

## **Afterword: On the Pleasures of “Muddling Through” in Ethnography**

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When I was younger and passing through the formalities and fashions that dominated the American social sciences during the 1960s, their insistent positivist rigor was regnant—and still is in core confident disciplines like economics and political science. I was so pleased and relieved to come upon the notion of “muddling through,” a plea for pragmatism in the cult of hyperformal rationalist discipline, and promoted by the political economist, Charles Lindblom (Lindblom 1959). This was a somewhat iconoclastic and mellowing break with views on and hopes for human action under the harsh rigor of formal rationality. Lindblom’s idea was that decision-making—the task about which many social scientists in that period were trained to comment and think (cognitive anthropology was the then popular anthropological counterpart)—was an incremental, contingent process, that progressed through hunches, educated guesses and correcting errors. Lindblom was a maverick thinker in his own domain, known for writing comparatively on market regimes and planned economies in the world of the Cold War. He was also leavened by his experience of participating in development projects in India. These were also the years of the U.S.’s technorationale for measuring success in Vietnam—the infamous metric of body counts—under Robert McNamara’s management.

Although moderated in its expression, “muddling through” was all about making mistakes, and learning from them, and was even about failure, although this was the word to be avoided.<sup>1</sup> As a social science in good standing, anthropology in its overt discourses, like the other social sciences, was about success and confidence in its methods to produce results. Yet, sotto voce, the culture of its method was always about “muddling through,” and not always in the hopeful language in which Lindblom described it. Told in anecdotes and tales of fieldwork, as part of the romantic, heroic nature that anthropologists cultivate, anthropology’s particular story of “muddling through” has sometimes involved admissions of abject failure (e.g., famously, Gregory Bateson’s admission of failure among the Baining, or Malinowski’s “confessions” in an appendix to the *Coral Gardens . . .* volumes), or expression of more extreme frustrations than the political economy of telling the same story allows. In anthropology, this veritable discourse of proceeding through failure or mistakes all went public, became self-conscious as the heroic, self-critical genres of fieldwork accounts from the 1960s onward, and were succeeded by the more analytically critical turn of the *Writing Culture* critique during the 1980s.

Disciplines that set themselves methodologically “impossible” tasks—and fieldwork in its spirit and ideal standard is certainly one of these—provide themselves with the means to rescue success from the jaws of defeat or failure. In fieldwork, for instance, doing well, or well enough, is a remarkable achievement. The morale of such a discipline depends on a certain respect and reward for trying. Anthropology is certainly a discipline where one can trace this tendency, especially in its well-established character of delivering critical interventions within spheres of authoritative but unreflected-upon knowledge practices. Anthropology’s contribution to other domains of knowledge-making has been to correct overly rationalist, inevitably reductionist perspectives on human action. But this function has created problems for its own self-confident production of results, which are legible in the virtue that it has made of “muddling through,” of making mistakes as a way of knowing, of rescuing success from near failure. Even so, anthropology had its positivist period that still lingers, and within it, an interpretive period or “turn” that allowed constructive doubt as a productive way—even a scientific one—to generate results from research. Indeed, the interpretive turn was anthropology’s version of the Lindblom intervention: a maverick, pragmatic break with anthropology’s own post World War II hyperrationalism. Geertz’ style was anthropology’s “muddling through” to attractive results. Every fieldwork story from Geertz on achieved what it did through some version of “muddling through.” Postmodernism was perhaps too difficult to take because it threatened excess, to diminish a good and secure result from such “muddling through” of the interpretive turn. It threatened to undermine the promise of a modicum of rationality in the Geertzian version of positivism so that the game could proceed. But this was the scare image of the postmodernist moment that anthropology experienced through the *Writing Culture* critique. The legacy of that postinterpretive period of supposed excess, going too far, has been to have put anthropological practices on a firmer footing by pressing a more candid understanding of the value of error in the course of fieldwork.

While it is thus no longer daring to admit “muddling through” as a valid, even productive, way to operate in fieldwork—that it is a norm of method by which anthropology produces some of its most important results from its mistakes—the discussion of failure, which is not so easily redeemed as is a mistake, and thus is more difficult to incorporate within a discourse on ethnographic method, remains more sensitive and disturbing.

The papers of this collection, which largely deal with what can be learned from breakdowns in communication in the course of fieldwork, are an advance on this tradition of “muddling through” discourse on method in anthropology in that they are dealing with more than simple mistakes as productive of knowledge-making in anthropology, but accept the larger challenge of failure that blocks inquiry and that is not always overcome and followed by success or resolution. Instead these papers stay rather courageously with the state of failure and breakdown and focus their attention on this predicament without promise of overcoming or positive results.

So what is the constructive outcome for anthropology if one refuses to tell the redemptive “learning from mistakes” story? Confronting failure, studying its conditions but not transcending it, says perhaps something about the limits of the ethnographic method overall in relation to the conventional standards of research in the social sciences and what is supposed to come from it. Creating a constructive discourse about the value of failure in ethnographic method requires not just a rhetorical “fix,” nor incremental tinkering or reform of the long-established culture of method in the tradition of Malinowski and Boas, but rather a rethinking of the entire process of fieldwork and the culture of professional attitudes that has grown up around it in anthropology. This reexamination would not necessarily lead to an end of the long-established protocols of ethnographic method, but to more subtle adaptations to new challenges in contemporary research than the comfort of a solid pragmatism in the guise of “muddling through” has allowed in anthropology.

In anthropology, the strategic ground or crucible for doing this rethinking is the dissertation process where the stakes are quite different from later “finished” work, and are more consequential, I would argue, for the progress of the discipline than the professional discussion and review of more mature work. In the latter, unlike the dissertation process, the frank coming to terms with failure in fieldwork and the productive or generative possibilities for the reform of method that this would offer is blocked by the careful management of the virtue of “success through learning from mistakes” in support of a necessary rhetoric of authority (mature work shows its face to itself and the world; dissertation or apprentice work is hidden from view, so to speak, and in this sense, offers a more probing and creative context for rethinking method unencumbered). Thus, the strategic importance of apprentice work is the context to recognize and discuss failure in fieldwork for its generative possibilities, at this moment in the history anthropology, after idealized positivism, after Geertz and the interpretive turn, and after the *Writing Culture* critique.

As mentioned, this tradition of “muddling through” verging on failure seems inherent in the way the papers of this collection set their occasions for writing. Does this “muddling through,” amount then to absolute failure of research? Not if failure provides the trope for an adventure or a plot of deeper emergent knowing that exceeds the usual satisfactory outcome in stories of analytic success from mistakes or failure that support the worth of anthropology in the fellowship of social science. The challenge is to produce results without always ensuring outcomes as success.

The alternative in the crucible of dissertation research (as the grounds of finished work by the fully credentialed) is incompleteness, openness, and contingency as an explicit strategy of the design of research rather than as a consoling rhetoric for the limitations of what has been accomplished, which is how such terms now function. In the rethinking of method, a norm of incompleteness demands a rigorous way to think about the management of limitation and failings that anthropologists have encountered as an integral and routine part of fieldwork from the very inception of the

ethnographic method. Incompleteness, openness, and contingency are values that anthropology has helped to make conventions of a more realistic understanding and practice of the process of inquiry that accumulates a different kind of knowledge than in the era when I entered anthropology.

## Notes

<sup>1</sup>Interestingly, failure has long been a completely acceptable result of research in experimental, natural sciences. I recall my Harvard dorm mate from graduate school—a very promising chemistry student from Argentina, who after four years presented a dissertation that reported negative results. It was not an auspicious beginning of a career but a completely acceptable one. To understand this value of negative results in natural science (failure which then is not exactly failure) one must appreciate the norms of speculation in research in an enterprise that has more faith or consensus about its foundations and methods than do the social sciences, let alone anthropology. Here it is useful to evoke recent philosophy of science from Kuhn/Popper to Feyerabend/Lakatos to Hans-Jorg Rhineberger. In the latter's focus on the nature of experimental systems and the pursuit of epistemic things, there is an aesthetics of inquiry that values, even requires, results that surprise. Correspondingly, in such an enterprise, disappointment and failure are inevitable, and it would be unwise to be too hard on the many who reach dead ends in research. The stakes of research in the social sciences are defined very differently so that failure is inevitably felt more personally than in a regime of research where it is an embedded and recognized characteristic of a collective pursuit. Anthropology is perhaps the one social science most clearly and most committedly willing to be frank with itself concerning this "complex" of the generic social science project in which it has been historically embedded.

## References Cited

Lindblom, Charles

1959 The Science of Muddling Through. *Public Administration Review* 19:79-88.