In the summer of 1975, Barry Laffan, then one of my doctoral students, called me at my weekend home in Columbia County, New York, to tell me that he had at last finished a draft of his dissertation, and to ask if he could drop it off as he travelled from Vermont to his own summer home near Catskill, New York. I said that I wouldn't be home that day, but that he could leave it in the mailbox. He said that he wasn't sure it would fit, and sensing that he might be anxious about leaving his precious document in a roadside rural mailbox, I made arrangements for him to leave it with the town justice, my next door neighbor, where someone was always home. She called the day after the drop off date to say that someone had left a case of liquor for me. Puzzled I drove over and found a Dewar's White Label liquor carton. Inside was a document which filled it fairly well: Barry's "thesis" was 2,000 typed pages long.

I spent the rest of the summer reading what Barry had written. I concluded that he had actually written three different books: one was a study of a particular commune which he called Jackson's Meadows, the other was a descriptive, historical, and comparative account of all the communes in the area he called Provincia, and the third, as I recall, was a historical treatise on the subject of communes in the United States from the 18th century to the present. Worried that Barry would be unwilling or unable to do so, I spent days that summer walking around my study sorting the manuscript into the three piles, and sorting each pile into its own beginning, middle and end. I then read each separately and made a judgment about which would be the most defensible to a committee of anthropologists.

I chose the first pile about Jackson's Meadows because it was the one that was most traditionally anthropological: it dealt ethnographically with a particular community over time, and was informed by extensive fieldwork and participant observation on site with the people of the community. It told their story, and the data upon which the story was based included first hand observation, detailed interview data with all the participants over time about their own and others behavior, and their hopes, feelings and aspirations. I called Barry and told him to come over and meet his new thesis. We spent the weekend together discussing his thesis, and he left in agreement that he had indeed written three publishable works, and that this was the one most likely to get him
his Ph.D. degree in anthropology and education from Columbia. The present book is a revision of that dissertation; parts of the second “book” have been incorporated, especially in Chapters 8 and 9.

Since this book makes arguments that people are located in their own particular cultural time, and that persons are malleable over time, it might help the reader to know more about Barry and the academic milieu in which he matured. I will, therefore, briefly explore the time and setting in which Dr. Laffan prepared for this study. The Columbia University anthropology and education program was then embedded in Teachers College’s Department of Philosophy and the Social Sciences; a collectivity which included other programs—sociology, history, philosophy, economics, and politics of education, and the college’s programs in comparative education. In 1967 Barry was enrolled in this department in the Ed.D. program in comparative education. His adviser was George Z.F. Bereday, a prominent figure in that field, and a charismatic teacher, whose scholarly interests were in comparative sociology. Barry’s interests reflected a previous experience in Japan where Bereday had also worked, and he was then on a career trajectory which would have led to a dissertation involving Japanese education patterns.

I came to the Columbia anthropology and education program in 1967 immediately following my own graduate work in psychological anthropology at Harvard’s Department of Social Relations. I was a specialist in a relatively new field, the anthropology of contemporary American society, and had studied sex-role and political socialization. William Dalton joined the program at the same time. Trained at Manchester University for his doctorate, he was from Boston and was a social anthropologist whose area of specialization included Libya and the Middle East. We joined the other anthropologist* on the program faculty, Lambros Comitas, a Columbia University trained cultural anthropologist and a well-known Caribbean specialist. My own interests in the anthropology of contemporary United States society were shared by Conrad Arensberg, a true pioneer in such efforts by virtue of his membership on the research team studying “Yankee City”. Arensberg was a senior member of the Graduate School of Arts and Science’s (GSAS) Department of Anthropology, with whom Barry would also study. Barry also studied with Margaret Mead, an adjunct professor in both the Teachers College and GSAS departments, who shared interests in psychological anthropology, but who managed to bring anthropology to policymakers and other non-anthropologists through her mass-media activities.

It was an exciting time. Comitas, then 40, Dalton, 30, and I, at 25, shared a desire to change the field of anthropology to increase its relevance to contemporary society and we proposed a new Ph.D. program in applied anthropology which, with Arensberg, Mead, and Marvin Harris, became a joint program between the Teachers College and GSAS departments. We were the first Ph.D. program in applied anthropology in the country. Whatever opposition purer academic colleagues may have had by the riotous events of the Spring of 1968 by police responses to student sit-ins, suspended, students, anthropology students, and their relevance of their academic programs to academic power structures, politics, and GSAS critics of applied science soon disappeared when students demanded they do something about real world concerns. There couldn’t be an academic program and a political moment.

During this period Barry was active and channelled his energy into helping form college, which he was quickly elected to everywhere. It seems natural, in retrospect, that anthropologists attracted by anthropology as a discipline seemed to be moving more rapidly than others, and relevance to larger political issues. We could change the world for the better with anthropology was an effective means to our orientatins, though we differed in more of a Marxist. I maintained a more patient and unconventional beliefs, th Marixist. Barry was the political activist seemed intellectually more influenced by Victor Turner.

Paradoxically, in light of these political differences, we shared many personal characteristics which Barry also had children. We maintained stable marriages, owned rural homes, and had all trace some Irish ancestry. Barry was still in his education behind, though this was, I think, because he was closely linked to Bereday and friends.

Our programs attracted a splendid group of students from anthropology and education and applied anthropology. They were diverse, energetic, intelligent, that period include MacArthur Fellows. Lubic, prominent applied anthropologist, John Carleton Kelley, Joyce Lewinger Schwartz, and academic anthropologists Melanie Dreher, and Glenn Hendricks.

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Introduction

purer academic colleagues may have had to this development was overwhelmed by the riotous events of the Spring of 1968 at Columbia. With the campus shut by police responses to student sit-ins, and the semester's classes effectively suspended, students, anthropology students among them, demanded to know the relevance of their academic programs to understanding the events of the day: academic power structures, politics, and, of course, the war in Vietnam. Even GSAS critics of applied science soon learned to point to the applied program when students demanded they do something about anthropology's relevance to real world concerns. There couldn't have been a better fit between a new academic program and a political moment.

During this period Barry was active in the political events of that spring, and channelled his energy into helping create a student government at the college, which he was quickly elected to lead. Calls for change seemed to be everywhere. It seems natural, in retrospect, that Barry became increasingly attracted by anthropology as a discipline: we had a critical mass of faculty who seemed to be moving more rapidly than those in other disciplines toward policy and relevance to larger political issues. If the faculty thought, in differing ways, we could change the world for the better, Barry came to believe our vision that anthropology was an effective means to that end. We were also anti-authority in our orientations, though we differed in style. Dalton was confrontational and more of a Marxist. I maintained a more conventional persona but had similarly impatient and unconventional beliefs, though I was more Weberian liberal than Marxist. Barry was the political activist, and was attracted by Marxism, but seemed intellectually more influenced by Anthony Wallace, and the later work of Victor Turner.

Paradoxically, in light of these political leanings, Barry, Dalton and I also shared many personal characteristics which were decidedly mainstream. He was also just 30; he had married young (by 20) as had we, and he had a child, as we had children. We maintained stable marriages, took child rearing responsibilities seriously, owned rural homes, and had strong career orientations. We also could all trace some Irish ancestry. Barry cast his lot with us, and left comparative education behind, though this was, I thought, a difficult decision for him because he was closely linked to Bereday, and their daughters were the best of friends.

Our programs attracted a splendid group of students in both anthropology and education and applied anthropology which also would have attracted Barry. They were diverse, energetic, intelligent, and motivated. His fellow alumni from that period include MacArthur Fellows Shirley Brice Heath and Ruth Watson Lubic, prominent applied anthropologists like Tony Barclay, Twig Johnson, John Carleton Kelley, Joyce Lewinger Moock, Claudia Rogers, and Frances Schwartz, and academic anthropologists like Carol Ascher, Joan Cassell, Melanie Dreher, and Glenn Hendricks.

Barry was a competent and hardworking student, but his true love was people. His gregariousness brought life to a gathering. He loved to talk, was equally adept at being a host or a guest, and he listened just as well as he talked.
He had an irreverent sense of humor, and puns abounded. He remained very active in student politics, and politics used up a lot of his time.

As his course work with us came to a close, he began to see the possibility of a dissertation involving fieldwork in contemporary America, and ultimately planned a study of the rebellions underway in parts of the country, and which he had experienced first hand at Columbia in the spring of 1968. He owned land in a number of rural areas, and heard about the communes developing near one in Vermont. He proposed, and we accepted, the idea of a dissertation studying such a commune. While he and we shared sympathy with the goals of such efforts, we were also professionally skeptical, and we were all far too mainstream to join them. Barry, and we, could have been charged with voyeurism for our interest in such counter culture phenomena, but all anthropology is inherently voyeuristic.

Even if more true in this instance than others, it was only a part of the motivation. Unique, ephemeral, and possibly non-repeatable events were happening. Applied, or even policy relevant, anthropology had to be there. The main point of the study was to describe what was happening.

The study took over his life for years. It is not fair to say that he went native. He always maintained some physical distance—in the field, e.g., he usually lived in his VW camper, not in the communal houses. As his comments about his field notes in the Prologue indicate, Barry maintained intellectual distance as well. He was deeply troubled by the immaturity, the conflicts, and the pettiness that he observed. His anti-authority stance also made him leery, if not critical of self-proclaimed leaders, messianic or otherwise, like James and Roy. He remained in the field until the outcome of Jackson’s Meadows had traveled its full cycle, and continued to follow up this research for years afterwards.

But the intellectual paradox of studying Jackson’s Meadows was that to study it you had to join it. The boundaries were fairly vague, and it didn’t take much to join, but Barry was in fact a member of the group he was studying as much as most of the people described in his research. His way of handling the inevitable split between LaFann as researcher and LaFann as de facto member was novel. He acknowledged that on several occasions he had an impact in a definite way on what he was studying: he stopped a fight, mediated a conflict, told DT he had to leave, etc. As ethnographer he assiduously reported his role, but gave the name Patrick to the actor he wrote about. This was never a deception on his part; I do not recall any effort to pretend to me or others that Patrick was anyone else. It was simply that this community required membership if you were to study it, and when he was acting as member, or solely as actor in this social milieu, not as an anthropologist, he was Patrick. It was the anthropologist Barry who would go back to his camper and type his fieldnotes every night, no matter how much “Patrick” had drunk during the evening. I came to see this as a necessary defense for him regarding the conflict inevitable between the participant and the observer that this setting intensified by insisting on membership. He separated them: Barry is the author; one of the people he observes, and dispassionately speculates about themselves how this works.

Since Barry’s fieldwork, some writers in “the Other,” not just study the other, in order to understand his group as a native would, in 12-18 months of fieldwork could anyone taken decades for the others to accomplish. Anthropologist pretend one’s previous life experiences. Barry’s case, however, he was studying such society as himself, and some had community which was unique to them—and only months old and still in formation through an understanding as an insider, the claim won to have studied Pukapuka. Finally, I think that he had an outsider perspective—his anthropologists—and that these have shaped people are about.

As a work in anthropology when it was and provided careful description of, the work Barry’s use of their words for many of his kids, over 30s, IBMs are good examples. To uses etic concepts and frames: social class, etc. These constitute a kind of dialectic between participant and observer, between the other and me.

Some readers may be troubled by Barry’s which occasionally puts you in someone else’s not taken place: “Peter thought that Methodologically these are indeed quite true claimed to be known. But he would write this, had in fact told him what he remembered this the participants so described had in fact vouched for it. While I would argue that they they were actually thinking at the time of the based upon self report data. Readers can make you see “Peter thought that...”, simply read it.

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He remained very observant and dispassionately speculated about, is Patrick. Readers can judge for themselves how this works.

Since Barry's fieldwork, some writers in our discipline urged us to become "the Other," not just study the other, in order to gain true understanding. This position has led some to formulate a sharp answer that becoming like your subjects is not a reasonable goal of the fieldwork process. Most of us today would argue that any anthropologist who claimed to have "become a native" or to understand his group as a native would, was engaging in self delusion. How in 12-18 months of fieldwork could anyone accomplish what socialization had taken decades for the others to accomplish? Further, how could the anthropologist pretend one's previous life experiences were no longer operative?

In Barry's case, however, he was studying people who had grown up in the same society as himself, and some had backgrounds similar to his. The community which was unique to them—and which he was to study—was itself only months old and still in formation throughout his fieldwork. If Barry claims an understanding as an insider, the claim would have more legitimacy than if he were to have studied Pukapuka. Finally, I think Barry was also always aware that he had an outsider perspective—his analysis sections are clearly outsider perspectives—and that these have shaped his understanding of what these people are about.

As a work in anthropology when it was done, the study was respectful of, and provided careful description of, the words and models of the participants. Barry's use of their words for many of his categories in this book, e.g., street kids, over 30s, IBMs are good examples. To this essentially emic frame, he also uses etic concepts and frames: social class, family of origin characteristics, etc. These constitute a kind of dialectic between Patrick and Barry, between participant and observer, between the other and the ontological frame.

Some readers may be troubled by Barry's poetic writing style in this version which occasionally puts you in someone else's head as if those dialectics had not taken place: "Peter thought that...Jessie believed that..., etc." Methodologically these are indeed quite troubling: we can't ever know what is claimed to be known. But he would write this only when the individual involved had in fact told him what he remembered thinking at the time, and that most of the participants so described had in fact read parts of the manuscript and vouched for it. While I would argue that this still told us nothing about what they were actually thinking at the time of the action, I take the descriptions as based upon self report data. Readers can make this correction for themselves. If you see "Peter thought that...", simply read it as "Peter said he thought that."

When Barry and I parted that weekend in 1975, the third of his manuscript which became his dissertation was still almost 600 pages long, and by the time his dissertation typist—no computers in those days—followed marginal requirements for the library the dissertation was over 1,000 pages. Dalton had become so anti-authority that he avoided perishing by, in the words of this book, moving from SS academics to Alteria to seek alternatives in a provincial college. The final defense committee consisted of me as sponsor, Conrad
Arensberg, Lambros Comitas, Herbert Passin (a GSAS sociologist of Japan from Barry’s earlier life), and Janet Dolgin (a young symbolic anthropologist who joined the faculty after Barry had begun fieldwork). Some of them were disturbed by the length of the document—longer than any of them had seen in their careers, and they complained to me in various ways; they were mollified when I told them that more than two thirds had been cut! Since it told a story that was complete, but had various stages, they had no cuts to suggest either. It was still a work on a grand scale, and the depth of data was seen as overwhelming whatever problems were in the manuscript. The committee not only approved the dissertation as written, but they nominated it for the Bancroft Award, the highest award for dissertations on topics relating to American history, and which would have come with assured publication. The Bancroft trustees award that year went instead to Elizabeth P. McAughey for her study of William Samuel Johnson: Loyalist and Founding Father.

After his defense Barry put the manuscript aside. I think he was actually fonder of some of the material we had taken out than what was included, and while I urged him to publish all three volumes, nothing happened. After all, the question of “was it anthropology” was not relevant anymore, and he agreed that what was most defensible for his committee was not necessarily synonymous with what was most important to publish. He talked about getting them ready for publication. He took a job teaching at Marlboro College and devoted increasing time to students and other applied projects, while continuing to update material on individuals.

After many years, he left Vermont, and resettled in Florida, teaching at Florida State University and becoming involved in many novel applied projects, many of which concerned community development in rural areas. I remember him calling me after the first year there to express delight in having left winter and mud season behind forever. In the end, the only manuscript he prepared for publication was this one, and he died before it came out. Joanna Mauer told me that she has found what looks like the remains of the other manuscript pieces and that they may be published in the future.

So this book could have and should have been published more than twenty years ago. Had it been, the events described would have been closer to their own time, and, I suspect, not as surrealistic as they appear with hindsight. Readers are about to embark on a unique journey to a time not so long ago, in a place not so far away. For those of us who lived as adults during that time it will be a reminder of that time, for those not alive then, I wonder what you will make of it. Will you find Jackson’s Meadows a very foreign place, or will you recognize these data as exemplifying more general human themes that render the bizarre familiar and predictable?

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