molecular Knowledge of Life," paper presented at the History of Science Society meeting, Seattle, 1990, and part of a larger book in progress.

25. Sinclair Lewis, *Arrowsmith* (New York: Grosset and Dunlap, 1925), p. 196.

26. Joseph Cain, "Common Problems and Cooperative Solutions: Organizational Activity in Evolutionary Studies, 1936–1947," unpublished manuscript (discussed in part at the International Society for the History, Philosophy, and Social Studies of Biology meeting, Evanston, 1991). Several unpublished papers by Vassiliki B. Smocovitis also discuss this point.

Collaborators who seek the help of an extra laborer should identify carefully what the contribution is expected to be and what benefits will result (such as pay, coauthorship, acknowledgment, etc.). Collaborators who work together to combine expertise and to gain greater credibility need to determine the responsibilities and expected benefits for each participant, and to clarify that all are expected to study carefully and certify the results. Those who work to create a scientific community should recognize that the effort may fail and that some individuals may succeed more than others, which may be frustrating to those who do not; or the effort may succeed in producing a community that some participants do not like. Each model for collaborating brings its own benefits and risks; each needs its own set of rules.

Historical reflection illuminates some of the diverse cases to help bring into focus some of the basic questions surrounding colaboring. As my colleague James Collins points out, there are immediate advantages for scientists today in recognizing the different types and purposes of collaboration. Acknowledging the vagueness of the concept, with its various primary and secondary forms, provides a warning for contemporary managers of laboratories. If scientists see collaboration as tied to different levels of responsibility, then managers who enter a collaboration must obviously assume the appropriate responsibilities for their project. More surprisingly for many scientists, they must also respect other collaborators' exercising of their rights and responsibilities. Thus, a principal investigator should not ignore what other collaborators see as problems and should accept the others as full participants. He or she must accept that others – whether primary or secondary collaborators – may decide to stop contributing.

Collaborators collaborate for a variety of reasons. Articulating those reasons to define the respective roles of all contributors, and recognizing whose interests are being served by the collaboration, can help science progress more smoothly.

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