NASA Revisited: Theory, Analogy, and Public Sociology[1]

Diane Vaughan
Columbia University

This ethnographic account of the rituals of risk and error after NASA's *Columbia* accident reveals the mechanisms by which sociological theory traveled across the disciplinary boundary to public and policy domains. The analysis shows that analogy was the instigator of it all, enabled by the social mechanisms of professional legitimacy, conversation, technologies, time, networks, and social support. It demonstrates the work sociologists do when theory travels from professional sociology to nonacademic audiences and what happens to the theory and the sociologist in the process. It reveals the tensions when professional sociology, critical sociology, public sociology, and policy sociology are joined. A study of sociology in the field, it shows how sociologists negotiate the meaning of their work in a nonacademic situation. Thus, this account contributes to research and theory on social boundaries, the diffusion of ideas, the sociology of scientific knowledge, and current debates about public sociology and the role of the sociologist, adding to the sociology of our own work.

At 9:00 a.m. EST on February 1, 2003, NASA's space shuttle *Columbia* disintegrated as it was streaking across the sky over Texas toward the landing site at Kennedy Space Center, Florida. Immediately, NASA declared a "shuttle contingency," executing the contingency action plan that the space agency had established after the *Challenger* accident 17 years before. As part of that plan, NASA activated the *Columbia* Accident Investigation Board (CAIB) to identify the causes of this second shuttle disaster. The technical failure, the CAIB concluded, was initiated 81.7

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seconds after *Columbia*'s launch, when a large piece of foam insulation broke off the shuttle's external tank. Traveling at approximately 500 miles per hour, the foam debris struck the thermal protection system of *Columbia*'s left wing, causing a breach. Upon *Columbia*'s reentry into the earth's atmosphere 16 days later, superheated gases penetrated the interior of the left wing, melting its aluminum structure and causing loss of control and breakup of the orbiter.

But like the presidential commission that investigated the *Challenger* disaster, the *Columbia* Accident Investigation Board discovered that the technical failure was triggered by an organizational failure of shocking proportion. On that fatal February morning, *Columbia* reentered the earth's atmosphere with known damage. Cameras at the launch site had recorded the foam debris strike, causing worried engineers to request high-resolution imagery from Department of Defense satellites so they could better assess the structural impact of the foam while *Columbia* was orbiting. Their request was turned down by NASA management, who concluded, without adequate data, that the debris strike was a "turnaround" issue only, requiring repair to the thermal tiles after landing, not a safety of flight issue. Equally alarming, the CAIB learned that NASA had been incurring foam debris strikes on every shuttle mission. Without proper hazard analysis, NASA repeatedly defined the problem as an acceptable risk.

Rituals of risk and error are the aftermath of every disaster. An official investigative body is constituted, given authority, and charged with identifying the cause or causes. Public spectacle ensues. A report is issued, blame affixed, corrective measures required. Because I had written a book on the *Challenger* accident (Vaughan 1996), I was viewed as an expert on NASA and shuttle accidents. As a consequence, my *Challenger* research revisited me, implicating me in the ritualistic processes that followed *Columbia*. I was consulted by the press, called to testify before the CAIB, invited to join the board as a consultant and staff researcher, and worked on the official report, authoring a chapter. Most surprising, after the CAIB report (CAIB 2003) was published, the space agency that I once studied from the safe distance of historical ethnography opened its doors, enlisting my advice about how to make the organizational changes recommended in the report.

How did a 500-page research monograph initiate such a journey? This article explores how theory travels—not only from one NASA accident to another, but across sociology's disciplinary boundary, from the pages of an academic book to institutions of power: the media, CAIB, NASA, and the U.S. Congress. Although my experience was surely idiosyncratic in both the extent and duration of its "publicness," these very qualities expose otherwise unavailable aspects of the process of going public with sociological work for analysis. My NASA revisit is an opportunity to analyze the mechanisms that enable sociological theory to travel across institutional boundaries, the work that boundary crossing entails, what happens to the sociologist and the theory in the process, and the implications for public sociology and sociologists. In doing so, this article adds to research and theory on social boundaries, the diffusion of ideas, the sociology of scientific knowledge, and bears upon current debates about public sociology and the role of the sociologist

Burawoy (2005) locates public sociology within the "division of sociological labor," consisting of professional, public, policy, and critical sociologies. Professional sociology is the generation of research and theory within the discipline: posing research questions, applying systematic methods, developing concepts and theories, and building cumulative knowledge. In contrast, policy sociology serves a goal defined by a client: investigating problems, providing solutions, or challenging or confirming existing definitions of a situation. Critical sociology examines the theoretical and methodological assumptions of professional sociology, initiating debate within the discipline. Public sociology carries professional sociology to nonacademic audiences, where it affects public discourse. Although Burawoy separates the four sociologies into ideal-types in order to define them, he emphasizes their relational complexity and the porous, intersecting nature of the divisions he describes (2005, pp. 9–15).

Public sociology can be of two general types, traditional or organic (2005, pp. 7–9). In traditional public sociology, the media may discover research, disseminating sociology to various publics, or sociologists may publish in outlets that reach nonsociologists. Initiating debates within or between publics, the traditional public sociologist may or may not actively participate in them. The publics thus generated tend to be invisible, diffuse, and, typically, mainstream. In contrast, organic public sociology contributes sociological expertise to help marginalized people achieve solutions to their problems. Organic public sociologists engage directly with visible, local, socially organized publics, using sociology in a dialogic exchange that is mutually educative and, often, dedicated to social change. Within these two general types, public sociology encompasses a variety of publics, practices, and value commitments by sociologists. Shared across these differences is a commitment to bring professional sociology to bear upon societal issues in venues outside the academy.

Theoretically and empirically, the practice of public sociology is inextricably linked to social boundaries, the sociology of scientific knowledge, and the diffusion of ideas. In the new sociology of ideas, scholars typically have explored the knowledge production process among intellectuals (Camic and Gross 2001). Localism—the pattern of relations among and between academic disciplines and universities—is central to understanding the formulation and dissemination of ideas (Camic 1995). However, the new sociology of ideas has itself been ineluctably local, neglecting the dissemination of scientific ideas across disciplinary boundaries into non-academic settings and how scientific knowledge might be transformed, accepted or rejected in the process. Further, the focus on the past has necessarily relied primarily upon archival research, precluding the possibility of ethnographic examination of microprocesses.

Lamont and Molnar (2002) define social boundaries as objective forms of social differences that manifest in groupings of individuals, resulting in material differences in resources and opportunities. They identify several current research trajectories, among them research on the professions and work that documents great efforts to construct, maintain, and defend professional boundaries against incursion (Sarfatti-Larson 1979; Collins 1979; Gieryn 1983; Abbott 1988; Gal and Irvine 1995). In contrast to this emphasis on separation and exclusion, Star and her colleagues demonstrate that boundaries can function as connective tissue between differently organized social life, facilitating bridging, crossing, and interaction (Bowker and Star 1999; Star and Griesemer 1989). In particular, "boundary objects"—the material focus of scientific inquiry—can bring together diverse communities and thus maintain a place in several social worlds. Their work on boundaries elaborates on an established research tradition in the sociology of scientific knowledge that recognizes the key role of informal social linkages in the making of science and identifies networks as a mechanism for the diffusion of scientific knowledge beyond the boundaries of laboratories, disciplines, and research institutions. Specifically, actor-network theory innovated by repositioning human agents, texts, and objects as equally agentic "actants" in networks (Callon 1986; Latour 1987, 1988; Law 1987). In this theory, things—the products of science, boundary objects—can influence human action. At the same time, the agency of human actors in this network activity is preserved.

Building on the new sociology of ideas, this article exposes the microprocesses of public sociology and boundary crossing. The focus is on local institutional settings and how sociological knowledge was disseminated and gained credibility and acceptance. I draw upon my experiences from February 2003 through February 2004, reconstructing them chronologically in an ethnographic account of the four stages of post-*Columbia* rituals of risk and error: the social framing of the news, CAIB public hearings and the dissemination of social theory, the production of the CAIB report, and the institutionalization of the accident's sociological explanation. My data are constituted in my participation at the CAIB and NASA and a chronology and content analysis of over 1,200 e-mails I received related to the *Columbia* accident. My "in" and "sent" mailboxes provide a moment-to-moment diary, recording lengthy continuing conversations with multiple publics and showing the patterns and turning points in the process described here. Also useful were 4 × 6 cards on which I recorded each telephone contact by the media and others by date and phone numbers, taking notes on questions asked and new information I received. I analyzed all newspaper and wire accounts relevant to the accident and to the CAIB and NASA activities available via the CAIB clipping service, and I examined Internet documents. Finally, I compared succeeding versions of the CAIB report chapters addressing the social causes of the accident.

Working inductively from this ethnographic account, I identify the mechanisms that enabled sociological concepts and theory to travel across the discipline's boundary to become meaningful in the public and policy realms. Analogy was the primary mechanism in the diffusion of sociological ideas. "Analogy" refers to correspondences in some particulars between things, otherwise unlike. The extent to which two things agree with one another or differ, however, is an empirical question. The revisit that *Columbia* initiated was more than metaphoric; it gave me the opportunity for a focused ethnographic revisit (Burawoy 2003) to NASA 17 years after I began my *Challenger* research, allowing me to compare the site with the "same" one studied earlier. Although the cast of characters had changed, the data showed that the historical, political, economic, institutional, organizational, and cultural causes of *Challenger* were analogical, empirically and theoretically, to those of *Columbia*. Thus, the causal theory explaining the first accident generalized to the second.

Hesse (1963), writing about theory in the natural sciences, argued that analogy was essential not only to the formulation of theory, but also to its extension into new domains. Analogy between theory and some new empirical model, she asserted, gave theory its dynamic quality, making extensions, modifications, and elaborations of theory possible. Also, analogy was fundamental to predictions about new phenomena, which was one of the traditional functions a theory was expected to fulfill. In other words, analogy was a mechanism that enabled theory to travel. But Hesse, a philosopher of science, focused solely upon the relationship between theory and empirical models, omitting the role of social mechanisms essential to the diffusion of ideas. As this analysis will show, professional legitimacy, conversation, technologies, time, networks, and social support also were essential mechanisms.

Elaborating upon actor-network theory, this account illustrates the work that sociologists do when sociological theory crosses into other domains. It shows how sociologists work out the meaning of their work and role in a complex and unfamiliar nonacademic situation: a case study of sociology in the field, applied. Verifying the centrality of professional sociology in the sociological division of labor, it also demonstrates the tensions when working in the interstices of professional sociology, public sociology, critical sociology, and policy sociology. Thus, it adds to the sociology of our own work. Along the way, it provides a look at the backstage of the social construction of media frames, news, and social problems (Gamson et al. 1992; Spector and Kitsuse 1977; Schudson 2003). A turning point in postaccident rituals of risk and error is the production of a report that creates an official definition of the situation. Because my informal association with the CAIB led to a permanent one, this analysis also is an inside view of the social construction of documentary reality (Smith 1974) and how policy recommendations are formed (Katz 1997).

RITUALS OF RISK AND ERROR

Public Spectacle: The Investigation and the Social Framing of the News

About 10:30 a.m. on Saturday, February 1, as I watched television replays of *Columbia*'s tragic disintegration unfold, I began receiving phone calls and e-mails about the accident. I was deep into a new project, so was caught off guard by the sudden NASA-related intrusion of media and interested others. I was not surprised by the accident: in the last paragraph of my book, I predicted another because the systemic social causes of the shuttle's technical failure were being reproduced even as I wrote. But I did not predict the consequences for me. The weekend was a harbinger of the months to come: a deluge of messages from the media and diverse publics. In the past, most book-related inquiries had been relayed by my publisher. Now the World Wide Web provided easy access and a direct route, implicating me in the unfolding public spectacle.

NASA, accused of cover-up after the first shuttle accident due to its infrequent, stonewalling press conferences, remedied that situation after *Columbia* by holding televised daily sessions on the progress of NASA's in-house investigation. A few days after the accident, I was riveted by NASA's revelation about the *Columbia* foam strike and the long history of foam debris from the external tank hitting the orbiter. The O-ring erosion responsible for the *Challenger* accident also had a long problem history. The day following the Columbia accident, the space shuttle program manager, faced etched with grief and fatigue, showed a large piece of foam the size of the one that struck *Columbia*. Discounting its importance as a probable cause, he acknowledged that foam debris had repeatedly hit the wings at shuttle launches, explaining "We were comfortable with it." I was astonished. The normalization of deviance—a key concept from my *Challenger* research explaining how NASA first accepted an O-ring anomaly, then accepted more and more, until flying with damaged O-rings became normal and routine—seemed to fit this second accident scenario. NASA's former solid rocket booster manager, who had played a major role in the *Challenger* launch decision and with whom I had lost contact, fired me an e-mail with the subject head, "Déjà Vu All Over Again!" Media inquiries ballooned.

The CAIB soon seized the reins from NASA to initiate its own independent investigation. The board's composition (ultimately significant in the framing discourse of the board's report, as I will discuss later) was a product of history and politics. As mandated by NASA's post-*Challenger* shuttle contingency action plan, seven members automatically were activated because they occupied designated government posts: six were directors of safety; one headed NASA Ames Research Center.[2] That same day, NASA administrator Sean O'Keefe appointed as Chair Admiral Harold Gehman (retired), former commander in chief of the U.S. Joint Forces who also served as cochair of the Department of Defense investigation of the 2000 Yemen attack on the USS *Cole*. The press lambasted the newly appointed board members for lack of independence: all the members were affiliated with the government. Between February 6 and March 5, citing its need to "manage its burgeoning investigative responsibilities" (CAIB 2003, p. 232), the board diversified, adding five members.[3]

As the story of what happened technologically began to come into focus, the CAIB investigation turned to NASA itself: Why did NASA continue to fly with known foam debris in the years preceding the *Columbia* launch, and why did managers conclude that the *Columbia* debris strike was not a threat to the safety of the mission, despite the concerns of their engineers? Remarkably, the 1986 *Challenger* investigation had pursued two identical questions: Why did NASA continue to fly with known O-ring erosion in the years before the *Challenger* tragedy, and why, on the eve of the *Challenger* launch, did NASA managers decide that launching the mission in such cold temperatures was an acceptable risk, despite the concerns of their engineers? As Admiral Gehman began to make public the board's discoveries about the organizational contribution to the accident, the the-

[2] Maj. Gen. John Barry, Air Force Materiel Command; Brig. Gen. Duane Deal, U.S. Space Command; James Hallock, Volpe National Transportation Systems Center; Maj. Gen. Kenneth Hess, U.S. Air Force Safety Center; Scott Hubbard, NASA Ames Research Center; Rear Adm. Stephen Turcotte, U.S. Navy; Steven Wallace, Federal Aviation Administration.

[3] John Logsdon, director, Space Policy Institute, George Washington University; Douglas Osheroff, Nobel laureate and chair, Stanford Physics Department; Sally Ride, professor, physics and space science, University of California, San Diego, former astronaut and member of the presidential commission investigating the *Challenger* accident; Roger Tetrault, retired CEO of McDermott International; Sheila Widnall, professor, aeronautics and astronautics, MIT, and former secretary of the U.S. Air Force.

ory of the *Challenger* book was increasingly relevant to the media because it fit the emerging data.

Media work had always been stressful for me. Densely concentrated around book publications, it was grueling. I did participate in interviews and other media activities because while teaching as a graduate student I saw how sociology could challenge or even alter people's understanding of their own and others' lives. Writing scholarship in a style accessible to nonacademics became, for me, another form of teaching. Media work was yet another, albeit dramatically different from the classroom, where we have control of the agenda, the quality and amount of the interaction, the direction and content of the conversation, and, to a varying extent, the final product. Not so in media work. We are dependent upon others to convey our sociology to a geographically dispersed, invisible, and largely silent public. Rarely do we control the final product. Information is systematically reduced. Exceptions exist—live broadcasts, op-ed pieces, the regular column or radio program—but often, my own experience was one of losing control over my ideas and, sometimes, in television, outright manipulation and exploitation. The reward was in the parallels with the best parts of teaching: the occasional rich discussion with an interviewer or when someone in that invisible public got in touch, letting me know that sociology had struck a chord—or dischord.

The *Columbia* accident was a historic event. Defining this as a professional responsibility as well as a teaching opportunity, I tried to respond to every inquiry. What I was teaching was the sociological perspective, using the theory and concepts that explained *Challenger*. In the first few weeks, many reporters were new to the space beat; they knew nothing about the first accident or how NASA worked. Others, because they usually covered NASA or had read my book or had interviewed me when the book was published, knew all too well. Like me, they quickly saw parallels between *Challenger* and *Columbia*. Because the investigation kept the accident in the news, many contacts with print journalists became continuing conversations, allowing me to reinforce sociological concepts and interpretations. I noticed that key concepts from the book—missed signals, institutional failure, organization culture, the normalization of deviance—began appearing in the press early in the investigation and continued, whether I was quoted or not.

What I initially imagined as an involvement of a few weeks went into a second month, my ongoing field research displaced by my public sociology. Three weeks of 12-hour media and e-mail days settled into eight hours a day, the contacts rising and falling after that in response to discoveries in the investigation. In contrast to my previous media experience, the advent of e-mail now empowered the public to initiate conversations with me. In addition to journalists, I heard from NASA engineers, current and past; NASA safety and quality assurance personnel; space buffs; non-NASA physicists, thermal experts, engineers, accident investigators, and safety and risk management personnel; people who had read my book or were responding to some print, TV, Web, or radio comment; students; NASA contractors; a whistleblower; writers and documentary producers; conference organizers; lawyers seeking an expert witness; colleagues and old friends.

The advent of e-mail also transformed the form, content, spontaneity, and duration of these conversations. Many wrote to say they had previously read my book and saw analogies between the two accidents. Some requested favors: the whistleblower, silent for years and no longer a NASA contractor, now wanted to tell his exposé story, requesting that I forward it to the CAIB.[4] Students wanted help with papers and dissertations. Surprisingly, in dialogic exchange many e-mail correspondents began teaching me. Scientists and technical experts sent lengthy analyses of NASA's organizational and technical problems. Many of these affirmed analogies between the social causes of the two accidents: for a second time, cost, hierarchy, and schedule appeared to have worked against safety. A NASA employee became a regular e-mail informant. Journalists sent copies of NASA documents obtained through the Freedom of Information Act, asking for interpretation. In interviews, they described stories in progress then sent advance copies. For Space Center City and East Coast journalists and National Public Radio, I became a regular source. Formal requests for interviews devolved into e-mails headed, "Gotta minute?" or a quick observation: "Stuff falls off the Shuttle all the time, and every time we run a story on it, sometimes several stories. The *New York Times* only comes down here [Florida] when it takes the Shuttle out of the sky." I heard from no one contradicting what I was saying or my sociological point of view. My public sociology was leading to continuing dialogic exchange with a select sample of the public that defined me as a NASA critic and likewise fell into that camp.

In late March, I was invited to testify in the CAIB public hearings in Houston. My testimony, I decided, would be built around a comparison of *Challenger* and *Columbia*. The CAIB investigation evidence that the social causes of these two events were structurally equivalent was growing. Based on multiple sources and the uncontested nature of most of the information available to me, I would make the argument that the accident resulted from a failure of NASA's organizational system: the repeating patterns across the two cases indicated that the social causes of *Challenger* had not been fixed. Using the Simmel-based method of analogical theorizing that guided my *Challenger* analysis (Vaughan 1992), I converted

[4] I referred him to the CAIB hotline publicized on its Web site.

my accumulating data on the accident into a more systematic comparison, beginning a focused revisit to the site of my previous study.

Was this accident another example of the normalization of deviance or not? If not, what explained it? If so, what contributed to it? The available information—the long history of foam problems, the phrase "we were comfortable with it" and NASA's public certainty that the foam could not have caused the accident, severe budget shortages, safety personnel cuts, disempowered engineers—all indicated a possible fit with the theory explaining the first accident. To identify both similarities and differences, I began systematically sorting my data into institutional, organizational, and decision-making levels of analysis.

I had been responding to the news; now I became the news. In press conferences, Admiral Gehman, influential not only by position but because he was a charismatic master of the spontaneous sound bite, repeatedly stressed the importance of the social causes of the accident. When he announced that I would testify before the CAIB in Houston, the *New York Times* ran "Echoes of *Challenger*," while the field's leading journal, *Aviation Week and Space Technology*, headlined "*Columbia* Board Probes the Shuttle Program's Sociology." My testimony was scheduled for the afternoon of April 23, 2003, to be preceded by a briefing of the CAIB on April 22. It was the best of all possible teaching opportunities: the CAIB would be making policy recommendations and NASA personnel were no doubt watching the ongoing televised hearings. My presentation had to be absolutely clear, the evidence convincing. However, between my end-of-semester obligations and the need to keep up with new developments in order to respond to CAIB questioning, time for my analysis and testimony preparation was scarce. I had no experience as an expert witness, let alone as a participant in an official government investigation of this scale. Unaware of the extent of the book's influence on the CAIB's thinking, I arrived in Houston anxious about my interrogation.

Backstage and Frontstage: Public Hearings and the Dissemination of Social Theory

The CAIB occupied a small two-story brick building on Saturn Boulevard near Johnson Space Center, site of Mission Control and the controversial management decisions about *Columbia*. Innocuous looking on the outside, building security was tight. Identification cards were required. Only the magnetized ID cards or combinations punched into keypads opened doors inside and out. Unable to proceed unescorted, I was assigned an aide, a recent political science Ph.D. who became my informant, tutoring me about CAIB members, the investigation, and the hearings. We passed several people in military uniform. A place alive with activity and conversation, the hallways displayed some of the work in progress. Two mural-size scale diagrams mapped the daily recovery efforts: one of *Columbia*'s debris field, the other a reconstruction of *Columbia*, based on the pieces found.

In addition to the public rituals of press conferences and hearings, the CAIB had its private ones. At 8:00 a.m., I attended the "stand-up" briefing, a military tradition.[5] The admiral and CAIB members sat at a long polished table, with about 20 of the 120 staff in chairs on the periphery. Board members and staff investigating off-site were connected to the stand-up by teleconference. The admiral reviewed the schedule for the day and his activities, then asked the other board members for status reports. The proceedings were relaxed, to the point, and punctuated with humor. At the conclusion, the admiral introduced me to the group, and as we dispersed I was surprised when several staff members approached me to sign copies of my book. I then learned that Admiral Gehman had read my book two weeks after the *Columbia* accident, saw empirical analogies with *Columbia*, and became convinced of the relevance of the sociological perspective and theory of the book for the investigation. Soon, copies of it and a theoretically true but de-jargonized management journal condensation that I wrote (Vaughan 1997) were circulating in the offices of the CAIB and its staff.

Expecting an adversarial grilling in Houston, I found the board already receptive to sociological analysis. Prior to the CAIB briefing that afternoon, I was asked to attend a meeting of Group II, a subset of CAIB members who were investigating NASA's foam decision making and the NASA organization. Not knowing what to expect, I was again surprised when Group II board members Major Geneneral Ken Hess, chief of safety, US Air Force, and astronaut/physicist Dr. Sally Ride asked me to give a sociological interpretation of their data. The group described their investigative process and revealed their findings in great detail. Working through the data with them, I suddenly felt at home. They were convinced that the foam debris anomalies were another example of the normalization of deviance. We discussed similarities and differences between the two cases. Not only were the empirical and theoretical analogies with *Challenger* deeper than I suspected, I was impressed by the strength of their evidence and their systematic research approach: they were analyzing other technical problems during the same time period when, under the

[5] For coordination, all responsible parties report their activities. Typically, the group is small and the meeting short, so people stand. We sat. The CAIB culture was professional, not military, however. Board and staff from other professions were more numerous than military members. Also, the structure was flat, participants had high task autonomy, and leadership practice and decision making were democratic, welcoming all opinions regardless of profession or status in the group.

same institutional and organizational conditions, NASA had not normalized anomalies, instead halting launches to fix them.

In the board briefing, I went through a longer version of my next day's testimony. My aide had warned that the CAIB's human factors–trained technical experts would challenge a sociological explanation, but that did not happen.[6] In the three-hour exchange, the similarities and differences between the two accidents again were central, but charged with producing policy recommendations for changing NASA, the CAIB also was concerned with how to prevent yet a third accident. We adjourned to a relaxed dinner in a small Italian restaurant nearby. I learned about CAIB process: the beginnings of their investigation, the excruciating debris search and recovery effort, NASA "push-back" against CAIB requests for information, breakthroughs, and unresolved puzzles. Although each board member was powerful in his or her own domain, the day's events and the dinner showed them engaged as equals, caught up in unraveling what had happened and why, keenly aware of the seriousness of their undertaking and its political consequences. I worked late, completing my testimony based on the evidence I brought with me without reference to the new data revealed to me in utmost confidence. What I had learned nonetheless validated my conclusions about the many similarities between *Columbia* and *Challenger* and the two important differences I had found. Nothing to date indicated that production pressure had a direct effect on *Columbia* decision making, as it had for *Challenger*. Second, the normalization of deviance was even more institutionalized for *Columbia.* Damage to the thermal protection system was expected as a consequence of environmental forces during space flight, so even before shuttle missions began, damage to the thermal tiles had been defined as a "maintenance problem," not a safety of flight issue.

The CAIB's public hearings began at 9 a.m. I spent the morning reviewing data with the Group II staff in their office, the "Lava Lamp Room." In a move that established a continuing informal connection, they asked if I would read and respond to drafts of their analysis via e-mail and telephone. Never had I intended such an extended diversion from my ongoing research, but now hooked by their puzzle and process, I agreed. I then rushed to the hearings, which were held in the ballroom of my hotel. On my way to the makeshift "green room," I opened the ballroom door a crack to watch. The setting announced the proceedings as formal, consequential, adversarial, and public. A wall of blue velvet draperies, centered by a huge PowerPoint screen with "*Columbia* Accident

[6] Human factors experts examine technical causes and human error due to individual attributes, such as incompetence, negligence, poor supervision, poor training, lack of sleep, physical or mental impairment, etc.

Investigation Board" imposed over the navy, gold, and red image of the crew patch for *Columbia*, was a backdrop for two long tables, adorned with white table clothes and water pitchers. The tables were distant and opposite each other, forming right angles with the crew logo. At the table to the left of the logo were two witnesses being questioned about managing aging aircraft; on the right sat seven members of CAIB, two in full military regalia. Brilliant television lighting set off the questioning area from the rest of the room. In the shadows were invisible translators who would convey what transpired to a large audience. At a small table to the left of the witnesses a transcriptionist sat typing a verbatim record. To the right of the CAIB, technicians operated banks of sound and lighting equipment. In folding metal chairs in the first three rows of audience seating were about 40 members of the press, heads bent to their work. Scattered in seats behind them sat perhaps a dozen spectators.

It is amazing how we, as students of rituals, can experience and respond to their symbolism and cultural meanings, conforming to what the setting requires despite our critical understanding of the social reality that rituals produce and how it is accomplished. Gone was the comfortable security with my presentation and my relationship to the CAIB instilled by the collegial welcome and the easy exchanges at dinner. This was interrogation in a public forum. I closed the door and hurried to the green room to cram. Soon the television monitor showed the hearing session concluding. Board members appeared for a break and sandwiches. They told me what had transpired in the first two sessions of the day, I met a former astronaut working as a liaison between NASA and the CAIB, then it was time to resume. As we lined up to enter through the velvet curtains, one CAIB member put on someone's military hat and saluted us as we walked by. Laughing, the board member in front of me turned to say, "Now you get to see our dog and pony show." With cold hands and keen awareness of this as the most literal deployment of Goffman's (1959) frontstage and backstage distinction that I had ever experienced, we passed through the parted curtain into the bright lights and, seated at tables in symbolic adversarial opposition, snapped into appropriate roles. The admiral welcomed me, asked me to introduce myself, then I made a presentation essentially the same as the one I gave the day before.

Setting up the parallels between *Challenger* and *Columbia* as a sign that the social causes of *Challenger* had not been fixed, I laid out the causal theory of the first accident using key concepts in a dejargonized, example-based version that explained *Challenger* as the result of an organizational system failure. A chart titled "The Trickle-Down Effect" (my wannabe sound bite) showed the connection between the parts of that system: how NASA's institutional environment—historic political and budgetary decisions by the White House and Congress—transformed

NASA's organization culture and structure, thus influencing the decision making about the O-rings and resulting in *Challenger*'s destruction. Next I pointed out the parallels between the two accidents. I used examples from both that plugged into the three levels of analysis in the trickle-down model, emphasizing the differences as well as the limitations of my data. Stressing the importance of connecting policy with the social causes of NASA's two accidents, I concluded with examples that addressed the systemic institutional, organizational, and decision process flaws that contributed to *Columbia*'s demise.

Instead of adversarial challenges, the CAIB's insights and questions reinforced my points, showing their understanding of sociological principles and their preoccupation with how to translate them into policy:

> *Dr. Widnall* I've mused over this issue of how an organization that states that safety is its no. 1 mission can apparently transition from a situation where it's necessary to prove that it's safe to fly, to one in which apparently you have to prove that it's not safe to fly. I think what's happening is, in fact, that engineers are following the rules but this underlying rule is that you have to have the numbers. . . . So that means that every flight becomes data that says it's safe to fly . . . and people who have concerns in one of these uncertain situations that you talk about, they don't have the data. So I think it may be getting at, in some sense, changing the rule to one that it is not okay to continue to operate with anomalies, that the underlying rule of just having data is not sufficient to run an organization that deals with risky technologies. (CAIB, public hearing transcript, April 23, 2003; pp. 53–54)

> *Major General Barry* Now, if we try to look pre- and post-launch, prelaunch is very formal . . . you've even alluded to it in your book. Postlaunch, it could be argued, less formal, more decentralization, more delegation certainly. . . . So I would ask really your opinion that, is there some kind of a delineation in your mind, from what you know to date, pre- and post-launch, that we might be able to provide solid recommendations on to improve NASA? (CAIB, public hearing transcript, April 23, 2003; pp. 56–57)

> *Admiral Gehman* Several things you said struck me, and they're related to each other. One is that you can't change the behavior unless you change the organization. You can change the people, but you're going to get the same outcome if the organization doesn't change. Yet in another place up there, you said beware of changing organizations, because of the law of unintended consequences. You've got to be real careful when you change organizations. What do you make of the post-*Challenger* organizational changes that took place, particularly in the area of more centralization and program management oversight? . . . Is that the kind of organization which would recognize mixed and weak signals and routine signals? (CAIB, public hearing transcript, April 23, 2003; p. 48)

The questions were difficult. Nearing the third hour, I felt it would never be over. Then CAIB member John Logsdon took a different direction. He said that the board knew my book had received a lot of publicity when published and had been used to help improve a number of safety programs, including one for nuclear submarines. He asked if I had been invited to speak or consult with NASA after the book's publication (CAIB, public hearing transcript, April 23, 2003; p. 52). I replied that I had been contacted by many organizations concerned with reducing risk, error, and mistakes, then ticked off a number of examples showing the variety. Relaxing with his easier question, I said, "It seemed like everybody called—my high school boyfriend even called—but NASA never called." The room erupted in laughter. The book's theory and concepts traveled farther as my testimony—like that of other witnesses—was shown live on NASA TV and video-streamed into television, radio, and press centers and the internet.

My testimony was summarized in major newspapers and distributed by wire services. However, the sound bite was not "the trickle-down effect," but my unscripted "boyfriend" comment. I viewed this as a teaching failure. NASA's chief flight director confirmed my fears in an e-mail titled "Sound Bite." His message read, "My high school boyfriend called, but NASA never called . . . a very cheap shot. You kind of had me interested until then . . . too bad." In a press conference, NASA administrator Sean O'Keefe, repeatedly queried about my testimony, commented bitterly, "Book sales must be up." One reporter e-mailed, "That just shows he doesn't know anything about publishing *either*." Then NASA called. An associate administrator, in a congenial but one-sided two-hour conversation, denied any similarities between *Challenger* and *Columbia*, carefully explaining to me all the post-*Challenger* changes that had been made, which he, in fact, had overseen. No similarities? Incredible! Was he sincere or was I getting the official line? Much later would I learn that many at NASA had rejected the *Challenger* comparison outright, but copies of my book began appearing on desks at NASA's Johnson, Kennedy, and Marshall space centers and at NASA Headquarters (Cabbage and Harwood 2004, p. 203).

The Social Construction of the Social in the CAIB Report

Between the twin public spectacles of official accident investigation and report release is a hiatus during which the data are analyzed, sifted, and sorted for inclusion or exclusion, and a framing discourse established. Gradually, my informal association with the CAIB became a formal one: I became an active participant in the production of the CAIB report and a collaborator in the creation of its sociological frame. During May 2003, as agreed, Group II sent drafts analyzing NASA foam decisions for feedback. In June, when the board moved from Houston to Washington, D.C., to conclude its investigation and write the report, Group II asked me to come for a few days.

The CAIB was in a small office complex in Arlington, Virginia, again in an innocuous-looking building, this time a high-rise also housing offices of U.S. Department of Homeland Security—not so innocuous after all. The CAIB and about 40 staff members occupied a floor laid out with a pattern of inner and outer offices in such a confusing stretch of long intersecting hallways that key junctures where one could go wrong were marked with easels bearing large signs indicating what locations would be the result of a turn at that point. Admiral Gehman had a modest but well-appointed corner office. Board members were two to a small, windowed office, an arrangement that worked well because some CAIB members continually were traveling for the investigation. Staff shared inner offices. A smaller version of the Lava Lamp Room had been resurrected in D.C. The hub of staff activity, however, was "The Bullpen": a large busy room in the middle of the office complex where 16–20 people worked at open desks and cubicles in the midst of photocopy machines and printers, while people crossed through the room as a shortcut to get from one corridor to another. Entry could be had only by punching in the lock combination. The security system was, in principle, the same as in Houston, with two exceptions: ID cards operated the elevators, permitting exit only at the CAIB floor, and one room housed a shredding machine that was busy every night.

Group II brought me up to speed on investigative developments. My assignment was to consult on data interpretation and give feedback on chapter drafts about NASA's contribution to the technical failure. Most of my time was spent with General Hess and his Air Force Safety Center staff, who had been assigned the chapter on organizational causes. Drawing on the social science literature on organizations, risk, and accidents, their drafts were heavily weighted toward social psychological theories, which, they explained, came from a belief in individual leadership instilled by the military. We began what became a continuing debate about the relative merits of individual learning and cognition versus structural and cultural explanations as a match with the *Columbia* data. I noticed that already embedded in chapter drafts were the concepts from *Challenger* explaining data from *Columbia*: weak, mixed, routine, and missed signals, organization culture, the normalization of deviance. Further, the outline of the report promised an extraordinary document. In contrast to the traditional human factors accident report focus, the CAIB report featured an "expanded causal model" giving social causes prominence. Part 1would present the accident's technical causes, part 2 the social causes, and part

3 the board's recommendations. The part 2 editor, a doctoral student with some social science background, explained how this innovation came about.

Admiral Gehman, while reading my book and article in mid-February, had enthusiastically championed the concepts and their application to the *Columbia* data to the existing board in hallway conversations and at lunch. Persuaded by empirical and theoretical analogies between *Challenger* and their early investigative findings, the admiral thought that a large part of the report should focus on social causes even before the final board members were appointed. Developing the report outline with the Admiral, the editor gave examples from the book, fleshing out the details. The topics of the three part 2 chapters paralleled *Challenger*'s three-part causal model: NASA's political/economic history, its organization structure and culture, and its decision-making processes.

Now in its sixth revision, the report outline distinguished these three social cause chapters by declining causal importance: "Beyond the Proximate Cause," "Factors that Contributed to the Loss," and "The Accident's Underlying Causes." These chapter titles were confusing. Also, I doubted that readers would understand the board's attempt to discriminate social causes by degrees of importance, nor were they likely to make sense of the implications of these distinctions for the changes that NASA would be asked to make. Would the board be viewed as engaging in a costly investigation with unclear findings? Moreover, theoretically and empirically, for *Challenger* the social causes were equally important and interrelated. The three in combination had precipitated the technical failure. And so it appeared for *Columbia*. Encouraged by the admiral's openness to sociology and democratic practices that defied military stereotypes, I proposed a different outline that gave these chapters substantive titles, made the three social causes equal, and showed they were causally connected.

The admiral endorsed the outline, but disagreed about chapter 5. "History is a scene setter, not a cause," he said. Giving examples, I explained how history had contributed to both shuttle accidents. First, historic decisions in NASA's external political and budgetary environment changed the agency's structure and culture, ultimately affecting risk decisions. Second, NASA's history of engineering decisions mattered. The first decision to fly with the anomalies set a precedent for accepting more such decisions; then repeated decisions created an empirical base and cultural belief in acceptable risk that influenced the tragic outcomes. He was dubious, so I proposed a writing experiment, to be used in the report or not at the board's discretion, that showed the causal links between the political environment, organization, and decision-making chapters. "How do you know you can do that?" he asked. "I'm trained to do that," I said.

When I returned to Boston the end of June to begin my writing experiment, I became a permanent part of the social control apparatus for the agency I had once studied as an "objective" academic. I was added to the CAIB staff and payroll as a researcher and member of Group II, but unofficially I became a member of the group, too, my insider status announced in an e-mail titled "Greetings, Lava Lamp Member" sending photos of us at the office. I was not "going native." I *was* native. I had taken a step toward policy sociology that immediately created tensions with both my public and professional sociologies. Because I was working with data from some interviews given under conditions of anonymity and considered privileged communication by the board, I was asked to sign a nondisclosure agreement. Having built a network of media connections based on my position as a professional sociologist, I now had to consider carefully the source of my information before I spoke.

I managed the conflict with deception, a strategy that felt like a violation of trust. I did not disclose my new status, suspecting that my affiliation might change the questions I received from reporters and others. I used my cell phone to disguise my location and declined interviews that clearly risked conflict of interest, hedging the reasons. There were other complications. My proposed chapter, like my testimony, was going to show that the social causes of *Challenger* had not been fixed by comparing the two accidents, but now my analysis could be based on the board's extensive original evidence. However, I did not control the direction of the research, its duration, or my final product, which, the admiral made clear, belonged to the CAIB permanently, to edit, use or not use, as they wished. This was a condition against the tradition of professional sociology but equally applied to all board members, six with PhDs, so I could not object—nor did I want to. The report was a collaborative product; I was part of the team.

Even this close to the report deadline, e-mails and chapter drafts showed new evidence important to the investigation. The most significant was a detailed report from an off-site field investigator with empirical evidence proving that production pressures had affected the *Columbia* foam strike decisions. The missing link was no longer missing. Analogical to *Challenger*, a direct relationship existed between NASA's political/economic history, an organization culture dominated by deadlines and schedule pressures, and crucial management actions. On July 21, I sent a draft of "History as Cause: *Columbia* and *Challenger*." The admiral immediately accepted it as a chapter, along with its implications for the board's expanded causal model. Elated, next day I returned to Washington for the duration of the CAIB's work.

Now permanently ensconced in the Bullpen, I saw that the pace had quickened. The admiral had extended the July 1 report publication deadline because the investigation was still underway and massive amounts of writing and editing remained. However, meeting the new August 26 date was essential because Congress needed time to read the report before reconvening after Labor Day. The political significance of the investigation was palpable. The admiral and board members were meeting with members of Congress, who objected to the board decision to keep some interviews confidential. The press, trying to uncover the details of the report, was ever present. There was concern about leaks: we were instructed not to leave documents on our desks overnight. Many off-site staff investigators were back, writing sections to be inserted into chapters. Their data and analysis were integrated into my chapter; sociological connections and concepts were woven into theirs. As the part 2 editor began integrating the parts into a whole and bridging gaps, the draft of the report revealed connections between the social cause chapters that had not before been made. For the editor's use, I wrote sample introductions and conclusions that provided continuity of sociological concepts and causal linkages between each chapter. The editor and I stayed in close touch, coordinating the integration of sociological themes.

The sociological framing of the report accurately reflected the interpretive schemes of those writing it. NASA released the transcript of the key mission management team (MMT) decision meeting for the *Columbia* foam debris hit, then had a closed-door, invitation only "mea culpa" press conference with Linda Ham, the MMT head who turned down the engineers' imagery request. A rising star at NASA, Ham had been transferred to a lesser position. The CAIB public affairs lead sat in the Lava Lamp Room reading her notes on the press conference to all who crowded in. The conversation after reflected the collective understanding that the causes of the accident were in NASA's organizational system: another manager holding that same position would have been exposed to the same normalization of foam anomalies, organization culture and structure, political and economic circumstances and thus, reacted the same. Ham was, as one said, "just the person who happened to be standing there at the time."

I had the usual balloon of e-mails from press, several sending transcripts of the MMT meeting for interpretation, thus reviving conflict of interest. In July, I did fewer interviews because I needed time to write my chapter in addition to my work on the others, but the MMT incident involved issues of hierarchy for which sociology has much interpretive power. The admiral had decided that the board's view of MMT actions was not to be discussed with the press; their view would be in the report. However, in response to my query the admiral concluded my position was different. As a scholar, I could speak on any issue, as long as I did not disclose privileged information or appear to be representing the board. The public affairs lead and the part 2 editor encouraged me, saying it was in the board's interest for me to continue a relationship with the media because my sociological interpretation and the board's coincided, and I could speak at times and venues when they could not.

In the production of documents and their discursive frames, the editing process is crucial. Everyone had to make drastic cuts in order to reduce approximately 1,000 pages of first drafts to a more readily digestible 250-page volume. Rank was meaningless; no one was spared. Completed drafts were put on a computer-editing program that permitted all board members to insert initialed comments. Revisions were subjected to the same procedure, the full board commenting on every draft. On July 23, one month before report release, board members convened for the plenary review of part 1, "The Accident," which consisted of four chapters analyzing the technical failure. In three long days of plenaries (only editors and key writing staff included), the penultimate drafts were debated for accuracy, clarity, and to assure the final version represented board consensus. For the report's "Executive Summary," the board composed a section entitled "Physical Cause Statement." They approved the revised outline of new chapter titles giving equal weight to the social causes of the accident; they accepted "History as Cause" as chapter 8.

The board reconvened August 6 to review the penultimate drafts of part 2, "Why the Accident Occurred." They wrote an "Organizational Cause Statement," unprecedented in an accident investigation report. With the exception of "an informal chain of command and decision processes that operated outside the organization's rules," this statement perfectly described the organizational system factors contributing to *Challenger*:

> The organizational causes of this accident are rooted in the Space Shuttle Program's history and culture, including the original compromises that were required to gain approval for the Shuttle, subsequent years of resource constraints, fluctuating priorities, schedule pressures, mischaracterization of the shuttle as operational rather than developmental, and lack of an agreed national vision for human space flight. Cultural traits and organizational practices detrimental to safety were allowed to develop, including: reliance on past success as a substitute for sound engineering practices (such as testing to understand why systems were not performing in accordance with requirements); organizational barriers that prevented effective communication of critical safety information and stifled professional differences of opinion; lack of integrated management across program elements; and the evolution of an informal chain of command and decision-making processes that operated outside the organization's rules. (CAIB, vol. 1, 2003, p. 9)

As before, only the editors and key staff doing the writing attended these plenaries, this time including me. The admiral, who in stand-ups had been announcing my presence to those on the teleconference line after he named the board members in the room, an acknowledgment not given to other staff members, now elevated my status physically by asking me to sit at the table, rather than with staff on the periphery as before. I was not confused by what was happening. He was not elevating me as a person. Instead, having himself made a commitment to sociology, he was signaling the board of that commitment, assuring the sociological framing of the report and thus, what was to become the official explanation of this accident.

I did not participate in the reviews of the technical chapters, but the changes in my chapter 8 are illustrative of the preservation of the integrity of the authors' analyses that also prevailed in part 2, chapters 5 and 6. Enthusiastic about my first draft content, the board's objections were to style ("bombastic and poetic") and length, which resulted in my cutting 25 single-spaced pages to 18 and changing language (delete "This chapter will connect the dots"), some of which the editors inserted in other chapters. The board's computer text edits corrected technical points, changed wording ("not economic—budgetary"), made some examples stronger (insert after "proceeded": "without thorough hazard analysis") and weakened others (delete "The ladder of responsibility reaches higher than middle-level managers like Linda Ham, Calvin Schomburg, and Ralph Roe"). Now, in a four-hour line-by-line review by 13 multidisciplinary readers that was the closest reading any draft of mine has ever had, the queries were toward accuracy and consensus on every point ("She writes, 'The board strongly believes. . . .' Does the board 'strongly believe'? Does the board believe at all?"). None of my original points were lost, however. The theoretical explanation of the two accidents and the causal linkage between its three parts remained intact

The sociological analysis did not entirely prevail in chapter 7, "The Accident's Organizational Causes," which covered both the organizational factors normalizing the foam problems and the structure of the safety system. This latter was a separate cause because it failed to intervene to change the construction of risk, thus perpetuating it. The chapter was made up of sections, each written by a different person. Assembled, it was long and unwieldy. Ken and his staff were still revising and I was still reorganizing and editing even as the plenary convened. The board discussion went to the lack of clarity and to a different organization of the chapter. Board member General Barry, who worked on the technical investigation, wrote an alternative outline of the chapter on the white-board that kept the substantive topics but lost the causal link between organization factors and the ineffective safety system. In an attempt to save the day, the part 2 editor proposed a compromise outline that re-organized with causal connections intact. The admiral left it to Ken to decide. Ken, now well read in organization theory and committed to the chapter structure, responded to the disapproval of his peers. We continued this 16-hour-day week, revising along the editor's model through the weekend.

In 11 days, the report had to go to the printer. With chapter 7 now in disarray and revisions due on my chapter 8, I had to leave to prepare for the annual American Sociological Association meetings that same week. Ken, I was devastated to discover, would also be away, no longer writing himself but directing his staff via e-mail. I would continue working on chapters 7 and 8 by e-mail, but my meeting commitments prevented a return to the CAIB before the report deadline passed. With Ken and me absent, the admiral also away, and the editors into a 20 hour-day final week, John Barry, acting in his capacity as CAIB executive director, took over the writing of chapter 7. Ken and I tried to salvage it long distance, but the deadline prevented major revisions. The final product remained a skillful organizational analysis, a reflection of the mastery of the literature on risk, disasters, and organization theory by Ken and the Lava Lamp Room team, but the within-chapter causal connection was lost.

On August 21, a small group of editors and staff flew in a NASA jet to Seattle, where an initial 500 copies of the report were printed, boxed, and returned with the group to Washington, D.C., on August 25. The Sunday *New York Times* ran a feature about the admiral, who announced that the report would give equal weight to the technical and social causes, outing me as the source of the board's approach and the author of chapter 8. My last official task was assisting with preparation of slides on the social causes of the accident to be included in a PowerPoint presentation individual board members would use as they responded to the many speaking invitations already pouring in.

The Institutionalization of Social Causes and NASA Revisited

The final stage of post-*Columbia* rituals of risk and error was the return of public spectacle upon the report's August 26 release. The board's explanation of *Columbia*'s demise became massively public, institutionalizing the expanded causal model as the official explanation. Orchestrated with military precision, the report "roll-out" began in three backstage gatherings of the board's most significant publics. In Houston, two board members met privately with *Columbia* astronauts' families to present them with copies and discuss the board's findings. In Washington, the admiral and Sheila Widnall delivered copies of the report to NASA Headquarters. And in a "press embargo" devised to assure equal access to the story, media representatives were admitted to a "reading room" where the closely held copies were available for reading from 6:00 a.m. until 10:00 a.m. The public spectacle began at 12:30 with the board press conference. I watched with excitement from a television studio in Boston. Carried live on all networks, NASA TV, and the Internet, the board made clear that the *Columbia* disaster, like that of *Challenger*, was a failure of NASA's organizational system. The board emphasized that unless the technical, institutional, organizational, and cultural recommendations made in the report were implemented, little would have been accomplished to reduce the chance of another accident.

Next day the language of sociology was generic in the press. Headlines pointed to a flawed organization culture; newspapers in NASA Space Center cities published entire sections about social causes; the *New York Times* excerpted all the part 2 chapters. The theory of *Challenger* and *Columbia* traveled next into the official policy-making arena, as the board presented their expanded causal model to the U.S. Congress, House Committee on Science. In his prepared remarks, the admiral read the board's organizational cause statement, concluding, "The Board believes that these factors are just as much to blame as the foam" (Gehman 2003, p. 3). Responsible for NASA oversight, the House committee inquired if the board would be willing to take part in the Congressional return to flight (RTF) evaluation of the changes NASA implemented. The board agreed that, if asked, they would be willing to reconstitute and serve.

This House committee later interrogated me on a teleconference about the social causes of *Columbia* and NASA's potential for change. They were concerned with accountability and blame. Missing or ignoring the policy implications of an organizational system failure, one person asked if I thought criminal charges were appropriate. Another became angry about members of Congress being implicated as a cause of the accident when I explained that unless budget allocations for NASA were increased to make the new goals for RTF feasible, the agency culture would continue to be plagued by scarcity and production pressures detrimental to mission safety. Apparently, they wanted heads to roll, but not their own. Next the board briefed a joint session of the House and Senate, receiving a standing ovation. The board then dispersed, with individual members conveying their expanded causal model in speeches in national and international venues.

I, too, continued to convey the sociological message, this time reaching an unexpected audience. In a televised press conference the morning after the report release, NASA's Sean O'Keefe declared that NASA would "fully comply" with the board's recommendations, then, deferring further comments until they had time to absorb the report, opened it up for questions. The first question was, "Have you read Diane Vaughan's book yet?" He replied," Yes, we're all reading it. Some people from here have contacted her, the first several months ago." Following up, an Associated Press reporter called me to verify. I told her of my sole contact with the NASA associate administrator after my testimony and his "no similarities" stance. She then called that same administrator to check. He concurred, admitting he had been wrong. Immediately NASA called, I assumed as a result of press leverage and to avoid another "NASA didn't call" story. Indeed, her AP wire story, "NASA Finally Looks to Sociologist" (Dunn 2003) reported that NASA had invited me to a dinner at headquarters to talk with top officials, who had shifted from denial to acknowledge that the systemic institutional failure that led to *Challenger* also caused *Columbia*. My revisit, heretofore at a distance, now was face-to-face. The dinner was in the ninth-floor dining room, recently renamed the "*Columbia* Café." Feeling as if I were walking into the proverbial lion's den, I was surprised to be met at the elevator by the very NASA associate administrator who had phoned me in April. Welcoming me, he earnestly and apologetically began explaining why he believed as he did at that time. Realizing I was there for symbolic reasons, I balked when I saw a gaggle of reporters at the end of the hall, but he whisked me by, saying "They are not here for you." The café was small and modest, with NASA photos and plaques on the wall, grim reminders of all NASA's accidents and lives lost. I met NASA elites, names familiar from discussions at the CAIB, who confided their connection with one or both shuttle accidents. Board member Roger Tetrault, invited because he had missed the board's briefing at NASA, arrived with prepared comments.

Place cards seated me at a table with two former astronauts, one now NASA's chief scientist, the other, director of safety. The fourth seat was for Sean O'Keefe, who was delayed at the White House discussing the report and "the future of space flight." Like the admiral, who also had a striking white-haired appearance and public charisma, O'Keefe later spoke to the group about the impact of the accident and NASA's vow not to repeat. When Tetrault took his turn, he repeatedly castigated them for what the board called a "broken safety culture." O'Keefe countered by explaining that the "*Columbia* Café" was named as a constant reminder of that broken safety culture, following with a statement of how they were processing the report's findings, concluding forcefully, "Let us get up off the mat!"

O'Keefe then whispered that I, too, could make comments but not to feel obligated as, unlike Tetrault, I hadn't been asked to prepare a talk—a fact that confirmed my hunch I was a late addition, invited by virtue of press leverage. From my conversations before and during dinner, what struck me most was that these people were grieving—for the astronauts, for their own responsibility in the accident, for NASA's lost goals, and for the agency's future. Further, they did not know *how* to implement the board's recommendations for structural and cultural change, especially the latter. Indeed, the board had left guidelines for cultural transformation vague, working more toward achieving it through structural change. Seeing this as the best of all possible teaching opportunities and winging it, I translated the report's findings, identifying necessary cultural changes indicated by each of the social cause chapters.

Suggesting they not wait until after an accident to discover cultural flaws, I recommended they get regular feedback on culture by funding a number of continuing fellowships for ethnographers—sociologists and anthropologists—locating them throughout the agency, replacing them every year with new ones to avoid acculturation. Discussion about changing NASA followed. They talked with great feeling about the difficulty of incorporating the board's main suggestion for structural change into the existing organization structure. As we were dispersing, two administrators pointed out "personality similarities" in a lead *Challenger* engineer and his *Columbia* counterpart who took their concerns to a certain hierarchical level then stopped; they asked me why changing engineering personnel was not the answer. They didn't get it. Seeing what a challenge making the required organizational changes was going to be, I later typed my comments, e-mailing them to all with a reading list of relevant literature on organization theory, risk, and disaster. Now the e-mail balloon was from NASA personnel.

In October, I was invited to speak at NASA's 40 Top Leaders Conference, a two-day retreat on the Wye River, Maryland, attended by key personnel from all NASA centers. The NASA administrator in charge began by advising the attendees to read the report carefully because "not everything in it is true," and that NASA headquarters was checking into what it would mean to "legally comply" as opposed to "fully comply" with the board's recommendations. Suddenly remembering I was sitting beside him, he uttered an unfinished caution, "Diane, don't you. . . ." Then he gave a dynamic talk to energize them about NASA scientific developments underway and the shuttle's return to flight. My presentation was a detailed explanation of *Challenger* (not all present had worked at NASA in 1986), a short comparison with *Columbia*, and an expanded version of my headquarters points about changing organizational systems, which unexpectedly triggered a two-hour discussion and a vote to continue by adding a session next morning.

Before he left the room, the administrator said, "We thought you were a NASA critic, so at headquarters we didn't know what to expect, but we saw that you were a social analyst." He asked me if I would help them. I was tempted—finally a chance to study NASA from the inside—but told him I would not due to conflict of interest: if the board participated in the RTF evaluation in fall 2004, I would be involved. Privately, I wondered if the administrator really wanted help, or if my joining them would be for symbolic purposes only. My work would be compromised if NASA intended to "legally comply" as opposed to "fully comply." Moreover, not only was I not trained to implement organizational change, but my analysis of the two accidents as organizational system failures predicted future accidents. Change could reduce the probability of another accident, but never prevent it because of the recalcitrance of systemic causal factors.

The next day only reaffirmed my prediction. The administrator absented himself while the Space Shuttle Program and International Space Station leaders met separately to discuss ideas about change. They composed a list of flaws in the agency culture. These leaders described a culture of hierarchy and disempowerment. NASA headquarters, they agreed, was the main obstacle to safety and change. They angrily reported the same difficulty convincing those above them in the hierarchy of their safety concerns as did NASA engineers in the *Challenger* launch decision meeting and after the *Columbia* foam strike. In frustration, one said, "Am I supposed to tell my people we are going to comply or that we are going to legally comply?" Changes now were being imposed so rapidly from above without explanation that many were confused about their responsibilities. At meals and breaks throughout the day, I met people with NASA at the time of *Challenger* who spoke of *Columbia* with tears in their eyes, devastated that they could have done it again, dedicated to doing everything they could to prevent a third accident. In small groups and private conversations, they also asked me to help. I saw they were sincere. I also saw what they were up against: my revisit showed the reproduction of the social causes of both accidents at this key transitional moment when the agency was trying to change.

However, there was a way. The program manager for the International Space Station (ISS) was enthusiastic about my suggestion for an ongoing cultural analysis by ethnographers. He wanted to get started because, unlike the sidelined shuttle, the station was still a going concern. The result was a plan for ISS research, for which I would apply, that would provide cultural feedback for the station as well as an ethnographic research opportunity for doctoral candidates and postdoctoral fellows in sociology. In January 2004, I wrote a proposal in response to his ISS posting on NASA's Web site for prospective applicants for an International Space Station Organizational Behavior study to begin in September. However, the month before, NASA headquarters had posted a request for proposals for an agencywide cultural analysis. Ironically, the CAIB requirements for change prior to resuming launches had reproduced the very production pressures that the CAIB identified as a problem. The announcement was posted December 16, 2003, with proposals due January 6, 2004, soon extended a meager 10 days. Requesting a three-year study, NASA required data on cultural change in six months (in time for the scheduled congressional evaluation of RTF readiness), then again at annual intervals. In February, the ISS program manager phoned to say that he was concerned our ISS ethnography project would be caught in agency politics if the results should conflict with those of the larger study conducted by a NASA contractor. Expressing regret, he decided it was an inopportune time to initiate it. So ended my NASA revisit.

HOW THEORY TRAVELS: ANALOGY, BOUNDARY CROSSING, AND PUBLIC SOCIOLOGY

In this ethnographic reconstruction of post-*Columbia* rituals of risk and error, I have traced how the sociological theory and concepts embedded in an academic monograph traveled across disciplinary boundaries, becoming grist for the mill of public discourse and policy. The process began with a book—a work of professional sociology, not written with a broad audience or policy making in mind. Lauder et al. (2004) suggest that a work of professional sociology becomes influential because of its relevance, the strength of its evidence, the architecture of its theory, and its ability to connect structure and agency. Initially relevant because it was timely, the *Challenger* book's continuing relevance originated in its ethnographic evidence, theory, and the macro-, meso-, micro fit with the *Columbia* data. Confirming Hesse's (1963) stance that analogy is necessary to the development of theory and its extension into new domains, analogy was the primary mechanism that enabled the theory of *Challenger* to travel. Stinchcombe (1978) argued that classifying two events, activities, or phenomena as analogous requires an empirical assessment that establishes a set of equivalence relations on a number of factors. If a great many elements true of one are also true of a second, the result is a deep analogy. The empirical analogies in the social causes of the two accidents were striking both in their number and kind. The theory and concepts explaining the *Challenger* disaster traveled to *Columbia* because of structural and processural equivalences at the macro-, meso-, and micro-levels of analysis (for the comparison, see CAIB 2003, pp. 195–204).

Consider ethnography's contribution to this process. Ethnography has the capacity to describe social life in sufficiently vivid detail that the relevant elements become alive for others. An account in ethnographic thick description can persuade because, despite a unique setting, it resonates with other events or circumstances. The *Challenger* book was historical ethnography, in which structure and culture were elicited from interviews and the documents created prior to the event in order to understand how people in another time and place made sense of things. Recognized for its strengths in exploring complex social processes at the microlevel in the present, ethnographic historical vision also was made possible by the unique data (Vaughan 2004). I was able to follow years of interactions and events documenting the incremental development of a debilitating construction of risk and cultural understandings that led to the accident. In addition, the data allowed upward vision, from social interaction to the layered structures and cultures in which it was embedded.

This upward vision was possible because the empirical evidence linking macro-, meso-, and microlayers of the social was observable in local conflicts, talk, rituals, rules and norms, emotions, and power and dominance relations—all the everyday stuff of everyday life to which the ethnographer attends. The result was an explanation of microlevel actions and outcomes with demonstrated empirical connections to history, politics, culture, and layered institutional and organizational structures. When empirical examples and sociological analysis are coupled with concepts and theories that stand in for them, matching and explaining the experience and observations of various publics, analogy cements that intersection of personal biography and public issues that Mills (1959) so importantly identified, enabling theory to travel across disciplinary boundaries. Sally Ride memorably voiced the sentiments of many e-mail correspondents, journalists, and the board when she said, "I think I'm hearing an echo [of *Challenger*] here" (CAIB press conference, April 8, 2003).

The first journey from *Challenger* to *Columbia*, and the role of analogy in it, initiated the second journey. The theory of *Challenger* traveled to other domains because analogy combined with social mechanisms: professional legitimacy, conversation, technologies, time, networks, and social support. Professional sociology was the springboard. A research-based book and my university affiliation worked as legitimating devices, giving me voice as an expert. Conversation, enhanced by technologies, also was a mechanism: telephone and e-mail facilitated contacts and rapid exchange of ideas with multiple publics, thus dialogic one-on-one teaching. Television, radio, print, Web, and wire service technologies disseminated sociological ideas widely, generating new contacts. Time became a mechanism as the eight- month duration of the investigation allowed me repeated conversations that reinforced sociological principles. These continuing conversations transformed some initial contacts into network ties with segments of the media and the CAIB.

Thus, the theory traveled to two tribunals of power with authoritative voice that were at once destinations and themselves mechanisms that enabled the theory and concepts of the book to travel farther, becoming increasingly public. Their command over public discourse eventually forced NASA's public acknowledgement of the relevance of a sociological explanation for the tragic fates of *Challenger* and *Columbia*, making the book required reading at NASA weekend retreats and workshops and opening agency doors to discussions about organizational change. The more public the definition of a situation, the greater the degree of institutionalization (Berger and Luckmann 1966, pp. 14–17). In the end, and unprecedented in an accident investigation, the equally weighted social causes of *Columbia*'s demise became institutionalized as the official explanation in the CAIB report, the records of Congressional hearings, NASA history archives, media archives, and documentaries.

It was the board's investigation that sustained the theory's presence in public discourse, so we must ask why sociological theory found this board a receptive audience. The championing of the social by Admiral Gehman, General Hess, and the part 2 editor was crucial, but looking beyond these individuals shows the importance of CAIB composition, structure, and process in the outcome. First, as directors of safety, six of the seven original members were professional accident investigators. Trained to look beyond technical causes to human factors, Ken Hess told me, made going from human factors to social causes an easy next step when, two weeks after *Columbia*, the evidence began pointing to political and organizational causes that reiterated *Challenger*'s tragedy. He said my book's organizational focus and concepts, new to them, helped make sense of their data and then led them to explore other social science research. The other members had either accident investigation experience, Ph.D.'s, or both, thus all board members were practiced in systematic research methods. The admiral's lead role in the investigation of the 2000 attack on the USS *Cole* and the controversial 1989 explosion of the USS *Iowa* gave him a critical perspective on government that (along with his retirement) made him staunchly independent in his thinking.

This independence was behind his decision to extend the report deadline from July 1 to August 26, with major effect. The board had the time to travel extensively, doing a lot of hands-on investigation work in the field. Empirical and theoretical analogies were verified by the board members as the ethnographic data in my book were reproduced in their own ethnographic understanding of NASA. Several times during the investigation and again to the press at the report release, the admiral stated, "There is nothing in this Report that we have not personally witnessed or experienced ourselves." The board's investigative skills and their extensive fieldwork—characteristics that distinguished them from the 1986 Presidential Commission—led to a working knowledge of NASA culture. The Presidential Commission, by comparison, was headed by a Republican political appointee, former U.S. attorney general William Rogers, who adhered to a three-month deadline imposed by then-president Reagan. The effect was an abbreviated investigation requiring extensive delegation of responsibility and less hands-on work by commission members, thus resulting in a report (Presidential Commission 1986, vol. 1) containing mistakes of fact and misinterpretation of NASA culture (Vaughan 2006).

The theory of the *Challenger* book functioned as a boundary object, a classification system able to cross boundaries and pull together otherwise unconnected people and groups in interaction (Star and Griesemer 1989). Boundary objects are able to maintain a core identity, even as they are tailored to suit the needs of the several communities of practice that they come to inhabit. Their malleability raises the question of what happened to the theory as it crossed sociology's disciplinary boundary into other domains. The myriad ways our ideas are transformed in thought and use are invisible to us, so I can only report what I was able to see. The theory of the book varied with the extent of my own hands-on involvement and time and space allocated for the presentation of information. The CAIB report shows the theory at its most coherent because with the board I had the greatest amount of time and input, the space dedicated to social analysis was extensive, and the printed word, once published, remained. Although my board affiliation allowed me direct influence on the content, I was still limited. The need for hands-on collaboration and reinforcement of ideas was clear because each time I was away, the sociological content and coherence of the report drifted. Also, the report was the work of a group. The final product was a compromise of many professional ideologies. Each person gained and lost ideas that she or he wanted, but the report retained the principle points of each, including its sociological frame, concepts, and interpretation.

In the media, the theory of what happened to cause the two shuttle accidents traveled in condensed form. In response to limited time slots on radio and TV, I reduced the theory to concepts, then editorial framing and editing processes further restricted, and in the case of TV, distorted, the sociology that reached the public. Even when time and space were ample, however, complexity and nuance, theoretical assumptions, and supporting evidence did not travel at all. Moreover, macrolevel conditions and their connection with organizational and individual factors that gave the theory of *Challenger* and *Columbia* its explanatory power were lost. Macrostructural principles are the hardest to teach, as evidenced even by the admiral (who, in one board meeting, gave a definition of culture equal to any a sociologist could give) when he protested, "History is a scene setter, not a cause"; the two people at NASA headquarters who suggested replacing key engineers with others with different personalities as a solution; and those on the House Committee on Science who called for individual punishments instead of structural change. Consistent with our experience in the classroom, various publics have their own theories about how the world works, and sociological explanations often rub up uncomfortably against American individualism and individual actor/rational choice models. Still, despite the expected slippage between the original theory and its public representation, its core identity remained constant in the media: the NASA organization had contributed to both accidents. The social conditions responsible were consistently conveyed by Blumerian sensitizing concepts working as sound bites, traveling independently of the theory.

In stark contrast to my previous public sociology experiences and those reported by others (e.g., Hammersley 2004; Becker et al. 2004; Stacey 2004; Ericson 2005; Beck 2005), the visible instances of misunderstanding, distortion, and misuse were remarkably few. In the media, rather than presenting multiple frames or distorting sociology to fit the prevailing media logic of the institution concerned, the sociological perspective *became* the frame. How to explain this unusual outcome? The initial media assignment was to report the research rather than seek an expert to get a quote or background information tangential to the expert's research, a situation that leads to greater accuracy (Stacey 2004, p. 141). Then analogies plus the duration of the investigation allowed my repetition of sociological ideas, contributing to the consistency. However, the unusual durability of the sociological frame over time across all media I could not have accomplished alone. It was undeniably tied to the board's authority, influence, and public reiteration of the social causes. The result was that no competing theory about NASA's contribution to the technical failure was entered into public discourse.

A consideration of how theory changes as it crosses institutional boundaries leads naturally to questions about the sociologist who travels with it, the work involved, and the transformative effects of the experience. As the theory crossed institutional boundaries, I physically went as well, relocating in exotic settings I had not previously inhabited. I also crossed boundaries of the sociological division of labor. A publicized sociologist the first few days, I quickly became a traditional public sociologist, then the course of events carried me, gradually and most unwittingly, into something more akin to organic public sociology. What did this work entail and what were its effects upon me?

Like classroom teaching, the work was based in discursive practices. Also like teaching, much of it was invisible to the public and to the profession. The work visible in the report and media does not begin to capture the hours and hours on the phone, e-mail, and Internet; research and data analysis for testimony; then daily meeting, writing, and editing with a new group. Nor does it capture the uncertainty of working on an unfamiliar turf. Although I had book-related media experience, nothing prepared me for this intensive, months' long engagement that thrust me into the policy realm. Never did I predict the extent of my involvement or the impact my work ultimately had. Often simultaneously doing professional, public, and policy sociologies and, less frequently, critical sociology, I found that working in the interstices of Burawoy's four sociologies was also emotional work.

Disciplinary acknowledgement goes, with little variation, to professional sociology. The tensions between professional sociology and public sociology began immediately, as I set aside my ongoing field research, juggled teaching and departmental responsibilities, and deferred professional writing and reviewing commitments. Technology enabled my sociology to reach beyond disciplinary boundaries but also it enabled others to reach me. While most sought me out for my professional expertise or to give me information, some saw me as a means to having their own voices heard, either by conveying their experiences to the media or the CAIB or getting their own writing into print; however, I felt neither abused nor used, for I was on the receiving end of data directly connected to my professional interests. What I experienced the most in this first stage and then throughout was overwhelming busy-ness, the conflicts between professional and public sociologies, and, when I had time to think, incredulity at having written something that, seven years postpublication, was useful.

The tensions between professional and public sociologies intensified upon my joining the board. They manifested in conflict of interest issues that required negotiation because continuing to speak to the press as a professional sociologist sometimes opposed the investigation's need to keep confidentiality. Not responding to or, indeed, deceiving my press network was another emotional stressor. As months passed, I feared I would never regain access to my field research sites, hard won in the first place. I had to back out of professional writing commitments, something I had never done before. I benefited from yet another mechanism necessary to public sociology: social support. It arrived first in the form of the Burawoy-initiated movement for public sociology, in which the very bestowal of this name gave what I was doing professional legitimacy in my mind. And the disciplinary division of labor he described gave me tools to understand the structural origins of my emotional conflicts. Further support came when I physically relocated, joining the board and its staff. All were professionals yanked from their usual environment for this temporary assignment and the deadline, the daily excitement, and common cause bound us all together. When removed from CAIB and among professional sociologists at ASA, the professional/public sociology tensions reached a peak. I could neither successfully shape the sociology in the final report draft nor adequately finish my meeting presentations because

I was still editing. That week, for the only time, I felt a loss of control over my ideas.

Ironically, my affiliation with the board inhibited my professional voice even as it was enabling it. As my role switched from outside consultant to inside consultant to paid member of the staff, my ability to speak as an autonomous expert was increasingly compromised. Further, my ability to criticize the board and the report was nonexistent. Not only was my research integral to their work, but I also was an embedded sociologist, seduced by doing professional sociology as a full participant in a like-minded group. So embedded did I become that when board and staff dispersed, I felt a loss. The political implications of my participation were not all clear at the time. I was in an environment where visiting Congressmen, combination locks, shredding machines, and leak concerns were constant reminders that this work had political ramifications. I was encouraged to speak as a professional sociologist on topics when the board could not, and I understood the symbolic significance I had to the CAIB as an apparently independent scholar. Other possibilities did not occur to me because I hung up my hat as a critical sociologist at the CAIB door.

I did not see how the transition to full CAIB member gradually changed my public sociology. Although I was working with elites rather than the counterpublics of organic public sociologists, like them I was engaging in dialogic exchange with a local, socially organized, active public. The combination of traditional and organic gave my public sociology complex purposes: teaching professional sociology to diverse publics; using sociological expertise to further a public process; advertising and promoting the sociological perspective as an intellectual stance and as a way of understanding social life; sociology in service of the political goal of reducing the probability of another accident; working to extend the shuttle's life rather than doing the program in. These latter two political directions of my activities were lost on me. My limited ability to apply my critical sociology to my own situation was no doubt curtailed by the speed and volume of events. Also, all my ethnographic work had been done in public agencies—prisons, a welfare department, NASA, and air traffic control—so being a professional sociologist in a government agency was not a new experience, although the CAIB was certainly unique.

What was unprecedented was my formal affiliation as full participant, but it was not being on the payroll that compromised my critical sociology. It was being a member of the group and my identification with group process and goals. Consider also that a summer of 12-hour days left no time for writing the field notes that build in daily reflexivity, I was isolated from colleagues who might provide criticism, and being useful is seductive in its own right. The experience of being useful transformed into the *wish* to be useful to the group. I self-defined as doing professional sociology in another setting: research, data analysis, and teaching; public sociology relevant to policy; *not* political activism working for institutional and organizational change. In fact, I was doing all three.

At NASA, my professional, public, and critical sociologies produced a different policy sociology outcome. I arrived with a public reputation as a NASA critic and a CAIB-acquired view of the space agency as the adversary—not the posture I held in the book. Always the distant analytic object for me, NASA became less the adversary, more human, and even more analytically complex as I witnessed the postdisaster grief, confusion, and the internal messiness of organizational dynamics as top leaders groped with externally imposed requirements for change. When invited to take part in policy implementation, however, I could not. The insights I gained at Wye about NASA headquarter's position on change made me fear I would be used for symbolic purposes only and that my ideas would be abused or not used at all. A related factor was my sheer incompetence at implementing policy sociology. I was not trained to go beyond sociological principles and examples to implementing change. Further, I was hoist with my own petard: my research predicted the reproduction of the system effects that were being reenacted before me, affirming the possibility of another accident. Finally, with the cessation of the CAIB and media attention after the report's publication, I lost my power base. I had no means of influencing the direction that NASA's implementation took. This was definitely not the classroom.

The implications for public sociology and sociologists are these. How theory travels is central to the sociology of scientific knowledge and the new sociology of ideas. In this example, analogy emerges as a bridge across institutional boundaries, enabling a sociological theory to travel to publics outside the academy. Analogy in combination with the social mechanisms identified here—professional legitimacy, conversation, technologies, time, networks, social support—seem to be ingredients fundamental to the diffusion of expert knowledge. Scholars interested in the production of knowledge and the diffusion of ideas have tended to approached the problem one of two ways: reputation or content fit (Camic 1992; McLaughlin 1998). The reputational perspective accounts for the rise and fall of ideas based on historical and cultural context, geography and national traditions, institutional, organizational, and network arrangements, or individual characteristics of the author and scholarly life. This case affirms the reputational model's findings about institutional arrangements and networks, adding to it by emphasizing the actor-network association (Latour 1987, 1988) and by suggesting that technologies of dissemination might be usefully incorporated into the study of knowledge production, regardless of type of knowledge or historic period. The second approach has explained the acceptance or failure of an idea by its content. Camic, discussing the role of the content-fit model in a theorist's selection of intellectual predecessors, implicitly suggests analogy: "The relationship exists chiefly because of the fit between the arguments, concepts, themes, materials, orientations, or methods of certain earlier figures and some aspect(s) of the work of the thinker under study" (1992, p. 423). The fit with the content of an idea is affirmed by the empirical analogies that enabled the theory of the *Challenger* accident to travel to the *Columbia* tragedy. These analogies and the structural equivalences between the two problems suggest that the *form* of an idea or theory in relation to its application to other empirical situations may also be significant in the legitimation of ideas, their acceptance or rejection and dissemination.

This case affirms the centrality of professional sociology in the sociological division of labor. It underscores the contribution that empirically driven sociological theory can make to public discourse and policy formation. It demonstrates that our concepts can be important in the diffusion of sociological knowledge—in this example even weathering the usual debilitating effects of "sound-bite sociology" (Stacey 2004, pp. 140–42)—to become sound bites that fell into the vernacular, conveying the sociological interpretation. Contradicting the notion that policy makers prefer research that is free of theoretical baggage (Lauder et al. 2004, pp. 5, 17), the CAIB report is informed by theory and concepts. The case also affirms the relational complexity of the four types of sociologies and the porous, overlapping nature of their boundaries. My book began as professional sociology, was reviewed in the press as a critical account of NASA, reentered public discourse post-*Columbia* as public sociology, and reached into the policy domain, ultimately regenerating my teaching, research, and writing.

Burawoy points out that the individual sociologist may engage simultaneously in more than one type of sociology (Burawoy et al. 2004, p.106). Not only do the four sociologies merge into blurred genres in my experience, as I was often doing all or several at once, but so do the two types of public sociology. Although my public sociology began as the traditional type—working with elites, temporary, with media visibility, and (once on the board) was salaried—upon joining the board the work itself had much in common with the less visible, continuous, often unpaid work of organic public sociology: a time squeeze on the diverse responsibilities of professional sociology, the uncertainty of working on unfamiliar terrain, uncertainty of outcome, the challenges and compromises of collaborative work, the importance of persistence over time and social support, and the lack of institutionalized rewards in the discipline (Gamson 2004; Ryan 2004).[7]

[7] Efforts to address inequalities in rewarding the different kinds of work sociologists

At the same time, this example reveals the tensions between the four sociologies that may be experienced while doing public sociology. Recognizing that public and policy sociology can produce data and theoretical insights that feed into professional sociology should assuage some of the tensions, especially for graduate students whose public sociology interests are discouraged by mentors concerned about publications necessary to fledgling careers. But research makes us all vulnerable. Social conditions and current events may bring our research to public attention immediately or years later. When empirical examples, analysis, and theory match and explain the experience and observations of others, analogy enables our theories to travel across disciplinary boundaries, stimulating public discourse. Technologies make this possible regardless of the proclivities of the sociologist to remain in the domain of professional sociology, for our work can now travel independently of any wish or activity on our part.

How far theory travels outside academia will depend both upon the strength of research evidence and concepts and upon the activities of the sociologist who initiates the social mechanisms that facilitate the diffusion of ideas. Not all will wish to engage in what some may view as entrepreneurial activity, although, as Ericson (2005) astutely notes, we already publicize our sociology in the classroom, textbooks, research monographs, guest lectures, conferences, government reports, and Web sites. Nor will all wish to engage in the extra work required to disseminate research findings to other publics. The absence of institutionalized rewards and incentives may be a barrier for many whose work has relevance beyond the discipline. But even the disinterested may be moved to engage in public sociology in order to mediate distortions when their professional sociology crosses into other domains.

For those who engage, whether as traditional public sociologists or organic public sociologists, it is important to consider the institutional boundaries crossed, the reasons for their permeability, and the effect on our sociology. Our research is likely to bring us into contact with select samples of the public. It renders some boundaries permeable while others may become impermeable or, normally closed to us, remain closed. The same boundary can be permeable or impermeable, or permeable to sociological ideas but not to sociologists. When institutional boundaries become permeable, admitting us as well as our ideas, we should consider why organizations and individuals find our research useful. How do they benefit? To what ends will our work be put? We must examine our own

do are currently underway in the ASA Task Force on the Institutionalization of Public Sociology (see Task Force 2005). They include guidelines for evaluating public sociology for promotion and tenure.

theoretical assumptions, the politics of our work, whether implicit or explicit, and their connection to the boundaries that we cross.

Burawoy notes that public sociology has no particular moral stance, but varies with the normative position of the sociologist who practices it. Ericson (2005) points out that all sociology has policy relevance because both social theory and sociological data have normative and rhetorical force for principled courses of action. By definition, then, taking sociology to other audiences is a political act. Of the select samples of publics we attract, the boundaries that open up to embrace our sociology and us along with it are the ones that offer the seduction of being useful. Taking critical sociology along is as important for traditional public sociologists whose work suddenly becomes policy relevant as it is for organic public sociologists who regularly engage with counterpublics while wearing their normative stance on their sleeves. In both activities, we may feel we are holding our values in check, but in joining a group of like-minded others we inevitably take a normative stance and are vulnerable to the seduction of being useful, which may diminish our critical awareness of the effects of group membership on us, the autonomy of our sociological voice, and the ways our work is deployed.

Lauder et al. (2004) argue for the potential of sociology to become a "new policy science." It is not unusual for sociology to have policy relevance but less is known about its capacity for policy formation and implementation. Other individual cases of professional sociology that traveled into the policy domain, also not representative because they had high media visibility, attest to the obstacles involved (Coleman 1966, 1975; Wilson 1996; Becker et al. 2004; Stacey 2004). Whether doing traditional or organic public sociology, we cannot assure that policy formation and implementation based upon our work is in keeping with the social analysis behind it (Steinmetz 2004; Wiles 2004; Beck 2005). Tittle (2004) warns that publics have interests they want affirmed, inhibiting the sociologist from pursuing evidence fully or contradicting the contentions of the group. We must also acknowledge our own limitations in policy formation and implementation. Our research is drawn from samples that are unique in time and place. When public sociology leads to policy sociology, we run the risk of constructing false analogies. How far can we go in applying our research to other sites or apparently similar problems without verifying its appropriateness to the new situation? What about other sociological research that indicates a different direction?

My position with the CAIB allowed me access to original data that assured me that my *Challenger* research could be legitimately applied to *Columbia*, but that kind of opportunity—both in terms of research access and time spent—may be rare. Also, we are trained to identify relevant social conditions associated with policy principals, but we are not trained to translate our research into specific policies enacted in new social settings or to anticipate the outcome of changes implemented in complex systems. In addition, the disciplinary incentives and rewards are not in place to sustain the long-term engagement necessary to see an activity through, and many sociologists, acting as individuals without resources or institutional support, will not have the necessary power base. Further, when research challenges received wisdom or dominant ways of approaching some problem or issue, its implementation in the policy domain is likely to be controversial. Finally, we have no cumulative data base analyzing what has worked, not worked, and why. For these reasons, sociology, although policy relevant, is unlikely to become the "policy science" that Lauder et al. recommend.

Attempts to measure the worth of public sociology by achievements in policy formation and implementation is setting too high a standard. The raison d'etre of public sociology is not in its successful implementation of specific policy prescriptions, but in its ability to influence policy by the very fact of its becoming public, thereby contributing to public discourse and policy debate. In published debates about public sociology, the first point of agreement is the desirability of doing socially relevant research and conveying sociological knowledge to publics across the discipline's boundaries (Gans 1989; Best 2003; Burawoy 2004, 2005; *Social Problems*, February 2004; *Social Forces*, June 2004; *Critical Sociology*, summer 2005; *British Journal of Sociology*, September 2005). The second point of agreement is that sociology has failed to live up to its promise to take sociological knowledge beyond the academy.

My experience affirms that sociologists have discursive opportunities to influence multiple publics, illuminating social problems, affecting public discourse and policy debate. To evaluate the success of public sociologies by considering only the effectiveness of policy formation and implementation is to restrict ourselves to the present, what is visible, and thus, measurable. As in the classroom teaching that we do, the full impact and the eventual ramifications of going public with sociological interpretations cannot be known. It may be that the only measurable impact of my public sociology was on the board and staff of the CAIB, as they returned to teaching, research, and accident investigation with a new awareness of the social. But I believe it did not stop there, based on my conversations with journalists, many NASA personnel, and e-mail correspondents who saw the analogies, got the concepts and theory, and took them further, applying them to other professionally and personally relevant situations. Although reward for public sociology is not yet institutionalized in the discipline, it lies in the local process and the people we teach and who teach us as we take our research to other publics. They, like our students, and like the board and media in this example, may be the ones who learn the most, helping our theories travel to wider audiences at the same time that they help us elaborate those theories.

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