# Rapid #: -20673668

CROSS REF ID: **6227665**

LENDER: **HUL (Harvard University) :: HD**

BORROWER: **HUL (Harvard University) :: Widener Library**

| | |
|---|---|
| TYPE: | Article CC:CCL |
| JOURNAL TITLE: | Knowledge |
| USER JOURNAL TITLE: | Knowledge. |
| ARTICLE TITLE: | Edited Excerpts from a Smithsonian Seminar Series |
| ARTICLE AUTHOR: | Borden, Carla M. |
| VOLUME: | 14 |
| ISSUE: | 1 |
| MONTH: | |
| YEAR: | 1992 |
| PAGES: | 110-132 |
| ISSN: | 0164-0259 |
| OCLC #: | |
| PATRON NOTES: | The section is authored by myself (Michael MJ Fischer) and Mehdi Abedi 14 |

Processed by RapidX: 4/26/2023 9:52:51 AM

**10** **Odyssey**

**Rapid #: -20673668** **odyssey.rapid.exlibrisgroup.com/ILL**

| Status | Rapid Code | Branch Name | Start Date |
|---|---|---|---|
| New | HUL | Widener Library | 04/24/2023 08:37 PM |
| Local Holding In | HUL | Widener Library | 04/24/2023 08:37 PM |
| Pending | HUL | HD | 04/24/2023 08:39 PM |
| Batch Not Printed | HUL | HD | 04/25/2023 07:39 AM |

**CALL #:** **H62 .K626**

**LOCATION:** **HUL :: HD :: HXK3A1**

| | |
|---|---|
| REQUEST TYPE: | Article CC:CCL |
| JOURNAL TITLE: | Knowledge |
| USER JOURNAL TITLE: | Knowledge. |
| HUL CATALOG TITLE: | Knowledge |
| ARTICLE TITLE: | Edited Excerpts from a Smithsonian Seminar Series |
| ARTICLE AUTHOR: | Borden, Carla M. |
| VOLUME: | 14 |
| ISSUE: | 1 |
| MONTH: | |
| YEAR: | 1992 |
| PAGES: | 110-132 |
| ISSN: | 0164-0259 |
| OCLC #: | HUL OCLC #: 4556737 |
| CROSS REFERENCE ID: | [TN:6227665][ODYSSEY:206.107.43.109/HUL] |
| VERIFIED: | |

**BORROWER: HUL :: Widener Library**

PATRON NOTES: The section is authored by myself (Michael MJ Fischer) and Mehdi Abedi 14

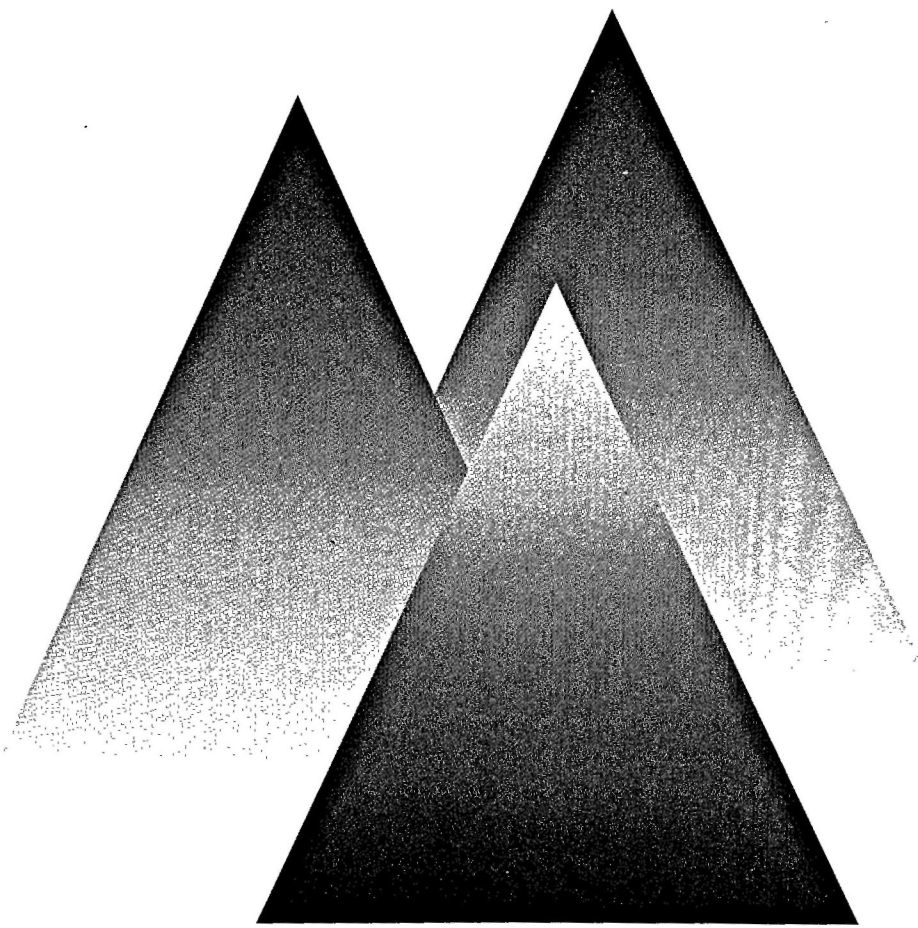
ExLibris RapidILL

Volume 14, Number 1 / September 1992

# Knowledge

Creation ▲ Diffusion ▲ Utilization

An Interdisciplinary Social Science Journal



SAGE Periodicals Press

KNOWLEDGE
*An Interdisciplinary Social Science Journal*
Volume 14, Number 1
September 1992



*Contents*



Sage Periodicals Press
A Division of SAGE Publications, Inc.
Newbury Park • London • New Delhi

*In this excerpt from a series of seminars on knowledge generation in the arts, the sciences, and the humanities, scholars of philosophy, history, and anthropology explore the meaning of collaboration in their respective fields. The seminars, held at the Smithsonian Institution during 1991, compared the goals, techniques, and myths of creative and scholarly collaboration.*

# *Edited Excerpts from a Smithsonian Seminar Series*

*Part 3: The Humanities and Social Sciences*

**CARLA M. BORDEN (Seminar Editor)**
*Smithsonian Institution*
**GEORGE R. LUCAS, Jr. (Speaker)**
*National Endowment for the Humanities*
**PETE DANIEL (Speaker)**
*Smithsonian Institution*
**STEVEN F. MILLER (Speaker)**
*University of Maryland*
**MICHAEL M. J. FISCHER (Speaker)**
**MEHDI ABEDI (Speaker)**
*Rice University*

## *Introduction*

*The following excerpts complete the presentation of a series of seminars on how knowledge is created through collaborations in the creative arts, sciences, social sciences, and humanities.*

*In the third seminar (held at the Smithsonian Institution, Washington, DC, 23 September 1991), scholars of philosophy, history, and anthropology described modes of collaborations in their fields.*

*—Carla M. Borden*

---

*George R. Lucas, Jr., is Assistant Director in the Division of Research Programs at the National Endowment for the Humanities. He has also been professor of philosophy at Emory University, the University of Louvain (Belgium), and the University of Santa Clara. His books include* Lifeboat Ethics: The Moral Dilemmas of Hunger *(1976);* Poverty, Justice, and the Law *(1986); and* The Rehabilitation of Whitehead: An Analytic and Historical Assessment of Process Philosophy *(1989).* *—M.C.L.*

***George R. Lucas, Jr.:*** When Carla Borden called to invite me to this colloquium on collaboration in the humanities, she remarked that she wanted me to "focus on the question of why humanities scholars don't routinely collaborate with one another."

My initial reaction to this request was defensive. "But, of course," I thought to myself, "humanities scholars *do* collaborate with one another!" As proof, consider the other distinguished participants invited to participate on this panel this afternoon: Steven Miller's project is funded in part by the National Endowment for the Humanities and represents a famous and long-standing collaboration. Pete Daniel has worked on the Booker T. Washington papers, a collaborative project lasting some twenty-five years. The National Endowment for the Humanities (NEH) sponsors the *Dictionary of Middle English* and the *Dictionary of Regional American English*, as well as a number of encyclopedias and reference data base projects, together with editions of papers of other famous Americans, including Frederick Douglass,

---

*Editor's Note*: This article represents the final installment in a three-part series guided by Carla M. Borden. Previous installments appeared as "Knowledge Collaborations in the Arts, the Sciences, and the Humanities: Edited Excerpts from a Smithsonian Seminar Series—Part 1: The Arts," which appeared in *Knowledge: Creation, Diffusion, Utilization*, Volume 13, Number 2 (December 1991); and "Knowledge Collaborations in the Arts, the Sciences, and the Humanities: Edited Excerpts from a Smithsonian Seminar Series—Part 2: The Sciences," which appeared in Volume 13, Number 4 (June 1992). Borden's summary of the seminars and the published excerpts appears elsewhere in this issue as Carla M. Borden, "Knowledge Collaborations in the Arts, the Sciences, and the Humanities: Collaboration, For Better or For Worse—Part 4," *Knowledge: Creation, Diffusion, Utilization*, Volume 14, Number 1 (September 1992).

*—Marcel C. LaFollette*

Mark Twain, John Dewey, and William James. All of those are, in an important sense, collaborative projects in the humanities.

The program that I will describe, however, is a program intended to support large, substantial, and highly significant interpretive research projects in the humanities which are collaborative in design, in contrast to individual research projects supported by small fellowships. . . . A project mentioned in Borden's introductory remarks to the seminar—the collaboration by Robert Bellah with his students and colleagues—is perhaps the NEH's single most famous collaborative project. The Bellah team's initial book, *Habits of the Heart* (Bellah et al. 1985), was widely reviewed and provoked a great deal of national discussion and positive response; that book was the result of a collaboration funded by the NEH, as is their current book, *The Good Society* (Bellah et al. 1991).

All this, however, is, in a sense, "spoken like a true bureaucrat." That is, to Carla Borden's initial inquiry about the relative infrequency of collaboration in the humanities, I have responded merely with defense of the programs supported by my agency. In fact, this really isn't the response that she had in mind. She was broaching a question that troubles many scholars, including those who work at the Endowment—and indeed, troubles many of us teaching and conducting research in U.S. colleges and universities today. Research in the humanities is generally carried out through the scholarly efforts of individuals working alone, rather than as members of some research team. When published, the "definitive" biography of Abraham Lincoln, or the "revolutionary" new theory of art history and aesthetics, will most probably bear the name of only a single author.

Why should this be so? What is it about the nature of "research" in the humanities that constrains this activity primarily to a solitary enterprise, in contrast, for example, to the natural sciences, in which research is customarily undertaken through collaboration?

I want to suggest that there is nothing peculiar about the subject matter of humanities disciplines themselves which favors individual over collaborative research. Rather, I want to suggest that there is something peculiar about the pedagogical "socialization process" by which new scholars are trained and brought into these disciplines—something peculiar about the structure of education, especially graduate education in the humanities, that discourages cooperation and collaboration in favor of individual effort. The pedagogical socialization process in the humanities stands in sharp contrast to that represented by graduate education in the natural sciences, in which new scholars are trained virtually from the outset to collaborate with peers and mentors in the pursuit of scholarly research. The social sciences lie in the interstices of this pedagogical divide: while I cannot pursue the matter in

detail, my account will suggest that the social sciences are not perceived as "scientific" on the basis of their subject matter or results so much as on their attempts to adopt the research methodology of the natural sciences, which is primarily quantitative and collaborative.

In order to illustrate this essential difference, I want to relate a "tale of two students." Each of the following stories is hypothetical, but fact-based. My student "cases" are drawn from the natural sciences (specifically, physics) and from the core humanities (specifically, philosophy), respectively. Let me begin with the physicist.

## *I.*

Physics students take a number of courses, both general survey and specialized upper-level topics in the subdisciplines of their field. The courses, and the students, are extremely competitive—only the best and brightest survive.

At some point, two or three years into their studies, however, students begin to develop a keen interest in one of the specialties of their field, say, nuclear physics. Our hypothetical student has attracted the attention of one of the brightest, most productive members of the faculty, who directs a large grant for the National Aeronautics and Space Administration. Our student is offered a place as a junior collaborator on a research team that conducts "scattering experiments," designed to study nuclear structure by aiming collimated beams of elementary particles at selected targets and studying the results. Because it is well funded, the team, consisting of, say, three faculty in his school plus seven graduate students (linked with similar collaborators in two other institutions), has money for the latest equipment and uses the latest data analysis and computer techniques. There is much to do, and much that can be learned, including ample opportunity to carve out a portion of the larger project for the student's thesis research. In addition, this student will be paid a living wage for the foreseeable future as a member of the team and will work on "frontier" topics side by side with some of the best senior researchers in his field.

If the physics student accepts this opportunity, he may expect to collect a reasonable salary over the next two to three years, spend many a night on the cot at the cyclotron, do his share of managing the larger group effort, and be given a portion of the experiment to work on for his thesis. Let us suppose that his work on nuclear energy levels in selected isotopes is productive: he discovers several new levels, makes a number of measurements for the standard reference works, and writes up the results, complete with theoretical

analysis and conclusions. The detailed study, unpublished, is his thesis; the several shorter papers that report specific discoveries are published in the main refereed journal in his field. His name, as principal author, is listed first, followed by his faculty mentors and student teammates at his school, followed by the collaborators who participated in any way in this overall effort at the other participating institutions. In some cases, there may be as many as twenty, and sometimes even a hundred names on such a publication.

Because of the excellence of his own work, and the fine reputation of this research team, he is offered a good position as a postdoctoral research assistant on another research team. After three to six years as a "post-doc," he is successful in landing a tenure-track position in a large department, whose nuclear physics team needs another experimentalist with his particular training and expertise. Tenure and promotion will come through teaching and through continuing to work and to publish as a member of one or another such collaborative research team.

## *II.*

Our hypothetical philosophy student also begins by taking a series of general survey courses, followed by specific upper-level courses in the subfields of his discipline. This course work is also quite competitive, and only the best and brightest survive. After two to three years, he decides that his interest lies in late Enlightenment moral and political philosophy; he decides to write a dissertation on the German philosopher Immanuel Kant.

Each of his student colleagues, like himself, is facing the choice of specialization and thesis topic. Although the students, and occasionally some of their faculty, meet now and then for beer and discussion and perhaps plan a more formal social event once or twice a year, for the most part each student is busy studying, writing term papers, and deciding upon an area for thesis research. There is a certain camaraderie and esprit de corps derived from the joys and miseries of graduate study; but there is nothing to correspond to the physics student's opportunity to join a collaborative team, let alone be paid for the privilege. Instead, each student finds a single faculty mentor who specializes in the general area of that student's specific interest. Let us suppose that our philosophy student decides to work under the supervision of an internationally prominent scholar of the Enlightenment on the graduate school faculty, with whom he meets to discuss his project and his progress on a regular basis. They agree on a thesis topic, on the materials to be studied, and on a plan for research. The student pursues most of this on his own, a solitary figure working till midnight in the campus library. Once or twice a

month he reports to his mentor and turns in a chapter of the thesis or discusses some problem he has encountered. His graduate student assistantship expires during this period, and he is obliged to take on part-time teaching at a nearby community college to support the final phase of his doctoral studies.

After two years of such solitary research, he presents a draft of his work to a committee composed of his faculty mentor and perhaps two other members of the department faculty, who serve as second readers. These latter may or may not have been involved in any direct way in the design and execution of the research plan. The student presents a largely exegetical and dialectical study of Kant's writings – that is, he endeavors to tell us what Kant really said or meant, laced with references to secondary scholarship by other isolated figures working in this same area over the past century, with a decided preference for recent work by famous contemporary figures in major departments, such as that of his own teacher/mentor. This thesis is microfilmed and subsequently embalmed for future scholars, while a single master copy is filed on the library shelves at his own graduate alma mater. It may never be read again. It is entirely a matter of initiative on the part of the student whether any scholarly articles are drawn from this work or whether the thesis itself is later revised and published as a book. Tenure (should our scholar be lucky enough to land a teaching post) will likely depend upon such independent initiative.

One final contrast bears mention. Because of the need to stay current in the field, and "up" on the latest discoveries and techniques, the physicist's multiple-authored paper stands about a 90 to 95 percent chance of being read and cited in similar work by other researchers. The book, or the article, by our philosophy student, should he choose to engage in the further effort to publish them to obtain tenure, stands only about a 2 percent chance of being read and cited by other scholars in his field (Hamilton 1991).

## *III.*

Why don't humanities scholars collaborate more readily? Notice that in these two contrasting educational paradigms there are no support systems, models, or modes of encouragement for collaboration built into the fundamental structure or into the socialization process in the humanities. There is simply no readily apparent cognitive category for envisioning such activity as a viable option in these disciplines: that is to say, collaboration is not the sort of activity that it would even occur to someone in the humanities to undertake as a normal "default" option.

In some areas of the humanities, collaboration *is* becoming the norm—usually in such fields as history and archaeology, which blur the distinction between what our German colleagues (after Wilhelm Dilthey) term the *Naturwissenchaften* and the *Geisteswissenschaften*. Residing on this boundary, the social sciences, in fact, consist largely of areas like psychology, sociology, and economics—formerly considered branches of philosophy and in which research was once pursued by individual scholars stressing historical, narrative, interpretive, largely qualitative research methodologies. These areas now tend to utilize quantitative, empirical methods and to stress descriptive, nomological accounts of evidence rather than "interpretation," and *research is pursued collaboratively*, as in the natural sciences.

But, in the main, humanities scholars do *not* collaborate on *basic* research in their fields. My "tale of two students" suggests that habit and pedagogical tradition are essential to perpetuating this state of affairs. There is little in the way of either precedent or encouragement for collaboration in the humanities; in fact, collaboration is sometimes actively discouraged. There is a tendency among humanities scholars to denigrate the significance of multi-authored works as somehow representing a "shortcut" to publication. Tenure decisions in the humanities are more difficult to make if the works are coauthored, let alone "multiauthored." How much credit for the work ought to be apportioned to the individual who is under evaluation, for example?

Indeed, the quintessential image of scholarship and research in the humanities is suggested in the vignette about the philosophy student above: the isolated, individual scholar, toiling in his or her lonely cell, pursuing the esoteric textual-historical project which, in the final analysis, interests primarily the scholar and members of his or her immediate family, and perhaps a few kindly disposed senior colleagues willing to review the publication in some scholarly journal, where it is quickly relegated to footnote immortality.

In the NEH, awards to individuals have tended to be perceived as the norm. The NEH Division of Fellowships and Seminars, for example, receives literally thousands of inquiries and as many as two thousand applications each year for individual fellowships and yet is able to fund only about 12 percent of these requests. By contrast, in the NEH Division of Research Programs, a program entitled "collaborative projects" is perhaps the best-kept secret in the Endowment. During fiscal year 1992, Congress has apportioned nearly $3 million to support collaborative research in the humanities. To date, this program has received a record high of 197 applications for these funds, 40 percent of which were for projects in archaeology. Opportunities for funding collaborative research are not well known in the core humanities; many humanities scholars would not even think of inquiring about them.

Happily, this situation is beginning to change. During the past fiscal year, NEH peer review panels of eminent scholars enthusiastically endorsed a number of significant and innovative research projects that are simply too large in scope and importance to be carried out by any one person alone. For example, two historians of slavery and three other research consultants are collaborating on a documentary and demographic study of the persistence and evolution of distinctive African cultural traditions in Spanish and Early American Louisiana in the seventeenth and eighteenth centuries. Basing their work on the rich documentary evidence available for slaves and free Africans in this period, the scholars bring to this research their special knowledge of the slave trade, African ethnic institutions transplanted to the New World, and African economic history. In another NEH-funded project, two distinguished scholars of American literature will organize an international team of experts for a collaborative study of the influence of the American poet Walt Whitman (1819-1892) on the national literatures of other countries around the globe. The team includes scholars from numerous countries in Europe, Latin America, Africa, and Asia who will cooperate in publishing a special centennial issue of the Walt Whitman journal and a volume of essays analyzing the reception and contemporary influence of Whitman's poetry and prose in the countries represented. Finally, a philosopher of science and a historian of science will study Johannes Kepler's intellectual development and accomplishments in astronomy and physics, examining the variety of natural philosophies available to Kepler early in his career, the university culture he encountered, and the intensely reform-minded religious context he absorbed. This study will clarify the pivotal role of Kepler's thought in the transition from Renaissance humanism to modern science.

As I suggested from the outset, there *are* good models of collaboration in the humanities. One *can* locate scholars in these fields who collaborate in their research in fundamental and highly productive ways. At the same time, I have suggested that there is something structurally at work in the education process that blinds scholars in the humanities to the option of carrying out their research collaboratively. I believe that my "tale of two students" begins to suggest what this structural element is, and why it has prevailed.

*In response to a question from a member of the audience, George Lucas later added:*

To scholars contemplating a collaborative undertaking, I would recommend that each be certain that his or her own individual "professional" identity was already reasonably well established, and that each be clear on the contributions he or she would expect to be able to make, and what each

would expect to receive as a result of the collaborative venture. In the absence of a reasonably well-established professional identity, the individual scholar may find that his or her entire career is dependent upon the success or failure of a large, complex, and often risky venture. This is not a comfortable situation.

When we think of collaborative research models in the sciences or in the humanities, we generally tend to envision, as representative examples, only those collaborations which have succeeded. We don't always hear about those which fail. Successful collaborations represent a resonance, a synergy between the collaborators, issuing in positive accomplishments and results in which all collaborators share. My account of the physics student represented the positive impact for one junior collaborator of the success of work undertaken by the team of which he was invited to be a part. Their success redounded to his enhanced professional reputation and to the enhancement of his own individual career.

So, likewise, when we read in the news about a high-energy research team at, say, Stanford or an astrophysics/astronomy team at Texas making a startling and revolutionary new discovery, and when the junior members of those highly successful teams leave those universities to apply for faculty positions elsewhere, their own reputations are enhanced by having participated in the successful collaboration at Stanford or Texas or wherever. The connection is rewarded and appreciated—even though it may not be clear exactly what their individual contribution was to the success of the project. It was enough that they were there.

This is not always positive, however. Recently, my graduate school roommate (now a leading high-energy particle physicist) told me about a calamitous project at a major research university which had failed. The reverberations of this failure were not confined to disappointment and wasted research funds; rather, the failure left a number of the younger collaborators without anything substantial to document on their professional *curricula vitae*. In one case, a young scientist had worked on this huge project for several years, written a doctoral thesis based upon his portion of the work, linked to the anticipated final results. He then obtained a tenure-track job at another institution, continued to carry out research as a faculty member on the original collaboration based at his graduate institution, and was awaiting the appearance of a number of journal articles that should have subsequently flowed from the original project, for which ultimately he would then be able to claim at least partial credit when it came time for his own tenure review. Thus, when the collaboration failed, this young scholar was left with little to claim in the way of positive (and tenurable) accomplishments for his own career, despite his long service and experience on the original research team!

In effect, he staked his entire career on one huge collaborative undertaking that failed, leaving his career in ruins.

As a result, we tend to recommend that young scholars in the humanities first complete a book or a series of substantial articles that, in effect, defines their own professional expertise. Only then, as more experienced scholars with a track record, should they venture on into the larger, bolder, and more risky (even if exciting) research that collaborative projects entail.

---

*Pete Daniel is a curator in the Division of Agriculture and Natural Resources in the National Museum of American History. His books include* The Shadow of Slavery: Peonage in the South, 1901-1969 *(1972);* Breaking the Land: The Transformation of Cotton, Tobacco, and Rice Cultures Since 1880 *(1985); and* Standing at the Crossroads: Southern Life since 1900 *(1986). He has worked on a number of collaborative projects, including the Booker T. Washington Papers and museum exhibits.* —*M.C.L.*

***Pete Daniel:*** Over the past twenty years, I have collaborated on some half dozen projects. Each case of collaboration is different; each developed from the person[s] I was collaborating with and the project. In each case I felt at times that I was carrying most of the burden, and I am sure that the other collaborators had similar thoughts. There is tension in collaboration because one is dealing with intellectual property, and those involved want excellence but fear that one party or the other will fail to achieve it.

In one sense, my most satisfactory collaboration was on the Booker T. Washington Papers, probably because I was there on the day of its founding as a graduate assistant assigned to the project; learned a great deal about historical research at the feet of Louis R. Harlan, the editor; participated in decision making (which documents to publish) with the editor; shaped editorial style; and helped put together the first volume of letters which was well received by reviewers. The key to this harmony was Louis Harlan, who told me as I did research that anything interesting that I came across I should pursue. One article resulted from this freedom, and also I discovered collections that helped me in other research.

Other projects have not been so harmonious. I would like to avoid specifics, but in one collaboration I was so outraged at the behavior of the publisher that I wanted to stop production and would have. My coauthor held back, and the result was disappointing. Had I been in charge, the project would have stopped until the publisher came to my terms. Thus independence, or lack of it, is one of the tensions that arises in collaboration. One is

restrained by the coauthor[s]. The collaborator shares both the glory and the grime, and, in the latter case, blame naturally arises, as in the former do claims of making the project excellent.

Perhaps the most subtle part of collaboration is that it links a number of people in intellectual harness. It is indeed difficult to find collaborators who share ideology, method, and temperament. In larger collaborations it is probable that some will not be intellectually as apt as others or perhaps not even be conversant with the expected vocabulary. *Official Images: New Deal Photography* (Daniel 1987), a 1987 exhibition at the National Museum of American History and a book, started out as a conversation I had with Merry Foresta, curator at the National Museum of American Art, about New Deal photography. We both agreed that the Farm Security Administration photographs were the only photos that most people ever saw, even though there were many other windows on the New Deal. We were fortunate that there were two Fellows in the area, Sally Stein and Maren Strange, who were leading scholars in the field of photography.

Even though we were intellectually compatible, there was substantial creative tension in this group. For example, I was to write the introduction to the book, which I drafted. Stein and the other members of the project were not satisfied with it. At last Stein asked if she could coauthor it. I agreed, and the result was much stronger than had I done it myself.

At points such as this, one's ego enters. With intellectual projects, the writer's ego is precious, and criticism (unlike the kind one gets in reviews) must be muted. This is not always possible. Several times in the editing of the essays that compose *Official Images*, I cooked dinner for my three collaborators. We would banter during the meal and then get down to bloodletting. At times the discussion was brutally frank and I am sure that some of us were bruised and sliced. Yet this was a tough group, and we all worked hard to make the book a success. We were all proud of it in the end, and had we not been so forthright about our misgivings, it would not have been so good.

If there is a secret to collaboration, it is not found in doing a project with a friend, in collaborating with someone whom you think you can dominate, or in thinking that there will be less work if you throw in with someone else. Mutual respect and forthrightness seem to me to be the most important ingredients.

Yet, as I mentioned, every project differs, every personal combination is unique, and even the best intentions can explode into chaos. While I have enjoyed most of my collaborations and am proud of them, the work that I have done alone has been the most rewarding. Then I can simply dismiss bad reviews as the narrow tirades of lesser minds who don't understand or accept

the wise evaluations of other reviewers. With collaborators, one must share and usually analyze such reviews.

---

*Steven F. Miller holds an M.A. in American Studies and is completing a Ph.D. in history at the University of Maryland, College Park. He has been an editor on the Freedmen and Southern Society Project since 1984. He is coeditor of two volumes of* Freedom: A Documentary History of Emancipation *(Cambridge University Press, 1982) and coauthor of two forthcoming volumes,* Slaves No More: Three Essays on Emancipation and the Civil War, *and* Free at Last: A Documentary History of Slavery, Freedom, and the Civil War. *—M.C.L.*

***Steven F. Miller:*** I'd like to say a few words, a kind of "show and tell," about my experiences and those of my colleagues on the Freedmen and Southern Society Project. For about sixteen years, the project has been engaged in writing a documentary history of the emancipation, using documents from the National Archives. For us, collaboration began in part as an ideal of how scholarly work should be organized, but also in large part for sheer practicality. When the project was initiated, my colleagues knew that there was a huge volume of documents in the National Archives—far more than one individual scholar could hope to assess over one lifetime, or maybe even several lifetimes. The project's staff didn't know at the time quite how large it was. Over the course of three very intensive years at the beginning, and less intensively subsequently, we have gone through about three or four million documents and culled out about fifty thousand or so. Moreover, the granting agencies were, for quite understandable reasons, unwilling to give start-up money for something that was unlikely ever to be finished by one lonely individual pawing through the Hollinger boxes at the Archives.

In the nature of things, the work of selecting and annotating the documents and preparing the volumes has involved many minds and many hands. But over the years, it has become something more than just a question of division of labor in order to get things done expeditiously. At its best, our collaboration has become an ongoing intellectual exchange among coequal participants. The nature of the material we work with and the mandates of the project itself are conducive to collaborative work.

Our task in this respect is different from that of many other historical editing enterprises. Editing the papers of a single individual is the model for the field. In that model, the first task of the editor is to search the various depositories in the country and find all the documents that were authored by

or written to a particular person. And once this universe has been defined, the editor in chief is responsible for winnowing documents and trying to make some final decision about which ones to use.

Our task, on the other hand, began with a mountain of documents and making preliminary selections on the ground floor, so we would have something that would be manageable to work with later. Selection was done on the spot by the person who first encountered the document. For that reason, he or she had to have enough independence and competence in the subject to make an on-the-spot decision about whether a document was something we wanted to use.

Another consideration that favored collaboration is that our documentary history was to be interpretive; that is, we wanted to do more than just compile several books' worth of interesting documents pertaining to emancipation. We set out self-consciously to select and present them with a view of telling the story of how American slaves became free men and women and what that process meant for the history of the United States.

As a result, we knew that as editors we would have to have (or come to) a similar understanding of what story we were telling—what the main themes were, who the chief characters were, and so on. This process is still going on. We readdress it every time we start a new volume and, at times, even when we go over particular chapters in particular volumes. It is reflected in the organization of work on the Freedmen and Southern Society Project. In essence, each editor on the project is a coequal participant in the production of volumes. We tend, to the extent possible, to work on different chapters in parallel fashion. We begin with collective meetings which discuss major things, the volume, the rough organization of chapters. Then the chapters are assigned to particular editors, each of whom brings certain expertise in particular subjects.

For example, our last volume was about labor and relief arrangements in the Union-occupied Confederacy during the Civil War. Different editors had, over the years, evolved certain regional interests and specializations, and we assigned chapters on the basis of those. After that, each editor was, at least for a while, on his or her own. The editor is responsible for accumulating a preliminary collection of documents, probably twice the total number that will end up being published, and for preparing a draft essay that elucidates the major themes for that chapter and connects them to what we think will be the themes of the volume. This is done largely on an individual basis, while other people are doing the same thing—coming to terms with their own subjects in their chapters. Quite regularly, we touch base, discuss the relation of one chapter to another, exchange documents, arrange the custody of a

document that might be suitable for one chapter but also for another one, and that sort of thing.

Besides these kinds of informal ways in which we interact in preparing the volumes, there are formal sessions as well. The main one is something we call "score carding," in which the preliminary draft of the chapter is circulated among the other editors for comments on the document selection and presentation. Editors come to their own conclusions about which documents seem best suited to telling the story, which ones seem most marginal. And this is done not only on the basis of what they see before them—a small sample of the documents pertaining to that subject—but also on the basis of independent knowledge that comes from working on their own chapters. They can see ways in which this social experience, say, plantation labor in the Mississippi Valley, is similar to or different from some experience somewhere else, say, the southern part of Louisiana. This is one of the most intellectually exciting aspects of working on the project, where we decide, on the basis of independent knowledge and collective discussion, how the chapters are going to shape up.

Similar discussions follow in the preparation of an introductory essay for the volume. Each of these stages, with the exception of the preparation of individual chapters, involves a large amount of give and take, learning from colleagues who have particular knowledge of the field, part of which they brought with them when they joined the project, part of which they have developed since.

I don't wish to be seeming to say that our way of doing things necessarily is applicable to other scholars in the humanities. We have had our own particular concerns from the first. The nature of the material shapes what we do in certain ways. It seems to me, though, that there are distinct advantages to organizing the work and approaching intellectual problems in a collaborative fashion. The chief one, I think, is that the finished product benefits enormously from the knowledge and talents of all the participants, each of whom has a tangible stake in the outcome that comes from being a full partner in authorship.

On the Freedmen and Southern Society Project, we recognize no distinction between a senior editor or assistant editors or editorial assistants. We find that our collaborative work is less isolating than individual scholarly work which is the norm in the humanities. And, at the same time, because the work is not hierarchically organized in a tributary fashion—with a junior sending material up to the superiors—it is less alienating; morale benefits from this organization of work. We learn from our colleagues in a fashion that, at least for me, is far more fulfilling than the usual kind of interchange

that happens between colleagues in which you write hasty comments on somebody's latest paper or have short lunchtime conversations about commonalities between your work and that of somebody else.

Not that collaboration establishes a utopian academic workplace; our enterprise is still subject to the usual human failings. But things are structured in ways that seem to render those failings somewhat less debilitating to the integrity of the whole enterprise. At its best, disagreements among colleagues which inevitably arise can be dealt with in a productive way rather than an internally disruptive fashion. The disadvantages of our collaborative work seem to stem less from the final product and more from the structure of the historical profession and its assumption about what constitutes acceptable scholarship.

Part of this, I think, goes back to the question of attribution: the question of who did what. That question is often unanswerable. A mere totaling up of pages in a volume, principally authored by each editor, without considering the work as a whole, does not accurately measure the contribution of each.

Another problem—and this is something we've been noticing more the last few years—is the difficulty of finding people who are able to come on to the project as coequal editors. That is, who are knowledgeable about the subject, who are interpretively at least roughly in sync with the editors on the staff already. It has also been difficult to find talented and ambitious people who are willing to work collaboratively in a profession that tends to dispense rewards and awards chiefly on the basis of individual achievement.

---

*At the close of the session, Michael M. J. Fischer (Professor in Anthropology and Director of the Center for Cultural Studies, Rice University) and Mehdi Abedi (Lecturer in Anthropology, History and Religious Studies, Rice University) offered their reflections on collaboration in a field that draws from both the humanities and the social sciences. The following excerpts are from the transcript during which they discussed their coauthored book,* Debating Muslims: Cultural Dialogues in Postmodernity and Tradition *(1990), and from additional material supplied for use in this issue.* —*M.C.L.*

***Michael M. J. Fischer:*** I believe in and argue for, both in my own work and in my administrative capacities, genuine collaborative research and writing projects in the humanities and the social sciences on two grounds. First, knowledge itself is always a cumulative collaborative project. And second, certain kinds of research are more securely pursued through collaboration, especially those that involve cultural analysis, cross-cultural linguistically

grounded research, and comparative social science that is interested in triangulating the multiple perspectives that come from different social or cultural positionings. Because anthropology has deep roots both in the humanities and the social sciences, it is perhaps well located to illustrate both the utilities and the necessities for collaborative work.

Anthropology historically has been involved in two types of collaborative projects: individual collaborations and team research. The individual research of anthropological fieldwork is often aided by collaboration with an insider to another culture, either an elder or a specialist with a lifetime of experience and knowledge, or a youth or marginal person who is willing to cross conventional boundaries of reticence. The single anthropologist in collaboration with a cultural insider is perhaps the best known image of anthropological work. But anthropology has at the same time also been formed in the crucibles of three kinds of team projects: students of a teacher with a vision, regional projects, and multidisciplinary projects.

The first generations of modern empirical, fieldwork-based, research anthropology were created in seminars around teachers with a vision. The seminars of Bronislaw Malinowski at the London School of Economics, of A. R. Radcliffe-Brown at Chicago, Sydney, Cape Town, and of Franz Boas at Columbia are places where the new field was created as a set of coordinated research questions, where theory was modified in relation to closely examined research data. The Malinowski seminar, for instance, which was always opened by Malinowski's demand for a clear "Problemstellung!" worked on the principles of social organization (e.g., does matrilineal social organization correlate with high divorce rates, does ritual serve as a charter for social action, and so on), questions which could not be answered by information from a single society.

In addition to these university-based seminars, important parts of anthropological knowledge have been created through regionally coordinated research projects. Max Gluckman's "Seven Year Research Project" (1940) for the Rhodes-Livingstone Institute in Northern Rhodesia is archetypical. It was an effort to create a mosaic of detailed studies of different tribes and communities in the "bush" which also supplied labor to the mining centers of the copper belt; the studies identified a base for evaluating economic development, including the destruction of traditional economies and the creation of man-made regimes of famine, the development of legal systems including such impacts as the breakdown of traditional mutual help systems and the organization of industrial labor. The Rhodes-Livingstone Institute was among the first of a series of Institutes for Social and Economic Research established across the British Empire and Commonwealth. Other examples of regionally coordinated projects were pursued by Harvard University

researchers—the Chiapas Project (in Mexico), the Southwest Values Project (in New Mexico), the Bushman Project (in Botswana and Namibia), and the Childrearing Project in East Africa.

Third, and equally important, there were multidisciplinary research projects: the Comparison of Civilizations Seminar and research projects at the University of Chicago run by Robert Redfield and Milton Singer with Ford Foundation money, the MIT project on development in Indonesia (in which Clifford Geertz's early work was formed), and the New Nations Seminar at the University of Chicago in the 1960s. All these involved an interaction of psychologists, anthropologists, economists, political scientists, social historians, as well as literary and religious analysts. Not only did people from different disciplines contribute to these projects but their research agendas, explanations, and theories also required the integration of perspectives from many fields, for the work to proceed.

My own training and experience illustrates the same rhythm. My first fieldwork, done in Jamaica, was part of the University of Chicago and London University Comparative Family Study Project in West Africa, the Caribbean, and the United Kingdom (under the direction of Raymond T. Smith, David M. Schneider, and Raymond Firth). My first book came out of the Islam and Social Change Project (under the coordination of Leonard Binder and Fazlur Rahman) at the University of Chicago, which was a comparative study of the education of religious leaders in six countries: Iran, Indonesia, Egypt, Turkey, Morocco, and Pakistan. Each country was allocated two principal investigators, one a native of the country and one an outsider.

Currently at Rice University, the Center for Cultural Studies carries on similar comparative, cross-cultural and interdisciplinary work. The center grew out of a collaborative discussion group, the "Rice Circle," which we in the Anthropology Department had established with participants from a number of humanities fields. The name popped up one night as George Marcus and I sat around the dinner table with my parents, reminiscing about the Vienna Circle, on the periphery of which my mother had participated as a student; the name started as an affectionate joke, but it stuck. Although the Center for Cultural Studies grew from a local discussion group, it is also reflective of a national debate over the changing needs of the university curriculum, the maturing of a number of interdisciplinary arenas, and the emergence of new fields of knowledge such as film and media studies and the cultural studies of science. The center serves as a Rockefeller Foundation funded residency site for postdoctoral fellows, part of a program to energize study of the transnational dynamics of culture, to connect these studies to cultural diversity in America, and to restructure area studies programs. The

Rice Center for Cultural Studies pursues these issues through a number of workshop, seminar, and lecture formats (e.g., our two-year seminar on moral sensibilities in historical and cultural context).

The Rice Circle was one of the collaborative grounds out of which a series of books emerged, including *Anthropology as Cultural Critique* and *Writing Culture* (Clifford and Marcus 1986). The latter, edited by James Clifford and George Marcus, was created through a weeklong seminar at the School of American Research in which anthropologists, literary critics, and historians of anthropology interacted, critiquing each other's work. *Anthropology as Cultural Critique* (Marcus and Fischer 1986), which I wrote with George Marcus, is one of those books which neither one of us could have written alone at the time, where some differences of opinion still show, but in which, in a very real sense, every sentence in the book was written (and rewritten) by each of us. *Debating Muslims* (Fischer and Abedi 1990), written with Mehdi Abedi, is a second such sustained collaborative writing project.

*Debating Muslims* is an intervention in the discussions about social change in the Islamic world in the aftermath of the Iranian Revolution. . . . More generally, *Debating Muslims* is an intervention in much broader discussions within cultural studies and in a world in which writing increasingly involves multiple audiences, a world in which those written about read what is written about them and reply. There is a general methodological challenge and opportunity that this situation opens up, one that we point out as a key form of cultural critique in *Anthropology as Cultural Critique*. . . .

What I've tried to illustrate so far is collaboration in the three different *moments* of scholarship: research, discussion, and the written product. . . . Many of these tasks of cultural translation perhaps could be done by one superbly trained bicultural person, but attention to metaphor, parable, and the real ways people use interpretations of cultural forms requires a subtlety, an ear, and an eye, that are not easy to acquire and that are much easier and more surely explored through working with co-elaborators who can point out the experiences upon which interpretations take on particular kinds of salience in some conditions and not others. What one seeks, after all, is not a definitive interpretation, no matter how detailed, that will stand eternally, but rather access to the competing discourses that through their vying create the cultural arenas that we would like to describe and that we perforce must participate in whether we wish to or not.

Salman Rushdie's *Satanic Verses* (1989) illustrates just such a point. There is little if anything in that novel which Rushdie has invented out of whole cloth. You can use the final chapter of *Debating Muslims*, for example, to trace out all the Islamic sources on which Rushdie draws (and as a reading guide to the novel itself). For Westerners to accept at face value the Muslim

fundamentalists' claims that the book in its title or its portrayals of various stories is blasphemous is like what philosophers used to call a "category mistake." The fundamentalists have quite valid grievances, but they do not reside at this surface level, they are about such issues as poll taxes, schools, and labor force discrimination in England, electoral politics in India and Pakistan, and so on. The Rushdie book thus serves as a symbolic token in the policing of what *may* be said (and how) inside as opposed to outside the community or, in the case of Iran's participation, it is a token in a global drama for symbolic control over the setting of agendas by Islamic leaderships in Iran as opposed to those in Saudi Arabia, India, or elsewhere. Rushdie's position as an Oxbridge-educated member of the British elite, alienated from lower-middle-class immigrants—about whom he would like to be sympathetic but who see him as patronizing—has more to do with his troubles than what he actually wrote in the novel. That most Muslims who are offended by his novel have at most read certain sentences and paragraphs taken out of context and inserted into another context which make them appear blasphemous only illustrates the point.

The point then—the challenge of cultural analysis in the contemporary world and the need for collaboration across intricate cultural terrains—as we say in our pivotal chapter on the Qur'an, is a challenge in "three ethical registers: the ethnographic effort to understand other people(s) in their own terms, the political effort to establish a public world where the rights and interests of all can be protected or negotiated, and the self-evaluative or self-reflective effort to break out of ethnocentrism and to place one's own perspectives in historical and dialogical relation to others" (Fischer and Abedi 1990, 97).

There has been a great deal of attention in recent years to matters of dialogue in writing about others: on natives' reading what is written about them and writing back—for example, among creative writers (Ashcroft 1989), among historians of the "Subaltern Studies" group, among African philosophers, and among translators. Anthropologists have long had genres devoted to accounts about their primary informants or collaborators (e.g., Joseph Casagrande's edited volume, *In the Company of Men* [1960]) or about fieldwork, but the idea of having informants "write back" is quite a different project, since the pen and the voice issue with quite different perspective and authority.

***Mehdi Abedi:*** . . . In my case, the effect of this dialectical process of fieldwork and interaction with non-Iranian perspectives has been tremendous . . . it has managed to help transform me from a high school dropout and bookbinder in central Iran into a cultural anthropologist, and it has made my

stable life and worldview a turbulent one. How "sure" I was about "our right way of doing things" in those days! I naively thought I was helping the foreign scholars correct the "wrong ways" of their society. I thought we were morally and culturally superior, lagging behind merely in material progress. How "uncertain" I have been ever since. I began dreaming of breaking away from my hometown, wife, and child in search of knowledge! For this "exciting ride on the roller coaster," as I often call it, I have paid a high price. Now when I read the myth of Adam, his eating of the forbidden fruit, his subsequent deportation from the Garden, his descent onto the earth, and his wish for return, I understand very well that my mythical ancestor and I have had similar destinies! I am terribly aware, more aware than Adam, that "return" is a golden wish with which I must be buried, here or back home. No, I can never become the young and innocent native that I once upon a time was. . . .

[When Abedi first met Michael Fischer, who was doing fieldwork in Iran] it bothered me that he mixed with Zoroastrians and Jews as well. I perceived no danger in such encounters—for neither of these two minorities were interested in converting him; but the Baha'is were—or at least I thought they were. Yet, as a Muslim, I was so well trained in debating the Baha'is that I knew I could undo their spider web by exposing the nature of their religion. The difficulty with [Fischer] was that one could hardly ever know what was going on in his mind. He was a listener. To him, everything was always interesting rather than good or bad. "*Mardomshenasi mishe* (it becomes anthropology)," he frequently said, and it took me a long time to find out he was more interested in the complex social dialectics rather than the intended and desired outcome.

[Fischer], knowing about my anti-Baha'i zeal, which had been so badly exposed in debate, never discussed with me his relationship with the Baha'is. Instead, Dr. Paknežad and I were very glad that, when I told [Fischer] about the books written in "refutation of Baha'ism," he showed much interest. . . . Yes, I was naively sure; but Fischer had merely learned that he should keep me uninformed about his Baha'i friends. Now, it was not only we who practiced mental reservation (*taqiyya*) in regards to the kind of things we did not want him to know about Muslims, he was playing the same game too, at least in regards to dangerous minorities. He was very good at making each of his informants feel himself as the only source of authoritative and correct information.

Having translated so many cassette tapes of the lectures of the most important and persuasive preachers of Islam for Michael [Fischer], I honestly thought of him as a potential Muslim. For a long time, I hoped that, all of a sudden, he was going to convert to Islam. And, what an honor could his

conversion be for me, the only man who had given him a new life. Several times, I dreamed of his conversion. In one dream, [he was] just out of his ritual purity bath of conversion, water still dripping from his face, wearing a long white garment. He and I were being carried on the shoulders of the public into the grand mosque. I was thinking of a public statement: "I did nothing, may it please Allah!" And there was already a dispute among several men, each of whom wanted his daughter to be married to [Fischer]. I remember very well that, the day after this dream, I asked Michael which religion in the whole world he preferred. Rather than giving me a straight answer, he played with my mind for several hours. I pressed him harder and harder, hoping to squeeze the word "Islam" out of his mouth. Finally, he disgusted me by telling me how wonderful "Buddhism" was, a religion about which I virtually knew nothing. Nonetheless, I was "sure" he was pulling my leg. How could someone who knows so much about Islam choose as obscure a religion as Buddhism? . . .

The last meeting I had with Fischer in the first fieldwork was on the occasion of the birth of Reza, my son. "He is beautiful," he said when I handed him my son. "No," I said, "you are not supposed to admire him even though he is beautiful." "Yes," Michael said, "you are afraid of the evil eye, which I do not have. I did not know whether to be American or Iranian." "I hope," I said somewhat jokingly, "you have not forgotten to bring *chashm rowshani*." He had not forgotten. In fact, like an Iranian, he had put it on the table rather than handing it to us like an American. It was a cloth, which, we could tell, he had just bought, and his rechargeable Sony tape recorder, which he knew I would love to have, was wrapped inside. "A Sunni recorder for a Shi'ite friend," I said. "Yes," he replied, "Sunni Japanese recorder for a Shi'ite Iranian man."

. . . In Qumm, during my second fieldwork with Fischer, we cooperated in finding all sorts of information from all sorts of people: genealogies of the clergy, class materials of the theological seminaries, classified information about corruption in public funds, myth and reality of temporary marriages, the relations between theologians and the masses, political unrest, and what have you. . . .

. . . Michael [Fischer] and I were on our way back from Tehran in the Land Rover of the American Institute of Iranian Studies. Rizvi, a senior Pakistani student of Ayatullah Shariatmadari, joined us. We decided, as the expression goes, "to throw a ladder in the way," that is, to make the trip seem shorter by having a conversation. *Al-Hiyal al-shar'iyah* (religious deceits) seemed a good subject, which I started. The debate heated up between Rizvi, a molla concerned with the flexibility and interpretability of the law, and me, who argued that religious tricks were sophist games for the purpose of

cheating God, which is not possible. Michael, driving with the ease of a patient driver more interested in the trip than in its destination, was a good audience, who encouraged both sides of the argument by throwing a word hither and thither. The debate boiled down into practicality (Rizvi's side) versus the shame that a Muslim feels when he claims to be a follower of the straight path on the one hand and has to admit that there is room for cheating in religion on the other hand. While admiring my faith, which I did not have, Rizvi tried to convince me that we were living in a real world in which the forces of evil are predominant. We Muslims have no choice because we did not create an economy of usury, unlawful salaries, and questionable ways of earning a living. "Then," I replied, "you are arguing that Islam is no longer a practical religion." "This," he said, "is what you have been arguing all along." We all burst into laughter. "On the contrary," Rizvi said, "I am arguing that it is practical because it is flexible. One must be smart enough to interpret it."

Later on in Qumm, Rizvi told me that he had discussed our debate with Ayatullah Shariamadari. "His Holiness," he said, "wants to see you. He thinks that in six months or so, you can be a *mujtahid* [expert whose independent opinion may be followed by laymen] who could propagate the cause of Islam in the West" . . .

Now I am an anthropologist. So often I dream of going back to Iran for fieldwork. But can I go back? Having helped American scholars, I am suspect. I might be arrested as an American spy. I am currently with no homeland. This is what fieldwork did to me.

***Michael M. J. Fischer and Mehdi Abedi:*** *"In hameh avazha az shah bovad/Garcheh az halqum-e Abdullah bovad* [All the voices are from the king/Even though it comes from Abdullah's throat]." In these famous lines on the nature of collaboration, the great mystical poet Rumi modestly refers to himself as Abdullah (a mere "slave of God," an Everyman), and his teacher and inspirer, Shams ud-Din ("Sun of Religion"), as king ("shah"). The lines put into play the dialectic or oscillating interdependencies between the spoken and the written word, between the one who dictates and the scribe, the one who inspires and the one who formulates. To make the point even clearer, Rumi not only places himself in this ambivalent relation to Shams but also to his own disciple and scribe, Hesamuddin ("Sword of Religion"). The king, of course, in an ultimate sense is the divine, and Abdullah is the human. The lines also serve as an epigram for the sort of collaboration that our book *Debating Muslims* represents, a title which also invokes three registers of collaboration and debate: between the two of us, between the "First World" and the worlds of Islam, and among Muslims holding different opinions.

## *References*

Ashcroft, B. 1989. *The empire writes back: Theory and practice in post-colonial literature.* New York: Routledge.

Bellah, R. N., R. Madsden, W. M. Sullivan, A. Swidler, and S. M. Tipton. 1985. *Habits of the heart.* Berkeley: University of California Press.

Bellah, R. N., R. Madsden, W. M. Sullivan, A. Swidler, and S. M. Tipton. 1991. *The good society.* New York: Alfred A. Knopf.

Casagrande, J. B. 1960. *In the company of men: Twenty portraits by anthropologists.* New York: Harper.

Clifford, J., and G. Marcus. 1986. *Writing culture: The poetics and politics of ethnography.* Berkeley: University of California Press.

Daniel, P. 1987. *Official images: New Deal photography.* Washington: Smithsonian Institution Press.

Fischer, M.M.J., and M. Abedi. 1990. *Debating Muslims: Cultural dialogues in postmodernity and tradition.* Madison: University of Wisconsin Press.

Hamilton, D. 1991. Research papers: Who's uncited now? *Science* 251:25.

Marcus, G., and M.M.J. Fischer. 1986. *Anthropology as a cultural critique: An experimental moment in the human sciences.* Chicago: University of Chicago Press.

Rushdie, S. 1989. *The satanic verses.* New York: Viking.

*CARLA M. BORDEN convened these seminars while she was a program specialist in the Office of Interdisciplinary Studies, Smithsonian Institution. She is now at the Smithsonian's Center for Folklife Programs and Cultural Studies. Borden has edited two books,* Contemporary Indian Tradition: Voices on Culture, Nature, and the Challenge of Change *(1989) and, with J. Jackman,* The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930-1945 *(1983). She holds degrees in sociology from Barnard College and Columbia University.*