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Blurred boundaries of science and advocacy: the discourse of scientists at a conservation organization

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Blurred boundaries of science and advocacy:
The discourse of scientists at a conservation organization

by

Neil Lindeman

A dissertation submitted to the graduate faculty
in partial fulfillment of the requirements for the degree of

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ABSTRACT

In this dissertation I argue that, in the field of conservation, the boundary separating science from advocacy appears to be undergoing a shift as the number of research scientists at conservation advocacy organizations grows. Drawing on data from interviews with scientists at a prominent conservation non-governmental organization (NGO), I identify and analyze the kinds of rhetorical work NGO scientists engage in as they attempt to participate effectively in the forums of both science and advocacy. I also analyze the publications of one scientist at the same organization to identify features of the discourse of NGO conservation science that suggest a shift—or at least a blurring—of the boundary between science and advocacy in conservation. My discourse analysis focuses on publications from forums of scholarship and advocacy including, as a representation of discourse in the latter forum, an example of gray literature. Gray literature refers to reports, books, and other texts produced and distributed outside the channels of the academic and publishing industry. The study highlights the types of “boundary work” NGO scientists are engaged in to establish their membership in the scientific community as well as specific features typical of their rhetoric that result from their occupying a “hybridized” cultural and professional space where science and advocacy overlap.
CHAPTER 1

INTRODUCTION AND REVIEW OF LITERATURE

In this dissertation, in which I document and analyze recent shifts in the discourse of conservation science, my analysis starts from the perspective that science is best defined not as an essentially distinctive intellectual activity, but as a cultural “space” delineated by “boundaries” that scientists and other actors continually redefine in the course of everyday activity, a process Gieryn has labeled “boundary work” (“Boundary-work,” “Boundaries of Science”). Gieryn, a sociologist, is interested in the concept of boundary work as it relates to a sociology of professions—in this case, the profession of science. Taylor, however, reminds us that boundary work, a fundamentally rhetorical practice, is also relevant to inquiry in the rhetoric of science (Defining Science). In subsequent chapters I argue that the boundary separating the cultural “spaces” of conservation science and advocacy is becoming increasingly blurred. My study is framed by the rhetorical concept of forum, which I see as a valuable resource for studying the discourse of bounded cultural spaces because forums are both concrete and dynamic. My study focuses on how the growth of science in conservation non-governmental organizations (NGOs) is changing the forums of conservation science.

In the sections that follow, I provide a fuller account of the various concepts and topics listed above. I begin with a brief overview of contrasts between essentialist and constructivist definitions of science in order to introduce the concept of boundary work. I then summarize perspectives on boundary work from both sociology and rhetoric. My focus then shifts to the concept of forum, especially as it relates to developments in rhetorical theories of audience and discourse community. This section also includes a brief discussion of forums in science. Finally, I summarize previous work on the growth of NGOs (especially
conservation NGOs) and the growth of “gray literature” as a venue for scientific and technical discourse in forums of conservation advocacy. I finish with an overview of the remaining chapters of my dissertation.

**The “Demarcation” Problem in Science**

Within the field of science studies, theorists have long struggled with what is commonly referred to as the “demarcation problem,” i.e., the problem of identifying the “unique and essential characteristics of science that distinguish it from other kinds of intellectual activities” (Gieryn, “Boundary-work” 781). Indeed, as Taylor observes, “the intellectual horizon is littered with attempts to come to grips with the constitutive character of science” (Defining Science 4). As a way of introducing and providing some context for the concept of boundary work in science, the following is a short overview of this “littered intellectual horizon,” focusing specifically on the three disciplines of philosophy, sociology, and rhetoric.

**Philosophical demarcations**

Some philosophers of science have taken an essentialist approach to the “demarcation problem,” attempting to identify those qualities that make science absolutely distinct from other intellectual activities. For instance, early 20th-century “verificationists” argued that science is unique because it produces knowledge claims that are verifiable by empirical evidence. Karl Popper objected to this fundamentally inductionist demarcation of science, arguing that science produces reliable predictions in addition to empirically verifiable claims. Popper thus argued that “falsification” rather than verification is the definitive quality of scientific knowledge claims. As Gieryn explains, from a falsificationist perspective, science
advances toward truth not inductively, through the accumulation of corroborative empirical evidence, but through a process of “bold conjecture and severe criticism” (“Boundaries of Science” 395). A scientific statement is one that is falsifiable in the sense that some empirical observation could, in theory, contradict or refute it. According to Popper, science “is not a confirmation game (looking for evidence to corroborate a generalization) but a refutation game (looking for evidence to shoot it down)” (Gieryn, “Boundaries of Science” 395).

In any event, verificationist and falsificationist demarcations both posit essential features of science. Another notable essentialist demarcation comes from Imre Lakatos. Concerned with demarcating science from “a curiosity shop where funny local—or cosmic—oddities are collected and displayed” (102), Lakatos offers the “methodology of research programmes [sic],” arguing, as Taylor explains, that a “scientific research program” requires several components: (1) a “hard core” of immutable hypotheses; (2) a “protective belt” of auxiliary hypotheses; (3) a “negative heuristic,” meaning unquestionable assumptions that form the basis for the hard core; and (4) a “positive heuristic,” or the conditions under which the program can be changed (Defining Science 31-32). It is noteworthy that Lakatos’s demarcation, unlike Popper or the verificationists, accounts to some degree for the social dimensions of science.

Thomas Kuhn was one of the first philosophers to analyze science in practical rather than methodological or logical terms, thus taking a descriptive rather than prescriptive approach to the demarcation problem. For Kuhn, the defining feature of science is “paradigmatic consensus,” meaning consensus among scientists in a particular discipline concerning the “background assumptions about the way the natural world works . . . coupled with methodological and theoretical exemplars or models that translate those deep
assumptions into working rules to guide the selection of problems and acceptable procedures
for their solution” (Gieryn, “Boundaries of Science” 402). Paradigmatic consensus marks
periods of “normal science,” where a field agrees on the puzzles to be solved, “the perimeter
frame, the cut-out pieces, and the spaces to be filled in” (Gieryn, “Boundaries of Science”
402). Periodically, scientific fields undergo a paradigmatic “revolution” as a particular field
adopts a new paradigm that better accounts for the anomalies that did not fit easily into the
old paradigm. Importantly, Kuhn emphasizes that paradigmatic revolutions are not
progressive—because new paradigms are products of their social, political, and cultural
context, they are not necessarily more “true” or “complete” than the paradigms they replace.

By basing his demarcation of science on observations of how science is practiced,
Kuhn takes the demarcation problem in a radically new direction. Nevertheless, Kuhn is
similar to his essentialist predecessors in that he presents paradigmatic consensus as a
necessary feature of science. As Gieryn notes, Kuhn’s view is that “once a paradigm is in
place, for a researcher to abandon its worldview without hopping to an alternative puzzle is
tantamount to leaving science” (“Boundaries of Science” 403). Or, in Kuhn’s words, “to
reject one paradigm without simultaneously substituting another is to reject science itself”
(79). Additionally, as Taylor reminds us, Kuhn does not embrace the notion that science is
“constructed” through paradigmatic revolutions. For Kuhn, “dominant paradigms may come
and go, [but] what they are paradigmatic of (science) retains an essential continuity”
(Defining Science 45).

Some critiques of Kuhn have thus focused on his failure to consider the constructed
nature of paradigmatic consensus, how it is “a matter of interpretation, negotiation, and
settlement—by scientists and sometimes other involved parties” (Gieryn, “Boundaries of
Science” 403). As Gieryn explains, from a constructivist rather than essentialist perspective, “consensus is a contextually contingent product of scientists’ variable interpretative procedures, which means that, for Kuhn to conclude analytically that consensus exists in a research community at a designated time, he must ignore potentially wide discrepancies in scientists’ own sense of the degree and kind of consensus they supposedly share” (“Boundaries of Science” 404).

**Sociological demarcations**

The previous section, while obviously only a small sample of philosophical definitions of science, illustrates the essentialism that has characterized many philosophical responses to the demarcation problem. But essentialist demarcations are not confined to philosophy: essentialism also characterizes Robert Merton’s pioneering sociological demarcations of science. Following Merton, however, some sociologists of science have rejected essentialist demarcations in favor of a constructivist perspective that highlights what Gieryn has labeled “boundary work.”

Merton’s 1973 book *The Sociology of Science* provided the sociology of science with “its first major paradigm” (Collins and Restivo 193). The most influential section of the book deals with the normative structure of the scientific community—what Merton refers to as the “ethos of science.” In particular, Merton claims that the modern institution of science is characterized by the following four social norms: (1) *universalism*, meaning that scientists judge knowledge claims based on “pre-established impersonal criteria” rather than the claimant’s personal or social attributes (race, class, political views, etc.); (2) *disinterestedness*, meaning that scientists’ commitment to the “higher cause” of science—
extending certified knowledge—supersedes their own self-interested motives; (3) *organized skepticism*, meaning that “the scientific investigator does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed”; and (4) *intellectual communism*, meaning that “the substantive findings of science are a product of social collaboration and are assigned to the community” rather than the individual (270-278). As Gieryn explains, Merton sees these norms as being “communicated and internalized during the socialization of scientists, and . . . reinforced by sanctions levied against transgressors and by rewards heaped on successful conformists” (“Boundaries of Science” 398).

Some sociologists of science have rejected Merton’s presentation of the norms of science as apparent absolutes, arguing instead that the seemingly essential qualities of science, be they methodological, logical, or normative, are subject to constant reinterpretation and revision. Cicourel, for example, argues that scientists in everyday practice continually reinterpret Merton’s norms—and sometimes deem them irrelevant to the context at hand. Mulkay echoes this claim, stating that “we should not assume that any norm can have a single literal meaning independent of the contexts in which it is applied” (112).

But sociologists who deconstruct essentialist demarcations of science are left with a conundrum: if nothing is essentially distinctive about science—if nothing categorically separates it from other types of intellectual and social activity—then what accounts for its unquestionable cognitive authority? Gieryn and others have attempted to address this problem by studying science not as it is represented in the lab practices of scientists or their accounts of nature in academic journals, but rather by studying episodes in society where the question “what is science?” is answered in concrete terms for particular purposes and to
particular audiences. Efforts to demarcate science in the “real world” have come to be called “boundary work.” Gieryn, who is most often associated with the term, defines boundary work as “the attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activity as non-science” (“Boundary-work” 782). Put simply, boundary work is the work done to either revise or reify the line separating science from non-science. As a concept, boundary work rests on the assumption that the distinctive features of science are not unique or absolute. Rather, “the separation of science from other knowledge-producing activities is [viewed as] . . . a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes” (Gieryn, “Boundary-work” 393).

Gieryn presents the concept of boundary work as an extension of theories aimed at explaining the dynamics of ideologies, which he divides into two main categories: strain theories and interest theories. Strain theories posit that ideologies “provide ‘evaluative integration’ in the face of conflicting demands, competing expectations and inevitable ambivalences of social life. They are symptoms—as well as symbolic resolutions—of role strain, contradiction, and disequilibrium” (“Boundary-work” 782). Interest theories posit that ideologies are “social levers’ or ‘weapons’ used by groups to further their political or economic interests amidst universal struggles for power and advantage. They are manipulations of ideas to persuade people to think and act in ways benefiting the ideologist” (“Boundary-work” 782). In contrast to these two general types of theories, Gieryn sees boundary work as a more encompassing explanation of the dynamics of ideologies because,
as he explains, it shifts the focus to stylistic variations in the rhetoric of ideological statements, and thus makes it easier to articulate the interaction of strains and interests.

Gieryn is mainly interested in boundary work as it applies to science. A principal theme of his work is “credibility contests” in science, which he defines as situations within which “ideologists” of science are likely to view boundary work as a stylistic resource for constructing statements of professional ideology. Gieryn identifies three general types of credibility contests: expulsion, expansion, and protection of autonomy. An “expulsion” credibility contest occurs when scientists in a particular field attempt to monopolize their professional authority or resources by excluding rivals. In these instances, the accompanying rhetoric of boundary work often relies on labels like pseudo, deviant, and amateur to define professional rivals as outsiders. An “expansion” credibility contest occurs when scientists compete with a rival authority, such as “religion, politics, ethics, or common sense” for “jurisdictional control over a contested ontological domain.” Boundary work in these instances tends to present science as a more reliable, truthful, or relevant source of knowledge about natural reality than its rival authority. “Protection of autonomy” credibility contests occur when “outside powers” attempt not to “dislodge science from it place of epistemic authority” but to “exploit that authority in ways that compromise the material and symbolic resources of scientists.” When legislators or corporations attempt to make science subordinate to political or market interests, for instance, scientists often use boundary work to put up “interpretive walls to protect their professional autonomy over the selection of problems for research or standards used to judge candidate claims to knowledge.”
Other work in sociology and critical theory could also be classified under the general rubric of boundary work studies. In his book *The System of Professions*, for instance, Abbott examines the boundary disputes that can occur when competing professional groups attempt to claim authority for three types of tasks: diagnosis, inferential interpretation, and treatment of problems. These professional boundary disputes can take place in the courts, in the arena of public opinion and media representation, or in the world of actual professional practice. Abbott contends that the groups who prevail in such disputes tend to have a well-organized national association and a knowledge domain that is not overly formalized or codified, which could lead to a shift of work down to “para” professionals. Moreover, their rhetoric tends to rely on one or more of the following strategies: (1) reduction, or showing that a certain task is the same as other tasks within the profession’s domain; (2) metaphor, or showing that a certain task is similar to other tasks within the profession’s domain; and (3) gradient, or arguing that control of severe instances of a problem should justify control over milder instances of the problem (98-102). Abbott also argues that professional boundary disputes can result in professions assuming full, zero, or divided jurisdiction over a particular task. In the case of the latter, jurisdiction can be divided through subordination (e.g., doctors delegating certain tasks to nurses), division of labor (e.g., engineers and architects splitting tasks as interdependent professionals), intellectual control (e.g., division between theoretical and applied knowledge), advisory control (e.g., priests working in a hospital ward, but only as advisors), and clientele differentiation (i.e., once-competing professions continue to do the same tasks, but for different clients and markets, often stratified by class and often under different labels) (69-79).
The “science-as-social-world” perspective within sociology is also relevant to boundary work studies. As Gieryn notes, Hughes first introduced the question “how does work get done?” to sociology, which in turn led to the study of science as a “social world,” or in other words as a site where a “diverse and often unexpected set of people” come together to accomplish the work of science (Gieryn, “Boundaries of Science” 412). Hughes was among the first to show that a central aspect of doing work is constructing a meaningful representation of the work itself. This, in turn, requires locating the work within a broader cultural space, a task that involves boundary work. In particular, the science-as-social-world perspective has contributed to boundary work inquiry by focusing, as Gieryn notes (“Boundaries of Science”), on the diverse and unexpected set of people connected with scientific work, and thus problematizing conventional assumptions about who is and is not considered a scientist. A central “social world” question is who does science? Or, in other words, who belongs to the social world of science? Scientists obviously belong, but so, arguably, do museum patrons, curators, janitors, and various sorts of administrators and managers. The social world approach is to create as comprehensive a list as possible of people who have commitments to the achievements of the science world, and then ask the question: why are some individuals on this list not conventionally defined as doing science? What boundary work is being done to define them as peripheral?

Boundary work inquiry is also relevant to the relationship between politics and science, two professional worlds that have historically tried to locate themselves as close as possible to each other while maintaining a clear delineation. Science needs to stay close to politics in order to maintain its cultural authority, which rests to a large degree on how relevant science is to political decision making. As Gieryn notes, “too great a distance
between science and politics threatens a critically important route for scientists’ legitimation via their perceived political utility—and in particular their claim on government funding for their research” (“Boundaries of Science” 435). Yet science must, through boundary work, keep itself distinct from politics; otherwise it risks damaging the objectivity upon which it relies for cultural authority. By the same token, politicians are better able to justify their policies if they are grounded in science, yet they are motivated to keep themselves separate from science lest political decisions become exclusively technical and thus beyond their control (Gieryn, “Boundaries of Science”).

Another important contribution to boundary work inquiry is the feminist critique of science as a gendered space constructed in ways that exclude or marginalize women. As Gieryn observes, feminist theorists have argued that the boundaries of knowledge and gender have coevolved as “centuries of double-boundary work have moved whatever counts as science toward the masculine, and whatever counts as feminine away from science” (“Boundaries of Science” 420). Moreover, by deconstructing gendered boundaries of science, feminism is itself an excellent example of boundary work in practice, “a project seeking emancipation in part through reconfigurations of science and politics, culture and nature, object and subject, male and female” (Gieryn, “Boundaries of Science” 424).

**Rhetorical demarcations of science**

At its core, the concept of boundary work represents a rejection of essentialist demarcations of science. A similarly constructivist perspective can be found in contemporary rhetorical theories of scientific discourse, which have generally rejected what Blyler and Thralls refer to as the “windowpane” theory of language commonly associated with logical
positivism—the theory that language and meaning are separate and that language, properly used, can provide readers or listeners with a clear view of the author or speaker’s meaning. Rather, many discourse theorists argue that meaning can never exist apart from language—meaning is inevitably mediated and determined by the language used to express it. With regard to the language of science, Bazerman summarizes the constructivist view nicely: “Scientific formulations are human constructions and thus heir to all the limitations of humanity . . . . [Scientific language thus] seem[s] to do all the social work of being human with no overt means of doing the empirical work which has been considered the work of science” (Shaping 294-5).

Bazerman lists several reasons to “distrust” scientific language, or, in other words, to reject the windowpane theory of scientific language in favor of a constructivist perspective. First, scientific language, like all language, is a semiotic system, and thus incorporates basic assumptions about the nature of reality that color not only how reality is represented in language but how it is perceived by rhetors (in this case, scientific authors). Scientific discourse is also inevitably laden with ideology and functions not just to communicate but also to exercise power by establishing and maintaining the authority of science as an institution. Moreover, because scientists tend to be fiercely competitive—both as individuals and as groups—for status, recognition, and resources, scientific language is often “partisan, argumentative, and manipulated for individual gain rather than an objective, dispassionate representation of things as they are” (Shaping. 294). In short, scientific language is rhetorical, and thus science cannot claim to be characterized by a uniquely objective language.

The question of what defines the discourse of science is implicit in much rhetoric of science literature, but among rhetoricians Taylor deals most overtly with the “demarcation
problem” as I’ve presented it in the previous sections. Claiming that Gieryn’s work does not adequately account for the “broader social contexts in which scientific practice (including demarcation) is conducted” (“Defining the Scientific” 406), Taylor focuses on the relationship between science and other rhetorics of the “public sphere.” He credits Overington as being one of the first rhetoricians to recognize that science is not a “discrete and asocial” endeavor but rather a communal enterprise. In other words, science is argument (Taylor, Defining Science). With this basic tenet established, Taylor continues, subsequent work in rhetoric has characterized the scientific discourse community as being either “internally contextualized” (Prelli) or “externally contextualized” (Campbell), meaning in the former case that scientific argumentation takes place within an internal context established by the prevailing authoritative community of science, and in the latter case that scientific discourse is “reciprocally related to . . . the larger social milieu in which it is embedded” (Taylor, Defining Science 108).

Taylor, like Campbell, prefers to treat science as “externally contextualized” and aligns his work with others who take a similar view. He cites Gross, for example, who, by examining Newton’s dispute with Leibniz over who deserved credit for inventing calculus, shows that the scientific norm of using dated journal publication to establish priority claims resulted from a rhetorical negotiation that was at least partly influenced by the larger historical and cultural context of the time. As Taylor explains, Gross’s study “suggest[s] that the construction of the social norm of priority was not an objective reflection of self-evidently appropriate scientific practice. It was rather quite clearly a matter of the rhetorical management of competing interests and perspectives within a particular historical context” (Defining Science 112).
Taylor also cites Farrell and Goodnight to show that many rhetoricians whose work focuses on the external context of science are concerned with “rescuing” the public sphere from an encroaching technical/scientific sphere of discourse. Farrell and Goodnight argue that during the events of Three Mile Island “technical reasoning” usurped the role of “social reasoning” and caused a rhetorical crisis due to the “deep-seated contradictions” between the goals and practices of technical and social discourse. The aim of technical discourse is prediction and control through the use of “non-reflexive procedures in order to solve puzzles integral to specialized codes,” while social discourse aims to solve situation-dependent problems and is characterized by self-criticism intended to “guide conduct toward a more perfect society” (97). Farrell and Goodnight conclude that, as in the case of Three Mile Island, when technical discourse displaces social discourse, the public’s ability to solve the problems at hand is severely constricted.

In contrast, Taylor claims that too often the public is depicted as being “at the intellectual mercy of . . . technical discourses,” and thus we ignore “the operative influence of public factors on technical debate” (Defining Science 129). Instead, he calls for a “reconceptualization of the basic analytic categories of social (public) and technical” because “practices typically thought to fall in the public sphere can exert directional influence on the corrosive technocracy that many rhetorical critics have decried” (“Science as Cultural” 75). He suggests that “what we have traditionally thought of as nonscientific rhetorics can, in many contexts, be read (and criticized) as among the important cultural practices of science” (“Science as Cultural” 76). One way to approach this project is by providing a rhetorical account of demarcation, which Taylor defines as the “search for the rhetorical processes in which [distinctions between different sets of social practices] are articulated and
legitimated.” Such an account would “illuminate and celebrate” the latent interconnectedness of the technical/scientific and public spheres of discourse (*Defining Science* 130).

**Science and the Concept of Forum**

Sociologists of science like Gieryn argue that science should be viewed as a constantly shifting cultural and professional “space” rather than an essentially distinctive intellectual activity. This space necessarily has boundaries separating it from other spaces on the cultural and professional map (e.g., politics, religion, engineering) but, importantly, these boundaries are not absolute. Rather, they are continually reiterated and revised through “boundary work” in response to challenges from any number of sources: groups may be competing for the legitimacy, authority, and resources that can result from successfully portraying themselves as scientific; or science may need to redefine itself in response to shifts in the larger cultural and political milieu. Taylor, in particular, emphasizes this latter point—that nonscientific rhetorics and spheres of activity inevitably overlap with science and influence how it is defined.

The forums of science, meaning the locations where the discourse of science is published or “made public,” are useful for the study of boundary work because, compared to the forums of many other professional communities, they have (at least in recent history) been well defined, with limits that are maintained through strict monitoring, or, one could say, active boundary work. In their book-length studies of communication within the sciences, Garvey and Meadows both highlight the efficient and organized nature of communication in science, noting that scientific forums “provide for a rapid, orderly dissemination of knowledge” (Meadows 55). Garvey’s description of scientific
communication as a “closed” system, isolated from outside influences and “maintained and used only by scientists to exchange scientific information” (29), illustrates how well-established scientific forums are as distinct spaces for specialized discourse. Frequent boundary work is inevitably required to maintain the distinct delineations of these forums. In this section, I summarize theories of discourse community and audience as a way of introducing a brief discussion of the concept of forum.

**Forums and discourse communities**

The rhetorical attributes, functions, and dynamics of forums become apparent when forums are viewed as key elements of discourse communities. The concept of the discourse community is rooted in the social constructionist view that a “writer’s language originates with the community to which he or she belongs” and that writers use language to either join new communities or solidify their membership in the communities to which they already belong (Bruffee 784). Porter argues that discourse communities are central to a social constructionist perspective of rhetoric, particularly the rhetorical concept of audience. As he explains

The social constructionist, or field view, conceives of audiences as a structure embodied in the sets of texts that define a given discourse community, as a discourse field or ground from which the writer’s text springs—a kind of “communal implied reader,” in other words. The field view grants the audience considerable power in the production of discourse; the audience is a “discourse community” constraining, defining, and in effect creating the writer. “The writer” is a role, a subject position, constituted by community constraints. (83)
Swales provides a carefully elaborated definition of discourse community that highlights how the term relates to the concept of forum. According to Swales, in order for a group to be defined as a discourse community, it must meet the following requirements: Its members must share common public goals, and, perhaps obviously, they must engage in discourse. As Swales observes, “the discourse community survives by providing information and feedback” (212). A discourse community also has its own evolving set of “discoursal expectations” which lead to “the genres that articulate the operations of the discourse community” (213). It also has “an inbuilt dynamic towards an increasingly shared and specialized terminology,” and it relies on a “critical mass of members with a suitable degree of relevant discoursal and content expertise” and a “reasonable ratio between experts and novices” that allows for the community to survive over time (213). Finally, and most relevant to this discussion, a discourse community requires a mechanism for communication among members, or, in other words, a forum.

Helpful as Swales’ definition is, it should nevertheless include the caveat that discontinuity, as Porter observes, is an “inescapable provenance” of discourse communities. Establishing an accurate demarcation of a particular discourse community is often problematic. Should it be defined in terms of academic discipline, institutional affiliation, social group, object of study, research methodology, frequency of communication, genre, or some combination of these or other distinctions? As Porter explains, discourse communities are like ecosystems in that an ecosystem is a convenient ecological space defined by certain characteristics that set it off from abutting systems. But shift your perspective slightly and the order of the original ecosystem breaks down, because ecosystems invariably interact with
systems abutting them. Discourse communities cannot be isolated from other
discourse communities any more than the writer can be isolated as an object of study
from his social field. In other words, we need to remember that discourse
communities overlap—and are flexible and locally constituted. They may cross
academic and institutional boundaries, and they may exist only momentarily. (86)

Rafoth echoes Porter’s view. While acknowledging that, in its most general sense, the term
discourse community refers to “an idea of language as a basis for sharing or holding in
common: shared expectation, shared participation, commonly (or communally) held ways of
expressing,” he is quick to add that we ought not to allow this emphasis on the
commonalities among members of discourse communities to obscure “the variety, conflict,
and anti-conventionalism that exists in most actual discourse communities” (140).

Given the conceptual “fuzziness” of discourse communities, Porter argues that the
notion of forum provides a “convenient and practical starting point for . . . inquires into
discourse communities” (95) because a forum represents “a local and concrete manifestation
of an entire network of discursive practices” (112). Berkenkotter holds a similar view, noting
that a forum “provides an enduring connection between rhetors and their audiences” and
“institutionalizes community norms through its conventions, thus both constraining writers
and providing them with resources for argument” (1990, p. 192). For this reason, forums are
a “rich source of information about the interactions between writers and their audiences in a
dynamic social context” (192), particularly in the case of academic or disciplinary discourse.

As concrete manifestations of a group’s discourse practices, forums—like the
discourse communities they reflect—exist in varying degrees of stability. As Porter observes,
they may or may not “provide well-defined speaking and writing roles for [their] members”
Similarly, forums may or may not manifest consensus on “assumptions about what objects are appropriate for examination and discussion, what operating functions are performed on those objects, what constitutes ‘evidence’ and ‘validity,’ and what formal conventions are followed” (107). A forum may also have a well-established ethos, or it may have “competing factions and indefinite boundaries” (107). In short, argues Porter, a forum can exist in what Kuhn would call a “pre-paradigmatic state,” with an “ill-defined regulating system and no clear boundaries,” or it can have “articulated and explicit standards and conventions” (107).

Scientific forums, though inevitably evolving, appear to have the attributes of well established forums, as Porter describes them. Indeed, rhetoricians often treat scientific forums as exemplars of what other emerging forums in scholarly disciplines are modeled on and striving to become. Bazerman illustrates the well established state of scientific forums with a comparison of journal articles from natural and political science. He observes that discourse in the natural sciences is based in a codified literature where “older texts have developed stabilized meanings and have been incorporated into the tacit assumptions of shared knowledge” (Shaping 283). As a consequence, natural science articles devote relatively little space to reinterpretations of prior work and generally give explicit mention only to recent literature. With a codified literature in place, new knowledge tends to take the form of solutions to recognized problems, reconciliations of previously identified anomalies, new accounts of previously identified phenomena, or extensions of previous work into new domains. Natural science forums are also marked by a methodological consensus that is noteworthy compared to other disciplines, where, as Bazerman shows, methodological innovations are more frequently emphasized.
Previous research on forums

Despite their potential as objects of rhetorical inquiry, forums have received explicit attention in only a few studies (e.g., Berkenkotter, Herrington, Porter). Implicitly, however, forums have been covered extensively in rhetorical literature, especially in research on the discourse of science. For instance, Bazerman (Shaping), in generating his history of the experimental article as a genre of scientific communication, provides a history of a key forum in which this genre evolved: the scientific journal *Philosophical Transactions of the Royal Society of London*. Illustrating Taylor’s point that the rhetoric of science is inevitably influenced by its external context, Bazerman traces the origin of *Philosophical Transactions* to the general improvement of postal services during the 17th century, which led to a more rapid diffusion of scientific knowledge via personal correspondence. Until this time, books were the principal outlet for scientific discourse, but these were a slow medium of communication, limited in distribution, that didn’t provide a forum for the kind of lively discourse—so central to science as it is currently practiced—associated with scientific journals. Rather, books tended to present “self-contained universes, accounts complete in themselves with little opportunity for response, except in the muffled comments of the unsatisfied reader” (Shaping 130).

A central figure in the rising correspondence among 17th century European scientists was Henry Oldenburg, who was named the first secretary of the newly formed Royal Society of London in 1662. Before receiving this position, Oldenburg had established regular correspondence with many scientists and eventually became a conduit for information exchange among scientists throughout Europe. As Bazerman (Shaping) notes, Oldenburg realized that the sharing of information—facilitated by his correspondence—encouraged
scientists to produce more information and share more of their work. Capitalizing on his role as a correspondent with the scientists of his time, Oldenburg started the first scientific journal, *Philosophical Transactions of the Royal Society of London*.

Initially *Philosophical Transactions* functioned as an extended newsletter where Oldenburg would simply summarize newsworthy scientific events, drawn both from his experience as secretary of the Royal Society and from his correspondence. However, as *Philosophical Transaction* became more broadly circulated, the relationship between Oldenburg and his readership changed. With those submitting letters that Oldenburg would draw on for the content of the journal becoming a smaller and smaller subclass of the readership as a whole, Oldenburg began giving contributors more of a voice. More and more lengthy passages from submitted letters were quoted, with less and less introduction from Oldenburg, until finally submissions were printed in their entirety, with no introduction from Oldenburg at all. Oldenburg thus moved beyond his initial role as correspondent and instead became the journal’s first full-fledged editor, and the journal itself came to resemble the kind of forum we now associate with scientific journals (Bazerman, *Shaping* 131-132).

The new forum of the scientific journal significantly influenced the practice of science. As the journal became the place to establish one’s priority claim to a particular scientific discovery, an ever-expanding communal body of knowledge began to emerge in science, and scientific activity came to include not just describing nature but also embedding knowledge claims within this body of documented knowledge. Subsequently, discursivity became a key feature of science. No longer was it enough to report one’s discovery to the rest of the scientific community, as if it were an item of news. Now knowledge advances took the form of claims that had to be argued for in the principal forums of science, scientific journals.
**Modern forums of science**

While Bazerman traces the origins of the modern scientific journal back to the 17th century, profound changes in science and, subsequently, scientific forums have occurred since World War II. One major change was simply the remarkable post-war growth of science. In the US, for instance, the proportion of scientists to the country’s total population more than doubled in the 30 years following the war. Consequently, money spent on research and development relative to the US gross national product increased dramatically, as did the sheer quantity of scientific publications (Garvey). In particular, commercial scientific journals became more and more prevalent, replacing society journals like the *Royal Transactions* as the principal forums for scientific discourse.

As the private sector began to take over scientific publishing, commercial factors began to exert much more influence on the shape of scientific discourse. As Feather observes, scientific publishing has become “an industry which has to function in the world of business where it is subject to all the financial, political and commercial pressures and constraints which are common to such enterprises” (60). One result of this shift is that subscription rates have inflated beyond the reach of most individual subscribers. As a consequence, research libraries—like the commercial scientific publishing industry itself—have become central to knowledge dissemination in science because they are now the main subscribers to scientific journals (Meadows).

Indeed, commercial publishers and libraries feature prominently in what is commonly referred to as the “information chain” (Figure 1.1), a model for describing the post-war system of knowledge dissemination in science. Within this model, each agent (researchers, publishers, libraries, and readers/users) has a distinct role: respectively, knowledge creation,
publishing, distribution, archiving and intermediation, and use. Importantly, the traditional information chain is a “print-centric” model for knowledge dissemination. As Owen observes, “In the era of print [the traditional information chain] has performed extremely well and has developed into a sophisticated co-operative system based on shared interests and mutual understanding of the various actors’ interests” (“The New Dissemination” 276). However, with shifts in scientific practice and the rise of electronic media, this “cooperative system” may be evolving into something new. In the following sections, I discuss these shifts.
**Figure 1.1.** Traditional “information chain” of knowledge dissemination in science

<table>
<thead>
<tr>
<th>Agent</th>
<th>Role</th>
<th>Function</th>
</tr>
</thead>
<tbody>
<tr>
<td>Researchers/research institutes</td>
<td>Knowledge creation</td>
<td>Data gathering, analysis, “write-up,” submission</td>
</tr>
<tr>
<td>Publishers</td>
<td>Publishing</td>
<td>Selection, certification, editing, printing, marketing</td>
</tr>
<tr>
<td>Subscription agents</td>
<td>Distribution</td>
<td>Marketing, delivering</td>
</tr>
<tr>
<td>Libraries</td>
<td>Archiving, intermediation</td>
<td>Cataloguing, storing, referencing, organizing</td>
</tr>
<tr>
<td>Users</td>
<td>Use</td>
<td>Acquiring, reading</td>
</tr>
</tbody>
</table>

Source: Owen, “The New Dissemination”

**Shifting Boundaries in Scientific Practice**

Modern scientific forums have been, and continue to be, dominated by the discourse of science carried out in universities and the private and governmental sectors. Yet the number of non-governmental organizations (NGOs) producing science, while still small compared to traditional institutions of science, is becoming more and more significant. As a trend, the growth of NGO science is part of an emerging emphasis on producing “socially accountable” knowledge in the context of application, rather than the traditional emphasis on building a body of disciplinary knowledge (Gibbons et al.). In this section, I provide some background information on the rise of NGOs and NGO science, followed by a brief discussion of this new approach to knowledge production.
The rise of NGOs

Salamon and Anheier suggest that the rise of nonprofit organizations may be the “greatest social innovation of the latter twentieth century,” an innovation similar in significance to the rise of representative government and public and private bureaucracy in the 18th and 19th centuries, respectively (1). As noted above, science as a social and professional institution grew dramatically in the latter 20th century, so it was perhaps inevitable that these two sectors of society would influence one another. Yet the rise of the nonprofit sector and its influence on science seems to have gone relatively unnoticed in science studies.

Salamon and Anheier, who have studied the nonprofit sector extensively as part of their work at the Johns Hopkins Comparative Nonprofit Sector Project, provide a detailed definition of the term nonprofit organization, also commonly referred to as non-governmental organization or NGO. Put succinctly, NGOs are organized, private, non-profit-distributing, self-governing, and voluntary. In other words, NGOs possess an “institutional reality” (1), are “institutionally separate from government” (1), do not return any profits generated to the organization’s owners or directors, are in control of their own activities, and are characterized by “some meaningful degree of voluntary participation, either in the agency’s activities or management” (2). Salaman and Anheier do not use the term NGO to refer to religious or political organizations (political in the sense that the organization is involved in promoting candidates for elective office), even though these may meet the definition listed above.

The scale of the global NGO sector is impressive. As Salamon and Anheier observe, NGOs are now “a major economic and social force, accounting for a significant share of
national employment and an even larger share of recent employment growth” (2-3). A few figures illustrate this point. As of 1990, a total of 11.9 million people were employed by NGOs in the US, UK, France, Germany, Italy, Sweden, Hungary, and Japan combined. Or, in other words, 1 in 20 jobs in these eight countries belonged to the NGO sector, as did 1 in 8 service-sector jobs. In the US, far more people work for NGOs than for General Motors. And this number does not include unpaid volunteers, millions of whom are also employed full-time by NGOs (Salamon and Anheier 3). NGO expenditures are also significant—Salamon and Anheier found that, in 1990, total NGO expenditures in the eight countries listed above equaled about five percent of the countries’ combined gross domestic product (4).

Within the conservation community, NGOs have been a significant force for quite some time. Indeed, Salamon lists the “global environmental crisis” as one of four main crises that have driven the dramatic growth of the NGO sector. (The others crises he lists are the failure of the modern welfare state to protect against old age and economic misfortune, the problems of economic development in under-developed countries, and the failure of socialism to satisfy social and economic needs.) Exact numbers are hard to come by because of the size and diversity of the environmental NGO sector, but a 1999 study estimates that in 1990 more than 100,000 NGOs worldwide were focused on environmental problems (Runyon). Thousands more have likely been established since 1990. While the vast majority of these organizations are small and relatively limited in their influence and scope of activity, many large and quite influential environmental NGOs have emerged. Within the US, their names are recognizable to many: The Nature Conservancy, World Wildlife Fund for Nature (WWF), The Sierra Club, the Natural Resources Defense Council, etc. To give some idea of the size of the most dominant environmental NGOs, the US division of WWF alone has more
than a million members and, according to its most recent annual report, annual operating revenues of $117.8 million in 2002 (WWF-US). In addition to its US office, WWF has 47 other offices worldwide, each with its own membership and operating budget.

As conservation NGOs—and, indeed, all NGOs—grow in number, size, and influence, they tend to be confronted with two challenges. The first is professionalization. Brown and Kalegaonkar observe that NGOs are often characterized by an amateurism that can limit their efficacy. The volunteers so fundamental to the vitality of NGOs often lack the technical competence needed to meet the organization’s program requirements. Sufficiently skilled technical professionals, moreover, can be difficult to attract to the NGO sector because of comparatively low compensation rates. The same lack of professionalism can affect how NGOs are managed, since NGOs are often founded by entrepreneurs and visionary individuals with little management experience to draw on as the NGO grows and scales up its operations. As Salamon notes, successful NGOs often eventually must confront tradeoffs associated with volunteerism and professionalization, between “the informality that gives [NGOs] their special character and the institutionalization necessary to translate individual victories into permanent achievements” (7). Another challenge is what Brown and Kalegaonkar refer to as “NGO particularism.” NGOs are generally unique in their core values and in the particular constituencies they serve, a characteristic that allows the NGO sector as a whole to respond to many different interests. At the same time, this particularism can be a weakness in that “NGOs [often] fail to respond to interests outside their narrowly defined constituency” (235).

Some conservation NGOs (and perhaps other NGOs as well) have addressed both of these challenges—professionalization and particularism—in part by turning to science.
NGOs increase their level of professionalism by adding scientists to their staffs, and, in turn, these scientists are able to produce research that helps the conservation community as a whole organize, prioritize, and coordinate the work of conservation. Indeed, conservation NGOs have now become a significant presence in the world of science. As some have observed, after a period in which academic institutions and government agencies have provided the bulk of scientific research on conservation topics, a growing number of national and international NGOs now employ scientists engaged in research on conservation problems (Fonseca). As a consequence, policymakers are increasingly turning to NGOs for environmental data and analysis (WRI).

The “new production” of knowledge

The rise of NGO science in conservation represents a larger trend in science that Gibbons et al. describe as the “new production” of knowledge. In simple terms, the “old” and “new” productions of knowledge—which Gibbons et al. refer to respectively as Mode 1 and Mode 2 knowledge production—are defined as follows: In Mode 1, knowledge production focuses on problems that “are set and solved in a context governed by the, largely academic, interests of a specific community” and is carried out in conformity with “the codes of practice relevant to a particular discipline” (3). Merton’s four norms of science (universalism, disinterestedness, organized skepticism, and intellectual communism) provide perhaps the best known account of the “codes of practice” that govern Mode 1 science. In contrast, Mode 2 science is “socially accountable and reflexive” (3) and is carried out in a context of application where knowledge is intended, above all, to be useful. Mode 2 science is rooted in growing social concern for issues like environmental degradation and public health, as well
as a growing awareness of the influence of science and technology, for better or worse, on society.

This fundamental social accountability influences the definition of research problems in Mode 2, resulting in problems that, in many cases, are best addressed by multi-disciplinary teams of researchers working in university as well as non-university settings, i.e., government agencies, consultancies, and, increasingly, NGOs. Yearley explains why many environmental problems are better suited to Mode 2 science, observing that “the customary ways in which scientific knowledge is produced [Mode 1 knowledge production] . . . render it less than ideally suitable for environmentalists’ uses, because most ecological problems are multidisciplinary while most research is not” (463). As an illustration, Yearley cites acid rain, a problem that must be addressed from the combined perspectives of atmospheric chemistry, meteorology, soil science, and ecology. A Mode 1 approach, where research problems are defined according to the interests of a single discipline, is not well suited to this type of research problem.

**Forums and the New Information Environment in Science**

As the boundaries of science shift to include the growing number of NGOs that are doing science, as well as a new, socially accountable and multi-disciplinary mode of knowledge production, the forums of science are bound to change as well. The main purpose of the traditional information chain has been to disseminate the results of research within disciplines that for the most part are institutionalized as part of universities. This forum is thus better suited to Mode 1 rather than Mode 2 knowledge production, where knowledge is disseminated differently. As Gibbons et al. explain, in Mode 2 the primary audience for new
knowledge is the knowledge creators themselves because the principal goal of knowledge production is not to add to a body of disciplinary literature but to solve “real world” problems. This knowledge is eventually disseminated more broadly not necessarily through publication in scientific journals or conference presentations but rather as its creators move on to new problem contexts and new (usually transdisciplinary) configurations of researchers. This is not to say that the knowledge is completely ephemeral because, as Gibbons et al. note, “even though problem contexts are transient, and problem solvers highly mobile, communications networks tend to persist and the knowledge contained in them is available to enter into further configurations” (4).

Owens (“The New Dissemination”) revisits the information chain model of dissemination in light of these and other shifts in how science is communicated. He claims that the information chain model, which relies on “physical information products” for the primary purpose of disseminating research results, is becoming obsolete for a number of reasons. Commercial publishers, who aim to derive a profit from distributing a “public good” (scientific knowledge), have become more responsible to their shareholders than to the knowledge-making community, as evidenced by the high costs of subscriptions and restrictive copyright practices. Partly in response to these constraints, and partly due to technological innovations in desktop and electronic publishing, knowledge creators are increasingly producing their own print and electronic information products. At the same time, various agents of the information chain (see Figure 1.1) are assuming mixed rather than separate roles. Knowledge creators are not the only ones assuming the publishing role—libraries are doing so as well by creating document repositories and e-journals. Similarly,
Publishers are taking on library functions like cataloguing, indexing, short-term archiving, and end-user services such as document delivery.

A more accurate way of describing how knowledge has come to be disseminated in science is what Owen calls “mixed-mode communication.” According to this mode, networks of scientists rely on “advanced technology” to create an “integrated information infrastructure which allows the sharing of source data, access to archival materials, and networked participation in or at least discussion of ongoing research activities, in addition to access to formal research results” (283). Mixed mode communication relies on a “digital library” rather than an “information chain” model, which Owen describes as follows:

The digital library model is very different from the traditional information chain. It makes no fundamental difference between author, publisher, and library functions, and it relates to a far greater richness of information formats and functionalities. In this model, traditional distinctions such as that between monographs and journals are losing importance: the digital library is based on information objects which could have any type or (often dynamic and distributed) formats. In addition, it is based on an integrated approach to the entire information cycle of information creation, distribution and use, and a new “human-centered” cyclical model of the scientists’ information behavior.” (283)

Owen argues that this new model of communication, in which knowledge creators are increasingly circumventing commercial publishers and instead are publishing and distributing knowledge themselves, represents a new information environment where “gray” is the primary distribution mode (“Expanding Horizon”). Gray refers to “gray literature,” a term not widely known outside the natural sciences and various fields of information studies.
Briefly, *gray literature* refers to information products that are not produced, marketed, or distributed by commercial publishing organizations. In Chapter 4 I examine some aspects of the relationship between gray literature and the forums of science, especially conservation science centered in NGOs because, as I note below, NGOs are prominent producers of gray literature. Here I provide some background on the term itself, since it appears to be almost completely absent from the current literature on the rhetoric of science.

The origins of gray literature can be traced to scientific and technical "report literature," a term that first became common in the 1940s (Auger). Examples of report literature date back to the early 1900s, but the term *report literature* was not coined until WWII-related research and development intensified, causing report literature to flourish. During the post-war era of government-sponsored research, government agencies were tasked with spreading non-classified knowledge and information to broader audiences in order to aid in technology transfer. To this end, the Publications Boards was created in 1945 to replace the US government’s Office of Scientific Research and Development (OSRD). The Publications Board published the first bibliography of government-sponsored technical reports in 1946. In 1970 it became the National Technical Information Service (NTIS), which publishes *Government Report Announcements* (Luzi).

Reports generally share a number of features. They nearly always have a sponsor of some kind who pays for the publication and has at least a degree of control over how the report is distributed (Meadows). Reports usually aren’t refereed, at least not in the formal way that is commonly associated with journal articles. They are also free of some of the generic constraints of journal articles. For example, unlike journal articles, reports may contain negative as well as positive experimental results. They also may contain vast
amounts of data that would be too lengthy to include in a typical journal article (Garvey). And reports allow for a freer rhetorical style that places fewer limits on speculation.

Gray literature is similar to but more encompassing than report literature. The term refers not just to reports but also to any type of literature not available through the normal channels of commercial publishing and distribution (Auger). While, as Auger notes, gray literature didn’t become a common term until the 1980s, Luzi observes that it was first given official recognition—among library and documentation scientists, at least—at the Seminar of York in the late 1970s. Gray literature shares many features with report literature, but it has several noteworthy differences. For instance, the bibliographic control of gray literature has historically been more haphazard than that of reports, which often have a code series or some other type of access number. Generic differences also exist: reports have relatively well established generic conventions (Rude), whereas gray literature can take many different generic forms. Finally, reports tend to be more easily available than gray literature. Several “announcement journals” containing details about reports (e.g., STAR or Scientific and Technical Aerospace Reports) are currently published, while gray literature tends not to be widely or systematically announced or publicized, although different organizations have made efforts lately to overcome this limitation (Auger).

As gray literature has become a more widely used term among scientists, librarians, and publishers, various attempts have been made to give it a concrete definition. These definitions have tended to focus on a number of distinctive features of gray literature. Luzi argues that when gray literature first emerged as a term, librarians and scientists saw acquisition as its defining feature—literature could be classified as “gray” if it were simply difficult to acquire. Much gray literature, however, has recently become easier to acquire as it
has found its way into databases and other cataloguing systems that were originally established solely for scientific and technical reports. With this development, the emphasis in defining gray literature shifted to focus less on acquisition and more on the dynamics of production (Luzi). Specifically, gray literature came to be defined as literature that is produced and controlled outside the commercial publishing industry. At the Third International Conference on Grey Literature, held in Luxembourg in 1997, participants agreed on the following definition:

Grey [sic] literature is that which is produced at all levels of government, academia, business and industries in print and electronic formats but which is not controlled by commercial publishers and where publishing is not the primary activity of the organization. (Farace qtd. in Aina 178)

While Auger acknowledges that gray literature is mainly defined by how it is produced and acquired, he notes several additional features that are frequently—though not absolutely—characteristic of gray literature. Much gray literature, for example, is produced outside the rigors of the refereeing system that is so central to conventional scientific publishing. Much of it is characterized by a nonprofessional layout and format and relatively low print runs. Finally, much gray literature is issued by what Auger calls “pressure groups,” who need to publish quickly and may have limited funds and “no scope for the niceties of sale or return and trade discounts” (2). NGOs can be counted among these “pressure groups” that produce gray literature.

Various scholars have speculated on why gray literature continues to grow as a category of publication in scientific and technical fields. Some have noted that it shares advantages with report literature, i.e., room for negative results and other details, more
freedom for speculation. Others have questioned the conventional wisdom that the quality of
gray literature is inferior to conventionally published science. Farace argues that

just because grey [sic] literature is not controlled by commercial publishers does not
mean that it has not undergone as severe a peer process as does commercially
published materials. A dissertation has an academic committee; a proceedings has a
program committee; a report has a project and editorial board; etc. Likewise, grey
literature goes through a process of indexing and abstracting, sometimes with tools
and standards developed especially for grey literature. (qtd. in Gelfand 74)

Auger notes that gray literature can be disseminated more quickly and with more flexibility
than conventional publications. Similarly, Luzi claims that, with the recent rise of the Internet
and the subsequent growth in electronic publishing, gray literature has developed into a
model of near-direct communication between literature producers and users.

The growth of science in conservation NGOs, part of the spread of what Gibbons et
al. have labeled Mode 2 knowledge production, has been accompanied by the emergence of
a “new information environment” in science, according to which knowledge-producing
organizations are developing and distributing more and more publications completely outside
the traditional channels of commercial and academic publishing. As I show in Chapter 4,
gray-literature publications represent an important forum for scientific discourse within
advocacy organizations like conservation NGOs.

Conclusion

My study examines recent shifts in the discourses of conservation science and
advocacy as a result of the growth of science in conservation NGOs. It is grounded in the
perspective that science is best defined as a cultural space with boundaries that are continually reified or shifted through the rhetorical process of boundary work. I focus specifically on discourse in the forums of science and advocacy in conservation to examine how the boundary separating these two discursive spaces may be blurring. The discourse I examine includes gray literature, which I consider an important object of analysis because it represents the evolution of knowledge dissemination from an “information chain” to what Owen calls a “mixed mode” or “gray” model whereby discourse takes place not just through commercially produced and distributed journals and books, but also through publications produced and distributed by researchers or knowledge-producing institutions themselves. The following is a brief forecast of the topics covered in the remaining chapters.

- **Chapter 2. Methods.** In this chapter, I provide a detailed description of my study design and methods. Briefly, I examined the work of seven scientists at a prominent conservation NGO, using interviews and discourse analysis to gather data. My study took place in two stages. During Stage 1, I interviewed each of the scientists to learn their perspectives on communicating science as members of an advocacy organization. During Stage 2, I carried out an in-depth analysis of the work of one of the scientists from Stage 1. I analyzed four of her publications—two from scholarly forums, two from advocacy forums—and then conducted a series of interviews with her to gather further information about the context of these four publications.

- **Chapter 3. Rhetoric and the New Production of Knowledge: Doing Science at a Conservation Advocacy Organization.** This chapter reports the results of Stage 1 of my study. I draw on my interviews with NGO scientists in
conservation to argue that these scientists are engaged in boundary work to define themselves as part of the world of science even as they take on various types of rhetorical work as participants in the forums of advocacy. I argue that the scientists I studied embody, in a number of ways, the “new knowledge production” introduced in Chapter 1.

- **Chapter 4. A Comparative Analysis of Discourse in Scholarly and Advocacy Forums for Conservation Science.** This chapter reports the results of Stage 2 of my study. I use data drawn from my discourse analysis and interviews to support the following points: (1) Discourse in advocacy forums is produced and disseminated according to a “gray” model whereby knowledge producers (scientists) often become involved in all stages of publication development, design, and distribution; (2) A key distinction between discourse in scholarly and advocacy forums is the extent to which advocacy scientific discourse is often modified, through internal pre-publication review and revision, to accommodate the political interests of sponsoring organizations; (3) The extent to which NGO scientists maintain—or choose not to maintain—a disinterested stance in their discourse is one distinction between scholarly and advocacy forums in conservation; (4) The limited use of the rhetoric of knowledge production across scholarly and advocacy forums may be evidence of a boundary shift; and (5) Another indication of a boundary shift is the author’s pervasive commitment to fulfilling the aims of advocacy, regardless of forum, as is reflected in her rhetorical choices.
• **Chapter 5. Conclusion.** Here I recap the central ideas and findings from the previous four chapters, identify some of the limitations of my study, and suggest possible directions for future research that would build on the work I present here.
CHAPTER 2
METHODS

In Chapters 3 and 4, I elaborate the central argument introduced in Chapter 1, namely, that the boundaries defining the cultural "space" of conservation science are shifting as the discourses of conservation advocacy become increasingly interconnected with the discourses of science. My support for this argument is drawn from interviews I conducted with several scientists working for an influential non-governmental conservation organization headquartered in Washington, DC, and my analysis of several publications authored by one of the organization's scientists. All of the scientists who participated in my study have been colleagues of mine at one time or another in the four years I have spent working full time as a technical editor for this organization. In the following sections, I describe the methodology of my study. I begin by describing the organization and the study participants and then continue with a description of my interviewing methods as well as my methods for selecting and analyzing a set of texts authored by one of the study participants. I conclude with a discussion of my "insider" status as a researcher.

Description of the Organization

My study focuses on scientists employed by a large, international conservation NGO (non-governmental organization) that I will refer to as "EarthConserve." EarthConserve is headquartered in Washington, DC, and has over 30 offices worldwide. EarthConserve’s DC staff currently number about 270, while its international staff is currently about 800. The organization was founded in 1987 with an original staff of 37 people, most of whom were previously employed at another dominant conservation NGO but had grown disillusioned
with that organization’s approach to conservation. They believed their former organization did not do enough to involve local people in creating and implementing conservation practices and policies. Thus, the original mission of EarthConserve was to follow an “ecosystem approach” to conservation, which meant recognizing the integral role local communities play in conservation efforts.

Approximately two years after its founding, EarthConserve adopted biodiversity conservation as its central focus. A conservation biologist was subsequently hired as EarthConserve’s president, and the organization’s board of directors eventually came to include prominent biologists. Under the leadership of its new president, EarthConserve began to direct its efforts and resources at protecting those regions of the world where restricted-range species are both highly concentrated and under extreme threat. Using quantitative measures of species ranges, species concentrations, and human impact on habitat, EarthConserve researchers eventually generated an ordered list of over two dozen regions worldwide that it considered top priorities for conservation. A few of the regions at the top of the list were Madagascar, the Philippines, Sundaland, Brazil’s Atlantic Forest, and the Caribbean.

EarthConserve’s use of quantifiable criteria to identify and prioritize areas for conservation illustrates the organization’s emphasis on science. Indeed, EarthConserve’s promotional literature makes frequent mention of the importance of “sound science” as the basis for conservation action. Early in its history, the organization introduced its “Rapid Assessment Program” (RAP) as a way of setting conservation priorities. RAP sponsors expeditions of scientists to conservation priority areas in order to conduct rough species inventories. These inventories yield useful data on areas where the level of biodiversity has
never been systematically measured. RAP expeditions are also an opportunity to train local field researchers in techniques for measuring biodiversity.

EarthConserve’s interest in conservation science culminated in the 1998 founding of a separate scientific research division within the organization, which I will refer to as the “Center for Conservation Science,” CCS, or the Center. CCS was created with an initial $35 million donation from a prominent Silicon Valley entrepreneur. The Center’s original mission was to serve as a “distant early warning center” for threats to biodiversity. At the time of the Center’s founding, EarthConserve staff and scientists were growing increasingly discouraged by the environmental community’s inability to spot threats before it was too late to do anything about them—CCS was created to address this problem. As one of the Center’s informational brochures states, CCS scientists work to “provide early and accurate diagnoses [of threats and] prescriptions for urgent actions.”

One of the Center’s principal strategies for developing scientific bases for conservation solutions is to sponsor research fellows. As of 2005, CCS is funding the work of over 30 research fellows, many of whom are full-time employees with offices at EarthConserve’s Washington, DC, headquarters. Others work at research centers and universities around the world and count CCS as only one of various funding sources. Many of these research fellows are conservation biologists of one form or another, although their particular fields vary greatly, from primatology to ichthyology to herpetology to marine biology. Other research fellows’ expertise lie outside of biology; for example, CCS has sponsored the work of conservation economists and sociologists.

Besides sponsoring research fellows, CCS also has a full-time staff of about 60 people—30-35 scientists and 25-30 management and support staff—who work at
EarthConserve’s DC offices. These staff work in various programs that fit into the following general categories:

- **Species.** Among other things, the species programs at CCS publish a specialized journal on primates and another on edentates (sloths, armadillos, etc.) and are working on comprehensive assessments of the distribution and conservation status of several species groups, including amphibians, reptiles, and mammals.

- **Field assessment and monitoring.** This program focuses on monitoring biodiversity in the field through rapid assessment surveys and field stations.

- **Regional analysis and GIS mapping.** Using satellite, aerial, and field-based observations, this program focuses on spotting regional trends in biodiversity in order to address large-scale threats. The program also provides geographical information and mapping services to other departments in CCS as well as to EarthConserve field offices throughout the world.

- **Human dimensions of biodiversity.** This program conducts economic, sociological, and demographic research in order to address the human factors that almost always play a role in threats to biodiversity and to investigate the links between human communities and biodiversity in order to find ways that both can be sustained.

- **Conservation synthesis.** This program synthesizes the research generated by other CCS programs in order to develop priorities for conservation action.

- **Publications.** The communications team within CCS includes two technical editors, two graphic designers, and a Web specialist who work on the Center’s
various print and online publications. Since 2001 I have been employed as one of
the technical editors.

- Executive. CCS has a small executive staff that oversees the work of the programs
  listed above.

To communicate their work to external audiences, researchers at CCS focus primarily on
drafting and submitting articles for publication in refereed journals. Less frequently but at a
regular rate, CCS produces publications in-house (gray literature) that are based on the
research being done by the Center’s programs and fellows. These publications are directed to
a combination of technical and lay audiences.

Study Design

My study generated data through interviews and discourse analysis, and was carried
out in two separate but related stages. Stage 1 consisted of interviews with seven staff
scientists at CCS over a period of several months, beginning in March 2002 and ending in
July 2003. Stage 2 consisted of a series of more focused interviews with one of the
participants from Stage 1, along with a discourse analysis based on four publications
authored by her.

Participants

Because I had been working as a technical editor at CCS since May 2001, by the time
I began my study in March 2002, I had established a collegial relationship with all of the
study participants. In some cases I had previously worked closely on publication projects
with some of the scientists whom I interviewed. All were happy to participate in my study
and were forthcoming in their responses to my interview questions.
Briefly, the backgrounds of the seven scientists who participated in my study are as follows (pseudonyms are used). All hold PhDs from universities in either the US or the UK. Six are natural scientists; one is a social scientist. Dennis is an expert on the systematics of fishes. Before joining CCS, he worked as a curator at a natural history museum and then at a small marine conservation NGO. Edward is an ecologist and ornithologist who, prior to joining CCS and after finishing his PhD, worked for a year as a post-doctoral fellow at an American university. Katrina is trained in economics and sociology and describes her field as conservation planning coupled with agricultural and rural development. She has spent her career doing research for various conservation and development NGOs. Peter's fields are biogeography and taxonomy. At the time of this study, he held a joint fellowship with CCS and a large conservation organization headquartered in Europe. Before coming to CCS, he spent his career working as both a researcher and an administrator at two prominent European conservation organizations. Carol is an entomologist who heads one of the biodiversity monitoring and assessment programs within CCS. Of all the interviewees, she has been at EarthConserve the longest—she joined the organization prior to the establishment of CCS. Her career before EarthConserve consisted of two post-doctoral fellowships with two different American universities and a brief stint at an entomological research institution in the US. Carlos is both a primatologist and an expert on the protected areas system in Brazil. He was educated in the UK and then spent his career prior to CCS in Brazil, working first as a research consultant for the Brazilian government and then as a professor at a Brazilian university. Mark is a landscape ecologist who, prior to joining CCS, spent most of

1 Katrina is this participant's real name. I interviewed her again in Stage 2 of my study and also analyzed four of her publications, which I cite openly in this chapter and again in Chapter 4. Because her publications are easily available to the public, I am not able to protect her identity and thus have not assigned her a pseudonym. I received her informed consent to publish her identity.
his career as an academic both in Brazil and in the US. At the time of the study, he was on leave as a member of the ecology faculty at a major US university.

**Interview methodology**

I chose interviewing as a principal method for gathering data because I wanted to understand better my participants’ perspectives on their own work and experience as scientists within an advocacy organization, rather than interpret their work solely through my own observations of it. At the time of the study, as I mentioned above, I, like my participants, was employed by CCS. I had taken the job with no prior experience working either with scientists or within an advocacy organization of any sort. I therefore saw myself, to cite a metaphor from Steinar Kvale’s book *Interviews*, as an “interviewer-traveler” wandering through a strange land and “enter[ing] into conversations with the people encountered” along the way in order to get subjects “to tell their own stories of their lived world” so that the researcher can then reconstruct those stories for “the people of the interview’s own country” (4). This conversational approach to interviewing stems from a postmodern epistemology in which knowledge is viewed as being constructed by researcher and subject together. Both contribute as participants in the conversation-as-interview.

While I viewed my interviews as conversations, they were not simply casual dialogue between workplace colleagues; they were structured conversations with a particular purpose, namely, to understand and investigate my participants’ views on the intersection of science, advocacy, and discourse. Kvale explains that interviews can range in format from rigidly structured—meaning a strictly followed script of standard questions—to only minimally structured—meaning a single opening question followed by improvised follow-up questions.
My interviews were semi-structured, per Kvale’s rubric, in that I conducted them with “a sequence of themes to be covered, as well as suggested questions” but at the same time I was open to “changes of sequence and forms of questions in order to follow up the answers given and the stories told by the subjects” (124). My goal in conducting the interviews was to elicit responses that, as much as possible, would meet Kvale’s criteria for high-quality interview data, which are as follows:

- “spontaneous, rich, specific, and relevant answers”;
- long answers to short questions; and
- abundant follow-up and clarification on the part of the interviewer so that his or her interpretations of the participants’ answers can be confirmed or clarified.

(145)

**Stage 1 interviews**

In specific terms, I conducted the Stage 1 interviews as follows. I prepared a set of questions in advance covering three general categories of topics: (1) the subject’s background (e.g., Can you give me a short overview of your educational background and career up to this point?), (2) comparisons of science at CCS and science in other settings (e.g., What differences are there between what you and your colleagues do here at CCS and what a similar group of scientists might be doing at a research university?), and (3) balancing science and advocacy (e.g., What are the challenges of establishing and maintaining credibility and prestige in the scientific world and the conservation world at the same time?). (Appendix 1 contains a list of the types of questions I asked.) However, in keeping with my semi-structured format, I was not strict about asking all prepared questions, necessarily, nor
was I strict about asking them in a particular order. Rather, I tried to be flexible in my interviewing style, improvising follow-up questions when my subject raised a point that I thought was worth pursuing further. In some cases I wasn’t able to cover all of my prepared questions in the time I had scheduled for the interview.

During Stage 1 of my study, I interviewed each scientist once. The interviews generally lasted 60-90 minutes and took place in either his or her office or in a nearby conference room, depending on where the interview could be carried out uninterrupted. All participants gave permission for me to make audio recordings of the interviews, which I later reviewed closely to generate detailed notes and, where I deemed appropriate, transcribed quotations. All quoted material in the following chapters is derived from the transcribed portions of my interview data. My other observations and points are drawn from my interview notes as well as transcriptions. In keeping with standard ethical guidelines for interviewing (Kvale 112-117), I obtained my participants’ informed consent and ensured them of confidentiality in my reporting of the data by asking them to read and sign a consent form (see Appendix 2).

As a side note, for Stage 1 of my study I also counted as data a particularly relevant editorial that the executive director of CCS wrote in 2003 for a leading conservation biology journal². The editorial addresses the new role of NGOs in conservation science. I treated this editorial as data because, while I did not interview the director of CCS, I believe it indicates how he would respond to some of the questions I posed to the other interviewees. I did, however, contact him via e-mail with a follow-up question to the editorial—I asked him if he

² I have chosen not to name the journal in which this editorial appeared in order to obscure the identity of both the author and the organization where he and the other scientists who participated in my study worked.
had any examples in mind when he identified "an emerging schism" between the academic and NGO communities in conservation science. I cite a passage or two from his e-mailed response in Chapter 3. I also referenced his editorial occasionally in my interviews because many of the study’s participants were familiar with it, and thus it provided a convenient way of introducing my interview questions.

**Stage 2 interviews**

Stage 2 of my study took place from July 2004 to March 2005. It consisted of a more extensive investigation of the work of Katrina Brandon, one of the seven scientists interviewed in Stage 1. I singled out Brandon for a number of reasons. First, because of my study’s focus on overlapping forums in science and advocacy, I found her position as a social scientist in an organization (CCS) and discipline (conservation science) dominated by natural scientists to be particularly interesting. She was also quite forthcoming in the Stage 1 interview, so I expected her to be an excellent subject for the second half of my study. In addition, I could tell that she had thought a lot about the challenges of communicating her work in the various forums of science and advocacy that she had belonged to throughout her career.

Brandon has spent her entire professional career working for multilateral[^3] and non-governmental organizations focused on both economic development and conservation. In many cases she has done consultancy work for sponsoring organizations; other times she has served as a full-time member of an organization’s staff. As mentioned above, at the time of this study, she was a member of the research staff at EarthConserve. In her career prior to

[^3]: A multilateral organization is an organization whose members consist of several national governments. The United Nations, the World Bank, and the International Monetary Fund are three well-known examples of multilateral organizations.
joining EarthConserve she was at various times employed by two other large international conservation NGOs as well as the World Bank, for whom she has also done consulting work on occasion. She has also worked as an adjunct professor at the University of Maryland. She holds an interdisciplinary PhD in development sociology, economics, and conservation. Her area of interest and expertise, as she describes it, is agricultural and rural development as it relates to conservation planning.

Stage 2 consisted of two parts: my analysis of samples from Brandon’s record of publications (discussed in further detail in the following section) and a series of interviews with Brandon in which I asked questions about the publications as well as general questions about her work as both a scientist and advocate for conservation. The purpose of my first interview with Brandon in Stage 2 was simply to gather information that would help me choose texts for my discourse analysis. During this rather informal interview, we discussed her publication record for about an hour, during which time I simply asked her to “tell me the story” behind each of her publications. Later I reviewed a tape of the interview and in my notes briefly summarized the context of each of her publications. Based on this information, I was able to choose four publications for analysis. Brandon showed a comparatively vivid recollection of the constraints imposed by the forum within which each of these four texts was published and thus I expected that our interviews about the publications would yield rich data.

After finishing a discourse analysis of these four publications, I conducted a series of interviews with Brandon in which I asked her to respond to various aspects of my analysis. I also asked her questions about the context and history of each publication. These interviews took place throughout March 2005. Because Brandon was residing in Argentina at the time,
we spoke by phone and I recorded the interviews, with her consent. I later transcribed the interview tapes almost verbatim, eliminating only various verbal pauses (ums and ahs) and passages of conversation that were completely off the topic of the interview questions (e.g., initial or closing banter). As with the Stage 1 interviews, I prepared a list of questions in advance but was flexible in how I conducted the interview. Instead of rigidly following my prepared list of questions, I asked follow-up questions to pursue topics that I found interesting and potentially relevant to my study. We discussed each of the four publications separately, and I asked basically the same questions in regard to each publication. Then, in our final interview, I asked Brandon a few general questions about her experience communicating her work within scholarly and advocacy forums. The questions listed in Appendix 3 illustrate the general topics I was interested in covering during the interviews but, as the sample transcript in Appendix 4 shows, I deviated from these general questions extensively as I asked Brandon to elaborate on or clarify her answers to my opening questions.

**Interview data analysis**

Kvale identifies five approaches to analyzing interview data: (1) condensation—longer passages are condensed to brief statements that encapsulate the principal sense of what was said; (2) categorization—the interview data are coded into categories, i.e., passages are coded as representing (or not) a particular phenomenon; (3) narrative analysis—the "structures" and "plots" of the "stories" told during the interview are foregrounded in the researcher's account of the interview; (4) interpretive analysis—the researcher "goes beyond a structuring of the manifest meanings of a text to a deeper and more or less speculative
interpretation" of it; and (5) ad hoc analysis—a combination of some or all of the preceding approaches or others, as the researcher sees fit (193-194). I took an ad hoc approach to analyzing the data from the interviews I conducted. Rather than condensing and categorizing topics in the data as the basis for my analysis, I simply reviewed the interview transcripts closely, looking for patterns, themes, and points of comparison and contrast. A more systematic analysis might be appropriate for a larger study intended to be more definitive. This study was limited in scope and offers only suggestive results.

Discourse analysis

As was noted above, my study also included a discourse analysis of four texts published by Katrina Brandon, one of the participants in the Stage 1 interviews. The interviewing segment of my study was aimed at capturing my participants’ perspectives on working in a context where science and advocacy overlap; discourse analysis provided another method for investigating this context. Barton defines discourse analysis as “the study of the ways that language is organized in texts and contexts” (“Linguistic Discourse Analysis” 57). This broad definition leaves room for much variation, and indeed, approaches to discourse analysis range widely in both their units and methods of analysis. The approach I used in this study emphasizes contextualization: I identified and analyzed features of the texts that I interpreted as having significance with regard to the forums within which the texts were published.

I see my study as an extension of a tradition of context-oriented discourse analysis in rhetoric studies that includes, for example, Huckin’s study of the discourse of homelessness (“Textual Silence”). In the study Huckin situates a corpus of news articles and editorials on
homelessness within the broader context of discourse on the subject in order to identify “manipulative silences” in the news and editorial texts. Another example is Smith’s analysis of governmental discourse, in which she presents a conceptual framework for analyzing the discourse of Congressional hearings which she then uses to illuminate the historical and political context of a hearing from the early days of the US Senate. Barton’s analysis of doctor-patient discourse also connects generic features of a particular discourse type to a broader context. In particular, she contextualizes what she identifies as the “oral genre of treatment discussion in oncology encounters” between doctors and patients to help explain, among other things, why the genre gives doctors the option of foregrounding, backgrounding, or altogether avoiding communicating a prognosis to the patient (“Discourse Methods”).

Artifact selection

In choosing the textual artifacts for my analysis, I first assembled a collection of all of Brandon’s publications, which I initially categorized as having been published in either a scholarly or an advocacy forum. I further sub-divided these two categories into (1) chapters in edited books and (2) journal articles (for the former category) and (1) chapters in edited books and (2) gray literature (for the latter category). (See Appendix 5 for a complete list of Brandon’s publications.) Table 2.1 shows how Brandon’s publications in each of these sub-categories are distributed over the course of her career. Because her publications are relatively evenly distributed among the four sub-categories, I hypothesized that she would be qualified to comment on the rhetorical aspects of both scholarly and advocacy forums in conservation.
Table 2.1. Number of Brandon’s publications per year in each forum.

<table>
<thead>
<tr>
<th>Year (nothing published in omitted years)</th>
<th>89</th>
<th>92</th>
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<td><strong>Scholarly forum</strong></td>
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<td>Journal articles</td>
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<td><strong>Advocacy forum</strong></td>
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<td>Book chapters</td>
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<td>Gray literature</td>
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<td>28</td>
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</tbody>
</table>

I decided to choose two artifacts from each forum to be the objects of my document analysis: one chapter and one journal article from those texts published in a scholarly forum and one chapter and one example of gray literature from those texts published in an advocacy forum. As a first step in choosing my artifacts for analysis, I conducted an informal interview with Brandon to help me narrow my choices (see section on interviews above). Based on the information I gathered from this interview, I was able to choose my four artifacts, listed below and described in further detail in Chapter 4.

**Artifacts from scholarly forums:**


Artifacts from advocacy forums:


Artifact analysis method

My discourse analysis follows guidelines from Huckin’s methodology (“Context-sensitive Text Analysis”) in that I began by selecting an analytical corpus, as described above, and then analyzed the corpus to identify salient patterns that represented, in Huckin’s terms, “interestingness” to the field of rhetorical inquiry. Next, I performed a rhetorical analysis of these patterns by attempting to explain why the patterns exist. In identifying salient rhetorical patterns in the texts, I applied an approach Barton describes as “rich feature analysis.” Rich features, she explains, are “those features that point to a relation between a text and its context” (“Linguistic Discourse Analysis” 66). A rich feature analysis can proceed in either a top-down or bottom-up fashion. In the former case, rich features are interpreted through a larger theoretical lens, e.g., gender theory or social construction. A bottom-up approach, in contrast, is inductive and data- rather than theory-based. It involves “looking at texts, inductively identifying their rich features and associated conventions, and then using these features and conventions as examples in a descriptive argument in support of some generalization(s) . . . about the interpretive relations between features, conventions, texts, and their contexts” (“Linguistic Discourse Analysis” 67).
Mine was a bottom-up analysis—I began by scrutinizing each text in search of features that, in my judgment, indicated something meaningful about the larger rhetorical context within which each text was produced, especially as it relates to contrasts between scholarly and advocacy forums. In my initial readings of the texts I noted virtually every feature that seemed remotely relevant to my analytical purpose, but as I reread the texts, I began to narrow my focus to the types of features I thought were most relevant to my study. Once I finished my analysis, I conducted several interviews with Brandon to enrich and contextualize the findings from the analysis.

Insider Status of Researcher

Another aspect of the study that, in addition to the various features described above, influenced how I executed my project and the kinds of data I was able to generate is my “insider status” as a researcher. I worked as a technical editor on the publications team at CCS from May 2001 to July 2005. I was thus employed at CCS for the entire length of this study. The publications team consisted of me and one other technical editor, two graphic designers, and a web designer. Our main responsibility was to work with CCS scientists and support staff to develop technical reports, brochures, information sheets, newsletters, posters, and other related publications. Working with commercial printers whom we would hire, we produced and distributed these products as CCS publications. In other words, we produced gray literature. Producing technical reports was a particularly big part of our job. Less of our time was devoted to producing other types of publications. The scientific staff at CCS, in contrast, devoted the vast majority of their communications work to developing articles and chapters for submission, respectively, to refereed journals or edited books produced by
commercial and academic publishers. So, while I was an insider to CCS as an organization, I was generally an outsider to the communications work my study participants were engaged in.

On the surface, it may appear as if I had positioned myself within CCS in order to carry out an ethnographic study. Certainly my study shared in the general purpose and spirit of ethnography in that, as a researcher, I had ventured beyond academia and taken a job at a conservation NGO partly because I wanted to learn from first-hand experience and observation about the social world of conservation science as it related to rhetorical practices. Yet in a number of respects my project is not an ethnography. As Doheny-Farina and Odell explain, a primary goal of ethnography is to discover patterns in the process of social interaction, which an ethnographer does by generating “thick descriptions” of social and cultural phenomena in a particular context drawing from a plurality of sources—observations, conversations, interviews, physical artifacts, audio and video recordings of activity, etc. This methodological plurality is a defining feature of ethnography. Moss notes that “no ethnography makes use of only one . . . method” (159). Yet the data I gathered on scientists at CCS was based solely on interviews. The discourse analysis segment of my study centers on artifacts that are not directly connected to CCS as an organization because the author wrote them all before joining CCS. Moreover, even though I worked at CCS for over four years during the course of the study, I never set out to observe and record the day-to-day actions of the people I worked with. Nor is my study focused on CCS, per se, as a unique cultural or social entity. Rather, I explored the broader question of how science, advocacy, and rhetoric intersect by using scientists at CCS as an example. For these reasons,
I do not consider my study a bona fide ethnography, despite my being a colleague to the study participants while I was carrying out my research.

Nevertheless, my status as an insider influenced the study in at least two ways. First, it gave me an insider’s perspective on the worlds of conservation advocacy and science that made it much easier for me to understand the kinds of work my study participants were doing. This, in turn, made me a better-informed interviewer, and consequently I believe I asked questions that effectively engaged my subjects’ thinking on the particular challenges of communicating science in scholarly and advocacy forums. Second, because I was a colleague to my study participants and not a stranger whom they knew little about, I believe they were much more candid in their interview responses. EarthConserve is a high-profile organization that has become increasingly concerned with its public image as it has grown in size and influence; thus, because of the potential for bad press, I doubt my interview participants would have been nearly as frank with an outsider about the tension between advocacy and science.

**Study Limitations**

My study, like all studies, has its limitations and flaws. For instance, although I have done everything possible to protect the identity of the study participants, many in the conservation science community as well as others associated or familiar with the world of conservation NGOs would probably be able to guess the true identity of EarthConserve and, by extension, of the study participants. I did, however, alert the participants to this possibility in the consent form they were asked to sign. The more fundamental limitations of the study have to do with the authority and validity of the data.
My study purports to explore the intersection of science, advocacy, and rhetoric in broad terms, but my claims are based on data from a handful of participants who all belong to the same organization. Thus my findings must be considered quite tentative, and additional study is needed in distinct organizational settings where science and advocacy intersect. This critique—easily applied to many qualitative studies—is particularly true in this instance because, as Brown and Kalegaonkar observe, NGOs are marked by a pronounced “particularism.” Even NGOs who, as a group, are focused on the same issue (such as conservation) tend to be noticeably distinct in terms of culture, values, and constituencies. For this reason, it is quite possible that the same study conducted with similar participants at one of EarthConserve’s peer organizations would yield significantly different results.

A second data-related concern is that the interview data are a sort of double construct. I have used these data to construct my own interpretation—with all its inherent subjectivity—of the phenomena in question, and the study participants did the same when offering accounts of their experience as scientists and conservation advocates. I rely on the first-hand observations of individuals whose experience I am most interested in documenting yet, as Doheny-Farina and Odell remind us, “the reports of observations done by others . . . are just as limited, just as influenced by their own perspective, as are a researcher’s observation” (506). To some degree I skirt this problem by focusing on the boundary work of my participants, the constructedness of which is a given and beside the point. But boundary work is not the only focus of my study, and therefore my data are dually constructed and thus doubly removed from being any kind of an objective account.

Despite these limitations, I believe the results of my study provide a valuable indication of trends in the discourses of conservation science and advocacy. The number of
study participants may be small, but they belong, as a group, to a highly influential conservation organization that has a reputation as a leader in the field of conservation science. Therefore, I believe their perspectives on the dynamics of communicating science from within an advocacy organization are significant. In the following two chapters, I summarize and discuss the results of Stages 1 and 2 of my study. Chapter 3 is based on the results of Stage 1, while Chapter 4 is derived from the research I did in Stage 2.
CHAPTER 3
RHETORIC AND THE NEW PRODUCTION OF KNOWLEDGE:
DOING SCIENCE AT A CONSERVATION ADVOCACY ORGANIZATION

As I argued in Chapter 1, the boundaries separating science from other spaces on the cultural and professional “map” are not absolute. Rather, they are continually reiterated and revised through “boundary work,” which Gieryn defines as “the attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activity as non-science” (“Boundary-work” 782). Because science is not isolated from its larger cultural, social, and political milieu, boundary work often occurs in response to the non-scientific rhetorics and spheres of activity that inevitably overlap with science and influence how it is defined (Taylor, Defining Science). One extra-scientific development that has prompted a shift in the boundaries of science is a trend towards a more socially distributed model of knowledge production (Gibbons et al.) as more and more PhDs take their knowledge-producing expertise to extra-university institutions such as the think tanks, consultancies, and non-governmental organizations (NGOs) that are growing in size, number, and influence (Salamon and Anheier). As knowledge production has spread to these new contexts, a new mode of “mission-oriented” as opposed to “curiosity-oriented” research has become increasingly prevalent (Gibbons et al. 23). Mission-oriented research is not focused necessarily on building a body of disciplinary knowledge, but instead seeks solutions to the particular problems—often related to social issues like environmental and public health, poverty alleviation, etc.—that various knowledge-producing institutions are working to solve.
Stage 1 of my study examines the rhetorical dimensions of this “mission-oriented” research through interviews with seven scientists at a prominent Washington-DC-based conservation advocacy organization. The participants’ pseudonyms and areas of expertise were as follows: Dennis—ichthyology; Edward—ecology and ornithology; Katrina—development sociology and economics; Peter—biogeography and taxonomy; Carol—entomology; Carlos—primatology; and Mark—landscape ecology. I refer to the organization by the pseudonym “EarthConserve.” The scientists were employed by EarthConserve’s “Center for Conservation Science” (also a pseudonym—henceforth CCS or the Center). At the time of the study, I also worked full-time for CCS as a technical editor. The interviews took place between March 2002 and July 2003. (More details about the study design, including profiles of EarthConserve, CCS, and the study participants as well as information about my data-gathering methods, can be found in Chapter 2.)

In the following sections, I draw on data from my interviews with these scientists to show how they are engaged in boundary work to redefine the forums of conservation science, and how they are confronted with and attempt to balance the (sometimes antithetical) rhetorical exigencies of conservation science and conservation advocacy. I conclude by noting the ways in which these scientists are engaged in the “new production of knowledge production” theorized by Gibbons et al. First, though, I provide a short overview of the growth of science in conservation NGOs as a framework for discussing the results of my study.

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4 Not a pseudonym. See Chapter 2 for explanation.
Overlapping Forums of Science and Advocacy

In some form or another, scientists are involved in finding solutions for virtually every problem human society currently confronts, including, of course, environmental problems. Environmental research, moreover, takes place in many different organizational settings, including academic institutions, government agencies, and private industry. Recently, a handful (if not more) of advocacy organizations committed to environmental conservation have established themselves as significant centers of scientific research. This represents a departure from the previously widespread split between conservation and science whereby conservationists in the advocacy sector tended to rely on research scientists in the academic sector to provide the scientific basis, where necessary, for their work. The dramatic growth of advocacy organizations (or NGOs), the new legitimacy bestowed on conservation in the academic sector, and the fact that more and more scientists are being trained exclusively in conservation are all factors in the rise of science in advocacy organizations.

With the explosion of the number of conservation NGOs over the last two decades, demand for science informing conservation action has increased, as has competition for conservation funding. Carlos noted that in 1989 only one conservation NGO existed in the entire nation of Brazil, whereas today it has more than 2,000. With this growth, conservation scientists in the academic world found that more and more NGOs were interested in implementing the science academics were producing. As Carlos explained, “the growth of NGOs . . . provided the managerial, administrative, [and] advocacy side whereby the academics [could] actually begin to see their wonderful paper in Nature or wherever have an impact.” The number of NGOs has now grown to the point, however, where some are no
longer content to simply consume (or implement) the science being produced in the academic sector. As the director of CCS observed, “the main . . . drawback to leaving research entirely to the academics is that there are too few of them.” Instead, more and more NGOs are actively producing the science they need to address the conservation problems they consider urgent. They are adding PhD-level scientists to their staffs and providing significant funding for in-house research on conservation problems. Consequently, as noted by the director of CCS, “for some years now, a good volume of conservation science is being generated by the NGO sector” including “much of the new and exciting research” in conservation.

An additional consequence of the recent explosion of conservation NGOs has been increased competition for donor funds. This, as well, is driving the growth of science in the advocacy sector. NGOs realize that establishing an in-house capacity for scientific research can give them a fundraising advantage over other NGOs. As the director of CCS explains, “many organizations are coming to perceive scientific research as an integral, ever-present aspect of their operation, even if it is only to survive in a very competitive market.” Because of this intense competition, NGOs must present potential donors with “credible strategies and specific, quantifiable outcomes, along with sound scientific underpinnings.”

A second factor driving the growth of science in conservation NGOs is the new legitimacy bestowed upon conservation within academia. Carlos recalled that, in the early 1980s, when he was a graduate student, conservation science was “completely pooh-poohed by the academic world. Anybody who dealt in conservation was a ‘technician’ or a manager.” Now, however, a growing number of academic departments and PhD programs in conservation biology, ecology, and related fields are emerging. Moreover, the number of scientific journals devoted specifically to conservation has increased, as has the attention
given to conservation in some of the most prestigious scientific publications, such as the journals *Nature* and *Science*.

A third factor, related to the second, is that more and more scientists are being trained specifically in conservation because academic programs in conservation science are now quite common. Carlos explained that, as conservation science was “put into the university,” graduates began to emerge who were actually trained in conservation, “and with conservation being very much an applied biology, many of the students would go straight into NGOs.” Moreover, many were “interested in maintaining their individual respectability as scientists” by continuing to conduct research and disseminate the results, which “the NGOs would encourage . . . because, as competition grew for funding for projects, they depended very much on the reputations of the people they got in there, their actual scientific capacity.”

In the field of conservation, then, advocacy appears to be increasingly grounded in science, while science, for its part, appears to be carried out increasingly in organizations whose predominant purpose is not to produce science but to enact conservation. Such a shift in the organizational settings for both science and advocacy is an occasion for redefining the forums associated with both activities.

**Advocacy and the Forums of Science**

The growth of science in conservation advocacy organizations has provoked what Gieryn would call a “credibility contest” as NGO scientists struggle to establish their epistemic authority within the broader discipline of conservation science. In this case, conservation scientists are in some cases unwilling to assign NGO science the same credibility that university-based science commonly enjoys. The director of CCS, for example,
described an “emerging schism” in conservation science that has resulted from attempts by
“scientific societies and academies . . . to exclude NGOs on the principle of independence.”
In other words, some have criticized NGO science for a lack of objectivity, arguing that
NGO scientists selectively interpret data to support conclusions that appeal to the values and
concerns of their donor base. According to this view, the split that has historically existed
between NGOs and science should continue—NGOs should limit themselves to the
consumption of science and leave its production to the presumably disinterested academics
whose research agendas are not influenced by the political goals of advocacy organizations
and thus are able to produce unbiased data and analysis. Katrina noted that in a recent article
she co-authored for a prominent academic journal she listed herself as the third author even
though she “rewrote the whole thing” and did “80 to 90 percent of the work.” The first author
is an accomplished academic social scientist, so listing her first was a deliberate move to
establish credibility because, as Katrina explained, when academics see that she belongs to
CCS, they tend to discredit her work because “she’s a social scientist in a conservation
organization.”

Credibility contests like this one between academics and NGO scientists in
conservation are occasions for boundary work, or attempts to redraw the boundaries of
science to include (or exclude) a group or individual whose work has traditionally not been
located within the cultural space of science. Scientists at CCS, not surprisingly, are engaged
in boundary work intended to locate themselves and CCS on the science side of the
science/non-science split. This boundary work focuses on the outcomes of the Center’s work
as well as the cultural norms of the organization itself.
Several of the scientists were quick to note the two organizational accomplishments that, in their minds, seem to most strongly prove the organization’s scientific worthiness, namely, its record of articles published and research monies raised. As Dennis observed, the rate of publication at CCS “rivals any major research department in an established unit in academia anywhere, meaning an article in Science or Nature or BioScience every six weeks or so by someone on our staff.” Edward made a similar assessment, noting that “in terms of the main measures of success in academia—publishing and raising grants—CCS is competitive with any ecology department in any university worldwide.” Echoing the director’s comment, discussed above, that NGOs are producing much of the new and exciting research in conservation, Katrina observed that conservation scientists at CCS and other NGOs have “performed an incredibly valuable role in helping . . . academic [conservation scientists] gain credibility” by publishing such a large number of research articles in high-profile journals like Science. In her view, in other words, the NGO scientists are doing work that boosts the credibility of their academic counterparts, whose field has historically been seen as less “scientific” than other fields in the natural sciences. The contrast in credibility between NGO scientists and academics is even more pronounced, she explained, in the social sciences, where academics researching conservation problems are publishing studies she doesn’t consider to be rigorous. One representative example she gave was of a recent study published by a famous academic geographer in a high-profile journal that, in her opinion, “was using the wrong data, it was constructed the wrong way—everything about it was wrong.”

In the view of these scientists, however, the case for the Center’s inclusion in the cultural space of science goes beyond the organization’s publication and fund-raising
successes. It also includes what Dennis described as a “science culture” that pervades the Center. Merton’s four norms of science, though vulnerable to various points of critique (e.g., Cicoural, Mulkay), provide a useful definition of the culture of science. Briefly, these four norms are (1) universalism—judging knowledge claims based on pre-established impersonal criteria, (2) disinterestedness—subjugating the narrow interests of scientists and their institutions to the broader goal of expanding the body of certified knowledge, (3) organized skepticism—subjecting all ideas and beliefs, whether sacred or profane, to critical analysis, and (4) intellectual communism—assigning the products of scientific research to the scientific community, not the individual scientist. As illustrated in the statements cited below, in one form or another CCS scientists implicitly referenced each of these norms as they made a case for defining their organization as one that, above all, does science rather than advocacy. These cultural norms, I believe, are synonymous with what Porter would describe as the ethos of the forums of science. In the analysis that follows, I discuss each of these norms separately as it pertains to the scientists’ perceptions of their work.

The norm of universalism poses a potential conundrum to scientists doing research in conservation NGOs: how can scientists gather and evaluate data using pre-established impersonal criteria if they are tasked with doing research that supports the values and goals of an advocacy-oriented organization? Put differently, how can NGO scientists avoid selectively interpreting their data to support the conclusions their donors would prefer? This dilemma goes to the heart of the critique of NGO science described above. While, as we shall see below, CCS and EarthConserve have struggled to reconcile the competing aims of scientific objectivity and conservation advocacy, its scientists still maintain that what they do deserves to be called science. Dennis explained that the Center’s scientific universalism is
not undermined by its larger advocacy-oriented mission because its scientists focus on developing, *a priori*, “algorithms” that “find relevance” to a particular goal—in EarthConserve’s case the extinction crisis—in the data and then working to apply these algorithms “uniformly across the board.” The uniform application is key, he explained, because it distinguishes science in NGO settings from “special pleading,” which is how he described the ostensibly scientific work of many other conservation organizations.

According to its scientists, the “science culture” of CCS also conforms with the norm of disinterestedness because, as Dennis explained, he and his CCS colleagues are no different from their academic counterparts in terms of scientific integrity. CCS scientists, he explained, have “exactly the same set of goals that any professional scientist in academia would have,” which, he continued, includes not just securing grants for funding and publishing research reports in peer-reviewed journals, but also “really getting the science right no matter what the outcomes might be.”

In many cases the scientists described instances where CCS was willing to “put itself out there,” in Dennis’s words, by subjecting its research to review by the larger scientific community and, more important, then taking any scientific criticism seriously rather than dodging it or hiding it from potential donors. Dennis recalled an exchange he had with a colleague from outside of EarthConserve who had disparaged the organization’s science as being “soft.” He responded by showing the colleague a CCS promotional packet that contained reprints not just of the organization’s research articles from prestigious journals but also of the criticism—in at least one case quite harsh—published in the same journals. Publicizing criticism from other scientists, he noted, is the “sociological expression” of the Center’s disinterestedness.
The Center’s claims to respect the process of criticism in science also connect to its claims to have established a culture of organized skepticism. Mark observed that CCS does not shy away from critical analysis of strategies and practices popular among conservation advocacy organizations. For example, CCS recently published a report that questioned the benefit of sustainable forest management, a conservation strategy that many NGOs endorse. As Mark explained, publishing this report exemplifies the organized skepticism of the Center’s scientists because “you would expect we’d do anything possible to support the notions of sustainable forest management.”

Finally, CCS scientists claim to have embraced the scientific norm of intellectual communism, both in how they have defined their research agenda and in the efforts they take to disseminate the results of their work freely and as broadly as possible throughout both the scientific and conservation communities. Several scientists noted that the Center’s research is intended to serve not just the organization’s interests but also to provide guidance to conservationists worldwide. Edward explained that CCS has a “broader role” than simply “serving science within the broader organization [of EarthConserve].” Rather, CCS has been given the resources and mandate to do innovative research that, as Carlos described, is “breaking the frontier of conservation biology.” Edward and Carlos both noted that scientists at other advocacy organizations often (Edward’s words) “get caught up so much in the day-to-day demands of the organization that [they don’t] have time for original research.”

Similarly, as Dennis explained, CCS has eschewed the ad hoc data-gathering and analysis exercises so common in NGO science in favor of a long-term approach focused on generating comprehensive datasets and analyses that will empower all conservation researchers and practitioners for long periods of time. As he put it, CCS is working to “get
the data together for everybody” because “once the data are really organized, it’s empowering to every conservation group’s analysis.” So, with regard to a project he recently began which focuses on freshwater biodiversity, his goal as he explained it to me is not to “work for three years [and produce] ad hoc species lists that nobody else can use, and none of the data can be traced to their source,” but rather to be able to say “we have learned X amount about the distribution of freshwater biodiversity and threats to it, but we have transformed the basis for any organization to do its own analysis of the question, and we’re doing better ourselves with every passing moment.”

Peter was doing work similar to what Dennis envisions for his freshwater analysis project. He is coordinating a comprehensive global assessment of amphibian species, soliciting data from hundreds of species experts and then organizing them into databases that will be made freely available to all researchers, conservation organizations, and the general public through the Web and other media. Other researchers at CCS doing site-specific rather than comprehensive work like Peter and Dennis are also committed to sharing their data as widely as possible. Carol explained to me that, within her biodiversity monitoring program, the goal is to give everyone access to all the data coming out of monitoring projects. Any scientist from outside of CCS who participates in a field survey knows in advance that the data they gather will be treated as public data and distributed to anyone who is interested, free of charge.

The importance EarthConserve places on establishing a reputation for excellence in science is underscored by the fact that scientists at CCS have pursued so active a role in the traditional forums of science. They have published prodigiously in prestigious scholarly journals, but, beyond that, they are quick to explain how the Center has embraced the cultural
norms of science as a way of positioning itself within the cultural space of science. Strictly speaking, neither the scholarly publications nor the “science culture” of CCS would be necessary to meet the narrow scientific needs of EarthConserve—CCS scientists could publish internal reports and other documents directed at guiding specific EarthConserve projects and never join the broader discourse of science. Katrina understands this point well:

We need to publish to have credibility. . . . We don’t need to publish to make [EarthConserve] do its work better, internally. That we could probably accomplish in much better ways. . . . If you said to me “All I want you to do is make field projects and implementation happen better” . . . I would do zero academic publishing.

That the organization has nevertheless chosen to participate actively in the traditional forums of science indicates that the boundaries of conservation science are shifting.

**Science and the Forums of Advocacy**

While CCS scientists are certainly focused on establishing a record of rhetorical successes in the forums of science (i.e., articles accepted for publication in journals, successful grant proposals) as well as defining their organization as falling within the cultural space of science and embracing the *ethos* of its forums, they still belong to a conservation advocacy organization, and are therefore subject to exigencies that in some cases contradict the conventional exigencies of science. They therefore serve as an example of how the work of science is being transformed by, and not simply transposed to, the NGO sector. In this new setting, applied knowledge is of particular relevance to the work of science—more so perhaps than in academic settings, at least in the view of CCS scientists. In addition, CCS scientists are focused on communicating their work in a way that gives them an advantage
over the organizations with whom they are competing for a limited pool of donor funding. They are also concerned with persuading lay audiences of the importance and urgency of conservation. To be sure, none of these rhetorical aims (producing applied knowledge, raising funds, and generating positive publicity for one’s work), in a general sense, is absent from the work of scientists in academic settings. However, the specific manner in which they influence the discourse of science at CCS appears to be distinct from the discourse of their academic counterparts, or at least these scientists saw it that way—they often noted distinctions between themselves and academics when describing the advocacy-driven elements of their communication practices.

**An emphasis on producing applied knowledge**

CCS scientists repeatedly stressed the Center’s focus on applied knowledge. Carol, who joined EarthConserve because its field-research program was devoted to “putting [their data] to use for conservation, whereas other [field research programs] were more of an academic exercise and didn’t actually use the data for conservation,” recounted her recent experience reviewing a research article for an academic colleague. She had commented that the article, which described a new method for measuring the sustainability of oil palm harvesting, neglected to explain how to implement the method. As she recalled, her colleague had responded, “I never actually think people will use the stuff I publish.”

Mark echoed Carol’s observation about the contrast between theory and application in academic and NGO-based science. What drives this contrast, he explained, are the different rewards systems in each setting. Because grants and publications are “the metric upon which everything is evaluated” in academia, “everything you do has to be publishable
in the peer-reviewed literature. . . . There’s no reward for you in academia for spending five years establishing a massive protected area in the Brazilian Amazon. It’s not something that’s part of the reward structure. It’s not something that would be evaluated.” So “academicians are focused more on conceptual, theoretical, or quantitative and methodological kinds of issues associated with conservation and do very little in terms of actual implementation of conservation in the field.” In contrast, the reward system in NGOs—or, in other words, “your ability to fund-raise”—is based on conservation outcomes, “your ability to actually enact conservation in the field.” Mark also noted a potential schism between academics and NGOs that illustrates the different perspectives on research between the two communities. Because NGOs tend to focus on conservation outcomes, some academics do not see them as valid producers of research: “if there’s a schism [between academic and NGO scientists in conservation], it’s that people in academia who are interested in conservation feel that NGO-based conservationists aren’t doing research. They’re doing applied things.”

The longest-running series of technical reports produced by CCS—a series of about 30 reports on rapid biodiversity assessment surveys dating back to 1991—illustrates the Center’s prevalent concern for doing science that leads to conservation outcomes. Carol oversees the production of these reports, and she explained that, at one point, she considered publishing the results of the surveys on CD-ROM rather than as printed reports. But EarthConserve’s staff in several developing countries favored the printed reports because they had a more persuasive impact on government decisionmakers and community groups. As Carol explained, “EarthConserve’s in-country programs use the printed reports as kind of a calling card. They can go in and say ‘This is the kind of thing we do . . . the kind of thing
we can produce.” And the reports tend to be persuasive because they have specific conservation recommendations and, in Carol’s words, “look official.”

These reports also show local government officials, local NGOs, and community groups that EarthConserve is interested in solving their conservation problems and not just gathering data for its own scientific purposes. As Carol put it, the reports show local groups that “EarthConserve can deliver, we can make a product.” As often happens in developing countries, she explained, scientists visit, collect their data, and then never report back the results of their field work. She noted that “several countries don’t want the Smithsonian to come back because that’s what they do.” In contrast, EarthConserve and CCS make a point of producing and distributing reports in the regions where they conduct these surveys, and the consequences of this practice have been quite positive for EarthConserve’s local conservation efforts. As an example, Carol noted that EarthConserve’s field staff in Papua New Guinea persuaded EarthConserve headquarters to fund a field survey in their country because the report from a previous survey had had such a positive impact on conservation there.

Communicating science to compete with other NGOs for funds

EarthConserve, like most conservation NGOs, relies on donations as its main source of revenue. In order to successfully carry out its conservation mission, therefore, it must convince potential donors to support EarthConserve instead of other NGOs. In this competitive fundraising environment, the claim that a particular strategy is grounded in scientific research can be highly persuasive. As Dennis noted, “a lot of conservation groups
have picked up on the notion that ‘science-based’ is a good adjective to have in front of advocacy.”

Yet the notion that the science precedes the advocacy is often misleading, as Dennis was quick to observe in reference to some of EarthConserve’s peer organizations. As he explained, many people at conservation NGOs prefer the “smoke and mirrors,” the “sounds and . . . the aura of science.” They are not “data-oriented people,” but prefer instead to deal in opinion: “If the data are out there, then you have to do work, and you don’t have control. So there are a lot of individuals and a lot of organizations that resist the development of a clear-cut data background for advocacy because it reduces their ability to use opinion. They like to give opinion.” As a result, Peter explained, many conservation NGOs put out “compromised science” because “they are worried about what their donors [and] supporters might think.”

The same is true, although perhaps to a lesser degree, at an organization as ostensibly devoted to disinterested science as EarthConserve. A fundamental example is the extent to which the organization’s conservation strategy—its strategic niche in the highly competitive fundraising environment of conservation NGOs—in some ways has preceded the science that the organization claims it is based on. EarthConserve’s hallmark approach to conservation is to direct all its efforts and resources at protecting those regions of the world where restricted-range species are both highly concentrated and under extreme threat. It purports to have defined more than two dozen of these regions based on quantitative measures of species ranges, species concentrations, and human impact on habitat. Based on these criteria, the Caribbean, for example, ranks as a particularly high priority for conservation because it houses many unique species and has already lost most of its pristine habitat to deforestation and other impacts of human activities. Yet Dennis recounted that, when he joined CCS a few
years ago, he began to scrutinize the way EarthConserve had delineated the Caribbean priority area and saw that south Florida had been included not because it has a unique, highly threatened biodiversity (it does not, at least not compared to the rest of the Caribbean), but because of its wealthy residents. As he explained to me, EarthConserve thought “donors in Florida might be more inclined to give money if you could say ‘you’re living in the Caribbean [priority area].’”

Carlos also joined CCS after EarthConserve’s central strategy had been in place for a few years and soon saw that the organization had adopted and promoted its strategy as having a scientific basis before any systematic research and analysis were done to support it. Instead, as he explained, once EarthConserve adopted the approach and suddenly decided they needed to have data to support it, they gathered data in an ad hoc fashion, making back-of-the-envelope calculations of, say, the number of plants in the Amazon or the number of mammals in the Congo Basin. Thus, when he joined the organization and asked to see the data behind the strategy, he was told that little existed. He noted that only now are the proper analyses being done, and that, where necessary, the strategy will be modified to conform with the data emerging from current research.

Yet another, though perhaps more subtle, illustration of how these scientists are influenced by the competition for donor funding that EarthConserve continually faces is that several of them talked at length about the strategic advantages vis a vis their fundraising competitors afforded by EarthConserve’s signature approach to conservation. Dennis described the EarthConserve approach as “an enormously successful paradigm . . . for fundraising” because, as a prioritization strategy, it focuses on determining how we “lifeboat” the largest number of species per dollar through the “bottleneck” of extinction that
the natural world is currently experiencing. And, he continued, “talk like that is exactly what appeals to people who are giving the dollars.” Edward claimed to be “completely inspired” by EarthConserve’s approach because of its immense practicality for conservation. He described the approach as providing “a perfect balance between the most rigorous science available and the most practicable and implementable set of conservation actions.”

Carlos was similarly enthusiastic about the approach, explaining that, in a competitive fundraising environment where NGOs need to put forth distinctive approaches to conservation, EarthConserve’s strategy allows it the flexibility to stay true to the data as they are refined. In other words, because EarthConserve’s list of priority areas is relatively short, it can easily “move them around if they want” as data are collected and analyses updated. Moreover, he observed, if further research indicates that the current criteria EarthConserve uses for determining priority areas are not valid, the organization can easily adopt another paradigm like “very important animals.” Several other organizations, in contrast, are committed to inflexible approaches that force them to make selective interpretations of data, interpretations that favor the approach they have already committed themselves to so forcefully. This, he appeared to imply, harmed their scientific credibility which, in turn, could diminish the appeal of their strategies to potential donors.

**Generating support among lay audiences**

Besides competing with other advocacy organizations for donations, conservation NGOs are generally concerned with generating public support for their cause, and at EarthConserve the scientists are caught up in this objective to various degrees. Many discussed their successes and failures publicizing their work, both within the conservation
community and in the news media. Dennis noted that, while his job at CCS does not require him to focus on generating good publicity for the cause of conservation—he explained that none of his attempts to work through the news media to draw attention to conservation problems were “handed down from somebody higher up”—he still looks for ways to attract media interest when he can. In one instance, he found out that a colleague was about to announce the discovery, in the Caribbean, of the world’s smallest lizard. He asked this person if he could “ride his coattails” during the announcement to publicize the fact that the Caribbean contains an extremely high concentration of the world’s biodiversity, including the smallest bird, lizard, snake, and frog. He described this as a “fairly successful” attempt to draw media interest to the Caribbean. He also described another publicity strategy for the Caribbean according to which he was planning to use the upcoming publication of a multi-volume set of Caribbean fish maps to “make policy points that will get a lot of hype in the press.”

Katrina talked at some length about lessons she has learned over the years regarding publicity. She compared a special issue she edited of a leading academic journal on economic development to a book she recently edited on protected areas. As she observed, both projects were “equally intensive and time-consuming,” but the first had a “huge impact” and was very popular, the second was decidedly not: “no one has heard of the book, it got zero publicity [at the organization where she did the work], so I feel like it has been irrelevant.” The lesson she learned from this experience is that “the packaging is really important,” that, as a scientific author and editor, she needs to devote more thought to the “PR side” of her work.

Finally, Peter noted that the goal EarthConserve and other similar organizations have of disseminating their messages as broadly as possible in order to enhance conservation can
be a source of conflict when the organization works with outside publishers. He has worked with them frequently and observed that “it’s always been unsatisfactory” because of the divergent interests of conservation NGOs, who want to “get the information to whoever wants it—to scientists, decisionmakers, whomever,” and publishers, who are concerned with making a profit on the publication. Because of this interest, publishers typically arrange a “buy back” agreement with the NGO that developed the content for the publication, allowing them to purchase a number of copies at a discount—usually about 65 percent of the listed price. According to Peter, buying back copies at this price and then distributing them for free usually works out to be more expensive than simply producing the publication in-house. As he put it, “it never pays” to work with an outside publisher.

**Shifting Boundaries in Scientific Practice**

Scientists at EarthConserve are focused, at the same time, on establishing and maintaining their scientific credibility and on fulfilling various rhetorical aims of advocacy, including competing effectively with rival organizations for donations, generating good publicity for the cause of conservation, and disseminating their work as broadly as possible so that it can be used to improve conservation projects. This particular combination of rhetorical emphases derives from the fact that scientists at EarthConserve occupy a new cultural and professional “space” that overlaps at least two older and better-established spaces and systems of discourse: conservation science, itself a relatively new field of inquiry, and the growing body of non-governmental organizations devoted to conservation advocacy. Within this hybridized discursive space, scientists at EarthConserve seek to maintain their status as effective interlocutors in the forums of science while taking on some of the new (to
many scientists at least) rhetorical work associated with conservation advocacy. Obviously any conclusions about larger trends drawn solely from the results of this study would be quite tentative. Indeed, the capacity EarthConserve has developed for producing science remains exceptional in the world of conservation advocacy—currently only a handful of conservation NGOs contain large research centers like the one at EarthConserve. Nevertheless, more and more conservation NGOs have at least one or two scientists on their staffs who continue to remain active in the forums of science, in addition to their work in the conservation world. As Carlos observed, an increasing number of conservation science PhDs “go straight into NGOs” yet remain interested in “maintaining their individual respectability as scientists” by continuing to conduct research and disseminate the results. So, while the discourse of conservation science may continue to be dominated by science carried out in universities, the number of conservation advocacy organizations producing science is becoming more and more significant.

The science that has begun and, I believe, will continue to infiltrate advocacy organizations exemplifies what Gibbons et al. have labeled “Mode 2 knowledge,” a mode in which knowledge producers are not necessarily focused on building a body of disciplinary knowledge but instead seek solutions to the particular problems the mission-oriented organizations they belong to are focused on. Mode 2 knowledge is socially accountable and reflexive, socially distributed, transdisciplinary, and is generated in “a context of application” (Gibbons et al. 3). In Mode 2, the requirements of the problem at hand dictate the composition of the problem-solving team. Teams of Mode 2 knowledge producers thus extend across disciplines and to both academic and non-academic institutions. With more freedom from the constraints of institutional boundaries, Mode 2 research teams are flexible.
in their composition, which allows for quicker responses to shifts in the context of research as researchers strive to meet the criteria for success in Mode 2. These criteria include not only scientific excellence but also application-oriented factors like efficiency and competitiveness in the market. Moreover, in this context of transdisciplinarity, de-emphasized institutional boundaries, and hybridized knowledge-producing communities, Mode 2 knowledge producers are increasingly called upon to communicate a “more 'vernacular' science than ever before” and “to speak in more than one language in order to communicate at the boundaries and in the spaces between systems” (Gibbons et al. 36-7).

The scientists at EarthConserve exemplify this new mode of knowledge production. Their principal institutional affiliation is determined by the problem they are working to solve (the extinction crisis) rather than any particular discipline of inquiry. Similarly, their research concerns are connected by this common problem, not a common discipline. Their concern for establishing both a record of and a reputation for scientific excellence is illustrated by their success in raising research funds and getting articles accepted for publication as well as their self-identified “science culture.” Yet, because conservation supersedes science in the mission of EarthConserve, these scientists’ commitment to scientific excellence as traditionally defined in the forums of science is tempered by an emphasis on applied knowledge, a concern for using the organization’s limited resources for conservation as efficiently as possible, and the need to compete successfully for a limited pool of donations in the crowded field of conservation NGOs. As a result, they find themselves, in the words of Gibbons et al., “at the boundaries and in the spaces between systems.” At the ever-shifting boundary between science and non-science, they are engaged in a credibility contest with some of their non-NGO colleagues in conservation science. And
in the space between science and the system of non-governmental organizations advocating and carrying out conservation, their work illustrates the sorts of rhetorical responsibilities scientists may increasingly find themselves taking on as science continues to infiltrate the world of advocacy.
CHAPTER 4

A COMPARATIVE ANALYSIS OF DISCOURSE IN SCHOLARLY AND ADVOCACY FORUMS FOR CONSERVATION SCIENCE

As discussed in Chapters 1 and 3, scientists in conservation organizations occupy a boundary zone of sorts in the overlapping and sometimes antithetical cultural and professional spaces of (1) conservation science (predominantly centered in the academy, but increasingly spilling over into non-governmental organizations) and (2) conservation advocacy and implementation (primarily centered in non-governmental organizations). I have argued that the discourse community of conservation scientists is evolving in response to the growth in both number and influence of scientists occupying this boundary. In particular, more of the community's discourse now arises from knowledge produced in a “new mode,” as described by Gibbons et al., in which multi-disciplinary teams in both academic and non-academic settings collaborate on applied research intended to solve a particular societal problem rather than contribute to a body of disciplinary knowledge. As a consequence, the forums of conservation science continue to expand beyond the conventional discursive spaces of the academy—i.e., specialized journals and academic books—to those of conservation advocacy and implementation, i.e., reports, books, and others types of publications developed, produced, and distributed by the NGOs on the “frontlines” of conservation. Among many scientists and librarians, much of this latter category of discourse is often referred to as gray literature.

This chapter presents the results of my analysis of one scientist’s participation in the overlapping discursive spaces of conservation science and advocacy, which I refer to as scholarly and advocacy forums. Specifically, my study focuses on the work of Katrina
Brandon, a social scientist at the Center for Conservation Science at EarthConserve. Brandon is one of the scientists I interviewed for Stage 1 of this study (see Chapter 3). I decided to focus more extensively on her work for my study's second stage because her responses to the Stage 1 interview indicated that she had thought a lot about the challenges of communicating her work in the various forums of science and advocacy she had belonged to throughout her career. Also, her position as a social scientist in an organization (CCS) and discipline (conservation science) dominated by natural scientists seemed to make for a fruitful investigation of overlapping forums. I chose four texts from her record of publications—two published in scholarly forums, two in advocacy forums—to use as the basis for a comparative discourse analysis. Because I cite these texts openly below, I chose not to use a pseudonym to hide Brandon's identity. I also conducted a series of interviews with Brandon in which I asked her to (1) respond to various aspects of my analysis, (2) describe the context and history of each publication, and (3) comment more generally about her experience communicating her work within scholarly and advocacy forums. The interviews were semi-structured in format in that I prepared questions in advance but then deviated from my questions liberally to follow up on aspects of Brandon's responses that seemed relevant to my study. (Chapter 2 contains a more detailed description of my study methods.)

In what follows, I draw on data from both my textual analysis and my interviews to show how Brandon adapts her discourse to the scholarly and advocacy forums within which she publishes her writing. I first discuss contrasts between Brandon's discourse in both types of forums, focusing on publishing methods, the constraints advocacy organizations impose on their scientists' discourse, and the dynamic of Brandon's dual identity as both conservationist and scientist. I then analyze the features of the texts and their various
contexts to show how the boundary between scholarly and advocacy discourse in conservation is becoming increasingly blurred. In particular, I describe the interplay between scholarly and advocacy forums in Brandon’s work, meaning her use of scholarly forums for what could be considered advocacy objectives and vice versa. I begin with a brief overview of the texts that are the basis of my study.

**Textual Artifacts**

My study focuses on 4 of the 28 journal articles, chapters, and reports Brandon had authored (or co-authored) throughout her career at the time of this study. These 28 texts are fairly evenly distributed between scholarly and advocacy forums, which I define as follows: scholarly forums include academic journals and books published by academic publishers; advocacy forums include books and reports published by non-governmental organizations. I chose two publications from each forum type. The scholarly publications I analyzed consisted of a chapter from a scholarly book and a journal article. The advocacy publications included a chapter from an NGO-published book and a report published by the World Bank. For brevity, I will refer to these texts, respectively, as SF1, SF2, AF1, and AF2. Each is described in further detail below.

**SF1: Chapter from scholarly book**

The first text representing Brandon’s discourse in scholarly forums is a chapter she contributed to Randall Kramer, Carel van Scheik, and Julie Johnson’s book *Last Stand: Protected Areas and the Defense of Tropical Biodiversity*, published in 1997 by Oxford University Press (Figure 4.1). Kramer et al., the book’s editors, were all professors in environment-related fields at the time the book was published. Brandon and the other
contributing authors initially wrote their chapters for presentation and discussion at an invitation-only conference on biodiversity conservation in the tropics. All authors were given the exact title for their chapters in advance. After the conference, authors revised and submitted their papers for publication in the book. The preface notes that the book is intended “to examine issues in biodiversity conservation that [elude] solution within traditional academic boundaries” (ix). Biologists, ecologists, economists, and sociologists all contributed to the book.

The 24-page chapter by Brandon, entitled “Policy and Practical Considerations in Land-use Strategies for Biodiversity Conservation,” is a strong critique of several assumptions that she claims are both questionable and widely held within the conservation community. She argues, for example, that, contrary to the common assumption among conservationists, local people in tropical forests do not always live in harmony with nature when isolated from the developing world, nor are parks and reserves always the best strategy for protecting biodiversity, nor are local communities always motivated to use their forests sustainably if given the proper support and training. Brandon critiques seven misguided assumptions in all.
SF2: Journal article

A second example of Brandon’s discourse in scholarly forums is an article from the journal *World Development*, which Brandon described as “the best-known journal on development issues.” Entitled “Planning for People and Parks: Design Dilemmas,” this 14-page article was published in 1992, with Michael Wells as co-author (Figure 4.2). Based on research Brandon and Wells did for AF2 (described below), the article introduces “integrated conservation-development projects” (ICDPs)—a relatively new and little-studied approach to conservation, at least at the time of this article—to the development field.

Brandon and Wells introduce and explain the ICDP approach, illustrate it by describing two examples of ICDPs—one successful, one not—and describe in general terms the different strategies ICDPs employ to meet their combined conservation and development goals. Finally, they devote several pages to discussing the trade-offs between conservation and development—the “design dilemmas”—that must be confronted when considering the ICDP approach.
AF1: Chapter from NGO-published book

The first example of Brandon’s discourse in advocacy forums is a chapter she contributed to a book entitled *Parks in Peril: People, Politics and Protected Areas* (Figure 4.3). Jointly published in 1998 by The Nature Conservancy (TNC), one of the largest international conservation NGOs, and Island Press, a non-profit environmental publishing organization, the book is edited by Brandon, Kent H. Redford, and Steven E. Anderson and contains chapters contributed by a number of different authors. The book reports and draws lessons from nine case studies of protected areas (or parks) in TNC’s “Parks in Peril” program, which seeks to bolster conservation in parks that, for various reasons, have historically been protected in name only. In particular, the book argues that “sustainable use” is often a problematic paradigm for conservation and that protected-area management plans are often poorly conceived, in large part because planners fail to understand the complex social and political contexts of the areas they seek to protect.

The chapter analyzed here (28 pp.) is entitled “Perils to Parks: The Social Context of Threats.” In it Brandon calls for a more realistic view of what can be accomplished through
establishing protected areas. She argues that all kinds of factors beyond the control of park managers constrain what the park can be expected to accomplish in terms of conservation. She notes, for instance, that many of the parks profiled in the book were established to stop the transforming effects on biodiversity of regional socioeconomic change, something parks are unlikely to accomplish in the absence of significant political will and technical and financial resources from the government. In areas with a policy of sustainable resource use by local communities, a frequent challenge is achieving consensus between park managers and local communities about what exactly constitutes sustainable use. Uncertainty about land tenure is another problem; local communities often begin to extract resources as fast as possible if their land tenure is not made clear after a new protected area is announced, because they fear they’ll soon lose all access to resources not only inside the park but in surrounding lands. Yet another challenge is the complicated problem of equity associated with protected areas as disputes arise over which groups benefit or are hurt the most by new conservation policies and programs. Finally, Brandon argues that the best land use policy for areas adjacent to parks varies from one park to the next, and in some cases may contradict the conventional wisdom that sustainable use and minimal development are always the best policies to pursue.

AF2: Gray literature

My final text for analysis, an example of gray literature, is a 99-page report entitled *People and Parks: Linking Protected Area Management with Local Communities* (Figure 4.4). The report was published in 1992 by the World Bank and jointly sponsored by the World Bank, the World Wildlife Fund (WWF), and US Agency for International
Development (USAID). Brandon, at the time employed by WWF, shares authorship with Michael Wells of the World Bank and Lee Hannah of USAID. The report examines the effectiveness of land-use projects that focus on reconciling the conservation-oriented goals of protected areas (e.g., national parks, wildlife reserves) and the socioeconomic needs of communities and individuals displaced by, living in or near, or otherwise connected to protected areas. The authors coin the term "integrated conservation development projects" or ICDPs to describe these projects. A typical ICDP might be a forest reserve that includes both a zone where habitat and species are strictly protected and a zone where the local communities who have historically relied on the protected zone for resources are allowed to engage in some form of sustainable land use.

The report is based on 21 case studies of functioning ICDPs—11 in Africa, five in Asia, and five in Latin America. The authors or their assistants visited each of these sites over a period of several months to learn about the sites’ features and histories and to evaluate how well their goals had been met in terms of promoting conservation and economic
development for local communities. As part of the site assessments, the researchers reviewed key documents associated with each site (proposals, evaluations, plans, policy statements) and interviewed management personnel. The report itself, organized into nine chapters and an appendix listing descriptions and evaluations of each case-study site, begins by introducing the concept of the ICDP and the study carried out by the authors, then moves on to discuss the features and diversity of ICDP sites (with examples), the challenges associated with designing and implementing them, the role various types of institutions should play in executing ICDPs, and ways of measuring the effectiveness of ICDPs. Two central points that the authors reiterate throughout the report are (1) that ICDPs have historically been more successful in accomplishing development than conservation, in part because the development components are often not designed with an explicit link to conservation outcomes, and (2) that, by initiating ICDPs, conservation groups are getting increasingly involved in the complex world of development, and as a consequence risk diluting their primary mission.

I chose these four texts for my analysis because they appeared to represent the types of discourse Brandon typically uses in both scholarly and advocacy forums. Different genres are included as well (chapters, article, report), thus providing a sample that extends across several of the print genres normally available to and used by scientists such as Brandon. In the sections that follow, I draw on my analysis of these four texts as well as Brandon’s commentary on her experience to illustrate both contrasts and similarities between scholarly and advocacy discourse in conservation. I begin by focusing on the boundary between scholarly and advocacy discourse in conservation and then move on to showing how I believe this boundary is becoming increasingly blurred.
Information Chain versus the “Gray” Model of Knowledge Dissemination

One prominent distinction in Brandon’s activity in the forums of scholarship and advocacy is her role in the process of developing, producing, and disseminating published discourse. Within scholarly forums, her role has typically been confined to the knowledge creation “link” in the information chain. I describe the concept of the information chain in Chapter 1—to recap, it refers to the dominant model of knowledge dissemination in post-war science, whereby the functions of knowledge creation, publication, and distribution are assigned to distinct agents: respectively, authors, commercial publishing companies, and libraries. Owen argues that science is increasingly adopting an alternative model of knowledge dissemination which he labels “mixed” (“The New Dissemination”) or “gray” (“Expanding Horizon”). Within the gray model—a reference to the ongoing growth of gray literature—organizations such as conservation NGOs are increasingly circumventing the information chain associated with commercial publishing and instead are not only developing the content of their publications but are designing, producing, and distributing them themselves. Brandon’s experience in scholarly and advocacy forums illustrates this duality of dissemination models.

Brandon’s involvement in anything but drafting and revising SF1 for contribution to the book within which it was published was so minimal that she showed virtually no knowledge or recollection of any other aspect of the book’s production. She did however note her frustration at being completely excluded from the process of disseminating the book after it was published. She explained that the authors and editors of the book (all scientists committed to conservation) undertook the project not to further their academic careers but to influence conservation policy and practice. She wrote SF1, she said, not because she “was
looking for things to pad [her] CV” but because she was “looking to try and do things that are going to get out there and somehow make a difference.” Yet the book had little of its intended impact, in her view, because its dissemination was controlled by the publisher: “[We] do these things and then hope the university presses will market them.”

With AF1 and AF2, in contrast, Brandon was much more involved in almost all the functions of knowledge dissemination, from creation to distribution. In describing her role as author of AF1 and other chapters as well as co-editor of the book, which was published by her employer (at the time), Brandon showed that she was deeply involved in conceptualizing, researching, writing, and reviewing the content of the book as well as making plans for its dissemination. In the earliest stages of development, she helped write the prospectus for the book, submitted it to Island Press (the book’s co-publisher, along with TNC), and incorporated changes based on feedback from reviewers at Island Press. She then commissioned and, in some cases, carried out the case studies that formed the basis for the book’s content. After authors submitted chapters, she managed an informal review process whereby she asked colleagues both within and outside TNC to look at chapters if they were familiar with the sites studied. This resulted in only minimal revisions. Finally, she was involved in discussions about dissemination, particularly with regard to readers in Latin America. The original plan was to translate the book into Spanish for distribution in Latin America, but the format was undetermined. Brandon said she pushed for either putting the entire translation on CD-ROM—to make dissemination cheaper—or distributing translated versions of the material in a more targeted way, giving relevant audiences “abridged versions of the synthesis chapters [along with] the case [study] on their park.”
Her role was also extensive in the various stages of developing and producing AF2, where she took on editorial and design work that under other circumstances might have been the domain of communications professionals. With the overarching aim of making AF2, as Brandon explained, “as accessible as possible to people in different countries and . . . of wildly divergent backgrounds,” she focused on the design of the text as well as the language. The report’s many boxes were one solution she came up with: “I remember when we came up with the idea of boxes, which we thought were so clever because I don’t ever remember seeing anything that had lots of boxes like this” (see Figure 4.5 for an example). She also dictated the design of the cover (see Figure 4.4). Throughout the process, Brandon emphasized, she and her two co-authors “were kind of all on our own” in making rhetorical decisions about document design: “We kept feeling like there wasn’t anyone to advise us.”

**Figure 4.5.** One example of the types of boxes used throughout AF2. This particular box provides an illustration of a general point discussed in the main body of the report. Text on the same page as the box states: “... some projects have had to consult and work with complex and frequently overlapping local and national government agencies (box 7.3)” (Wells and Brandon 50).

**Box 7.3 Complexities of governmental relations at Talamanca, Costa Rica**

ANAI, the small local nongovernmental organization executing the Talamanca project in Costa Rica, has limited staff resources. The organization has agreements with, and receives funds from, several government ministries, including those for natural resources, justice, and agriculture. For a land-titling project component, ANAI has worked with additional public and private agencies. The project area includes several different categories of protected area—including a biosphere reserve, a wildlife refuge, Indian reservations, and a national park. Separate government offices administer each of these. In addition to these four agencies, ANAI must coordinate its activities with at least ten more government agencies. The elections in 1990 necessitated the building of a completely new set of relationships with incoming personnel in each government agency.

In commenting more broadly on the role NGO scientists should play in the various stages of communicating their work, Brandon identified an ongoing tension between science
and rhetoric that she appears not to have resolved yet to her satisfaction. On the one hand, she noted, NGO scientists at her organization consistently fall short in making sure the “political arguments” that will reach audiences beyond those belonging to scholarly forums do not get “buried in the science” of their discourse. Yet she is hesitant about relying too much on the communications staff at EarthConserve to “translate” her work for broader audiences—she would like scientists to control this process “to make sure [the information] is correct.”

Political Constraints in Advocacy Forums

One of the strongest distinctions between Brandon’s discourse in advocacy versus scholarly forums is the extent to which she has had to grapple with the political implications of her rhetoric for the conservation movement in general and the NGOs sponsoring her work in particular. As these four texts show, her research and writing tend to focus on identifying problems in how a particular aspect of conservation is either conceptualized or executed and then suggesting solutions. As a scientist, she appears to see it as her role to provide conservationists with an analytical perspective on their work, which of course often entails critiquing the status quo. In addition, all other considerations aside, she prefers to be direct in delivering criticism. She commented that she is known for “being a little too blunt,” but she modifies this aspect of her writing “depending on where it’s published.” She and her scientific colleagues at CCS give and receive criticism “in a positive, peer-review kind of spirit,” whereas the NGO community as a whole, because “they don’t have that tradition of peer review, . . . [often] get really upset” about the kind of criticism Brandon views simply as “analytical thinking.”
SF1 illustrates how Brandon prefers to present analysis when writing outside the forums of advocacy. As she explained, in this chapter she was free to be direct in her criticism because she was between jobs and thus was not writing as a representative of a particular conservation NGO. To be sure, in keeping with the dispassionate, “scientific” tone she uses in all her publications (more on this below), she relies heavily on the passive voice to express numerous criticisms without assigning responsibility to any one individual or group, as in these examples (emphasis added):

Efforts to preserve biodiversity *are being promoted* in a wide array of land-use projects. (91)

... local people *are often regarded* as ‘the problem’ or the proximate source of threat .... (93)

The danger is that these positions *continue to be reiterated* on a national and international basis . . . (95)

Notwithstanding her common use of passive voice to obscure blame, Brandon does identify responsible parties more frequently in SF1 than in the advocacy-forum texts, as the following examples illustrate:

The danger is that these positions continue to be reiterated on a national and international basis, as reflected in such policies and planning documents as *Caring for the Earth* (IUCN et al., 1992), the 1992 World Parks Congress, and the Brundtland Report (WCED, 1987). (95)

One clear example of the failure to promote strong policy reform as a serious initiative comes from the World Bank. (97)

Without careful analysis, IUCN [The World Conservation Union, a prominent international conservation organization] adopted the position that it was appropriate to promote the ‘decentralization of power’ to local communities. (101)

Shifting our focus to AF1 and AF2, however, we see that Brandon frequently endures complicated, drawn out, and in some cases tense negotiations with internal stakeholders at NGOs as she attempts to publish critique in advocacy forums. As she put it, “you never know how much an institution is going to hold you back.”
Connections between the discourse of AF1 and AF2 and the activities and goals of the two publications’ sponsoring organizations are explicit in various attributes of the texts. The text of AF1 contains numerous references to the publishing organization (TNC), especially those TNC staff involved in theorizing the design and implementation of protected areas, i.e., the “park planners” named in this statement of AF1’s purpose: “This chapter reviews some of the ‘perils to parks’ that are clearly rooted in the social context, in an attempt to explicitly guide park planners to recognize the social and political nature of their actions” (417). TNC is also explicitly referenced in these and other examples throughout the chapter:

A program such as PiP [TNC’s Parks in Peril program] can accomplish a great deal on all of these fronts. (415)

... programs such as PiP need to be sensitive to this context when they begin the process of implementation. (426)

... these case studies confirm that the Nature Conservancy and its partners are often doing the extraordinary. (437)

AF2’s connection to its sponsoring organizations is on open display in the cover and first few pages of text. The cover (Figure 4.4) includes logos for the World Bank, WWF, and USAID featured prominently at the bottom of the page. The masthead includes a lengthy disclaimer stating that the text represents the views of the authors, not the publishing organizations, and that “the designations and presentation of material in [the maps] do not imply the expression of any opinion whatsoever on the part of the World Bank ... concerning the legal status of any country, territory, city, or area, or of the authorities thereof, or concerning the delimitation of its boundaries or its national affiliation” (ii). Finally, a foreword is also included, signed by representatives of each of the sponsoring organizations, in which the report is linked to the broader mission of biodiversity conservation.
Brandon’s explanation of how these texts were developed further illustrates how the discourse was shaped in response to the aims and interests of their respective organizations. AF1 was part of a book published by TNC that was based on assessments of parks associated, either through their origins or management, with TNC. As a result, writing and editing the book meant negotiating, in Brandon’s words, a “political minefield.” Things became politically complicated from the very start because TNC sent Brandon and other researchers to assess sites that “TNC thought . . . were doing really well in most cases,” but the researchers quickly found evidence to the contrary. As Brandon explained, “I thought [most of the parks] were just a mess. But I couldn’t say ‘Gee, TNC, your projects are a mess.” Or at least she was told not to say that after writing a first draft that was critical of the parks, and therefore she ended up changing the draft considerably.

She was careful to note that she still reported back to TNC staff that their sites were having problems, so the “information wasn’t being lost,” but in the book this information had to be presented in a way that did minimal harm to TNC’s reputation. So, for example, Brandon noted that one of the “perils to parks” she describes seems obvious and non-controversial in how she presents it for the book’s readers but is based on controversial findings that she couldn’t report explicitly without harming TNC’s image. In the chapter, Brandon explains that a park’s “circumstances of origin” are a potential peril or, in other words, “how a park is formed is going to last for a really long time,” which, she was quick to add, “doesn’t sound like rocket science.” But what she really found in her research was much more incendiary: “What I didn’t want to say was ‘In nine of these places, you kicked people off their land [when creating a park]. Of course they’re still pissed 20 years later.” In contrast, Brandon sometimes gives specific examples when giving praise. She sees the
difference not so much in terms of public relations for TNC but as a way of presenting bad news constructively:

I think a fair way to put it was that I wanted to include what the problems were as a way of getting people to try and fix them. But I wanted to hide people’s names for anything that was done wrong. However, in places where somebody seemed to be doing something well, it made sense for me to call attention to that because . . . just like with little kids, praise gets you further.

AF2 went through a similar internal review process, though much more tumultuous. Indeed, the process ultimately led to Brandon leaving the organization she was working for at the time. The problem began in the early stages of the project. WWF, the World Bank, and USAID, as well as Brandon and her co-authors, began the study thinking they would be evaluating “everyone’s best projects.” The study and report were “supposed to be skewed towards the best conservation projects that were helping people in the world.” Brandon and her co-researchers designed a rigorous methodology for evaluating the hand-picked case studies: “We spent a good chunk of time and developed a really detailed, serious research tool.” Once they got to the field, however, they realized their methodology was useless because so much of the data they wanted to analyze were simply unavailable—people had no idea what their budgets were or how money was spent, the projects themselves never established baselines for evaluating their progress, etc. More important, Brandon et al. quickly realized that the projects weren’t exemplary at all: “We went out to the field and went ‘Ah, shit, these projects suck’ . . . I was just going ‘If this is the best, this is terrible.’”

The first draft they wrote reporting the disappointing results of their study was “critical and disparaging” of the case study sites. Brandon described the draft as being much
more “in your face” than the final, published version. After circulating this initial draft for review, the leadership of Brandon’s organization tried to stop publication entirely: “I was actually called in front of the president of WWF and told that if I published what was in there I’d be fired.” Internal reviewers, she said, were saying things like “Are you trying to destroy our fund raising?” Initially WWF was not simply asking for major revisions—they “didn’t want anything to come out” at all.

Brandon, however, was unwilling to conform with this demand so she quit working from WWF and became a consultant for the World Bank. (She didn’t comment on resistance from the World Bank and USAID, which might indicate that objections came primarily from WWF.) No longer an employee of WWF, Brandon was able to go ahead with the report but, as she explained

I’m a reasonable person, and so I listened to their concerns for the second version of the report. . . . The second version ended up being something that was more, I won’t say the criticism’s not there, but I think it’s a little more constructive in how stuff is framed. . . . I would say we toned it down a little bit, but I didn’t feel like we changed any of the messages at all.

Although she wasn’t privy to internal discussion at WWF after she left, Brandon noted that “somebody [at WWF] had to have given the final sign-off because WWF decided to put their logo on the final report.”

These two texts are not exceptional examples of the extent to which advocacy-forum science is subjected to revision due to the political concerns of sponsoring organizations. Brandon noted that one conservation NGO she worked for was “pathetic” in its concern about the politics of publishing her research. While employed there, she had at least four or
five “huge, good” publications that were never published “because they had review
committees and they never came out of the review committee because they couldn’t even
decide who should be on the review committee.”

Balancing Identities: Disinterested Scientist versus Committed Conservationist

As a scientist, Brandon’s commitment to providing disinterested accounts of her
research, even if it is critical of the activities of her sponsoring organization, has often led to
difficult pre-publication negotiations with internal stakeholders at NGOs. Brandon’s role as
disinterested scientist is also on display in certain rhetorical features of her published
discourse, although with some variation as we compare her work in scholarly and advocacy
forums. Not surprisingly, Brandon adopts the dispassionate language of science when
publishing in scholarly forums. For instance, SF1 and SF2 are both characterized by
widespread use of passive voice. A few examples from SF2 are listed below, drawn from
dozens in both texts (emphasis added):

The value of traditional enforcement activities, however, is increasingly being questioned as a long-
term solution to the protection of many critical ecosystems. (557)

... the benefits generated from tourism have been poorly captured and distributed to the local
community. (560)

Governments, multilateral development organizations, and NGOs were all found as either
implementing or funding agencies. (561)

Used in this way, the passive voice obscures human agency in the study and downplays the
subjectivity of the study and its results, thus giving the discourse a tone of scientific
objectivity.

When Brandon shifts to presenting her work in advocacy forums, she maintains this
tone of neutrality and scientific objectivity. Her writing in AF1 and, even more, in AF2 is
similarly steeped in passive voice and other sentence constructions that obscure the
involvement of humans in gathering and interpreting data, such as the placement of inanimate objects in the subject position of sentences. These are just two examples, taken from AF2, that represent dozens others available in both advocacy texts:

Most protected areas were originally established with little or no regard for local people . . . " (1).

The report explores the social, ecological, technical, and institutional issues that arise . . . " (ix).

Yet, even though Brandon maintains a tone of disinterested objectivity throughout her discourse in both types of forum, contrasts between her discourse in scholarly and advocacy forums show, if not a tension, then a fluctuation in the persona she assigns herself in relation to the world of conservation. In other words, the extent to which she counts herself as one who is committed to the cause of conservation or simply a disinterested observer and analyst varies throughout her discourse.

In SF2, Brandon maintains a clear distinction between herself and the conservation movement, referencing “conservationists” in third person throughout the article, as in these examples:

An emerging view among conservationists is that the successful management of protected areas (PAs) must include the cooperation and support of local people. (557)

Among conservationists, there has been increasing awareness of the needs of impoverished local people who live adjacent to PAs and depend on these resources for their livelihoods. (558)

Only in the conclusion and footnotes do first-person pronouns appear, as in these examples (emphasis added):

These questions indicate that we are still a long way from knowing how to design and implement an ideal ICDP project. (conclusion, p. 567)

We can also agree that poverty amelioration should be a goal worldwide. (conclusion, p. 567)

We need only note the current US budget deficit, the inaction on acid rain and the dilution of the Clean Air Act to demonstrate that this [choosing to use resources unsustainably] is the prevalent form of decision making. Why should we expect poor people to make “better” choices than we do? (footnote, p. 568)
However, these pronouns include the authors in either the community of researchers or the community of world citizens—not necessarily in the community of conservationists.

In other texts from both types of forums, the distinction between insider and outsider is preserved intermittently. In SF1, she alternates throughout the text between including and excluding herself—and, presumably, her primary audience of conservation scientists—in/from the world of conservation advocacy and implementation. In a number of instances, she uses the pronoun *we* to assign agency (emphasis added in all examples):

> In practical terms, *we* would like to think that *we* are knowledgeable about how to plan and execute conservation activities . . . (95)

> *We* are now realizing that many of the assumptions, despite their good intentions, are flawed. (96)

> The lesson is that if *we* do not pay attention to both policy and field-based incentives, *we* are unlikely to achieve success. (98)

Yet, sometimes on the same page, she switches to the third person in assigning the same type of agency (emphasis added):

> These shortcomings are largely due to a belief among *conservationists* that what *they* are doing is conservation—when, in fact, *they* are really doing large-scale social interventions in complicated settings. (95)

> . . . *conservationists* have often repeated a common failure of rural development projects by neglecting to consider whether the people want the technologies . . . (104)

> *Conservationists* have not learned to take consumption patterns and desires into account adequately. (105-6)

Alternate first- and third-person references aside, Brandon’s choice of topic in SF1 appears to align her more strongly with an outsider perspective. Steeped in hard-nosed criticism of various aspects of the conventional wisdom among conservationists, the article was especially appealing to its immediate audience of natural scientists. (The conference at which Brandon presented the pre-publication version of the chapter was attended predominantly by biologists.) Brandon explained that biologists are generally put off by what
they see as a lack of realism in conservation: “The biologists tend to like what I say because what I say for the most part resonates with the world they see. That it’s not all nice and wonderful and easy and that this stuff [conservation] is really hard.” They also appreciated the broad scope of Brandon’s chapter, her attempt to analyze as she put it, “the whole idea of sustainability,” an idea, she noted, that many biologists struggle with. By her own admission, she “covered a lot of territory squashed into one place” which was another feature (density) that made the chapter appealing to the biologists in her audience. Finally, AF1 also illustrates Brandon’s dual insider/outsider identities through alternating first- and third-person references to conservationists (emphasis added):

An illusion exists among conservationists that what they are doing is conservation—when the case studies make it clear that they are really doing large-scale social interventions in complicated settings. (416-17)

But we have lost sight of what we are really trying to do with parks. In trying to make them socially acceptable and “accepted,” we are holding parks responsible for curing structural problems such as poverty, unequal land distribution and resource allocation, corruption, economic injustice, and market failures. (418)

Brandon’s roles and the features of her discourse in the scholarly and advocacy forums of conservation are not identical. When participating in advocacy forums, she appears to assume a more extensive role in the various stages of developing, producing, and disseminating publications. She tends to be drawn into much more internal negotiation and, in many cases, significant revisions to accommodate her NGO managers’ concerns about the political implications of publishing self-criticism. She also varies in the extent to which she acknowledges her own commitment to the cause of conservation. However, my study also shows that Brandon is deliberately consistent in certain key rhetorical choices regardless of forum, which I believe suggests that an increasingly blurred boundary divides advocacy and science in conservation.
Minimal Use of the Rhetoric of Knowledge Production

One general rhetorical feature of Brandon’s discourse in both scholarly and advocacy forums, manifested in a number of related features in the four texts, is her minimal use of the discursive conventions that tend to be associated with the rhetoric knowledge production. Specifically, she generally rejects the meticulous claims-backed-by-detailed-evidence arguments typical of scientific texts aimed at establishing new knowledge claims, nor does she provide readers with detailed accounts of her study methods and the results as a way of buttressing the validity of new knowledge claims. Instead, she tends to write authoritatively, explaining her various points in an informative rather than argumentative fashion; statements that in other contexts might appear as controversial claims requiring supporting evidence are often treated as accepted knowledge. References to secondary sources are infrequent in all four texts. As Table 4.1 shows, average citations per page are 2.1 or lower in all publications, regardless of forum. This suggests that, in these four texts at least, Brandon is not particularly concerned with embedding her knowledge claims within a larger body of disciplinary literature.

Table 4.1. Citations per publication.

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<th>AF1</th>
<th>AF2</th>
<th>SF1</th>
<th>SF2</th>
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We might expect Brandon’s discourse in advocacy forums to take this shape, since the primary purpose of such a forum is not necessarily to establish knowledge claims in the
meticulous fashion typical of the rhetoric of science. And, indeed, the advocacy-forum texts bear this point out. AF2 is a particularly good example since it purports to report the results of Brandon’s primary research on the effectiveness of a particular conservation strategy. Yet the text lacks the features we commonly expect with publications that report research. For instance, it does not confine itself to reporting only the results of the 21 case studies the authors carried out. Instead, the report is presented as a seemingly comprehensive treatment of the subject of ICDPs. The issues associated with designing and implementing effective ICDPs are foregrounded and dictate the organization of the report more than the methods and results. Methods are described, but only in general terms: seven short paragraphs are devoted to describing the study’s methods, not in a distinct chapter of the report but at the end of the introductory chapter, after the authors provide three pages of background material on the integration of conservation and development.

Similarly, study results are not presented in a chapter or section dedicated solely to that purpose. Instead, they are cited periodically throughout the report as examples that support the arguments the authors make in favor of or against a particular approach. For instance, in a section on the importance of thorough information gathering, the authors first present their claim that “ICDP design should be based on detailed site-specific studies of the local socioeconomic, political, and cultural contexts” (13) and then list several brief examples from the case studies that support their claim, e.g., “In Costa Rica the Boscosa project carried out socioeconomic surveys, land-use studies, and forest inventories, which were used to initiate planning with the local community and to provide baseline data for the project. The process of collecting this information provided an important opening to effective
local participation" (13). The authors’ interpretations of their study data, in other words, are presented as claims rather than conclusions.

The report’s apparent purpose of functioning as a comprehensive treatment of its subject is also indicated by the numerous categorization schemes created by the authors throughout the report. Because ICDPs (itself a term coined in this report) were so new at the time the report was published, the new categories of attributes, etc., that Brandon et al. provide to their readers likely facilitated future discourse about the topic. In Chapter 4, the authors classify ICDP components as (1) protected area management, (2) buffer zones, and (3) local social and economic development. In Chapter 5, local social and economic development is divided into five sub-categories: (1) natural resource management outside core protected areas, (2) community social services, (3) nature tourism, (4) road construction for market access, and (5) direct employment. Chapter 7 classifies the three major roles for organizations participating in ICDPs as (1) project implementation, (2) management of the protected area, and (3) source of funds. Additional examples are scattered throughout the report.

As I noted above, these features of advocacy discourse are not necessarily surprising, given the purpose of the forum in which the discourse is published. More relevant to my argument that the boundary between advocacy and science is blurring in conservation discourse is evidence that the same is true of Brandon’s scholarly discourse, that is, Brandon chooses not to use much of the knowledge production rhetoric described above when addressing her work to scholarly forums. In SF1, for instance, Brandon commonly presents sweeping claims and generalizations supported only minimally—if at all—by backing evidence, as in the following examples:
environmental NGOs, in testimony before the U.S. Congress, have provided specific recommendations on how to increase [the World Bank's] attention to poverty alleviation. (97) [No transcripts of testimony are cited.]

The success of some communities, often indigenous ones, in managing resources have gained widespread notice. (101) [No examples of such successes or notice are given or cited.]

Parks are not being well maintained. There has been diminished attention to conservation basics, such as collection of basic ecological data, infrastructure development, pragmatic management planning, boundary demarcation, guard training, and enforcement. (107) [No examples or citations are given to support this claim.]

To be sure, many claims and generalizations are supported in a number of ways. But the chapter is not characterized by what I would consider a meticulous use of evidence.

SF2 is similar in that it contains numerous passages full of statements for which the scrupulous conventions of scholarly discourse might lead to us to expect some documentation or support. A section entitled “Evolving Conservation Approaches,” for instance, contains this passage, in which I’ve marked with an asterisk statements that seem potentially controversial rather than accepted knowledge:

Many of these protected areas, particularly those in the tropics, are experiencing serious and increasing degradation as a result of large-scale development projects, expanding agricultural frontiers, illegal hunting and logging, fuelwood collection and uncontrolled burning.* In most situations, park managers have inadequate resources to do anything to counter these forces.* Agencies charged with management responsibilities lack the inclination or capability to identify or address people-park conflicts (Hough, 1988).

The lands adjacent to parks are often remote and marginal, which has contributed to their protection.* Yet increased pressure from human encroachment is now a problem in many of these areas as a result of increased population growth in traditional communities and/or migration and settlement.* The people in these areas are often extremely poor, with limited access to government services and no political power. (558)

Much of the article is similar to this passage in tone and purpose: the authors offer statements as information, not as claims, and thus supporting evidence in the form of examples or references is included infrequently.

Detailed references to data are also infrequent in passages in which the authors reference their research, as illustrated in these two examples:
Among the 23 sites visited, the most common ICDP strategy was promoting social and economic development among communities adjacent to protected area boundaries. (561)

Most projects generated some employment, although it was generally on a small scale. Improving the local ability to capture the benefits of nature tourism was an important component in a number of projects worldwide. (561)

The conventions of argument in scholarly journals (SF2 is a scholarly article) might lead readers to expect some reference to data proving, say, that promoting social and economic development in communities adjacent to protected areas was the most common strategy, but such references are uncommon in the article. According to the same conventions, we might expect the authors to describe their methods in some detail in order to prove the validity of their data, but methods are described only in general terms. Study methods are summarized in a single paragraph that lists the number of sites studied, the basic criteria for their selection, and a single sentence describing the method of analysis: “An analysis of ICDPs was recently completed to identify what strategies these projects have pursued and the extent to which investments in ICDPs represent cost-effective, sustainable or replicable approaches to the management of protected areas and certain categories of forested lands” (558).

Advocacy Regardless of Forum

Brandon’s limited use of the rhetoric of knowledge production in advocacy forums as well as scholarly forums suggests a shift in the boundary dividing conservation science and advocacy. Even stronger evidence of this shift, at least to the extent that Brandon’s case is representative of a larger trend, is the degree to which Brandon uses all the forums for her work—both scholarly and advocacy—for the purposes of advocacy. This was a dominant theme both of my analysis and of Brandon’s responses to my interview questions. This trend is manifested in a number of ways in Brandon’s discourse.
As Brandon explained, she expected AF1 and, more broadly, the book AF1 was published in, to have a significant impact on how conservation was being practiced. First, she thought her own organization at the time, The Nature Conservancy (TNC), would be influenced by it: “I thought it would make some kind of change in how TNC did business or conceived of things or something like that.” Second, she thought that, after being translated and distributed to conservationists in Latin America (where the nine parks that were the focus of the book’s case studies are located), the book would “really . . . have major ripples throughout Latin America.” But Brandon’s goal of influencing on-the-ground conservation went unfulfilled in this instance. In evaluating the impact of the book, she noted with some disappointment that people at TNC, who were “one of the first target audiences” for the book, paid no attention to it: “I don’t even think anyone [at TNC] has read it. So . . . I don’t think it did a whole lot within The Conservancy to get them to think differently about their projects in any way.” As for the book’s impact in the field, TNC’s failure to translate any part of the book, despite their promise to do so, “completely undermined” Brandon’s hope of influencing Latin American readers.

Nevertheless, her overriding goal of impacting the practice of conservation by influencing not only her colleagues at TNC but the broader world of conservation practitioners, especially in Latin America, influenced many of Brandon’s rhetorical choices as contributing author and editor of the book. She named park managers repeatedly and frequently linked her points to the types of work they do, as in the following examples:

So why does the social context matter? What can we say about it that is meaningful to park managers . . . ? (415)

Upon taking the job, a park manager’s first sensation has to be a tremendous sense of, What have I gotten myself into? (417)
Park managers can increase support for parks, and minimize local conflict, by helping to identify the reasons for scarcity . . . (430)

She was also careful to use simple sentences and minimal jargon, something, she explained, she has tried to do in everything she writes: “It doesn’t matter if it’s a journal . . . I try really hard, if I can, to have simple sentences.” In this particular book, as editor she insisted on adding summary paragraphs at the end of every section despite her co-editor’s objection that these were redundant. She wanted to include “synthetic materials” to keep readers “on track.” She described the book’s writing as “workmanlike,” observing that “in the first chapter, [which she wrote], it’s like ‘Okay, we have nine case studies and 13 themes. I’m going to go through each one of these 13 themes and I’m going to give you an idea about what the topic is, what we found there, and a little bit about what it means.’ In her view, these features of the text make it more accessible to readers beyond her community of scientists and other technical experts who are accustomed to reading more specialized discourse, and thus were intended to boost the text’s impact in the broader world of conservation advocacy and implementation.

Brandon also noted that the community of conservation advocates and implementers—not academics—were the primary audience for AF2. She and her co-authors had planned a separate book for the academics. With this primary audience in mind, Brandon focused on making the writing simple, direct, and accessible “so the people in Latin America would be able to follow it,” because she felt like “that was [her] constituency.” She described the process of accommodating her various audiences in some detail:

We recognized a lot of the terms were going to be jargony for some of our audience and so [we tried to make it] a simple, straight story that wasn’t too long. . . . And divided in sections. We were trying to make it so you knew where you were going.
detailed table of contents so that you would be able to be guided through the whole thing. I think we started with it like in three chapters and then we kept on breaking it into more chapters to make them shorter—each chapter was shorter so that it was a little bit more self-contained. But we were very consciously trying to simplify things. None of that was done by anybody else. It was all us trying to go “All right, how do we guide the reader through it.” And I think we also tried to pay attention to topic paragraphs and a first sentence—we weren’t always good with it, but that was very, I know that was explicit in the writing.

Indeed, the report appears to be designed to meet the needs of many different types of readers. As Brandon noted above, the table of contents is highly specific, listing not just chapters but major sections within chapters. A three-page summary of the report’s contents is included before Chapter 1. Summaries of study data are available at three different levels of detail: “Brief description of the projects at each case study site are included in box 1.1 and their locations are shown on map 1.1. Summaries describing and analyzing each case study, and site maps, are included in the appendix. Extended versions of these summaries are available from the authors” (4). The descriptions in Box 1.1 include just three-four sentences per case study. Descriptions in the appendix, in contrast, are much more detailed. Each is several paragraphs long covering the following standard categories per site: area name and size, project name, implementing organization, responsible government agency, funding amount and source, project scope, region, project activities, and evaluation. A professional-quality map is also included for each study site. In addition, boxes are used frequently throughout the report (23 total) as a rudimentary device for dividing details useful to some but not all readers from the main body of text.
Another important rhetorical pattern linked to the advocacy orientation of AF2 is its forward-looking rather than evaluative presentation of the study. The authors move from summarizing information and describing their findings to interpreting their findings and prescribing appropriate ways to act on them early in the text, instead of waiting until the final chapter to draw lessons from the study. The early chapters, to be sure, are predominantly descriptive. Chapter 2 describes the basic features of ICDPs, drawing on examples from the case studies, and Chapter 3 continues in a descriptive mode, providing extensive descriptions of three contrasting ICDPs in order to illustrate the diversity of these types of projects. As early as Chapter 4, however, the mode becomes more deliberative and prescriptive. Here the authors argue, among other things, that buffer zones (lands adjacent to protected areas where use is partially restricted to provide an additional layer of protection) are considerably problematic in their implementation, and that ICDPs yet have much to learn from the field of rural development. In Chapter 5 they argue that ICDPs succeed more at fostering development than conservation. Chapter 6 shifts back to a predominantly descriptive mode, drawing on study data to explain different methods of involving local communities in ICDPs. Chapter 7 is more deliberative, arguing for certain types of involvement by supporting organizations. Chapter 8 builds on the central claim of Chapter 5 by arguing that ICDPs should be judged on explicit conservation outcomes. Chapter 9 is similarly prescriptive in that it offers several recommendations for improving ICDP design and execution.

A related rhetorical feature is the authors' frequent use of the imperative mood to direct readers in acting on the report's results. The following are a few examples (emphasis added):
It is not enough, then, for the social and economic development components of ICDPs to avoid the pitfalls of rural development; the ICDPs must also organize their activities to enhance—or at least not threaten—nearby protected areas. (29)

To facilitate the identification, dissemination, and adoption of sound technical practices, appropriate social and institutional arrangements must also be established. (33)

In the long term, local participation, as defined in this section, should be sought as much as possible. (47)

The imperative mood is used regularly throughout the report—it is not only found in the final chapter’s recommendations section.

Problematizing the notion of a distinct boundary between academic and advocacy discourse in conservation, Brandon explained that AF2 was widely read and referenced in both scholarly and advocacy forums: “It’s pretty much cited in almost anything about people-kinds of issues and conservation projects” and “had very high impact in conservation organizations . . . as well as in academia,” she said. Even a pre-publication draft of the report, which was distributed broadly within the report’s three sponsoring organizations and also among some of Brandon’s colleagues in the field, is still in circulation. Brandon commented that in her travels since writing the report she has often run into people who still “have Xeroxed copies [of the pre-publication draft], not even the ones that we sent, that had like a cardboard cover stapled to it, a heavier weight.”

The authors’ original plan was to produce two separate publications—a shorter report for readers in the field and a book-length treatment of the subject which “would be much more the thing for academic value and a bigger-picture kind of a framework.” The report was published first, and before the authors were able to start work on the book “the report [had] already made the crossover into the academic world.” Consequently Brandon et al. decided that, while “a book would be nice for our glory,” it was no longer needed because “the messages already got there” (i.e, to the academics).
The report was seen as a seminal work on the topic of integrating conservation and development for at least two reasons. First, it was ground-breaking. As Brandon pointed out, “it was the first thing published that was sort of a broad, sweeping review [of the subject]. There was nothing else out at the time.” Second, it was published at a time when conservation was just beginning to emerge as an academic discipline. In the early 90s, a time, Brandon noted, when conservation biology—the most established conservation discipline—was itself a new field, academics were just beginning their attempts (still ongoing) “to define academic disciplines that would cover . . . the social aspects of established disciplines.” So because, in Brandon’s words, her report was “aiming to be somewhat interdisciplinary,” it caught the interest of readers in many different disciplines linked to conservation.

Shifting to scholarly discourse, we see that Brandon’s purpose of influencing and guiding advocates and implementers of conservation is unwavering, even when she uses an academic forum to publish her work. She appears to believe that scientists at advocacy organizations—her colleagues at the Center for Conservation Science (CCS) in particular—are publishing their work in prestigious scholarly forums in order to impress current and potential donors and generate positive media coverage (in the media outlets of the US and other developed countries) for a particular conservation issue, but aren’t doing enough to reach people in the field:

Unfortunately we’re still on this thing where, What impresses [the director of CCS] and [the organization’s main benefactor]? Well, [articles in] Science and Nature. And . . . in the press they are picked up, and they get to one audience. . . . But if I’m out talking to people who are park managers and I’m like . . . “Did you hear about this big thing in Science recently?” and they all go “Huh?”
In her own work, she seems to be committed to overcoming this problem.

For example, Brandon envisioned the audience for SF1 as being much broader than the conference of conservation scientists to whom she presented a pre-publication draft. Like most of her scholarly discourse, she explained, she wrote SF1 not because she “was looking for things to pad [her] CV” but because she was “looking to try and do things that are going to get out there and somehow make a difference.” Making a difference in 1992—the year the conference took place—meant targeting a different audience then than it does now. Back then, she had international NGOs, the World Bank, and the “donor community” in mind as a primary audience, with the idea that “strategic thinking about how we need to do these things has to come from above.” Now, 10-plus years later, she believes the same type of book should also be targeted to the “user community,” i.e., park management professionals and authorities in developing countries who only recently have become a “viable audience” for these kinds of publications because they now have the technical capacity and training to understand and implement this kind of knowledge.

Similarly, Brandon explained that her primary audience for SF2 was not the academic community but rather the NGO community: “I thought that the article, because it was shorter [than AF2, where her research was initially published], it would hit the development community [the World Bank, etc.] by and large, [and] would have a better impact at improving project quality.” In the months and years following SF2’s publication, however, she was surprised to see that it had a significant impact on the academic community and, to her disappointment, minimal impact on the practitioners: “It was really well received and looked at by the wrong people. I shouldn’t say wrong—by my non-target audience.” The article is frequently cited, she explained, by academics in fields like international agriculture
and development sociology, but rarely by authors in NGOs, especially conservation NGOs. Occasionally, Brandon told me, she will check the references of a relevant publication to see if SF2 or AF2 (both based on the same research) is cited. Generally, academics cite SF2 and authors in advocacy organizations cite AF2, despite her desire to reach non-academics with SF2.

Brandon’s audience of advocates and implementers guided a number of SF2’s rhetorical features, including the text’s emphasis on the practicalities—as opposed to the theory—of executing successful projects integrating conservation and development objectives. Drawing on first-hand observation and analysis of these types of projects, Brandon and her co-author carry their discussion beyond the expository sections introducing the ICDP concept to include an extensive consideration of how the “performance” (562) of ICDPs might be improved. A key rhetorical device in this section is the unanswered question. The authors pose over 30 questions at various points in their discussion that are left unanswered, presumably because project designers must confront them on a case-by-case basis. For instance, in a passage where they discuss balancing the need to research a project versus the need to act to stave off irreparable environment damage, the authors ask: “What is the appropriate tradeoff between information gathering and urgency? How can these be balanced? What does this tradeoff mean for selecting sites? Should areas where the problem is urgent be excluded from the ICDP approach?” (563). The same rhetorical pattern is repeated at least seven more times before the article ends. It appears to show that implementers—individuals and groups on the “frontlines” of conservation—in addition to academics are a primary audience for this text.
For similar reasons, she wrote SF2 with a “checklist-y” structure (her term) which she described as a “really non-academic” feature. The following passage is an illustration:

These systems within indigenous cultures are prone to breakdown . . . under the following conditions, if: (i) there is a substantial increase in the local population; (ii) the area available for exploitation is substantially reduced; (iii) a few commodities increase in value and become more heavily exploited.

Several similar passages appear throughout the text, and they were included for the specific benefit of people in the field doing conservation and development work. As Brandon explained, she used lots of lists as a way of saying “Okay, everyone doing these projects . . . I’m assuming you’re not going to read [a long book on project design] so here are the seven things you’ve got to do.”

Finally, Brandon explained that, in an attempt to reach the people on the frontlines of conservation, she tried to be more “direct” in her writing, especially in describing negative examples of projects to support her points. She also tried to make the article “really clear” and avoided making too many assumptions about what her readers knew. Finally, she kept the focus on practice rather than theory. As she explained, “If I was writing it to try and get tenure I would have couched it in completely different ways. I would have tried to tie it to some theoretical basis.” She reflected on the irony that her first article in the premier journal of her discipline was so devoid of academic theory: “If I’d been in grad school and you’d said ‘Oh, what’s the first article you’ll ever do in World Development?’ I would have said ‘Oh, it’s got to be political ecology . . . and here I do something that’s devoid of any theory at all, which would have been shocking to me in graduate school.’
Conclusion

I designed Stage 2 of my study to investigate, in a more focused way, the central finding of Stage 1—presented in Chapter 3—which is that scientists at organizations such as CCS represent a new mode of “mission-oriented” knowledge production that takes place on, and is redefining, the boundary that divides forums of advocacy and science in conservation. My aim in Stage 2, which comprised a close analysis of and semi-structured interviews about one scientist’s published discourse in scholarly and advocacy forums, was to illuminate features of discourse in these two types of forums that appear to be more or less stable.

The results of my study suggest, first, that one key distinction between scholarly and advocacy forums in conservation science is simply the model of knowledge dissemination. Scholarly forums continue to conform to the “information chain” model, whereby researchers are confined to their role as knowledge creators and are minimally involved in designing, producing, and disseminating the publications that contain the knowledge they produce. Such a model is understandably frustrating to individuals like Brandon and organizations like CCS whose overarching purpose in writing and publishing their work is to disseminate new knowledge to those audiences who can best use it to improve conservation. For this reason, the “mixed” or “gray” model (Owens 1997, 2002) appears to be growing hand-in-hand with NGO science; mission-oriented researchers and organizations like Brandon and CCS can circumvent the conventional information chain and target their discourse based on their rhetorical purposes rather than the profit-driven business model of commercial and academic publishing. One important consequence of this is that scientists like Brandon may find themselves deeply involved in aspects of technical communication and information design, whether or not they are qualified for or interested in that kind of work. This model also
presents a new set of challenges as NGO scientists learn to balance the standards and conventions of their professional discourses and those of the communications professionals—public relations experts, etc.—who may be called upon to “translate” an advocacy organization’s science for non-technical audiences. Brandon appears to still be grappling with this tension; she knows scientists generally do a poor job of reaching non-scientific audiences, but is nervous about relinquishing control of the message to her colleagues in the communications department at EarthConserve.

Second, my study indicates that the internal reviews of research reports within advocacy organizations are often intense, complicated, and frustrating processes for NGO scientists who may often be caught between adhering to the discursive norms of science and accommodating the political interests of their organizations. Because advocacy-forum publications function, either explicitly or implicitly, as evidence of both the sponsoring organization’s policies and efficacy, their authors may find themselves expressing their work in a more nuanced fashion than they might prefer. Scholarly forums, not surprisingly, offer more freedom for NGO scientists to engage in discourse about conservation without concerning themselves as much with what the political consequences might be.

A third finding is that NGO scientists may assume a disinterested, objective persona when engaging in scholarly discourse more often than they do in advocacy discourse. This isn’t a particularly surprising fact, given that science is culturally defined in part by a norm of disinterestedness. However, by finding specific instances where Brandon’s dual persona as objective observer versus committed advocate were manifested in her rhetorical choices, I hoped to show a significant difference between discourse in scholarly and advocacy forums. This is a concrete distinguishing feature of the texts; the previous two findings relate more to
the context of discourse in these two types of forums. Considered together, I believe they show that the distinction between scholarly and advocacy forums is a meaningful one in conservation discourse.

However, I also believe my study indicates that the boundary between advocacy and scholarly discourse in conservation may be shifting—or at least blurring—in significant ways. Brandon’s discourse in both advocacy and scholarly forums is characterized by a noteworthy lack of the sort of rhetoric we tend to associate with knowledge production, i.e., an argumentative structure whereby claims are well-supported with evidence from the researcher’s study, detailed descriptions of methods in order to give one’s findings validity, and extensive references to supporting literature. All of these features are less common in Brandon’s publications than I initially expected to find. Given the purpose of advocacy discourse, which is more focused on knowledge implementation rather than production, this finding perhaps is not all that surprising as it applies to AF1 and AF2. But I was surprised that Brandon’s scholarly publications were quite similar to her advocacy discourse in this respect. Such a finding, if true generally and not just in Brandon’s work, would represent a significant shift in the rhetoric of science in conservation. My study is only suggestive, of course, because of its limited scope. An expanded study of this particular characteristic of conservation science discourse would help confirm its significance.

Another significant similarity between Brandon’s discourse in scholarly and advocacy forums is the overriding rhetorical purpose she identified for everything she publishes, namely, to further the cause of conservation by communicating her work to those who are in a position to implement it. This was evident in Brandon’s comments on her work, in which she consistently noted her goal of reaching a particular audience of implementers. It
was also evident in her rhetorical choices for the texts—she appears to be continually concerned with making her discourse as accessible as possible to a broad audience so that her ideas and research can have a magnified impact on how conservation projects are designed and executed. As I mentioned above, this particular feature of her discourse is not all that surprising when associated with publications in advocacy forums, but because it appears to be equally prominent in her scholarly discourse, I believe it may indicate a shift in how the discourse of science in conservation is delineated from other spheres of rhetorical activity. I was interested to note that, in her view, none of her publications had met this particular rhetorical goal to her satisfaction. A fruitful extension of this initial study may be to examine whether other NGO scientists share Brandon’s consistent frustration with this aspect of their discourse, and why.

As I have been careful to note all along, this study is, at best, only suggestive of a meaningful shift in the boundary separating science and advocacy discourse in conservation. Because I designed Stage 2 of my study to focus on a single individual’s work, it is quite possible that the features I’ve identified as trends in the discourse I analyzed are idiosyncratic to Brandon’s use of rhetoric rather than a general indication of how NGO scientists are communicating their work within contrasting forums. The best I can hope for is that my findings will provide useful starting points for a broader examination of this subject.
CHAPTER FIVE

CONCLUSION

In my dissertation I have argued that, in the field of conservation, the boundary separating science from advocacy appears to be undergoing a shift as the number of research scientists at conservation organizations grows. Drawing on my interviews with scientists at a prominent conservation non-governmental organization (NGO), I tried to identify and analyze the kinds of rhetorical work NGO scientists engage in as they attempt to participate effectively in the forums of both science and advocacy. I also carried out a focused analysis of the work of one scientist at the same organization to identify specific features of her discourse in contrasting forums that may indicate a shift—or at least a blurring—of the boundary between science and advocacy in conservation. In this concluding chapter I summarize the main points and significant findings presented in each of the previous four chapters and discuss various limitations of my study as well as several possible avenues for future research on the topics presented here.

Key Framing Concepts

In Chapter 1, I summarized scholarly literature on a number of topics relevant to my study. I have highlighted the most important concepts framing my study below.

Boundary work and the demarcation of science

An argument such as mine, that the boundary defining conservation science is shifting, rests on the assumption that science is best defined as a constructed cultural space rather than an essentially distinctive intellectual activity. I began my dissertation, therefore, by establishing the basis for this assumption. I first surveyed key essentialist demarcations of
science. Some have focused on qualitative aspects of scientific statements; various philosophers have argued that science requires statements that are verifiable by empirical evidence, or, in Popper’s view, falsifiable, meaning that some empirical observation could, in theory, contradict or refute it (Gieryn, “Boundaries of Science”). Constructivists counter that any ostensibly essential feature of science—be it some aspect of its knowledge claims or social norms—does not exist apart from the historical and cultural context within which science is practiced. Seemingly essentialist norms and features, thus, are actually continually reinterpreted through the daily practice of working scientists.

Gieryn argues that, once we accept that science is culturally and socially constructed, one useful way of understanding how it is defined within a larger social and cultural milieu that encompasses many other distinct professional spheres is through the concept of “boundary work.” Boundary work is “the attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activity as non-science” (“Boundary-work” 782). Gieryn is specifically interested in “credibility contests” in science, which are situations where individuals or groups vying to be classified as scientists construct “statements of professional ideology” (Cultural Boundaries 7) that, if successful, expand or protect their professional authority or exclude rivals. Taylor extends Gieryn’s notion of boundary work by arguing that it occurs not just as a result of contests for authority between rival professional groups, but in response to the “larger social milieu in which it is embedded” (Defining Science 108).
Forum

Because mine is a rhetorical study, I chose to examine shifts in the boundary defining conservation science by examining the discourse of scientists whose work represents the shift. This emphasis, in turn, led me to focus on forums, meaning the locations where a particular group’s discourse is published or “made public.” Forums are useful objects of rhetorical analysis because of their connection to corresponding discourse communities. As Porter notes, forums provide a “convenient and practical starting point for . . . inquiries into discourse communities” (95). The bounded cultural space of science posited by constructivist demarcations of science could also be labeled a discourse community. The world of conservation advocacy is another cultural space and discourse community that scientists are increasingly active in. My study focuses on the boundary separating the two cultural spaces of science and advocacy in conservation. I analyze this boundary by examining a handful of scientists’ participation in forums linked to both of these cultural spaces/discourse communities.

Mode 2 knowledge, the new information environment, and gray literature

The growth of science in conservation NGOs is part of the spread of what Gibbons et al. have labeled “Mode 2” knowledge production. Mode 2 is “mission-oriented”; Mode 1 (what we most often associate with traditional science) is “curiosity-oriented.” Research in Mode 2 is directed towards solving real-world problems—like environmental degradation—rather than building a body of disciplinary knowledge. Mode 2 is not necessarily disseminated in the traditional forums of science, e.g., scholarly journals and books. Rather, its primary audience is the knowledge creators themselves and those in a position to apply
the knowledge towards solving the problem at hand. Thus, Mode 2 is complemented by the “new information environment” in science, as identified by Owens (“The New Dissemination”), according to which knowledge-producing organizations are producing and distributing more and more publications completely outside the traditional channels of commercial and academic publishing. These sorts of publications are commonly referred to as gray literature. Gray literature represents an important—and often overlooked or discounted—forum for scientific discourse within advocacy organizations like conservation NGOs. One of my aims in carrying out this study was to highlight attributes and functions of gray literature as part of a larger analysis of the types of discourse NGO scientists in conservation participate in.

**Methods**

To investigate my central research question, i.e., how is the discourse of conservation science changing as a result of the growth of science at NGOs?, I studied the work of seven research scientists employed full-time in the Center for Conservation Science (CCS) at EarthConserve (both pseudonyms), a prominent international conservation NGO headquartered in Washington, DC. I designed the study to take place in two stages. Stage 1 consisted of 60-90 minute semi-structured interviews with each of the seven CCS scientists, where I asked for their perspectives on practicing and communicating science from within an advocacy organization. In Stage 2 I examined more extensively the work of one of the scientists from Stage 1. I carried out a discourse analysis of two of her publications published in scholarly forums and two from advocacy forums and then conducted a series of semi-structured interviews with her to discuss the context of and reasons for the rhetorical features
I identified in my analysis. I analyzed the interview data in an ad hoc fashion, reviewing transcripts in search of patterns, themes, and points of comparison and contrast. My discourse analysis followed Barton’s model of “rich feature analysis” (“Linguistic Discourse Analysis”) in that I identified discourse features that appeared to point to a relation between text and context. I took a bottom-up, inductive, data- rather than theory-based approach to my analysis.

In addition, I was employed as a technical editor at CCS during the time of the study, a position that influenced the project in at least two ways: (1) equipped with an insider’s perspective on the worlds of conservation advocacy and science, I was a better-informed interviewer than I would have been as an outsider; and (2) because my research subjects were also my colleagues, I believe they were comfortable giving candid answers to my questions.

Results

Drawing on ideas about boundary work, forums, and the intersecting concepts of mission-oriented (Mode 2) knowledge, a new information environment in science, and gray literature, I designed a two-part study of a group of NGO scientists in conservation to investigate the implications for discourse of the intersection of science and advocacy. Chapters 3 and 4 present the results, respectively, of Stages 1 and 2 of my study. In what follows, I summarize the key findings from both stages of my study.

Stage 1 findings:

- **Boundary work in NGO science.** I found evidence that scientists at CCS are doing boundary work as part of a credibility contest, to use Gieryn’s term, with some of their academic counterparts, who in some cases consider the work of
NGO scientists to be biased. In their boundary work, CCS scientists emphasize both the organization’s scientific accomplishments and its culture. The organization has been very successful in publishing its work in prestigious scholarly forums and in raising money for research. Indeed, the drive to publish in prestigious scholarly forums appears to be motivated above all by the organization’s desire to establish and maintain its credibility as a scientific organization—one scientist noted that scholarly forums are not needed to carry out EarthConserve’s specific conservation projects. In the view of the scientists I interviewed, CCS has also cultivated a science culture by embracing the norms of universalism, disinterestedness, organized skepticism, and intellectual communism.

- **Scientists’ involvement in advocacy forums.** While scientists at CCS seek to emulate the social norms of academic science and participate actively and successfully in its forums, they take on additional rhetorical work as participants in the forums of advocacy. In particular, they emphasize applied knowledge in their published discourse, they strive to communicate their work in a way that gives them an advantage over the organizations with whom they are competing for funding, and they are focused on persuading lay audiences of the importance and urgency of conservation.

**Stage 2 findings:**

- **Gray model of knowledge dissemination.** The “gray” model of knowledge dissemination common within advocacy forums—which rely on self-produced
and self-distributed publications (gray literature) to communicate their work—
often results in NGO scientists’ being involved in all stages of the publishing
process, from conceptualizing publications to researching and drafting them,
editing them, dictating document design, and developing plans for distributing the
publication after it is printed. This contrasts with the “information chain” model
associated with scholarly forums, a model that NGO scientists often find
frustrating because it can stifle dissemination of applied knowledge to
conservation implementers.

- **Political constraints in advocacy forums.** A key distinction between discourse
  in scholarly and advocacy forums is the extent to which advocacy scientific
discourse is often modified, through internal pre-publication review and revision,
to accommodate the political interests of sponsoring organizations. This constraint
appears to frustrate NGO scientists, who feel bound by the norms of science to
provide a disinterested account of the results of their research. Scholarly forums,
not surprisingly, are not as constrained in this way, and NGO scientists may
therefore find them more useful as venues for open discussion of conservation
problems.

- **Maintaining disinterestedness.** As we might expect, NGO scientists may be
more apt to identify themselves as conservation advocates when participating in
advocacy forums than in scholarly forums. However, the distinction is not entirely
clear—in the discourse I evaluated, at least, the dispassionate, objective language
of science was a common feature of advocacy texts (and not just scholarly
discourse) and first-person references to conservationists were common, though less frequent, in scholarly texts.

- **Minimal rhetoric of knowledge production.** All of the texts I analyzed, regardless of forum, were characterized by a limited use of the discursive conventions that tend to be associated with the rhetoric of knowledge production in science, e.g., claims meticulously supported with empirical data, extensive references to a larger body of disciplinary literature, or detailed descriptions of methods and results. We might expect discourse in advocacy forums to take this shape since these types of forums are not necessarily devoted to creating new knowledge. But the examples I examined from scholarly forums shared this characteristic, which may be evidence that the line separating science and advocacy in conservation discourse is shifting.

- **Advocacy regardless of forum.** Perhaps the clearest indication of a boundary shift in the discourse of conservation science is that the overarching purpose of all the discourse I analyzed for Stage 2 is to further the cause of conservation. The author may publish her work in the forums of both scholarship and advocacy, but she always sets out to impact the practice of conservation—and not just the theory—with what she writes. She always strives to create simple, direct, accessible publications that meet the needs of people on the frontlines of conservation—usually park managers in developing countries and similar audiences. Her article in a premier scholarly journal illustrates this point well. She deliberately avoided any mention of theory in it and instead kept the emphasis on
practicalities because she considered implementers and not academics as her principal audience.

Study Limitations

At different points in the previous four chapters, I have emphasized that, because of various limitations, this study offers only suggestive results, and that a broader, more systematic analysis would be needed before drawing definitive conclusions about shifts in the discourse of conservation due to the growth of NGO science. Here I list several limitations that are clear to me now, as I reflect retrospectively on my study:

- **Sample limitations.** Perhaps the most obvious limitation of my study is the small number of participants. I only interviewed seven scientists for Stage 1, and Stage 2 focused on the work of a single scientist at the same organization. Any study, of course, is more conclusive if it has numerous participants. But the fact that my participants all belonged to a single organization magnifies this limitation. As Brown and Kalegaonkar found in their broad study of the non-governmental sector in developing countries, NGOs are characterized by a pronounced “particularism.” Even NGOs committed to the same general mission, such as conservation, are generally unique in their core values and in the particular constituencies they serve. Any study that purports to show how scientists at conservation NGOs communicate their work should examine individuals and groups from a plurality of organizations.

- **Dual construct of interview data.** Interview data are doubly constructed and thus inherently limited. These data are constructed first by the research subjects, who offer
subjective accounts of their experience and observation in response to interview questions. The data are then constructed again when the researcher interprets the data as part of his or her account of the study. Because I rely extensively on interview data to support my analysis, my study is vulnerable to this point of critique.

- **Second-hand perspective on academic discourse communities.** Much of my analysis focuses on contrasts and overlaps between the science of NGOs and that of academics. Yet my study is based solely on the experiences of NGO scientists. To be sure, my research subjects are well-acquainted with the academic world of science—they all hold PhDs and, in several instances, have held jobs in academia. I therefore believe their accounts of academic science are useful. However, I expect that my analysis would be deeper if I included conservation scientists working in academic settings.

- **Only one participant in Stage 2.** In Stage 2 of my study, I focus extensively on the work of a single individual, examining her discourse in publications that I argue represent the conventions of scholarly and advocacy forums. I draw conclusions about the influence of advocacy on conservation science based on patterns I identify in her discourse and in her commentary on her rhetorical choices. However, it is possible that at least some of these patterns are better explained as the author’s idiosyncrasies. Moreover, her training and experience as a social scientist have of course shaped her perspective in ways that are different from someone trained in, say, conservation biology or climatology. A single social scientist obviously cannot speak for the diverse world of science in conservation. A broadened version of this study should include subjects from a plurality of disciplines.
Despite these limitations, I believe the results of my study provide a valuable indication of trends in the discourses of conservation science and advocacy. My research subjects, while small in number, belong to one of the world’s largest and most influential conservation organizations, especially in terms of its scientific resources and accomplishments. Their perspectives on the dynamics of communicating science from within an advocacy organization therefore carry a lot of weight and ought to provide a solid basis for future research on this subject.

Directions for Future Research

The limitations listed above suggest numerous directions for expanding this study. One obvious direction for future research would be to examine the same basic research questions drawing on a broader sample of participants from a range of disciplines and organizations, both academic and non-governmental. My study could also provide the basis for a true ethnography of scientists in conservation organizations, with methods that include not just interviews and document analyses but observations aimed at generating “thick descriptions” of the social and cultural aspects of their activity. Yet another direction would be a study that compares and contrasts science in conservation with another field that is characterized by significant academic and advocacy components—a number of public health issues such as HIV/AIDS would likely provide an interesting comparison.

Gray literature is a topic that I introduced and examined briefly in this study, but that could be investigated much more broadly, as this category of scientific and technical discourse was initially unfamiliar to me and, as far as I can tell, is unknown to most scholars of professional and technical communication. More attention ought to be given to describing
the particular rhetorical exigencies this type of discourse addresses, as opposed to other conventional types of scientific and technical publications. A robust analysis would include samples from a range of fields, not just conservation, and a range of organizations. Much gray literature, for example, is produced by government agencies in addition to NGOs.

Finally, I believe it would be quite useful to study the one audience my research subjects seemed to be continually frustrated in reaching: the implementers—the people on the frontlines of conservation projects, often in remote parts of developing countries. What are their characteristics as audiences for scientific discourse? What publications have been successful in influencing these audiences and why? Why do NGO scientists continue to struggle to communicate with them effectively through their publications? These and many other questions could provide the basis for a study that I suspect would provide a genuine service to scientists like those I studied, who constantly grapple with the rhetorical challenges of communicating science in a way that improves conservation.

Conclusion

The scientists whom I investigated for this study represent a growing trend whereby scientific and technical fields are becoming increasingly intertwined with political advocacy. As this study shows, scientists who leave the academy to work in support of a cause they believe in, such as conservation, often confront rhetorical challenges their academic training and experience have not prepared them for. To better prepare new scientists to meet these challenges, they should not just be taught to design and execute research properly in their particular disciplines; they should also be taught to investigate the context that leads to arguments in science, to understand how arguments can be effectively structured to persuade audiences in the various forums connected to science and advocacy, to analyze and respond
effectively to the dynamics of information control within organizations whose mission includes advocacy, and to consider both visible and invisible audiences. As Waddell argues, “Our ability to address successfully many of the most pressing problems we face today—problems such as global warming, ozone depletion, habitat destruction, and hazardous-waste disposal—requires sophistication in both scientific and humanistic disciplines” (55). If scientific training includes increased emphasis on the rhetoric of science in non-academic contexts, scientists will better understand how to assess and deploy the rhetorical resources at their disposal as members of advocacy organizations.
APPENDIX 1: TYPES OF QUESTIONS ASKED IN STAGE 1 INTERVIEWS

Background
1. Can you give me a short overview of your educational background and your career up to this point?
2. Why did you pursue a scientific career at CCS rather than at a more “disinterested” institution such as a university?
3. Why were you interested in coming to work for CCS?

Comparisons to science in other settings
1. What differences are there between what you and your colleagues do here at CCS and what a similar group of scientists might be doing at a research university?
2. How is the rewards system different for you as a researcher at CCS than it would be in the academic world?
3. What are the differences between the science at CCS and the science in an industrial setting, say, in a pharmaceutical or biomedical company, for example?

Balancing science and advocacy
1. What is the Center’s standing within the larger scientific community? What is its reputation? How did it get that reputation?
2. Is there a schism between NGO and academic scientists? How would you describe it? What is your direct experience of it?
3. What are the challenges of establishing and maintaining credibility and prestige in the scientific world and the conservation world at the same time? What are some of the competing interests of the two worlds? What criticisms are likely to be directed at CCS and similar organizations from the scientific community? From the conservation community? How successfully is CCS managing its reputation in both worlds?
4. How is CCS regarded within the larger conservation community?
5. How does the work CCS is doing compare to the work of its peer organizations?
6. In your opinion, what should the relationship between science and advocacy be within the field of conservation?
7. Do you see any tension between your role as advocate and your role as scientist? How does this tension manifest itself? Any examples of conflicts or problematic situations that have arisen in your work as a result? How do you reconcile the two roles, if you see any tension?
8. Have there been any instances where you had to adjust or modify your research agenda and goals in order to better conform with EarthConserve’s stated mission?
9. How would you characterize the difference between basic and applied research? How much of your research is basic? Applied? Is applied research undervalued in the academy? If so, why?
10. How does the fact that CCS relies on donors for resources influence the type of science it does?
Publication forums
1. In a recent editorial in *Conservation Biology*, the director of CCS noted that “in-house printouts” (or gray literature) from conservation NGOs are widely adopted and used throughout the world by governments and funding agencies. Can you think of any specific examples of this? Why has NGO gray lit been influential (if it has) among governments and funding agencies?
2. How do you evaluate the credibility of published knowledge in various venues (print, electronic, gray lit)?

It seems to me that science and politics have competing rhetorical styles: scientific discourse is marked by a level an uncertainty and qualification aimed at preempting refutation from the scientific community, while political discourse needs to sound more certain in order to be persuasive. Would you agree with this characterization? Is this a tension that you’ve had to deal with in your work? Any examples of it in things you’re working on/have worked on?
MEMORANDUM

To: .......... From: Neil Lindeman
Date: .......... Subject: Consent to participate in study of advocacy in science

You are being asked to participate in a study of the communication practices of scientists working to promote a cause through scientific work. I ask that you read this document and ask any questions you may have before agreeing to participate in the study.

The purpose of this study is to explore the ways scientists respond to the competing and sometimes contradictory demands and conventions of science and advocacy. If you agree to participate, you will be interviewed for approximately 30-45 minutes. In this interview, you will be asked several open-ended questions about the scientific work you have conducted and published as a member of a conservation advocacy organization. Interviews will take place in your office building, preferably in a location free from disruptions.

I have obtained written permission from XXXXX, [CCS] Executive Director, to conduct these interviews, and your participation in this study is completely voluntary. To protect your identity, a pseudonym will be used in place of your real name and the names of the [Center for Conservation Science] and [EarthConserve] will not be revealed. Any discussion of your research activity will be as general as possible in order to protect your identity; however, it is possible that someone very familiar with your work or the work of CCS/EarthConserve might identify you as a participant.

Any questions or concerns can be addressed to me or to my supervising professor:

Margaret Graham  
Department of English  
Iowa State University  
Ross Hall 203  
Ames, IA 50011  
(515) 294-5203  
mgramham@iastate.edu

I am undertaking this project in order to give people—laypeople as well as research scientists—a better understanding of how scientists incorporate advocacy into their work, and I hope you will participate. If you agree to participate, please sign the statement below.

-------------------------------------------------------------------
I have read this memo and give my informed consent to participate in the study.

Signature Date
APPENDIX 3: TYPES OF QUESTIONS ASKED IN STAGE 2 INTERVIEWS

I customized the interview questions to connect to specific points of my textual analysis, but the sorts of topics my questions were intended to cover are indicated by the generalized versions below.

- How was this chapter received by its audiences, primary and secondary?
- Any particular decisions/principles/objectives guiding your use of sources? Was there any process for subjecting your sources to scrutiny prior to publication?
- What sort of pre-publication screening process did the publication go through, either formal or informal?
- In this publication, why did you make the choices you did in terms of specialized (or non-specialized) terminology?
- In this instance, why did you present (or NOT present) this point of critique in this particular way?
- Why did you use evidence to support your claims (or not) in this particular way?
- How did your commitment to doing research that furthers the cause of conservation influence how you approached writing this publication?
- The hypothesis that I’ve put forth in my dissertation is that there’s a “boundary dispute” going on in the field of conservation science as scientific capacity moves into the NGO sector, or, in other words, a credibility contest between the academics and the NGO people. Do you think that’s an accurate assessment of what might be going on? Do you have any experience of that?
- How is the fact that scientific capacity is moving more and more into the NGO sector changing the publishing practices and expectations of conservation scientists?
APPENDIX 4: SAMPLE OF TRANSCRIPT FROM STAGE 2 INTERVIEWS

NL=Neil Lindeman, KB=Katrina Brandon

NL: How was this report received by its target audiences? Was it effective? It sounds like, from we talked about earlier, that it’s had a pretty high impact.

KB: It’s pretty much cited in almost anything about people-kinds of issues and conservation projects, although now because of its 1992 date it’s getting really old, but it was really the seminal work on it—it was the first thing published that was sort of a broad, sweeping review. There was nothing else out at the time. So I think it had very high impact in conservation organizations—they’d be familiar with it, USAID, World Bank, a very broad number of people [thought?] that way, as well as in academia.

NL: Oh really?

KB: Yeah. The sad thing is the people in the field didn’t see it, but I’m not sure they see or have time to read much. That’s part of why...we had envisioned two products—I think I told you this—a shorter report that could be something for people in the field that wasn’t so long and then there was supposed to be a longer book that we thought would come out through Hopkins Press that had all the long case studies in it that we’d done.

NL: Oh that was the original plan?

KB: Yeah, so the thing that might interest you is the thing that we thought would be a greater value to people in the field actually ended up being the right length for people in Washington.

NL: Oh, I see what you’re saying. So the report was actually the shorter treatment of the subject matter and you were also considering a book. And the report was directed primarily to people in the field.

KB: Yeah, that’s how we had envisioned it. The book would be much more the thing for academic value and a bigger picture kind of a framework. But when we got the report out, there were a number of things that happened. One was funding and we kind of ended up going on to other jobs and stuff and our support at the World Bank ended. But maybe the bigger thing is that the report already made the crossover into the academic world really fast so we just said ‘well, do we really think we need’...I mean, a book would be nice for our glory but the messages already got there. Although they didn’t get to the audience we wanted.

NL: Do you have any thoughts in retrospect about why it didn’t to the target audience?
KB: I think part of it was distribution problems. That was before the Internet and if you’re sitting off in some field project somewhere it’s pretty hard to get information, especially pre-Internet.

NL: And why did it exceed your expectations in terms of popularity with the academic crowd and the NGO crowd?

KB: Well at that point there really wasn’t any discipline in this stuff in academia. It may be that it’s actually been more used recently—I mean I don’t know if people can even still get copies of it—but, as a citation—and I don’t even know if people read it anymore, they just cite it because they think they’re supposed to. That would be my guess. Because I don’t know if it’s in print anymore, even. It’s an interesting thing in that it’s kind of a seminal thing that you should cite but I don’t know if people can even get it easily.

NL: But it’s a standard citation in publications that are written for academics, not just gray literature and stuff like that.

KB: Right. Because it as the first one and so it became popular because there wasn’t anything else there, number one. And number two: I think in academia people have been trying to define academic disciplines that would cover sort of what I do—the social aspects of conservation. I mean, conservation biology was emerging at the same time as a discipline—there weren’t really departments in that, either. And so, you know, it was something that sort of aiming to be somewhat interdisciplinary.

NL: The publication itself?

KB: Yeah.

NL: And so that was why it had a broader impact is that it was being picked up from people in a number of disciplines. It caught the interest of people from a number of disciplines.

KB: Lots of different disciplines.

NL: Do you remember there being any sort of decision made or principles that guided your use of sources when you put together this report? Is that something you paid much attention or thought about?

KB: Yeah, actually, I do. I would have the been the one that put together almost all the sources because I was coming out of a PhD fairly recently and I was the one with all the rural development experience and background and so in doing this, in the way we sort of divided up the thinking and the write-up, I did the parts that were bringing forward things from academia into the process. Whereas Mike Wells, my co-author, had been a finance guy who had done a career switch.

NL: He wasn’t an academic.
KB: He didn’t know any of this stuff. He’d gone off and volunteered to work on an elephant project and somehow finagled his way into the World Bank as knowing something about this. And he was great analytically and committed to doing this, but he didn’t have any background in development to compare it to anything. So I was the one sort of bringing in the literature so I think a lot of what I would have brought in would have been stuff that I had read. I was bringing in stuff I would have known from grad school, probably. Again, nobody was publishing on the nexus of this stuff at all. Well, there were some World Bank publications that were relevant that I also would have known those publications and of those people from academia. Okay, I found my copy. Let’s see what I cited. Okay, there’s a couple things in here that Mike might have found.

NL: Uh-huh, but most of it’s from your previous academic work?

KB: Yeah.
APPENDIX 5: COMPLETE LIST OF BRANDON’S PUBLICATIONS

Publications in scholarly forums, reverse chronological order


**Publications in advocacy forums, reverse chronological order**


Brandon, K. “Moving Beyond Integrated Conservation and Development Projects (ICDPs) to Achieve Biodiversity Conservation.” *Tradeoffs or Synergies? Agricultural...*


REFERENCES


