SECTION II: HISTORICIZING EXTENDED CASES

PREFACE

Historicizing the Extended-Case Method

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In 1949, Gluckman was appointed to the new Chair of Social Anthropology at the University of Manchester, with the intention of founding a new department. At the time, he was teaching at Oxford, in Evans-Pritchard's department. During the visit there of a Dutch colleague, Gluckman was introduced to him as leaving shortly for Manchester. He responded: "Ah, in the same way as X has left the department at _____ to go to _____." Evans-Pritchard remarked: "No, not in the same way. X is a refugee; Gluckman is a colonist" (Gluckman 1972: x). Gluckman, the colonial and colonist, remained devoted to Evans-Pritchard, his mentor, and hankered from time to time to find his way back to the Oxbridge ecumene.

Yet something else took intellectual shape in Manchester, something that had begun long before and much farther away than Oxford. Gluckman had begun his voyage at the anthropological periphery of South Africa, colonized twice over, as the offspring of Jewish immigrants from Russia in a colonial outpost of Northern Europe. Deeply affected by his experiences there, he had attended Oxford as a Rhodes Scholar (he was an excellent athlete) and had returned to forge and temper his path-breaking thinking in the British colonies of Central Africa. Before taking up a lectureship at Oxford in 1947, he had headed the Rhodes-Livingstone Institute for Social Research in Northern Rhodesia, recruiting students and colleagues who later became the research core of the Manchester School. Evans-Pritchard sent him down to build Oxford in Lancashire, yet Gluckman had another agenda, one quite unlike that of the anthropologies of Oxbridge, one that steered away draft by draft from the symmetrical abstractions of social structure that dominated Oxford, away from the pretty clockwork aestheticism that always pervaded the Cambridge anthropology of Edmund Leach, his great enemy, who was upper-crust British to the core.

When Leach (1984: 20) wrote of Gluckman, "If anyone had asked me then [1938–1939] or later what I thought of Gluckman, I would probably have said that I considered him to be an uncivilized and fundamentally uneducated

egocentric whose attempts at theoretical generalization were of quite puerile incompetence,” this was not merely a statement of personal antipathy toward yet another foreign *parvenu* but one that opened to quite distinct aesthetics of practicing the discipline of anthropology. Despite Leach’s (ibid.: 12) inane claim that, *inter alia*, Gluckman remained wedded to “homeostatic social equilibrium,” it was indeed Edmund Leach who, despite his disclaimers, remained mired in, as it were, High Church structural principles and structuralist transformations until the end of his days. Gluckman, as we argue in the introduction, moved toward studying what we are calling the practice of practice, the study of the hurly burly of social life in its myriad complexities, contradictions, and always emerging dynamics. Little by little, though never fully so, this steering became self-steering, at least for a while, and this is why, if only for a little while, one can refer to the Manchester School of Anthropology.

The contributions to this second section show how deeply Mancunian dynamics were embedded in broader intellectual climates and how integral such dynamics were to the doing of this kind of anthropology, and not solely in field research. David Mills takes a social-constructionist perspective on how Gluckman fashioned (and perhaps self-fashioned) claims to a distinct and distinctive approach that came to be viewed as a uniquely Manchester anthropology. He argues that it was only in the 1950s, long after the bridge-opening articles had appeared, that Gluckman started to write about the case-study approach as an innovation lodged in the Rhodes-Livingstone Institute, and that he did so perhaps in response to the interest that others, especially Clyde Mitchell, but likely Victor Turner as well, expressed in these articles—and likely no less in response to the criticism of his peers, who preferred the delineation of abstracted bony principles of social skeleton, with bits of apt illustrative flesh adhering here and there. Mills perceives Gluckman’s 1961 article, “Ethnographic Data in British Social Anthropology,” as an attempt to fashion a distinctively anthropological genealogy for thinking about the extended-case study. He mentions that Fred Eggan took issue with Gluckman’s pointed focus on case materials. This is an interesting twist, since Eggan, influenced by Radcliffe-Brown during the latter’s stay in Chicago during the 1930s, was perhaps the major figure in post-war American anthropology who called for the integration of British structural functionalism and the American study of ‘process’ and history (Eggan 1954: 745). In this instance, the Manchester approach lined up against both the British and most of the American, finding more affinity with Chicago sociology.

Thus, Mills shows that Clyde Mitchell, early on, was fully aware of W. I. Thomas’s idea of the definition of the situation, which Mitchell referred to as the ‘situational approach’, and probably with Thomas and Znaniecki’s path-breaking sociological monograph, *The Polish Peasant in Europe and America* (1918), in which they discuss the significance of studying “concrete situations.” In other words, the Chicago School of Sociology—which studied social situations through case studies (but not in any sense of an extended case)—had a significant, if embryonic, influence on the fashioning of a distinctive Manchester anthropology. A related question not addressed is whether the

post-World War II Chicago contemporaries of the Manchester group, with their continuing appreciation for “the nature and obdurate character of the empirical world” (Colomy and Brown 1995: 29), had any impact on the emerging Manchester School.

Mills mentions that the case-study method “was as much about an approach to writing as an approach to research itself.” The point is worth emphasizing. All of us know that the most difficult act of anthropological writing is the evocative yet substantive detailing of ethnography itself. The social situations of obdurate life are simply the most complex of social phenomena. Writing social situations positioned Manchester anthropology early on at the crux of turning the dynamics of social life into those of its inscription, even as Gluckman argued the value of literature for anthropology (see Frankenberg, this volume) without entombing culture within text-as-text, as Geertz and his apolitical American lit-crit hit mob had succeeded in doing by the 1980s in their quest for poetic (yet more often poetistic) shortcuts to doing ethnography. (Gluckman and his wife, Mary, devotees of Shakespeare, would declaim the Bard together in the privacy of their home.) This did not mean that all Manchester anthropologists were equally up to the task, though Victor Turner certainly was, as he developed the extended case into the social drama (see especially Turner 1974). Nonetheless, the problematic was recognized, salient.

Marian Kempny asks what it was that made Manchester anthropology fully a ‘school’, given that “theory as such was not a subject of inquiry,” and given that Manchester anthropology had not generated a distinctive body of theory adopted by its members. Kempny makes useful points in this regard, outlining factors that held Manchester anthropologists together despite their numerous differences. Here we highlight two of these that were quite distinctive to and productive for Manchester anthropology, helping to shape the Manchester perspective yet never receiving the attention they deserved: the seminar and the reanalysis.

Post-war American academia turned more and more to corporate managerial models for its self-fashioning. Going into the university was going to the office; specialized committees and sub-committees proliferated; deans became professional bureaucrats; and the departmental seminar became a polished set-piece, an artful presentation of the individual self in the scholarly boardroom, responded to with degrees of polite comment, question, critique, and wit, depending on the status of speaker and respondent. American anthropologists, like American academics generally, were (and are) thin-skinned in discussion and debate, and little of intellectual substance came or comes from their seminar occasions. The individual positions the self before the intellect; the self is wounded; the intellect offers no buffer (David Schneider at Manchester was one case in point; see Frankenberg, this section).

Manchester was quite the contrary, always intensive and highly social (see Kapferer and Frankenberg, this volume), and Manchester seminars often were learning experiences, more endeavor than set-piece. Gluckman instituted the Seminar Week—a week of seminars, morning and afternoon, for five consecutive days during the last week of each term. The presenters were faculty, PhD students

returned from the field, and guest speakers. The department practiced and lived seminar during that week, in the rough-and-tumble of discussions in which the position of speaker might be co-opted by others midway through a presentation that turned into a discussion—on topics varying from the nit-picking to the far-ranging. Often the presentation and discussion were on field materials, on how they did and did not fit together, the significance of either, or both. By and large, with Gluckman's tutelage, department members learned to think together with one another, while buffering their sensitive and precious souls from the substance of perspective and critique, though always retaining the right to accept criticism—or not. Both of us came to a parting of the ways with Gluckman over our PhD theses, but his bottom line was, as he expressed this to one of us: "Finally, it's your work, not mine." In other words, the decision is yours, as is, no less, the responsibility.

Not to put too fine a point on this, seminar discussion was no less the practicing into existence of anthropology through praxis, not the formulation of theory as meta-design, but the relentless, theorizing search for dynamics and patternings of complexity through what Peirce would have recognized as the logic of abduction, crucial to any anthropology of discovery and sadly lacking from today's anthropologies of ideological premise, presumption, and pronouncement. Not for nothing did Gluckman call anthropology a craft.

At his intellectual peak, Gluckman had the wonderful capability to listen to an argument in seminar and then, taking those selfsame materials, produce a counter-argument that re-explained those very materials that he then handed back to the lecturer. He did this not by going outside the parameters of the materials presented, nor by invoking another theory as more powerful to the task at hand, but by paying infinite attention to the minutiae of ethnographic detail, to the practicing of ethnography as it grew and took shape in the seminar room, and to the patternings implicit in this that could be highlighted to understand the field materials in a radically other way. Occasionally, a seminar would emerge as an embryonic extended case, as it was practiced into existence inside and outside the seminar room. Doing the ethnography of ethnography as it was collected in the seminar room was no less in its own way the praxis of theory and method—and it was fundamental to the idea of reanalysis.

Manchester taught the reanalysis of the classics, as Kempny writes, yet it went way beyond this. In the first instance, in Gluckman's view one learned from the ethnographies of others. He made a point of stressing just how easy it is to critique destructively, often by invoking countervailing theories, yet, appositely, just how difficult it is to critique constructively. He would say that there was always something valuable in the work of another, and that our task was to discover this. The crux to doing so was to learn to respect ethnography for its own sake, as a site of the obdurate social, cultural world in its myriads of complexity. Ethnography was to be learned from, yet in and by that very learning we were to make it our own, so that we could then rethink the details of ethnography in terms of what there is, not in terms of what there should be, could be, would be, used to be, must be.

Thus, first and foremost, as we stated above, analysts must stay within these parameters in the reanalysis, in intensive rather than extensive labor, exhausting the capacities of the data as much as possible from within themselves, afterwards adding to this while continuing the reanalysis. A powerful tool for learning and practicing the analysis of ethnography, and for continuously honing one's skills, reanalysis in the Manchester mode complemented the praxis of the extended case and the Manchester seminar. Both of us, much more than most anthropologists, have invested time and effort over the years into doing a wide range of reanalyses, from which our own anthropologies have only benefited (Evens 1984, 1989, 1994; Handelsman 1998).

Ronnie Frankenberg is a lateral, transverse thinker, and his highly personal memoir juxtaposes much insight and information on positioning Gluckman in South Africa, Rhodesia, and Manchester—on Gluckman tensing between Marx and Freud, the most powerful thinkers on conflict in the conflicted nineteenth century—on Gluckman moving to rhythms of conflict and asymmetry yet trapped to degrees within those of functionalism. Above all, Gluckman was a highly politicized and political man, raised in South Africa, alienated from the system he was part of, and, no less, an anthropologist studying that very system, yet one that was both the problem and the problematic. Frankenberg argues that in writing the bridge articles, Gluckman was making a deliberate intervention in the language of anthropology as it was constituted in South Africa and in the anglophone world: he sought to undermine the absolute cultural difference that characterized and therefore legitimated the colonial practice that became apartheid and, too, its support from the prevalent anthropology there. Conflict came to the fore in Gluckman's work *because* he insisted on the singularity of multiplicities, on open systems that could not ignore the asymmetries and clashes between multiplicities, so that one could not refuse to analyze practices before one's very eyes. So, too, the anthropologist was one of these multiplicities, albeit a tiny mote of a multiplicity, but nonetheless a subject among subjects, shaped, moved, acting, acted on. In our understanding of Frankenberg's reading, Gluckman advocated studying the practice of practice long before many of the Americans discovered what they think is practice theory.

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