

# Ecology Emerges

## *A Disciplinary Social Drama*

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### **Introduction**

Alan Gross has turned a few times to the social anthropologist Victor Turner to analyze public scientific controversies. Specifically, he has adopted Turner's model of "social dramas" to examine the recombinant DNA controversy of the 1970s and the "battle over black lung disease" in the late 1960s (Gross, 1984, 1990, 2006). Each controversy was handled in a way that was meant to "contain conflict, to see to it that public controversy leads not to revolution, but to a reaffirmation or reordering of existing social values" (Gross, 1984, 397). Working toward this end, each case followed the four-phase structure of the social drama: first, a "breach" occurred when one party defied some social routine at the expense of the interests of another party; second, the breach escalated into a "crisis," and the two parties grew more polarized; third, the crisis escalated into the "redressive action" phase, which involves some kind of outside adjudicator to settle the issue; and fourth, redressive action lead to "reintegration," whereby the two parties either established a new status quo together or remained stuck at odds. "Each phase," Turner points out, "has its own speech form and styles, its own rhetoric, its own kind of nonverbal languages and symbolisms" (Turner, 1974, 43). By focusing on the texts central to each phase of the recombinant DNA and black lung controversies, Gross's Turnerian analyses call attention to the broader tensions motivating public scientific controversies, encouraging us to "think of these debates in a wider political context, a legitimate perspective, since they are less about knowledge than about power" (Gross, 1984, 407). In what follows, I argue that, for these same reasons, Turnerian analysis is useful for understanding disciplinary controversies, that are, perhaps counterintuitively, less about knowledge than about power.

Gross's Turnerian analysis of the recombinant DNA controversy exemplifies a social drama wherein the balance of power between scientists and an offended public was called into question (Gross,

1984, 1996). The first phase, the breach, occurred when geneticists broke social convention by unilaterally creating and judging their own ethical standards and procedures. The second phase, the crisis, emerged when the parties crystallized into polarized factions as “the opponents of recombinant research wrested the issue from the control of the scientific community and successfully brought their case into the relatively uncontrolled arena of public debate” (Gross, 1996, 188). In the third phase, redressive action, the debate was brought before federal courts and local legislatures—“arenas expressly dedicated to social closure” (Gross, 1996, 188). Finally, the fourth phase, reintegration, did not play out as either the “healthier” or “less positive” versions of reintegration Turner lays out. Instead of the breach either being resolved (the “healthier” version of reintegration), and instead of the larger social recognition and legitimization of the breach as a simple fact (the “less positive” version), the social drama of recombinant DNA was left unresolved. Gross concludes that the recombinant DNA social drama is “one of a set of recurring conflicts concerning science and technology, all of which embody a similar clash of purposes: Americans want the benefits of a nearly totally protected science and technology, and none of the risks that nearly total protection entails” (Gross, 1996, 190). In other words, the recombinant DNA social drama is a manifestation of a larger social drama between the conflicting interests of a pluralistic public. But Gross’s analysis offers another important lesson: although “the public outcry [was] largely hysterical,” the drama could have been mitigated or avoided were it not for “the arrogance of experts,” who created the breach in the first place in “their attempt to circumvent in their own interests the checks and balances of an open society” (Gross, 1996, 192). Expertise was no barrier to a concerned public, who demanded a say in what goes on behind the usually closed doors of geneticists’ laboratories.<sup>1</sup>

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<sup>1</sup>Although Turner applies this kind of analysis to social dramas in contexts as varied as the Mexican Revolution of 1810, the Icelandic *Njál’s Saga*, and the incorporation of the Ndembu into the Zambian nation (Turner, 1974, 38-41), he and Gross emphasize that the point is not to reveal some fundamental similarity between all societies (Gross, 1996, 181). “The phase structure of social dramas is not the product of instinct,” insists Turner, “but of models and metaphors carried in the actors’ heads. It is not here a case of ‘fire finding its own form,’ but of form providing a hearth, a flue, and a damper for fire” (Turner, 1974, 36). Put in less poetic parlance, the ubiquity of the four-phase-structured social dramas is not due to an innate characteristic shared by all humanity; rather, it is the product of societies’ tendencies toward stabilizing routines and away from destabilizing conflicts.

The social drama of the recombinant DNA controversy reminds us that any perceived boundary between the science and society can be dismantled through intense rhetorical effort and a seemingly stable balance of power between the two can be upended. Gross's Turnerian analysis of the black lung controversy demonstrates how some sciences—in this case medical science—are more “socially saturated” than others; that is, some scientific practices and objects are more motivated by public and political concerns than by “a pure intellectual pursuit of best answers” (Bazerman and De los Santos, 2005). In the case of the black lung controversy, coal miners aligned with medical scientists to oppose coal industry leaders in a battle over whether black lung should be redefined as a disease. Ultimately, the miners, medical scientists, and a cadre of other sympathetic parties altered the very idea of what might be counted as a disease, and the miners were compensated appropriately. The volunteers who helped the miners achieve their goal continued trying to extend the successes of the black lung case into other industries, such as quarry and textile workers, but the miners were not interested in such sweeping social change, and so they no longer offered their support to such efforts. Moreover, politicians worried that more federal aid for workers compensation could quickly become too expensive. Thus, with faltering support and political resistance, the social status quo quietly reasserted itself (Gross, 2006, 153-61).

Gross's analyses illuminate an important point about boundaries between sciences and societies: because sciences are more or less socially saturated, such boundaries are negotiable. In other words, boundaries between sciences and publics are not fixed, but are rhetorically enacted and reenacted. In the decades leading to the recombinant DNA social drama, geneticists had enacted and reenacted a firm boundary between themselves and their publics. Through rhetorical effort, though, members of the public enacted a new boundary. Moreover, the enactments and reenactments of these boundaries enact different realities. The reality of occupational disease prior to the black lung social drama did not include black lung. But miners and their supporters enacted a new reality in which black lung is enacted as a disease. Any boundary between science and society is, as Annemarie Mol puts it, “part of a practice. It is a reality *enacted*” (Mol, 2002, 44, emphasis in original).

Disciplinary boundaries are also continuously enacted and reenacted through practices. Many scientists, however, argue that their disciplinary and sub-disciplinary divisions are the result of fundamentally different perspectives on a singular, fixed nature.

These scientists understand their differences as primarily epistemological rather than as a matter of enactment. This is true of many contemporary ecologists who, in their efforts to address the impending threat of ecocide, insist that their discipline must find a way to transcend epistemological differences to form a unified approach to planetary problems. Whereas RoS is not equipped to resolve epistemological disputes over which way of knowing is best, we are well equipped to mediate in social dramas where disciplinary fragmentation is enacted and reenacted.

In a recent clarion call to the field of rhetoric, technology, science, and medicine (RTSM), Carl Herndl and Lauren Cutlip write,

RTSM will thrive if it builds interdisciplinary alliances, engages with our colleagues in science to help manage uncertainty and the threat of ecocide, and develops specific strategies and tools to put into practice our disciplinary intentions to make a difference. We should move from talking *about* science to *doing* science. (Herndl and Cutlip, 2013, 7)

Herndl and Cutlip's goal is tied to their proposition that RoS scholars set aside epistemological questions about how we can better *know* something and focus instead on questions of how we can better *act* together—a proposition at the heart of what Scott Graham and Herndl call a “post-plural” rhetoric of science. Graham and Herndl argue that “[i]nterdisciplinary work has become a virtually inextricable part of contemporary scientific practice” (Graham and Herndl, 2013, 104). But the discipline of ecology, which is known for its cross-disciplinary reach, has struggled to cohere as a discipline in its own right. Graham and Herndl's work suggests that, when investigating how ecologists might achieve greater disciplinary cohesion, rhetoricians should shift their focus from what the epistemological differences are toward how disciplinary differences came about. “The rhetorical question,” they argue, “ceases to be ‘What is your disagreement?’ and becomes ‘Where does your problem come from?’” (Graham and Herndl, 2013, 123). Their post-plural rhetoric of science is rooted in Mol's theory of multiple ontologies, which, in contrast to incommensurability theory—“a theory of seeing and knowing”—is “a theory of doing and being” (Graham and Herndl, 2013, 110). Graham and Herndl conclude that those of us seeking to intervene in scientific controversies should take Mol's advice: “Don't attend to what is loudest, the fight, but shift your attention a little, widen it, and try to see what all this noise is part of” (Graham and Herndl, 2013, 123, referring to Mol, 2002).

Gross's analyses of public debates as Turnerian "social dramas" takes a step toward developing a post-plural rhetoric of science. Accordingly, I apply a Turnerian analysis to the social drama that occurred when a group of American ecologists lobbied to participate in the International Biological Program (IBP) during the 1960s and 70s. These ecologists thought that participating in the IBP was a major opportunity for ecology to establish itself as a legitimate scientific discipline in its own right. But it was an uphill climb for them to secure funding to participate in the program, and to overcome the widespread skepticism among their colleagues in biology who worried that the field of ecology was not mature enough—not coherent enough—to avoid an expensive and embarrassing public failure. Participation in the IBP helped legitimize the discipline of ecology among scientists and to quickly gain widespread social prominence as *the* scientific basis for addressing some of the most pressing issues facing humanity (Kwa, 1987). But the social drama from which ecology emerged is still reenacted in disciplinary controversies today. Ecology is an increasingly fractured discipline of sub-fields. I argue that many of the purported schisms that exist in ecology today are not a matter of un-aligned perspectives; rather, these schisms are differently staged reenactments of the same social drama.

## **Breach**

The social drama of the IBP began in the early 1960s amid a disciplinary milieu where divisions had sprouted between "new" and "old" approaches to biology. In his 1962 and 1964 annual reports to the Biological and Medical Science department of the National Science Foundation, Harve Carlson referred directly to a "breach" in biology between "proponents of the new and the old, of the molecular approach versus the classical approach, of the lab biologist versus the field biologist. In one school, one side dominates; in another the other side dominates. Good people are forced to leave or retire early in order that sweeping innovations may be made" (qtd. in Appel, 2000, 207). By acknowledging that the breach between two technical approaches to scientific objects had social and political ramifications for scientists. Carlson implicitly acknowledged that the technical, the social, and the political are not isolated spheres of activity, but converge in disciplinary practices. He implied, that is, that the boundaries between science, politics, and society were dissolving or had dissolved. In the early 1960s ecosystems ecologists were included among proponents of the "new" biology, while evolutionary and population ecologists and other biologists working in more

established sub-disciplines were proponents of the “old” biology. This breach helped set in motion the social drama of the IBP.<sup>2</sup>

Proponents of ecosystems ecology lauded the field as a revolutionary but underused science. To fulfill its potential called for unprecedented levels of funding and international cooperation. Some U.S. scientists saw the IBP as a chance to finally secure funding to implement their visions for large-scale ecosystems analysis. The timing for the program was, in some ways, especially apt. The International Geophysical Year (IGY), which took place 1957-1958, had been a recent success in terms of both international collaboration and public perception, and the IBP meant to capitalize on the goodwill that program had garnered by following the positive example of “big science” set by the IGY with its own example of “big biology” (Appel, 2000, 179-80; 226-34). Some American ecologists who saw the IBP as an opportunity to unveil their vision of “big ecology” took the first tentative steps toward joining the international program. In 1963, the National Academy of Sciences (NAS) assembled an *ad hoc* U.S. National Committee on the International Biological Program (USNC/IBP), which set about ascertaining the level of interest in the program among American scientists via questionnaires and solicited responses from members of the Ecological Society of America (ESA).

As Frank Blair, a member of the *ad hoc* committee, later noted, “These procedures, not unexpectedly, produced a mixed bag of

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<sup>2</sup>It is worth mentioning that, despite my repeated reference to “the social drama of the IBP,” the controversy I detail here was quite unique to the lead up to the United States’ participation in the international program, and was not at all typical of the IBP as a whole. No other countries had such a hard time agreeing on whether or not they should participate, and once the project was underway many scientists reported that interdisciplinary cohesion developed seamlessly. One scientist, Bill Heal, working on the Tundra Biome project wrote excitedly in his notes about how well the international, interdisciplinary work was going:

Thus we developed new ideas through exchange of information [which] is summarized by the equation:

$$2+2=5$$

I.e., the whole is greater than the sum of the parts or put the pieces together! This exchange of ideas, from different nationalities, different environments and different research backgrounds gave us insight into the fundamental processes of decomposition and the value of “synthesis”. It has had a large influence on my career development (qtd. in Coleman, 2010, 19-20).

responses” (Blair, 1977, 6). From the 104 replies from members of the ESA, more than 100 scientists from a wide array of specializations expressed interest in participating. Among the solicited and mostly unsolicited responses the committee received, however, a strong anti-participation sentiment was voiced. One such letter—described by Blair as “very critical and pessimistic”—from ESA member Lamont Cole read:

I have heard enough discussion of IBP to be certain that two suspicions are widely held. These are:

- a) It is a boondoggle designed to ride the coattails of IGY.
- b) It is a scheme to raid the U.S. Treasury, largely for the benefit of foreign scientists.

I am not concerned with the truth or falsity of these views. The important point is that they will appear to be confirmed if the program is undertaken and is not an outstanding success (qtd. in Blair, 1977, 8).

Worried about the reputation of their fledgling discipline should the IBP fail, Cole and other ecologists wrote to complain that U.S. efforts to participate in the IBP were a blatant example of “me-too” money-grabbing that invoked the IGY by name but lacked the careful organization that made the IGY so successful. This ethical critique called into question the program’s intentions and raised the possibility that the IBP might be exposed as a fraud thus delegitimizing ecology by association.

Tom Park—himself a member of the *ad hoc* committee—noted similar reservations in a cautionary memo. Park wrote, “My points are these:”

1. If there had not been an IGY there would not now be projected an IBP.
2. The IGY had meaning. Relatively simple measurements could be defined and taken. These data, in turn, contributed *conceptually* to geophysics.
3. The IBP does not enjoy this meaning:
  - a. No such simple measurements can be taken.
  - b. There is little, if any, conceptual framework onto which the measurements that are taken can be apportioned (qtd. in Blair, 1977, 14, emphasis in original).

Park’s points are similar to Cole’s in that they express concern that the IBP was disingenuously riding the coattails of the IGY. But

Park's points underscore a crucial tension that coursed through the social drama of the IBP, and that emerges again and again in ecological disciplinary debates. In classical rhetorical terms, this tension has to do with the "transformation of practical wisdom into accredited techniques, of *phronêsis* into *technê*," which, as Steven Mailloux explains, "becomes part of the conditions of possibility for the paths of thought in any disciplinary community" (Mailloux, 2006, 5). U.S. ecologists' involvement in the IBP was doomed to fail, according to its detractors, because they lacked accredited techniques.

The crucial difference between the IBP and the IGY, as Park understood it, was that the IBP could not expect to reap the same conceptual benefits that the discipline of geophysics reaped from the IGY; the IBP lacked both the conceptual disciplinary coherence that geophysics enjoyed and the methodological repertoire that helps establish such coherence. So, despite the excitement among some ecosystems ecologists about the prospects of large-scale studies of ecosystems, such a project lacked "meaning" in two senses: (1) ecosystems ecology was not developed as a disciplinary conceptual framework from which meaning could be derived; and (2) the methods by which disciplinary meaning is made were untested. Responses such as Park's and Cole's indicate that ecosystems ecologists were faced with a catch-22: they could not develop a coherent disciplinary framework without developing accredited techniques, but when given the opportunity to develop those techniques (which required a large-scale program like the IBP), their colleagues argued that they lacked a coherent disciplinary framework.

Park and Cole were not alone. Even other members of the *ad hoc* committee itself expressed ambivalence toward the IBP. But the committee was confident that ecosystems ecology was mature enough to prove itself by participating in the IBP project, and they felt the positive responses to their inquiries reflected that confidence well enough. The ayes had it. The committee ultimately decided to favorably recommend U.S. participation in the IBP to the NAS, which in turn sent a delegation to Paris to participate in the IBP organizing assembly. That delegation returned with the recommendation that a U.S. National Committee for the IBP (USNC/IBP) be established under the NAS. The recommendation was approved, and—ready or not, "boondoggle" or not, "me-too" cash-grab or not—the U.S. was participating in the IBP.

Turner notes that the breach phase of a social drama is initiated according to the "altruistic intent" of the offending parties. "A dramatic breach may be made by an individual, certainly," Turner



writes, “but he always acts, or believes he acts, on behalf of the other parties, whether they are aware of it or not. He sees himself as a representative, not as a lone hand” (Turner, 1974, 38). The selection and rejection of representatives is thus an important point of analysis in the breach phase of a social drama—a point that warrants some attention here before we move onto the crisis phase.

The offending parties in this case were the representatives of the *ad hoc* committee and the NAS, who, despite having heard the dissent ringing throughout the scientific community (and even from within their own committee), decided to go ahead with the plan to hitch American ecology’s reputation to the IBP’s wagon. The bureaucratic measures taken by the NAS and the committee were meant to minimize any offense at the committee’s decision. But among the offended parties, some raised ethical questions about the competency of their representatives. For example, in the same letter quoted from above, Cole objected to the representative role the NAS was meant to play in the decision to participate in the IBP: “I suppose it is logical for those unfamiliar with the situation to assume that NAS can speak for all science, but, unfortunately, ecology is a field in which NAS is not highly competent, and in this case apparently didn’t know where to turn” (qtd. in Blair, 1977, 8). Arguments such as this one, which effectively questioned the validity of the NAS committee’s claim to representative status, cast serious doubts on their ability to represent ecology as a discipline.

The members of the USNC/IBP soon discovered that Cole was right to be concerned about the NAS’s lack of competence in the field of ecology. The Academy appointed, to Blair’s dismay, a non-ecologist (indeed, to Blair’s further dismay, a *non-biologist*) as chairman of the USNC/IBP. In a letter to NAS President Frederick Seitz, Blair noted that he was “shocked” to learn of the appointment, writing, “My experience with the planning of IBP to date clearly impresses me with the certainty that the program is largely an ecological one. It is very difficult to visualize adequate guidance of the committee by one, however able in other respects, who is not only not an ecologist, but not even a biologist” (Blair, 1977, 21). Seitz responded that he and an advisory group had appointed someone “widely conversant with the international scene and a diversity of international problems affecting science” (qtd. in Blair, 1977, 21). Blair and his like-minded colleagues in the ESA managed to convince Seitz to name two ecologists as co-vice-chairmen of the USNC/IBP.

This incident was a miniaturized version of the larger social drama in which it was embedded. The NAS breached the trust of its constituents. The constituents responded, escalating the breach

into a crisis wherein one party (the ecologists) argued that their disciplinary interests were at stake and so deserved representation from an ecologist. The other party (President Seitz and his advisory group) argued that familiarity with the international science “scene” was more important in tackling such a massive international project. The crisis was essentially a dispute over whether disciplinary expertise or social influence should take precedence in appointing disciplinary representatives. Appropriate redressive bureaucratic measures were taken, and something like the status quo was reassumed with the eventual appointment of two ecologists as co-vice-chairmen of the USNC/IBP. Summarized more simply, this miniature disciplinary drama pitted arguments for the importance of technical acumen against arguments for the importance of social and political clout, and it resulted in what Gross calls “the healthier” version of reintegration, which “involves the incorporation of warring groups and their conflicting ideologies a new social synthesis” (Gross, 1996, 189). But the resolution of this miniature drama did not foreshadow the end of the larger social drama of the IBP.

## **Crisis**

Over the next few years, USNC/IBP planning moved along sluggishly. The planning phase for IBP projects was to last until 1967, at which time the operations phase of each project was to launch. The USNC/IBP was established only in September of 1964, leaving American scientists just barely over two years to coordinate an unprecedentedly large-scale research plan and secure funding for it, and prospects for funding were dwindling. By 1966, the National Science Board had only tentatively agreed to fund some very scaled-back research into “two or three of the most urgent problems . . . whose effective study requires international collaborative effort” (Appel, 2000, 229), and the NSF had notified the NAS that it would not provide a block grant for the IBP as it had done for the IGY.

Some of this fiscal reticence had to do with skepticism toward the program—many still insisted it was a disingenuous boondoggle—and some of it had to do with skepticism toward the NAS, which Harve Carlson, then chairman of the NSF’s Department of Biological and Medical Sciences, “thoroughly distrusted . . . as a powerful institution wanting to take over policy-making from NSF” (Appel, 2000, 230). Thus, NSF historian Toby Appel notes, “the year before the U.S. program was to begin operation, the U.S. National Committee found itself in a frustrating position.” She explains,

IBP at this point was to consist of a series of smaller ecological projects within the general framework adopted by the international community. But if NSF would not provide block funds for the Academy or give IBP proposals special standing, then the USNC could do little beyond certifying unsolicited projects (many of them ongoing) as relevant to IBP when certification conferred no acknowledged advantage. On this basis, it was impossible to organize diffuse projects into a coherent program (Appel 2000, 230).

The NAS responded to this situation by holding a meeting of the USNC/IBP in Williamstown, Massachusetts, where the focus shifted toward the formation of an “Integrated Research Program” on “Analysis of Ecosystems” within six “biomes”—eastern deciduous forests, tropical forests, grasslands, tundra, and desert (McIntosh, 1985, 216; Appel, 2000, 230).

These projects were controversial among U.S. biologists—whom the British ecologist Conrad Waddington characterized as the “toughest biological community into which to launch the [IBP]” (Worthington, 1975, 8)—for at least two reasons. First, the biomes to be studied were massive units of analysis by the biological standards of the time. The U.S. biological community was dominated by molecular biologists and microbiological geneticists who, Waddington notes, were quick to mention “in the hearing of government or the academy, that any organism bigger than *E. coli* serves only to confuse the issue” (Worthington, 1975, 8-9). Second, the Analysis of Ecosystems projects relied on systems analysis methodologies that were highly suspect to many biologists. Systems analysis offered a new approach to ecology—termed “systems ecology”<sup>3</sup>—that used computer models and differential equations to predict changes in highly contingent, large-scale ecosystems. Facing a lack of funding and a surplus of criticism from colleagues, as Robert McIntosh observes, “The U.S. program did not develop without crisis” (McIntosh, 1985, 216).

According to Turner, during the crisis phase of social dramas, “paradigms become transformed into metaphors and symbols with reference to which political power is mobilized and in which there is

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<sup>3</sup>Systems ecology is a somewhat ill defined approach to ecosystem ecology, and should not be confused with ecosystem ecology itself. Robert Lilienfeld, in *The Rise of Systems Theory* (1978), identifies six different sources of systems theories that have influenced systems ecology, from the cybernetic system theory of Norbert Wiener to the game theory of Von Neuman and Morgenstern (McIntosh, 1985, 232).

a trial of strength between influential paradigm bearers” (qtd. in Gross, 1984, 399). At the crisis stage of a social drama, the breach widens to a degree that is “coextensive with some dominant cleavage in the widest set of relevant social relations to which the conflicting or antagonistic parties belong” (qtd. in Gross, 2005, 44). The dominant cleavage in ecology in the 1960s was between the more established biologists, who studied ecosystems in terms of their functional components at a single level of organization, and ecosystems ecologists, who studied ecosystems in terms of the relationships between those functional components across levels of organization. During the crisis phase, both sides emphasized their differences, effectively polarizing the two approaches.

One influential ecosystems ecologist, Eugene Odum, attempted to mobilize all ecologists around the concept of the ecosystem, proclaiming in a 1964 article titled “The New Ecology,” “Ecologists can rally around the ecosystem as their basic unit just as molecular biologists now rally around the cell . . . *The new ecology is thus a systems ecology*—or, to put it in other words, the new ecology deals with the structure and function of levels of organization beyond that of the individual and the species” (Odum, 1964, 15, emphasis in original). Through rallying cries like these, ecosystems ecologists such as Odum sought to mobilize an audience of ecologists interested in what they called a “holistic,” or sometimes “integrated,” approach to ecosystems, which they touted as “the new,” or sometimes “revolutionary,” ecology (for many examples of this kind of language, see McIntosh, 1985, 193-241).

But Odum knew he could not simply assume his audience identified with his cause. Recognizing that he was making a somewhat incendiary statement, Odum predicted two responses from two “groups” of people. One group would take it as obvious that a whole ecosystem is not the sum of its parts, so they would see nothing novel about “the new ecology.” “The other group,” he wrote, “remains unconvinced that there is anything really new or different at ecological levels that can not be ultimately explained either by the reduction of the whole into even smaller parts or by expanding knowledge gleaned from parts directly into the whole” (Odum, 1964, 15). Odum argued that this group subscribed to a “reductionist philosophy,” according to which complex large-scale processes could be explained in terms of the processes of their components. Alluding to those aforementioned reductionists who thought that any organism larger than *E. coli* complicated the issue, “if anyone thinks that bird or human behavior can be understood by reducing the population to macromolecules, I would like to learn how this might be done” (Odum, 1964, 15). Thus we can see the

two ecological groups crystallizing as they take their own positions as given and argue that the other position is untenable.

Gross notes that Kenneth Burke's rhetorical theory is indispensable for "identifying the styles of conflict within the four Turnerian phases" (Gross, 1984, 398). In the social drama of the IBP, Burke's concept of identification and Maurice Charland's corollary theory of constitutive rhetoric seem especially useful for understanding how people rhetorically enacted the separation between the new and the old ecologies, and how they became entrenched in one sub-discipline or another during this crisis phase. Identification eschews the idea that audiences are pre-existing groups of people simply waiting to be persuaded, and instead calls attention to how audiences come to identify themselves as persuadable subjects in the first place (Burke, 1966, 301-2; 1950, 19-46). Burke observed that identification has profound implications for activities that are commonly understood as "autonomous," such as scientific disciplinary activities:

The fact that an activity is capable of reduction to intrinsic, autonomous principles does not argue that it is free from identification with other orders of motivation extrinsic to it. [. . .] The human agent, *qua* human agent, is not motivated solely by the principles of a specialized activity, however strongly this specialized power, in its suggestive role as imagery, may affect his character. Any specialized activity participates in a larger unit of action. "Identification" is a word for the autonomous activity's place in this wider context, a place with which the agent may be unconcerned (Burke, 1950, 27).

Charland's theory of constitutive rhetoric accounts for the processes at work in Burkean identification. "If," as Charland writes, "it is easier to praise Athens before Athenians than Laecedemonians, we should ask how those in Athens come to experience themselves as Athenians" (Charland, 1984, 134). In a disciplinary social drama such as that of the IBP, much of this constitutive rhetorical identification is carried out during the crisis phase when disciplinary actors recognize a breach, notice that it is widening, and find they must choose a side on which to stand.

Odum's revolutionary rhetoric was necessary for, as his colleague George Van Dyne phrased it, putting systems ecology "on the board" (qtd. in McIntosh, 1985, 221). But once systems ecology was on the board—that is, once it had established itself as a seemingly autonomous activity—the revolutionary rhetoric faded somewhat and systems ecologists shifted into a more magnanimous

position. Thus, when Van Dyne announced the founding of a new Systems Ecology program at Oak Ridge National Laboratory in a 1966 report, he included Odum's statement that "the new ecology is thus a systems ecology" (qtd. in Van Dyne, 1966, 9), but only well after acknowledging the controversy surrounding systems ecology:

Neither is this area of work in ecology clearly defined nor do all ecologists view it equally. As with any new field, systems ecology is beset with vociferous skeptics (largely those who have done well under the old conditions) but supported primarily by lukewarm champions (largely those who may do well under the new conditions). [. . .]

Often we feel that our own work and interests are of extra importance, but I do not propose systems ecology to be a new panacea, nor that we neglect more conventional approaches in ecology. (Van Dyne, 1966, 2)

Van Dyne's dialed back rhetoric was a sensible move for promoting a fledgling sub-discipline that wanted autonomy but not isolation. Many ecosystems ecologists would continue to strive for a similar balance in the next phase of the social drama of the IBP, but some would continue to overstate their case.

## **Redressive Action**

Instead of appealing directly to the funding agencies that had repeatedly spurned them, the USNC/IBP turned to Congress to seek federal funds directly. Two important representatives for ecosystems ecology, Frank Blair and Roger Revelle, arranged a set of hearings before Representative Emilio Daddario's Subcommittee on Science, Research and Development, which is where redressive action phase of the social drama of the U.S. IBP took place.

Revelle, who was identified by his colleagues as "a real mover and shaker . . . [who] really shone when it came to getting the process started" (Coleman, 2010, 25) and "someone with the reputation of a real thruster" (Worthington, 1975, 10), opened the 1967 hearing by submitting a prepared statement to the subcommittee. His statement began with a sweeping discussion of the self-inflicted biological problems facing the planet. Humans, Revelle observed, have "alter[ed] the face of the earth through technology," and they have also altered their own biology by extending their life expectancies considerably. The problem with all of this change is that "[o]ur technology has outpaced our understanding, our cleverness has grown faster than our wisdom" Therefore, argued Revelle, increased *understanding* and *wisdom*,

of the kind that only ecosystems ecology could provide, were necessary:

Because of our limited understanding of the relationships among living things, we are limited in our ability to predict the effects of technical change or to help the technologists conserve the values and utilize the abundance of the world of life. Our goal should be not to conquer the natural world but to live in harmony with it. To attain this goal, we must learn how to control both the external environment and ourselves. Especially, we need to learn how to avoid irreversible change. If we do not, we shall deny to future generations the opportunity to choose the kind of world in which they want to live. Greater understanding will make it possible for man to respond to opportunity as well as to react to need. To gain such understanding is the underlying purpose of the International Biological Program (IBP, 1967, 2).

Revelle's statement goes on to detail the objectives of the IBP and the U.S. program's plan to address those objectives.

The subcommittee focused on the specifics of the projects later in the hearings. But, Revelle's argument for an ecology aimed toward *control*—environmental control, population control, and self-control—was more persuasive to Daddario and his fellow Congressmen than the precise details of the project's implementation. Daddario read a passage from a report that had been commissioned by the subcommittee the previous year, titled "A Challenge to Science and Society," which speaks to the persuasive power of this argument:

Ecology generates a viewpoint or an attitude which, simply stated, involves wise use of our environment for the benefit of man. It does not imply a balance of nature or avoidance of change in the landscape. Rather, ecology encourages the manipulation of nature, but with knowledge of the interacting forces and immutable laws, not haphazardly or indiscriminately (IBP, 1967, 11).

Daddario immediately followed his quote with praise: "We look with great favor, Dr. Revelle, on what you have been trying to do, not just in support of this resolution, but in your work, which is recognized internationally" (IBP, 1967, 11). As the earliest statements of the hearing indicate, Daddario and his subcommittee were quick to align themselves with the ecosystem ecologists' goal of environmental control and manipulation.

Given the lack of alignment among ecologists themselves, how was alignment between the ecologists and politicians achieved so easily? Certainly Revelle carried some clout as a political and scientific “mover and shaker” with him to the subcommittee hearings. The presence of a cadre of other supportive scientists backing him did not hurt. But Chunglin Kwa argues that we cannot simply attribute Revelle’s USNC/IBP’s success to lobbying and coalition building. More important was the use of metaphor of ecosystem-as-cybernetic machine, which grounded the ecologists and Congressmen’s shared understanding. The ecosystem-as-cybernetic machine metaphor had found traction among the broader public through books such as Rachel Carson’s *Silent Spring* and Paul Ehrlich’s *The Population Bomb* (Kwa, 1987, 431). The environmental movement of the 1960s had popularized the ecosystem-as-cybernetic machine metaphor and its corollary concepts, such as homeostasis and feedback. Thus the cybernetic vernacular common to ecosystems ecology had also become fairly common among the American public and their representatives. For ecologists, the machine metaphor, “provided an heuristic for ecological research: systems ecology was to reveal the structure of the machinery of the ecosystem. It also set out a goal for ecology: specifying the conditions under which ecosystems could remain stable” (Kwa, 1987, 428). Moreover, the machine metaphor offered a heuristic for political action: once the structure of the machinery of the ecosystem was “revealed,” the public could go about deciding how best to repair it.

The cybernetic metaphor of ecosystems as machines served its persuasive purpose well in that, as Kwa argues, it “fulfilled an intermediary role between ecologists and politicians” (Kwa, 1987, 414). But when ecologists went so far as to suggest that they could be the ones to determine how to best manipulate the machine, they veered into the extra-scientific realms of politics and philosophy. Some ecologists made untenable claims about how ecological understanding of environmental problems might be applied in addressing those problems, inadvertently overstepping their boundaries. When, for example, Carlton Ray, a pathologist at Johns Hopkins University, suggested that the Analysis of Ecosystems projects were aimed not at understanding the cause or effects of pollution but instead at political action to mitigate the problems of pollution, Representative George Brown pushed back:

You are getting suspiciously close to a field which is outside the scope of science, one of normative judgments. . . . Are you contemplating some philosophical aspect of this approach to the biological



program? Are you assuming maybe that this ought to be sort of an international philosophical year as well as an International Biological Program? (IBP, 1967, 196)

Ray responded that he “was merely pointing out that the International Biological Program does not separate itself from the problem of human welfare”). To this Brown replied, “I think your point is correct. I am merely making the point that you have certain preconceptions about the nature of human welfare which are not necessarily a part of any scientific program” (IBP, 1967, 197). Later in Ray’s testimony, Rep. Brown summarized his argument:

I sense in the witness a nostalgia of the old days in which man and nature are in balance and everything right with the world. . . . To determine that there is some natural balance as you seem to be hinting at is a highly philosophical question (IBP, 1967, 199).

Representative Brown’s statements stood out among his fellow Representatives’ as especially critical, and he downplayed their bite by suggesting that he was “playing the devil’s advocate” (IBP, 1967, 199). But these sorts of arguments were even more common among scientists who were increasingly wary of ecosystems ecologists’ perceived philosophical overreach. For example, an article in the British journal *Nature* from December, 1967 reported:

Consideration of the philosophical bases of the International Biological Program (IBP) has so preoccupied scientific leadership in the United States that observers elsewhere have wondered when American biologists might get down to specific projects, and what these might be. A stream of stately essays has been the main output so far; yet Phase 2, or the operational part of the IBP, was supposed to start last July. At that time, however, the National Committee was in the midst of Congressional hearings on the IBP and its funding (qtd. in Blair, 1977, 26).

Similarly, as the Daddario subcommittee hearings published a report on the 1967 hearings, the U.S. journal *Science* reported that the Analysis of Ecosystems projects suffered, somewhat ironically, from “ecological sprawl” that spread it thinly over “a wide variety of seemingly unrelated projects” (Boffey, 1968, 1332). Herbert Curl, an oceanographer, wrote to *Science* to “add to [the] concise report on the ills of U.S. participation in the International Biological Program”:

It is highly improbable that a group of individuals who cannot agree on what constitutes a community can agree to get together for international cooperative research on communities. Not only is this an inauspicious time to commence major projects requiring new funds, but there is reason to believe that the field of ecology is not mature enough to benefit from a large-scale, coordinated program. This double misfortune is particularly disheartening since we are already in very deep ecological trouble (Curl, 1968, 1065).

The *Nature* and *Science* articles, and Curl's response to the latter, echo the doubts that polarized the old and new ecologists during the crisis phase. The journal articles characterized the project as disorganized, unfocused, and too philosophically flighty, and Curl's response attributes these failings directly to the "immature" discipline of ecology.

These doubts rarely found their way into the Congressional hearings, and after nearly a decade of the social drama of IBP, redressive action finally came in the form of Public Law 91-438, which was signed by Richard Nixon on October 7, 1970. The law fully funded the Analysis of Ecosystems projects, funneling 40 million dollars into the large biome projects and a few peripherally related smaller projects between 1970 and 1974. The U.S. contribution was the largest of any country participating in the IBP.

## **Reintegration**

Turner writes that, for the analyst of a social drama, reintegration is "an opportunity for taking stock. [The analyst] can now analyze the continuum synchronically, so to speak, at this point of arrest, having already fully taken into account and represented by appropriate constructs the temporal character of the drama" (Turner, 1974, 42). Various accounts of the U.S.'s involvement in the IBP have engaged in such stocktaking. Many have recognized the U.S./IBP as a watershed moment for ecosystems science, and the discipline of ecology more generally. In a 1981 statement in the *Bulletin of the Ecological Society of America*, R.L. Burgess wrote, "In all probability, the single most important event for U.S. ecology in the last thirty years was the participation in the International Biological Program" (qtd. in McIntosh, 1985, 214). This event was important in several ways. For one thing, the collaboration between scientists and the Congressional Subcommittee on Science, Research, and Development secured massive funding for large-scale ecological projects. U.S. participation in the IBP was also important in that it helped legitimate ecology as a discipline.

Ecology did not enjoy much institutional status prior to the IBP. Prior to the 1970s, no U.S. universities granted degrees in ecology, and ecologists held very few positions on influential policy-directing committees (Kwa, 1987, 416-17). Much of that changed once the U.S. projects were endorsed by the IBP. The American ecologist Frederick Smith credited the IBP with “lifting a minor subject to a position of major status” (qtd. in Worthington, 1975, 10). Finally, U.S. participation in the IBP was important because it helped establish ecology, and the study of ecosystems in particular, as the basic science that could solve our complex and increasingly dire environmental problems. The environmental movement in the 1960s and 70s had begun to coalesce around the ecosystem concept, and the swelling movement may have partly motivated Congress and the President to put their faith, and their (that is, the public’s) money, behind ecosystem ecology. In sum, U.S. participation in the IBP established a source of funding for ecological studies where there had been none before, which in turn helped established ecology as a discipline, and which in turn helped establish ecosystem ecology as *the* foundational science for managing environmental crises.

Nevertheless, the reintegration of “new” and “old” ecologies has not been fully realized. The social drama of the IBP resulted in “Turner’s less positive notion of reintegration,” whereby the parties recognize and legitimate a schism between them. In this case, the breach that Carlson first recognized between “old” and “new” approaches to biology were exacerbated during the crisis phase to an extent where the redressive action undertaken in federal government chambers was not enough to repair it. For one thing, amid the grandiose rhetoric employed during the subcommittee hearings, some ecologists had made claims about their discipline’s “predictive power” and its ability to “improve world-wide productivity” and help solve the problems of overpopulation. Thus, writes McIntosh, the discipline suffered “when more was claimed for it than it could deliver. . . . The solid achievements of ecology in the IBP ecosystem programs were sometimes masked by criticism of its failure to achieve the impossible” (McIntosh, 1985, 221). The biome projects made some large strides toward developing the kind of understanding Revelle and his colleagues sought, but there was little or no chance that the projects, by themselves, could accomplish everything that some ecologists had touted.

Positive integration continued to elude ecologists as, in the years following the IBP projects, ecologists continued to identify themselves according to the polarizing constitutive rhetoric of the crisis phase. For example, in a 1976 review of a major volume of

systems analysis, limnologist Frank Rigler took up Odum's dichotomy between old and new ecologies and repeatedly referred to himself and others who find systems analysis impenetrable as "oldies." On the sections of the volume having to do with biome modeling Rigler wrote:

It is a biology with a new language—a new paradigm. In the old days, despite our professional fragmentation, ecologists could understand and be interested in the work of geneticists, embryologists, molecular biologists, and others although we were inadequately trained to make original contributions to these fields. Now, it seems that a branch of our own discipline is beyond some of us because the chapters on biome modeling left me bemused. Eventually the source of the difficulty became clear. Systems analysis modelers have a totally different publication paradigm. Whereas we (the oldies) tend to publish the results of experiments we have actually done, they (the modelers) seem to publishing the equivalent of experiments they intend to do (Rigler, 1976, 481).

Using Kuhnian terminology in his critique, Rigler argues that the methodological differences between ecological "branches" amount to paradigmatic incommensurability. Sub-disciplines cannot reconcile these differences, and thus have no use for each other.

Reintegration is an ongoing process in ecology. Tensions between sub-disciplines have abated in some cases,<sup>4</sup> and many have sought, and are still seeking, a unified discipline. But as new ecological approaches emerge, familiar disciplinary divisions continue to be reenacted. For example, in 1988 Heinz Stolp, a microbial ecologist, lamented "the still existing gap between micro- and macroecology" (Stolp 1988, 282). More recently, Loreau, Mouquet, and Holt, complained that "the traditional divide within ecology between the perspectives of population and community ecology on the one hand and ecosystem ecology on the other hand" has re-emerged in, and fractured, the sub-field of spatial ecology (Loreau, Mouquet, and Holt, 2005, 418-19). Assessing the prospects for a more unified "big ecology," David Coleman writes,

When viewed through the lens of three to four decades since the heyday of the IBP Biome programs, several features stand out. The main point that comes across is

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<sup>4</sup> See, for example, Bazerman and De los Santos's study of the gradual unification of toxicology and ecotoxicology.

the sheer magnitude of the accomplishments by only a few hundred scientists working in concert over four decades. The second thought is: how much more could have been accomplished had there been a more concerted effort to involve a greater number of scientists, and to educate several hundred more graduate students, during that period (Coleman, 2010, 180).

Although the U.S./IBP was geared toward just such a concerted effort to cast a wider net for ecology, this effort was diminished during the social drama that ensued.

## **Conclusion**

Just as Turnerian analysis reveals diverse public debates about science and technology “as analogous events, perhaps even parallel enactments of the same social drama,” it also exposes disciplinary debates as analogous events and perhaps even parallel enactments of the same social drama. Since its earliest discernable beginnings as a formal science in the late 19<sup>th</sup> century, ecology has grown and split into several separate sub-disciplines. Though the creation of such sub-disciplines is important for creating specialized knowledge, it has some drawbacks. Specifically, it has become increasingly difficult for sub-disciplines to communicate with one another. Michel Loreau, a contemporary French ecologist, notes that this is particularly true when it comes to community ecology and ecosystems ecology, two sub-disciplines that, according to Loreau, “[i]n a way, provide two different perspectives on the same material reality” (Loreau, 2010, ix). To solve complex environmental problems, scientists such as Loreau argue, we need to reconcile these perspectives. But though the problem may seem a matter of perspectival dispute, Turnerian analysis gives us a way of looking beyond “what is loudest, the fight,” and instead “trying to see what all this noise is part of” (Mol 2002, 123).

Gross observes that “Turner’s concept [of social drama] best elucidates the ways in which societies attempt to contain conflict, to see to it that public controversy leads not to revolution, but to reaffirmation or a reordering of existing values” (Gross, 1984, 397). Calls for disciplinary unity, such as those made by Stolp and Loreau, can be understood as continuing attempts to contain conflict and to reorder and reaffirm existing ecological disciplinary values in the wake of the social drama of the IBP (Stolp, 1988, Loreau, 2010). However, this conflict extends back to the disciplinary emergence of ecosystem ecology in the late 1960s and since that time sub-disciplinary factions have become fairly entrenched. A Turnerian analysis of the emergence of ecosystem

ecology and its aftermath helps us rhetoricians, and may help ecologists themselves, understand their disciplinary divisions as continuous reenactments of a massive social drama.

A large body of ecological literature is dedicated to bridging the disciplinary gaps in ecology—gaps that some ecologists may perceive as natural, ancient, and formidable as the Grand Canyon. But seen as the reenactment of a merely decades-old social drama, the disciplinary divisions appear less like the result of incommensurable ways of knowing and seeing and more like different, though not necessarily incommensurable, ways of doing and being. According to a post-plural Turnerian analysis, disciplinary gaps may still seem formidable, but they are always traversable. If ecologists understand their disciplinary differences as different ways of looking at a fixed reality, they are likely to search for reintegration by asking how we can better know that fixed reality. If, however, ecologists understand their disciplinary differences as ways of enacting multiple realities, they may search for reintegration by asking how they can better act in the face of uncertainty.

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