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On the relevance of ethnography for the production of public sociology and policy

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In 'For Public Sociology', Michael Burawoy describes the internal complexity and porous boundaries of our discipline's division of labour, noting that the individual sociologist may 'assume a trajectory through time among our four types of sociology': professional sociology, public sociology, critical sociology, and policy sociology (2005:11). In February 2003, when NASA's Space Shuttle Columbia disintegrated upon re-entry to the earth's atmosphere, I was literally torn from my usual routine as a professional sociologist to embark upon this trajectory. Because I had previously written a book about NASA organizational failings that led to the disastrous 1986 Challenger launch (Vaughan 1996), I was deluged by media immediately. A publicized sociologist during the first few days, I quickly became a public sociologist in dialogic exchange with multiple publics for the eight months of the official accident investigation and after.

My comments in print, on radio, and tv drew over a thousand e-mailed questions and responses from the general public and, surprisingly, data about the NASA organization and its technology from former and current NASA employees and contractors, space buffs, and non-NASA scientists and engineers. Called to testify before the Columbia Accident Investigation Board in public hearings, I was then invited to join the CAIB as a staff member and worked on the report, authoring a chapter. When the report was published, NASA contacted me for advice about solving the organizational problems that the CAIB identified.

Although the frequency and duration of media attention to my research made my public sociology atypical, my experience bears upon ongoing debates about the potential for public sociology and 'a new policy science' (Lauder, Brown and Halsey 2004). Lauder et al. write 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

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agency. Initially, my 1996 book was relevant because it was about a previous NASA accident and therefore timely. However, its continuing relevance originated in its ethnographic evidence, theory, and the structure/agency connection it depicted. In combination, these characteristics of the work were the primary source of its influence on policy.

My book was an historical ethnography: a cultural analysis, drawn from interviews and archival data. Written in thick description, it reconstructed the meaning of events to NASA engineers and managers as they made decisions about the technical anomaly that caused Challenger's demise. Inductively developed, the grounded theory of the book explained why NASA continued to fly despite worsening problems in the five years before Challenger and in the critical Challenger launch decision. It linked macro-, meso-, and micro-levels of analysis thus: Decisions made by powerful others in NASA's political/institutional environment altered the NASA organization structure and culture, ultimately affecting decisions by managers and engineers doing the hands-on technical work. Demonstrating the connection between structure and agency, the analysis showed how individuals reproduced power relations, culture, and structure, as they made choices that incrementally moved toward the accident.

When Columbia disintegrated, NASA's own internal investigation immediately revealed a striking similarity between the two accidents. In both, the technical failure triggering the accident had a long history, during which NASA managers and engineers launched with an anomaly that was not permitted by shuttle design specifications, then flew despite worsening damage. When the CAIB took over the investigation, CAIB press conferences began revealing major social causes of this decision history for Columbia that were analogous to those of Challenger's.

As my engagement with the press intensified, key concepts from my book – the normalization of deviance, missed signals, institutional failure, organization culture, structural secrecy – fit the data about Columbia and thus appeared repeatedly in the media to explain breaking developments. I later learned that the chair of the CAIB read my book two weeks after the accident and found the sociological explanation of Challenger relevant for Columbia. Months before my testimony, he decided that much of the CAIB report would focus on social causes, including three chapters that, like the book's organization, would cover each of the accident's macro-, meso-, and micro-level causes: NASA's political/institutional environment, the organization, and the history of decision making.

The 'positionality' of qualitative research (Lauder, Brown and Halsey 2004: 14) was not the source of the appeal of the book's explanation to the media and the CAIB.[1] Ethnographic thick description presents details that convince, enabling readers to recognize patterns and make that important connection between personal problems and public issues. Further, ethnography is particularly suited for showing complex social relations, exposing the intersection

of history, institutional forces, culture, and structure as they affect every day interaction and the meanings of social life to individuals.[2]

Throughout the investigation, I was connecting theory and concepts to empirical examples from Challenger and Columbia in continuing dialogic exchange with the media and other publics through e-mail, telephone, and personal conversations. Many in the media told me they had covered the Challenger accident or had read my book, thus grasped the relevance immediately; others saw the connection with new evidence because of the empirical match at all levels of analysis. Early on, concepts came to stand in for what happened for the media, which began using them in their stories, independent of conversations with me.

CAIB members and staff told me that the theory of the book and its concepts helped them make sense of their data. Moreover, they saw its empirical evidence and theory reproduced as they interacted with NASA during their own extensive fieldwork. Contradicting the suggestion that policy makers prefer research that is free of theoretical baggage (Lauder, Brown and Halsey 2004: 5), the CAIB integrated the book's theory and concepts into their report (CAIB 2003). Further, the CAIB report connected policy recommendations to the layered social causes of the accident, including placing accountability with government: powerful leaders in Congress, the White House, and at NASA who made decisions that changed the space agency's structure and culture, ultimately contributing to Columbia's demise.

This example verifies the potential of ethnography to influence public debate, policy, and make government accountable (Lauder, Brown and Halsey 2004; Hammersley 2004). Further, it confirms that the core of effective public and policy sociology is professional sociology, and that the boundaries within the discipline and between the discipline and non-academics are extremely porous (Burawoy 2005: 7). None the less, several aspects of this example must be kept in mind when considering what it means for a sociologically-informed policy. Public sociology is bringing the academic and the non-academic into dialogue. Essential to this exchange is what Burawoy calls 'back-translation': sociologists actively taking professional knowledge back to those from whom it came (2005: 2).

Back-translation entails invisible work usually unrewarded professionally. Hence, doing it is a luxury that the untenured seldom have, but even the tenured may hesitate. My status as a tenured professor allowed me to respond to every contact for the eight months of the investigation and after, setting aside an ongoing research project to do so. However, I believe it was the *repetition* of sociological concepts connected to ethnographic data in continuing conversations that resulted in the accurate use of the sociological perspective by both media and the CAIB. The more typical experience of public sociology is that the researcher tends to lose control over ideas (Hammersley 2004: 442; see, e.g., Stacey 2004; Vaughan 2002; Burawoy 2005: 7).

The result was that mine was not the only voice engaged in back-translation: I had powerful allies. The media and the CAIB dispersed the sociological framing of the Columbia accident to a variety of publics – NASA included. Ultimately, it was the media and CAIB that made the White House, Congress, and top NASA leaders publicly accountable; then the CAIB, by the policy in its report, made them *officially* accountable. These things I could not have accomplished alone.

Whenever a work of professional sociology becomes relevant, qualitative and quantitative sociologists alike have the opportunity to contribute to debates about social issues of local, national, or international interest. However, whether the sociologist is willing to so engage, and whether that engagement leads to a sociologically-informed policy and government accountability may be thwarted by the absence of rewards for public sociology in the discipline and/or the inability to connect with a supportive power base, whether it be a social movement, Board of Education, community organization, labour union, government unit, media or other (see, e.g., Gamson 2004; Ryan 2004).

With a revision of the disciplinary reward structure to support the invisible work of public sociology, we can increase participation in public debate and thus the potential for sociologically-informed policy. But achieving science-based or sociologically-informed policy is not the same as achieving 'a new policy science', a Lauder et al. goal that seems to imply disciplinary change that makes the connection of research and theory to policy itself a science, in the positivistic, normative Mertonian sense (Merton 1973).[3] For public sociology, we can strengthen our ability to write clearly, publish in sources that give non-academics access to our ideas, and do the necessary back-translation when our work becomes relevant (Becker et al. 2004; Best 2003 2004; Lauder, Brown and Halsey 2004), but we must acknowledge our limitations regarding policy.

Our research is based upon data gathered at particular historic moments, in samples that are unique in time and place. Although our concepts and evidence may *seem* to be appropriate to a specific social problem, we have to be aware of the power of our own theoretical assumptions and beware of constructing false analogies: critical sociology becomes crucial (Burawoy 2004: 105). When public sociology leads to policy sociology, how far can we go in applying our research to other sites or apparently similar problems without verifying their appropriateness for the new circumstance? Once I became a member of the CAIB staff, I had access to all the original data and research process of the Board, thus was able to do a systematic comparison of Challenger and Columbia and extract the sociological principles on which CAIB policy was based. But how often do we have that kind of opportunity?

Second, we can introduce sociological principles and evidence about how social systems work and what to do to alleviate social problems, but we are

not trained in the practical skills necessary to translate our work into policy enactment in new social settings. Consulting with NASA about implementation of CAIB policy to change organization structure and culture, I found a NASA struggling to fit the prescribed new structure into the existing operation and evaluate how that might affect the rest of the operation. Without extensive additional research, I could not help them. Further, my research on the two accidents predicted that the external political/economic constraints on the agency would persist, perpetuating the many flaws that contributed to the NASA disasters – regardless of changes to organization structure.

The goal to convert the discipline into 'a new policy science' of the Mertonian sort is likely to be subverted by the very complexities of the worlds we analyse. Advances in this direction will be more craft than science, made on a case by case basis. But in making these advances, professional sociology, with its rigorous methods, cumulative knowledge base, theories and concepts, insights, and legitimacy, is indisputably central to both public and policy sociology.

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Notes

1. I sent my penultimate draft of the ms. to NASA personnel, contractors and others who had participated in the research for comment; their input was integrated into the text, correcting it, but not identifiable to readers as such.

2. Hammersley, contesting the Lauder, Brown and Halsey point about positionality, makes a similar point about all qualitative research (2004: 443).

3. These two terms are not distinguished in the text, but are used interchangeably. In the concluding discussion, however, 'a new policy science' appears to be assigned a different meaning.

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