"Doing History": A Personal and Anthropological Odyssey, 1979–89

We first came to the Republic of Ireland for a few weeks in the summer of 1979 to find a rural locale for a lengthier stint of field research the following year.[1] Having done some background reading in Irish history, we had concluded that the general historical context of rural Ireland was fairly straightforward—for the delineation of this context was located in a well-established historiographic tradition that had its roots in the nineteenth century, was linked to an impressive array of organizations dedicated to the study of Irish history, and even generated historical overviews of its own historical writings.[2] From this literature, we had learned that rural Ireland was a land of farmers and that its past was one of unremitting nationalism led by agrarian agitation and fallible leaders. The more recent literature added an important subtlety: this came from the ideas of economic and cultural "dualism" and regional variation. The Republic, it seemed, contained at least two regions: the east-southeast and the west. In the former could be found commercially viable agriculture, "modern" values, and vibrant community life. In the latter were subsistence or marginal agriculture, "traditional" values, and decaying community life.[3]

We believed at that time, in 1979, that the explanation for such regional variation did not lie in functionalist explanations, in notions of cultural persistence or breakdown, or in the structure of the agricultural sector alone. Instead, so we believed, it was necessary to explore social, cultural, and economic factors together and to link them all to a broader world context. Most important, we believed that it was essential to look at the past to understand the structural and cultural differences of the present. In the late 1970s, this meant "doing local-level political economy": both the present and the past were explicable through an analysis of political economy as applied to local-level and regional arenas. The central foci, and the analytical thrusts, were the concepts of "articulation" and "class formation" in the context of dependency and world systems theory.[4] This concern with local-level political economy also meant that we were determined to avoid the usual anthropological predilection in Ireland of turning a locality, a parish, a village, or an island into a "community"—a bounded, closed, and culturally homogeneous place.[5]

At the beginning of our sabbatical in June 1980, we established ourselves in a rural locale called Thomastown in County Kilkenny. We had purposefully located in the southeast, away from the main

anthropological traffic in the supposedly traditional and presumably esoteric west.[6] Because we needed to set boundaries within which to collect data, we deliberately defined Thomastown using two administrative-electoral units that contained the small town of Thomastown and a rural hinterland around it.[7] We did this so that we could use offical materials: population censuses, agricultural reports, electoral returns, and poor law union records were all compiled using such units.[8] Our intention was to study this local area exhaustively, from 1901—the date of the earliest census for which individual household returns had survived—until 1980. We intended to do the usual participant observation and informal interviewing, and to collect all available documents for the period from 1901 to 1980.[9]

We began by obtaining permission from the parish priest to copy the parochial records of baptisms and marriages. Every morning for several months we sat in his dining room generating hundreds of index cards.[10] We also began to seek people out, to become involved as participant observers. But because, like most anthropologists in the early stages of fieldwork, we knew few people who would talk seriously with us, we spent many an afternoon immersed in Irish history books. We were looking for documentary source materials; equally important, we were trying to pin down the outline of "Irish history." In both endeavors, we had little difficulty, for the history books provided not only numerous archival references but also a coherent chronology of events that appeared to form a connected thread through Irish history. Seemingly, a great deal began with the famine of 1845–49, which was followed by a period of economic recovery, and then, in sequence, by economic depression, Land Wars and Parnellian politics, renewed prosperity and political conciliation, the War of Independence and the Civil War, the polarization of the 1920s, the so-called Economic War of the 1930s, World War II, economic depression, and finally, membership in the EEC and economic expansion.

From this reading of Irish history, 1901 did not appear to stand out in any way. It marked no major event or process, and we soon came to think that it was not a good base point. So we decided to step back a bit and established a new base point in 1879, the year the Land Wars began—the political agitation that led to agrarian land reform and the creation of a landowning "peasantry" in Ireland. We decided to read the newspapers for the Land War period and to look for news about Thomastown and possibly County Kilkenny. We started with the nationalist papers published in Dublin. What we found were reports from various parts of Ireland but almost nothing from County Kil-

kenny. So we searched out the two county newspapers from the period. Even they contained reports only of events, meetings, and violence from other parts of Ireland—mainly the west—with only an occasional story about a Land League meeting near Thomastown or in south Kilkenny.

We concluded that the Land Wars had not been a major event in Thomastown and again decided to push back our temporal boundaries. This time we picked the years just preceding what some of our history books declared to be the "great watershed of modern Irish history"—the 1845–49 famine.[11] Once again we approached the County Kilkenny newspapers:[12] they were filled mainly with reports of famine from other parts of Ireland. Then, over the following months, as we read the newspapers for all the years between 1840 and 1980, looked at other archival materials, and elicited stories from people about the past, it began to seem that—regardless of what we looked at—either very little or "nothing ever happened in Thomastown."[13]

We raised this issue at a multidisciplinary seminar at University College, Cork, in 1980. We expressed great perplexity: seemingly, Thomastown lay outside Irish history. The historians were somewhat amused. They knew, and readily admitted, that so-called Irish history was an amalgam of local and regional events combined to create a unified and coherent whole held together by nationalist (and later, revisionist) ideology. They did not find it surprising that a particular local area or region never experienced all or even any of the events that later became part of so-called Irish history. They also conceded that such lacunae were more likely in the southeast.

What the historians told us was what we had already concluded from our experiences over the previous months. However, what we found most disconcerting, and intriguing, was that the historians seemed utterly untroubled by our objections and concern. This experience crystallized for us the fragility of the past, the capriciousness of historiography. For surely Thomastown, although it had been placed outside historiography, was located in history and had a past.

To recover this past, to do this history, we faced two immediate problems. First, we realized how serious it was that the primary sources (such as newspapers, parliamentary commissions) were not only patchy but also contaminated in indeterminate ways. They had focused not only on the newsworthy at the time but also on the issues and events of concern to the producers of such documents.[14] Consequently, during the famine and Land Wars, Kilkenny county newspapers carried little of local interest, compared with extensive coverage about these events from other parts of Ireland. Thus, even as the past was happening, the skeleton of "national history" was being constructed while the experiences of localities such as Thomastown were being ignored.

Second, for Thomastown, the particular locality on which we focused, many of the kinds of documents that historians use had never existed or had not survived. For example, we unearthed no estate papers, personal diaries, letters, or memoirs from the nineteenth century, and important British parliamentary commissions often had not solicited evidence from property owners or inhabitants of the Thomastown area.

With such constraints, we had four questions:

1. What events, if any, were experienced at the local level in Thomastown and in what ways were local experiences affected by events in other localities—by so-called national and international events—and by Thomastown people's knowledge of them?
2. How can so-called national history be used by anthropologists if its construction is arbitrary and its content uneven?
3. Since Thomastown's history was not a localized reflection of the events that had been constructed into a national history, what was it?
4. If the Irish past was partly made up from Thomastown's history and the histories of many other similar local levels, how could all these histories be made congruent?

To continue our work, we realized that we had to confront what history was; we could not simply study the past or do history. In 1980, in anthropology, this was still a relatively unexplored idea, and it raised two central problems. First, in doing history, Irish historians have expanded the quantity and range of their documentation by unconcernedly using materials from numerous and dispersed local areas. A historian studying, for example, landlordism might have used estate papers from one part of a county, valuation records from another part, and conveyances from another county altogether. However, as anthropologists with our local focus, the vagaries of documentation on the past meant that we were severely limited in our ability to explore the key issues that formed the topical and chronological agenda of Irish history—such as, in the nineteenth century, the nature of landlordism, the trajectory of rents and evictions, tenant land purchases, and so on.

Second, through documents and participant observation, we began to find categories of people in Thomastown who had seldom made it into the Irish history books. The clearest case was that of the rural, industrial proletariat, some members of which, as far as we could tell,

had been landless for at least two centuries. We also encountered the millers, maltsters, brewers, and tanners who had hired these laborers and the numerous, often self-employed, artisans who had lived and worked in the locality. At the same time, we discovered many economic and political activities, organizations, and cultural ideas never mentioned by Irish historians. Clearly, Irish historians, in their concern with events related to their own topical and chronological agenda, had constructed their own very partial version of society.

The problems with Irish historiography and its relation to our own concerns at the time were not solved by referring to the sociological or anthropological literature on Ireland. There we found three biases unacceptable. First, we found a heavy concentration of social studies in the west. It was partly this western bias that had led to a general view of rural Ireland as poor, "peasant," and demoralized. From our Thomastown vantage point in the southeast, rural Ireland looked very different: good tillage land, large farms, a retail sector, and an articulation with an international market that extended centuries into the past, local industries founded during an industrial revolution at the end of the eighteenth century, and the presence in 1980 of foreign-owned enterprises. Instead of drawing from the social science literature, therefore, we found that we had to confront its assumptions about rural Ireland as these had been generalized from studies done west of the river Shannon.[15]

However, the image of a poor and demoralized rural Ireland that came out of anthropology and sociology at the time was not simply a reflection of regional differences. It was also the result of the particular approach that underlay these studies and that provided a second reason for our sense of isolation from Irish ethnography. This was the approach of anthropologists who viewed rural Ireland as comprised of distinct "communities" and who therefore took a so-called community as a basic and natural unit and studied its contemporary culture to ascertain whether "tradition" was persisting, wearing away, or being reinforced. To us, as political economists, the idea was absurd that there could be bounded and isolated local places that had culture but no economy, tradition but no history.[16] As we tried to approach the past in the context of a particular locality, we found little help in these stereotypical "community studies."

A third bias underlay anthropology and these community studies in the west. This was the theory of modernization that also informed, both implicitly and explicitly, the vast majority of economic, social, and historical analyses of Irish society. In this view, Ireland was in the process of "catching up" with the rest of Europe, and it was only a matter of time before it "developed." In direct contrast, we had

notions from political economy and dependency theory that, at best, regarded modernization assumptions as wrong and, at worst, postulated that Irish conditions had more in common with the structural underdevelopment of Third World societies than with the developed countries of Western Europe.[17] At the time of our research, however, only a few macrostudies used this model, and certainly no local-level studies in Ireland analyzed dependency on the micro level.

Thus, with our southeastern study of a rural locality in both past and present, with concepts from political economy and dependency theory, we found ourselves confronting history, sociology, and anthropology. In this effort, we decided, as an early task, to write a local history for Thomastown people. This is what we had promised our informants, friends, and curious onlookers. We also thought to use the opportunity to try an initial and comprehensive survey of our data before approaching more purely academic and theoretical issues. For this task, doing history was a sorting through and a review of our ethnographic materials. However, as we were to realize only later on, this overview of our data from 1840 to 1983 became a way not only of doing history but also a way of creating it.

We wrote *In the Valley of the Nore: A Social History of Thomastown, 1840–1983*. To do so we combined three elements that, at the time, seemed straightforwardly and simply derived from our own interests and predilections. First, we took the events and chronology of so-called Irish history as our major section headings. These were the categories Thomastown people used when they talked about the past and were those they had learned in school. So they spoke about bad landlords and evictions, about good landlords who helped people; they also spoke about the War of Independence and the Civil War, the economic war and hard times of the 1930s, and so on. Local people very clearly conceptualized the *chronology* of their past in the same way as did their history books.

Yet it also was clear that sometimes what people remembered or took care to remember had not necessarily happened, whereas they had forgotten (or failed to remember) things that had happened.[18] A striking case was a large farmer who spoke vehemently, as did many farmers, about the numerous tenant farmers who had been evicted by landlords in the nineteenth century; again, like most farmers, he was unable to name a single case in the Thomastown locality. When pressed, he asserted somewhat impatiently that the evictions had all happened "up Kilkenny way." Interestingly, this farmer did not know (although we had learned it from a document) that his great-grandfather had indeed been evicted in 1850. Thus, the chronological periods of Irish history with which people compartmentalized the

past only partly corresponded to what they knew, or did not know, to be actual events and particular people. These two ways of structuring and conceptualizing the past coexisted in Thomastown. In writing *In the Valley of the Nore,* we provided actual events and the names of real people, as best we could, in the context of the accepted and dominant historical chronology.

There was a third element. Throughout our time spent living in Thomastown, we were struck, virtually every day and in numerous ways, by the centrality of class differences in the locality. In fact, Thomastown people had their own explicit categories that they used to organize a *class-based, social map* for their everyday interaction.[19] The map contained laborers, artisans, shopkeepers, and farmers; once there were landlords and gentry as well. An important feature of these categories was that they involved fixed structural ideas, not temporal ones. They were never used to organize chronology or to order sequences of events in time. Instead, the categories were timeless—they had always existed, they continued to exist; therefore, they were descriptive as well as explanatory. This meant that the categories could be, and were, extrapolated backward in time both to describe and to explain, simultaneously, the past and the present. To take an example: several laboring men, independently, while trying to tell us about the intensity of class difference in Thomastown, illustrated it by telling us that their fathers had never received IRA pensions after the War of Independence because of discrimination against workers. They also always added that "no laboring man ever got his pension." In other words, an unchanging, timeless class structure—seen through a personal event and a general principle—both described the past and the present while it explained that past and this present. A timeless past and present intersected in the here and now through the use of class categories.[20]

In writing *In the Valley of the Nore,* we linked these categories to actual local events and people and to national chronologies. As a result, Thomastown's history was for the first time constructed in the partial ways that it probably was experienced: that children during the economic war had no shoes was the experience of laborers and small farmers; that some Thomastown men became British soldiers during World War I was true only for the working class and the gentry; that emigration was central after World War II was true for everyone, but laborers tended to emigrate to England, whereas farmers and shopkeepers had sufficient capital to go to North America.

In making these kinds of linkages—among chronology, event/people, and class—we essentially constructed a new and different way

of seeing Thomastown's past. In doing history, we had created history. Children from Thomastown's schools have borrowed our book from the local library and have painstakingly copied our story in which the key actors were class based and in which real events only partially confirmed the dominant chronology. Similarly, Thomastown's parish newspaper has reproduced sections of the book for a more general readership.

Our aim in writing the local history had been to reciprocate the help we had received in Thomastown over the years and to have a first go at our materials. It was only after the book was completed in 1985 that we slowly realized that we had "made history."[21] When we began to question why this had occurred (in other words, when trying, still and once again, to understand how to do history) we decided to hold a conference[22] on anthropology and history. Through it, we hoped to discover the different ways in which other anthropologists approached the past using Irish ethnography.

An Overview of Historical Anthropology: From the Past to the Present

Our struggles with the past in both Ireland and Thomastown were but a single instance of a general trend in anthropology and of a growing concern among anthropologists with history. This did not, of course, happen overnight; it was a result of attempts by anthropologists to get away from earlier, increasingly unsatisfactory frameworks.

It is scarcely necessary once again to describe and criticize an erstwhile anthropology (and sociology) that was monopolized by synchronic, structural studies and analyses in which both dynamic process and history were either simply ignored or positively eschewed. The case against that earlier kind of work in anthropology, as in the social sciences generally, has been sufficiently, and repetitively, made, although too often without sufficient recognition of the importance of the detailed, perceptive, and often empathetic studies that were produced within that framework during the 1930s, 1940s, and 1950s. Indeed, it would not be difficult to argue that the foundation of present-day anthropology was well laid as a result. However, for present purposes it is sufficient to recognize that the neglect or rejection of history was not the consequence of any single theoretical approach in earlier anthropology, although too often, misguidedly, the cause of this neglect has been attributed solely to functionalist theory.[23] Rather, it was the result of a general, ill-considered ap-

proach to the description of sociocultural milieux that, inter alia, offered the apparent ease and simplicity of the snapshot of the "here and now" or the "there and then" in other societies.[24]

At the same time, it would be a historical mistake to assume that anthropologists have only recently turned to a consideration of dynamic process and of history and their implications for research, analysis, and understanding. The explorations into Boasian culture history and neoevolutionism and the emergence of the so-called ethnohistory of the 1930s, 1940s, and 1950s stimulated concern for one kind of history, although these can scarcely be seen as forerunners of a later historical anthropology, which is our present concern. In that matter, a crucial moment came in 1949 with the publication of Evans-Pritchard's historical monograph on the Bedouin of Cyrenaica.[25] This was followed in the next year by the same author's declaration that anthropology and history shared common methods and aims despite some differences in technique and perspective.[26] In 1961, Evans-Pritchard reaffirmed Maitland's century-old assertion that anthropology had to choose between becoming history or being nothing.[27] The message was only gradually heeded, but a trickle of anthropological studies began, in the 1950s and 1960s, in which sociocultural change through some period of time was a major preoccupation, even though it was sometimes uncomfortably linked with synchronic analysis.[28] Then, during the 1970s and 1980s, historical anthropological studies became a flood, to the extent that they have undoubtedly established a mode in ethnographic presentations.

This gradually increasing interest in historical studies was, of course, a product of and a part of a number of trends and innovations in anthropology. This is not the place for a history of anthropological thinking and experimenting, and it must suffice briefly to note the trends (with a few illustrative references)[29] that seem to have been particularly influential in the growth of historical anthropology. Setting them out starkly creates a danger of seeming to present them as discrete intellectual developments; but of course they have overlapped and influenced each other in manifold ways. These trends began at different times from the 1950s onward, and most are still active genres in social and cultural anthropology.

One trend was a growing concern, after World War II, for the study of social and cultural change. This was not a purely intellectual and theoretical development, for it was also related to practical and ethical issues and to ideas about the political relevance of anthropology in the poorer parts of the world, particularly the non-Western parts. Initially at least, the concern was for the consequences of

regimes of colonial domination.[30] Anthropologists shared with other interested parties[31] a desire to know what was happening to particular social and cultural institutions in empirical contexts: to households and kin groups, chiefship, patterns of cooperation, religious practices and beliefs, for example, and to economic production and standards of living. By the 1950s, it had become increasingly difficult for anthropologists to ignore what was actively occurring among the peoples they were studying, and it began to be unacceptable to concentrate on a so-called traditional stable order, relegating remarks on contemporary changes to a section or chapter tacked on to the main analysis. This concern for recent social change, and the need to make it integral to research and analysis, encouraged anthropologists to investigate further back in time and to acknowledge the importance and the possibilities of the extended collection of historical materials and of diachronic studies.

Unfortunately, this kind of interest led some anthropologists to conceive of social change (other than minor adjustments) as tantamount to a breakdown of the social system.[32] This conception has been particularly prominent in Irish studies, which purported to show how "tradition" was crumbling and becoming lost. A later example of this persisting viewpoint was Brody's 1976 description of Inishkillane. In his composite and allegedly typical "community" in the west of Ireland, he presented a picture of demoralization, anomie, and disintegration, quite failing "to recognise the diversely creative and innovative processes through which people currently constitute their economic and political lives."[33] With such processes in mind, anthropologists have more realistically investigated and described what innovations have been imposed or offered, how and why, and with what consequences.[34] From such specific and empirical interests, the acknowledged need for analytical and theoretical sophistication has arisen.[35]

More or less separately, a concern for social processes and the dynamics of social life began to develop. The early stage of this concern can conveniently be linked to Firth, who proposed, in 1951, the concept of social organization—people's choices and actions and the processes involved—in contrast to an underlying social structure. At first this concern was manifested in studies of repetitive processes within an essentially static structure, for example, in the domains of kinship, politics, and dispute management.[36] However, the artificial limitations of this soon became apparent and unacceptable; and a positive concern grew for such processes through time and in real life, together with a recognition that social reproduction did not

necessarily lead to mere repetition. That is, as social anthropologists sought to understand various social processes, it became necessary for them to look at ways in which social systems, institutions, roles, values, and patterns of interaction actually operated through time; and that required anthropologists to take account of real time, the passage of time, and history.[37]

Another development occurred as some anthropologists came to study peasant societies. Initially, following the lead of Redfield in Mexico, attention focused on synchronic studies or analyses of recent social change.[38] However, as peasant studies were extended, particularly to Europe, anthropologists entered a novel situation in which their units of study—peasant villages—had long been part of political states whose hegemonic rule had produced archival materials. These allowed anthropologists to extend their inquiries further back in time than had hitherto been possible, taking them beyond short-term studies of current social change and inducing a greater awareness and concern for the past.

The introduction of the Marxist paradigm into anthropology in the late 1960s brought new concepts that were amenable to local analysis (e.g., petty commodity production) while it provided others that required considerable modification before they could be applied to local studies (e.g., mode of production, class, superstructure).[39] The efforts to apply or revamp such concepts required a practical recognition of the significance of historical and regional analysis in the context of a wider social formation. New conceptual and empirical vistas opened up in anthropology. The commitment to historical anthropology through the approach of political economy (in varying degrees influenced by Marxist theory) has been apparent in the large proportion of historical ethnographies written under its influence from all geographical areas.[40]

The Marxist paradigm converged with the growing anthropological concern with dependency and world systems theory.[41] This too demanded a historical orientation. But it also brought a positive reaction from anthropologists against studying history from the top down and against the implication that local and regional populations merely reacted and adapted to national and world movements, almost in automatic and identical fashion. Such assumption was unacceptable to the specialists in local-level studies who saw that it was unjustified. As Cole and Wolf put it:

> We know that a study of small populations will not reveal all there is to know about the total societies in which they are embedded, and we are similarly aware that the study of total societies will not in and of itself provide grounds for predicting how small populations react to more wide-ranging systematic processes . . . a small settlement [is not] a replica of a larger whole in miniature.[42]

Moreover, because of different local and regional conditions, local populations have been affected and have reacted in different ways.[43] Thus, in the context of dependency and world systems theory, anthropologists reasserted the importance of microanalysis and of local and regional variations. They have called attention to the value of the study of history from below and have sought to demonstrate it in their studies.

Meanwhile in Europe, historians and sociologists had begun investigating family structures, household composition, and social life in "past time." The early work by Laslett[44] led historians[45] into kinship and domestic domains that had generally been the peculiar specialism of anthropologists in their studies of contemporary societies. This incursion, in turn, induced anthropologists to bring to the past their expertise on kinship,[46] and conferences brought anthropologists and historians together to examine household and domestic processes.[47] Further cross-disciplinary fertilization occurred as anthropologists began to look explicitly to social history[48] and "people's history"[49] for ideas and stimulation. All this has influenced anthropological thinking, sometimes specifically and overtly, as, for instance, in Kertzer's study of kinship in late-nineteenth-century Italy[50] and in the case studies in this volume.

At the same time, anthropologists were becoming more self-critical and self-reflexive, more aware of the historical and contextual conditions in which their discipline had developed. Whether anthropology has simply been a "child of imperialism" and the degree to which a scholarly discipline has been controlled by the sociocultural context of its practitioners have been, and are, matters of considerable debate.[51] The point here, however, is that anthropologists have been induced to consider the historical context within which they worked and, therefore and by extension, the historical dimensions of the peoples they studied.

With the flood of monographic studies in historical anthropology during the 1980s, and from both sides of the Atlantic, it is reasonable now to assume that for many anthropologists it has come to be taken for granted that it is both necessary and invaluable to look for and at the past—to "do history." However, although this is the case, it does not mean that anthropologists are clear or agreed about the range of

implications of those now accepted necessities. How should anthropologists deal with time, in research and in description and analysis? What are the results likely to be for theory and understanding?

Historical anthropology has become sufficiently well established that a range of interests and foci is fairly clear. In presenting here what looks suspiciously like a firm typology, we have in mind only to facilitate an overview of current interests by suggesting categories that are not intended to be watertight and discrete. Thus, we identify two broad categories within historical anthropology—historical ethnography and the anthropology of history.[52] Although some particular studies fall more or less clearly into one or another of our categories, most studies have the characteristics of more than one as data and interests have required.

I. Historical ethnography	*II. Anthropology of history*
(a) How the past led to and created the present	(c) How constructions of the past are used to explain the present (history as ideology)
(b) Synchronic and diachronic studies of a past time	(d) How the past is created in the present (the invention of tradition)
	(e) How the past created and recreated the past

A *historical ethnography* provides a description and analysis of a past era of the people of some particular, identifiable locality, using archival sources and, if relevant, local oral history sources. The ethnography may be general, covering many aspects of social life during that era, or it may concentrate on specific features, such as social ecology, politics, or religion. It was this kind of ethnography that at last brought anthropologists away from long-established, clumsy devices and assumptions such as the ethnographic present, autarchic "communities," and stable "tradition."

Most commonly, and for good working reasons, social anthropologists have been concerned to link past and present, chronologically and processually, in order to explain and understand the present by reference to the past. That past may have been a generation, several decades, a century, or a longer period, as anthropological interests and the availability of data dictated. As some anthropologists have said during field research, in an explanation to the people involved, there was a desire not merely to record the past for its own sake but to discover and show "how things have come to be the way they are

now." Thus, the anthropologist in her or his work among a contemporary, living people, not only garnered information about the "way they are now" but also, for fuller understanding, worked back in time, constantly relating contemporary conditions and institutions to past events, conditions, and processes. In the final historical ethnography, as it appeared in published form, the anthropologist, like any historian, may well have presented data and analysis more or less chronologically. This has not disguised the historiographical intent to explain the present through understanding the past. Therefore, this kind of historical ethnography has given a bias in the articulation of the historical process. This is an orientation and a concern rarely shared with historians.

In addition, there have been historical ethnographies of periods entirely in the past for which only archival materials were available. Such materials usually included the views and ideas of some of the individuals of that past time. Whether the ethnography was in synchronic or diachronic form depended a great deal on the availability of data. Such historical ethnographic work has been, of course, a more straightforward invasion of the historian's field and not directly related to conventional anthropological research in a present-day situation. As a result, these kinds of ethnographies have been less common. However, a few anthropologists have ventured away from a present-day attachment to make a diachronic study of a wholly past period (e.g., Hastrup's Icelandic ethnography, 1400–1800).[53] In some cases, the period was chosen largely as a matter of convenience (e.g., Vincent's analysis of marriage, religion, and class in nineteenth-century Fermanagh, Northern Ireland). In other cases, the period was dictated by the historical conditions, as in Silverblatt's study of Inca and colonial Peru or Dening's study of the Marquesas from 1774 to 1880 (the end date being the year that French colonial domination was finally established).[54]

Synchronic ethnographies of a particular past time for which archival materials happened to be available have attracted much less anthropological interest. Historians seem to have been readier to undertake that kind of study (typified by Le Roy Ladurie's well-known works).[55] Perhaps the reason may have been the growth of anthropological concern for social dynamics, process, and change, which has made an account of a petrified society scarcely attractive. If, because of limitations of historical materials, it was impossible to take account of major changes through a period of time, it has come to be expected that the ethnography should nevertheless depict a society in which people were active and interacting, making decisions, follow-

ing or avoiding "rules," and creating adjustments. Thus, Donham advocated first the identification of the underlying structure—an "epochal analysis"—before proceeding to a "historical analysis" that was dynamic in character but concerned with changes within the system rather than with fundamental changes or changes of the system: "What I mean by historical explanation is not simply an account of the connections between events over a period of time. A leads to B leads to C. . . . Historical analyses must be located *in* time. They must capture what might be called historicity, but they do not necessarily have to deal with large-scale changes *through* time."[56]

As monographs and journal articles in historical anthropology have proliferated since about 1970, it is obvious that the production of historical ethnographies has been the principal interest in historical anthropology. The aim has been to compile analytical histories that, ultimately, have been outsiders' constructions. Thus, although properly cautious, historical ethnographers have not been afraid to exercise responsible authorial authority. Nevertheless, they have neither advocated nor practiced neglect of the so-called native point of view concerning the history of the people involved. Oral history, human memory, and native explanations have invariably augmented archival sources in valuable ways. Moreover, these historical ethnographers have been prominent in demonstrating the gross error of assuming a single "native point of view" in an assumed homogeneous society or culture. Any society, large or small, is heterogeneous with regard to status, class, age, gender, group affiliation, and distribution of power and resources. Therefore, the anthropologist always needs to consider many "native points of view," which provide an entry into the complexities of real social life and real people.

This does not mean that historical ethnographers have produced artificial syntheses of all the "native points of view" into a single version of history. Rather, it suggests that they have explored the variety of points of view, of native versions of history, together with empirical archival material that may well have been unfamiliar to or rejected by the people under study and possibly contradictory to some or even all native points of view. Indeed, the nature and causes of such contradictions have become important in the construction of a dynamic history. It is clear, then, that historical ethnography has gone beyond "native points of view" as a result of both access to archival materials and use of analytical expertise. Thus, it is clear that any particular historical ethnography has been a construction by the anthropologist—a suggested version of a possible reality—which,

however, did not willfully ignore or contradict native points of view and did not claim to present any final, complete truth or reality.

However, some historical anthropologists have gone beyond taking serious account of native points of view to focus primarily on the ways and the cultural rationale by which a particular people have envisioned, created, and re-created their own past and related it to their perceived present. This we call the *anthropology of history*. Its concern has been to record and describe the insiders' views, assumptions, and perceptions and to show them in the insiders' own sociocultural terms. There has been, then, little or no attempt to produce an "objective" history. Rather, the interest has been in what people know and remember about their past, and how and why, and how people make sense of the past and relate it to the present.

It is important to recognize that the anthropological concern here has been more than just history for history's sake. People's own versions and evaluations of their past are a retrospective product of their present. Moreover, those versions tend to change from generation to generation. Thus, they are important for the anthropological understanding of a people and of changes in their sociocultural contexts.

The most straightforward endeavor has been for the anthropologist to set down the native versions of their own past, linking these to their contemporary cultural conceptions and social arrangements. In an extreme case, the anthropologist may consider available archival materials irrelevant and therefore ignore them. For instance, Sharp and Hanks related that, for the Thai village they studied, there were few documentary materials

> and most of them were never consulted. A visit to the district office . . . revealed a thousand neatly tied bundles of yellowing land deeds. In the deeds were listed changes of ownership that had long been forgotten in Bang Chan [the village studied], but more serious for our study were the transfers of ownership that were acknowledged in Bang Chan but unrecorded at the district office.[57]

It appears that, for these anthropologists, only the information retained in the village was considered relevant and usable in the discovery of the villagers' current visions of their own history. Logically, Sharp and Hanks may have been correct insofar as the introduction of documentary evidence from earlier years was not pertinent to their avowed purpose. On the other hand, consideration of such evidence could have led to a recognition that villagers' own history could itself

change and that, for whatever reasons, villagers were selective in what they remembered and put together and what they ignored and forgot.

People explain the past to themselves, just as they explain, rationalize, and justify their present. From this perspective, history is ideology, and like any ideology, it is open to manipulation and reformulation while it is believed by many to be "true" and correct. In this matter, Parmentier called for and sought to practice an "ethnographic study of the modalities of history," taking account of the connections between notions of time; historical memory; the distribution of power to control, create, and destroy historical ideology; and the range of cultural codes involved in historical consciousness.[58] Moreover, he emphasized that the "inclusion of the intentionality of people who create and interpret their own past is essential, rather than supplementary, to adequate ethnographic study."[59] Parmentier used and demonstrated his ideas through his exploration of the local story about the establishment of the political order in Micronesian Belau.

It is not easy to generalize cross-culturally about the degree of deliberate action by those with power to reinforce and/or re-create historical knowledge and perception to their own expected advantage. In at least some documented cases (but surely in many others too), the initial invention of tradition and history was begun by individuals with little or no power who must have seen some advantage to be gained from their invention and some prospect of its being accepted. In successful cases, their inventions proved most congenial to those with power or seeking to gain it who therefore promoted them. A well-known example was that of the creation of the Highland tradition in Scotland described by Trevor-Roper.[60] There, for a variety of reasons, those with influence and the general population colluded, so that the new tradition was universally accepted. Another instance, among many that could be cited from the current literature,[61] has been the creation by Indonesian historians (under the protection and encouragement of political leaders) of new culture heroes (as, of course, was often done in Europe in the thirty or forty years before World War I).[62] Hoskins described the elevation of a headhunter and famous warrior to the status of Indonesian hero. In 1909, this man led opposition to the Dutch "pacification" of one part of a smaller island in what later became Indonesia. "The rebel who opposed Dutch control has, with some irony, been used as a tool of a new kind of ideological control: the integration of distant regions into the [new] nation state through [invented] assertions of a shared past."[63]

A weakness in the many studies of the invention of tradition and

of new visions of the past has been the impression often given that, once tradition was invented, that was more or less the end of the story. However, if the past can be invented once in response to changes in the present, then it can be (and has been) reinvented later on in further response. There is no need to posit inevitably continuous reinvention or to deny its possibility. It depends, in any case, on what is meant by invention: something wholly new or modifications in various degrees of preceding visions of the past. It has been a long time since it was said that each generation rewrites its history. True as this may be, the rewriting can occur in less than a generation, or it may take longer. Contemporary anthropologists have been strongly reminded by Sahlins of the potentiality of the continuous re-creation by a people of their own history.[64] He introduced his *Islands of History* by noting that history "is culturally ordered, differently so in different societies, according to meaningful schemes of things. The converse is also true: cultural schemes are historically ordered, since to a greater or lesser extent the meanings are revalued as they are practically enacted."[65] What Sahlins called "practical revaluations" of sociocultural things may be a continuous process as people live their lives, make decisions, and act with ineluctable reference to their culture and therefore to their past, whether or not they are conscious of this. There is a "symbolic dialogue of history—dialogue between received categories and perceived contexts, between cultural sense and practical reference." Thus, "there is always a past in the present, an a priori system of interpretations" and, therefore, always a present in the past as that in turn is interpreted.[66]

Engaging Other Disciplines: "Little Localities" and "Big Problems"

Locality has been a key concern in contemporary anthropological analysis.[67] Whereas an earlier anthropology focused mainly on "a people," "a culture," or "a society," most anthropologists today are concerned with "a place." This emphasis probably developed alongside the growing anthropological interest in so-called peasants during the 1950s and 1960s. The early anthropological (and evolutionary) classification of non-Western peoples was based on a juxtaposition of mode of livelihood, settlement pattern, and political regime. Hunter-gatherers, pastoral nomads, and swidden agriculturalists—in so-called acephalous societies—were physically mobile; they may have moved within a large and known territory, but they did not have small, permanently settled hamlets, villages, or towns. Anthropologists took

sociopolitical groups, not locality, as the framework of society. Where tribal peoples were in permanently settled locales, usually in the context of more centralized political regimes (chiefships), anthropologists continued to emphasize the groups that comprised the society or culture: territory and space were regarded as secondary—as reflections of the kinship and political system. In contrast, so-called peasants were defined as permanently settled agriculturalists linked in various ways to a wider state and/or urban-based society. By anthropological definition, then, the peasant mode of livelihood was inextricably linked to a fixed, settled locale and to a wider political regime or "great tradition."[68] When anthropologists moved in—both with their definition of peasant and with peasants—they reinforced the importance of locality both for themselves and for peasantries. During the conference, historian Nicholas Rogers commented several times that he was truly struck by the fact that "anthropologists have a compelling sense of place."

At best, we have found that this anthropological commitment to locality has raised questioning eyebrows among historians and sociologists. More often, though, we have found that the anthropological concern with a "little locality" has been severely criticized, and most seriously, it has been used as a reason for dismissing anthropological findings and understandings. Samuel Clark, a historical sociologist, insisted at the conference that "anthropologists ignore the major transformations, the big problems. They also don't look at what's happening outside the community which they're studying."

We believe that much of the criticism and dismissal has come from the fact that nonanthropologists have often failed to understand the ways in which anthropologists nowadays are trying to use locality. The former often have the outmoded idea that an anthropological study based on a locality was associated with closed boundaries and with a reification of the notion of community.[69] In fact, we argue later that the anthropological use of space contains a theoretical logic and only seldom, today, implies closed systems and esoteric findings. We also argue that the contemporary anthropological use of space and spatial boundaries is no more and no less arbitrary than the conceptual management of space in other disciplines.

Anthropologists in the field have tended to surprise other academics, local people, and archivists with the doggedness with which they have pursued every lead that might provide information on a particular locality. When offered a newly discovered, 150-year-old census tract or a 90-year-old informant from two parishes over, an anthropologist may only with reluctance pursue the lead, bemoaning all the

way there the ill luck that made such a document or such a person not survive from his or her more immediate place.[70] Local historians, or social historians, seem to have been less rigid. When there have been no data for a particular locality, then materials from an adjacent or other nearby localities have been used. In producing one of the very few local studies at the parish level by a historian in Ireland, O'Neill routinely used material from parishes all over County Cavan to study the parish of Killashandra.[71] In historical sociology, another tack has been followed: a sufficiently large political unit or region has been delineated that, deliberately or not, obviated problems of data shortages.[72]

The unique (and seemingly neurotic) anthropological fixation on relatively small-scale localities has a rationale that can be summarized succinctly as *contextualization* and *comprehensiveness*. It has three key aspects. First, the intensive focus on the small scale allows a deep understanding of the phenomena being analyzed, and which constitute the true purpose of the study. This permits the inclusion of "real people" along with an exploration of the interdependencies of sociopolitical patterning, economic conditions, and cultural belief. As the essays by Birdwell-Pheasant and Gulliver in this volume illustrate, this can raise serious questions about the validity of conclusions based on data that have been drawn from larger and less contextualized areas. Documentary or oral evidence from and about an adjacent locality is always only of limited utility because its sociological and cultural context is not known or only incompletely known and, therefore, its meaning cannot be adequately assessed and its implications entirely understood.[73]

Second, the focus on a particular place allows anthropologists to expand into a wider area as they follow the relevant processes, networks, or constraints outward from the particular locality. For example, in this volume, Silverman moves from Thomastown (inward and outward, up and down) to the appropriate arenas and levels, following the processes of privatization in the inland salmon fisheries. Thus, contrary to the popular image derived from an earlier anthropology, an anthropologist today often varies the way in which locality is delineated according to the specific analytical interest being pursued at any particular point. This has the effect of giving the anthropologist's locality an organic, living quality; it also means that the anthropologist is *not* analyzing it as a closed, insular system. Indeed, by choosing a small-scale locality, the anthropologist maintains the option of keeping the boundaries permeable because an expansion outward is manageable. Ironically, that other disciplines

choose larger regions to study does not resolve the issue of boundaries, permeability, or closed systems; it just hides the issue under the guise that a bigger area is a better area. For just as there are no closed localities, there are no closed regions; and just as there are no closed regions, there are no closed nation-states.

Third, the anthropological use of little localities should allow for the explanation of phenomena. Indeed, anthropologists have often been criticized for not explaining why. Part of the problem is, of course, how causality is conceptualized;[74] another part of the problem is that anthropologists have not been listened to. For insofar as it is the anthropologist who is closest to real people, events, and cultural meanings; who has an intimate knowledge of so many interdependent variables; and who sees the intersection of structure and agency in action; then clearly those in the social sciences who seek historical explanation for the "big problems" should find some of what they want in historical anthropology.

What this means, of course, is that anthropologists today seldom study a locality purely for its own sake—to do yet another stereotypical, so-called community study or to provide even more descriptions of the esoteric minutiae of everyday life. It is equally important to recognize that any so-called local level is differently defined, depending on the particular anthropologist, the purpose of the study, and the availability of data sources. To take examples from historical anthropology, Cole and Wolf, the Schneiders, and Stoler each began their analyses at what geographers would call a regional level and then moved to smaller units as they traced out the logic of their particular research problems.[75] In contrast, other anthropologists began more locally and expanded outward as the need arose, or as they moved between localities, or as they used comparative data from other localities.[76] Therefore, it is crucial to recognize that no longer do the majority of anthropologists work with analytically closed, local systems. Indeed, we argue that the contemporary concern with historical anthropology is a definitive statement about the final demise of the primordial anthropological notion that closed local systems can exist anywhere. By moving back in time, historical anthropologists discover that they are unable to find true beginnings for the phenomena that they are studying,[77] and therefore true boundaries in space.[78]

"Little localities" thus form a context within which "big" sociological and cultural problems can be investigated. For such big problems or so-called major transformations as the rise of capitalism, the founding of the modern world system, and the decline of the landed aristocracy had local manifestations and unintended as well as intended consequences that are visible only from a micro perspective. In addition, the sociospatial networks that encompassed the globe had terminal nodes within local places.

This idea that localities are where networks intersect was suggested in the 1950s by Wolf: "Communities which form parts of a complex society can . . . be viewed no longer as self-contained and integrated systems in their own right. It is more appropriate to view them as the local termini of a web of group relations which extend through intermediate levels from the level of the community to that of the nation."[79] Since then, however, it has proved exceedingly difficult to conceptualize and to elaborate on this multidimensional idea.[80] Thus, thirty years later, in 1985, when Carol Smith criticized the anthropological use of the world systems approach in which all local change was treated as exogenous, she asked, once again, "How does one examine and analyze a dialectical process that involves the articulation of different layers in a multi-layered system?" Smith's solution was to analyze "structures that mediate between the local community and the world system."[81] Indeed, the concept of "mediation" has perhaps been the most fruitfully used over the years to cope with this dilemma of levels, layers, and local termini; and often it has been used in historical analyses.[82] However, the dilemma remains, as does the fact that "local nodes," and what happens there, are crucial and somehow must be treated as such. The Catholic Church in Ireland, for example, was (and is) both international and local, as was the operation of the network of economic relations. Conversely, that Thomastown's farmers traveled to Brussels as part of an Irish agricultural lobby was an example of local networks engaging the world system. Particular localities, then, were—and continue to be—where such myriad networks intersected and where the effects can be clearly traced. The local level thus is a context for testing ideas, for generating new interpretations, and for developing new hypotheses. Local case studies, therefore, can provide the framework for comparative analyses in the future.

Most important, though, is the fact that the response and actions from a myriad of local places propel the so-called big processes and major transformations.[83] To say this does not of course resolve the problems of conceptualization that have plagued all disciplines. However, this does mean that the experiences of localities must be made congruent with the findings of the macro-oriented people from other disciplines.

Thus, with roots in a local unit, anthropologists find it possible, desirable, and necessary to analyze large-scale processes. By locating

in a local place, an anthropologist not only achieves expertise in the problem at a manageable and contextualized local level but also can use this as a base for casting both the spatial and conceptual net more broadly. Thus do anthropologists adjust the boundaries of their unit spatially and/or conceptually as they explore the local manifestations of big processes and networks. An excellent example of the kind of contribution that can be made with this strategy has been in the study of local-level political economy of the past two decades, which has done much to explain the nature of agrarian transformation—both in the present and in the past, in Europe and elsewhere. There is now a vast amount of literature telling us, for example, how—and why—peasants become (or do not become) proletarians. We have learned that this process has not been a simple one; it has had numerous permutations, depending on particular contexts and on how the groups were demarcated and the processes conceptualized in the first instance. This literature provides an excellent example of what can be the anthropological contribution to the big problems, dealing as it does with big political-economic processes in the context of open, little localities.[84]

How does this fit with the "big problems" that have formed the basis of Irish historiography? How do we, as historical anthropologists, fit in? As historical anthropologists working in Ireland—or indeed in any state with an extensive and entrenched historiographic tradition—we believe that we must address Irish history and historiography. However, we do not believe that this requires us as anthropologists either to write local histories to provide data for historians or to rewrite national history. The former would have little analytical utility and would be of limited interest, whereas the latter is buried under too much interpretive argument, ideological debate, and hegemonic construction. Equally important, and perhaps as a result, is our recognition that, in the Irish context, so-called national history has consistently failed to engage the fact of local experiences and local histories. One historian put it as follows:

> Almost universally, Irish historians have been guilty of what is best termed the "fallacy of cross-grouping." That is, almost all groups within Irish society, even deviant and dissident groups, have been studied in terms of the nation-state or the national culture. . . . Unfortunately, this perspective . . . has woven yet another deep-running fallacy into Irish historiography, namely an ethnomorphism wherein the entire nation has been conceptualised in terms of the Dublin administrators. Historians of Irish life in the nineteenth century, even the most nationalistic, have taken the same viewpoint as the former British administration in Dublin in emphasising patterns discernible only from that imperial centre. By focussing upon national patterns of governmental and religious administration, we have wrongly projected upon local communities the belief that national concerns, not local issues, should be at the forefront of local consciousness.[85]

At the same time, national historians have constructed a past that has entered the local level and has formed the lenses through which people in local places see and in turn interpret their own local experiences.[86] The result is that complex inside views of the past coexist with, and affect, our complex outside perspectives.[87]

To state the obvious, events in a locality such as Thomastown occurred not in a vacuum but in response to events elsewhere and to impositions, opportunities, and ideas coming not only from more macro levels but also from other localities. For example, the series of Land Acts after 1880 that progressively gave Thomastown farmers ownership of the land on which they had been tenants owed very little to anything that Thomastown people did. The sources were in London, Dublin, and other parts of Ireland. At the same time, Thomastown's tenant farmers obtained their farms not at the national level but at the local level,[88] in interaction with particular landlords and land agents in the context of local political, agroeconomic, and cultural frameworks. Such local conditions subsequently played a large part in affecting the success of farm reproduction over time. The patterning of these successes and failures from varying, diverse, and numerous localities was later aggregated into a unified and homogenized Irish agricultural history that could say little about what had actually happened in Thomastown and about the conditions that had produced the aggregated patterns.[89] Moreover, the culmination of what some historians have seen as the "revolutionary" event that created a so-called peasantry had little meaning in Thomastown.[90] In 1981, most farmers were scarcely aware, first, that they had recently made a last payment to the Land Commission and, second, that this gave farmers unencumbered tenure for the first time in over seven hundred years. As another example, marriage and residence patterns have been the grist of many a historical mill. Yet marriage choices occurred at the local level, not at the national or regional level at which generalized patterns have often been discerned;[91] and such choices have been very much subject to prior household histories as well as to particularly local socioeconomic and local demographic conditions.[92]

What this means is that any national history has to be perceived as

constructed partly from, and therefore made congruent with, Thomastown history and the histories of many other local levels. Thus, we are arguing that locality-based history must be taken as a building block of national history and that historians must account for the "dependency" of national history upon local histories.[93] Moreover, they must simultaneously confront their own past and the historical constructions of their own making.[94]

At the same time, it is essential to recognize that the historical anthropological endeavor is not to provide grist for Irish historians. As a participant aptly said during the conference, "I don't see my work as an effort to fill in the gaps in Irish history." Thus, as historical anthropologists working in Ireland we believe that Irish history and historiography require work to which historical anthropologists can make a contribution. However, historical anthropology in Ireland has its own past, its own present, and its own agenda, which makes it distinct from that of Irish historiography. More important, it links historical anthropology in Ireland, via the cross-cultural tradition of anthropology in general, to the historical anthropology currently being done, and that has been done, in other cultures, societies, and periods.

Historical Anthropology as a Strategy for the Past: From the Cases, the Commentaries, and the Anthropological Present

In approaching the past, anthropologists have brought with them a distinctive cross-cultural tradition rooted in the academic and political history of the discipline, in the personal and professional histories of its practitioners, and in the internal differences—both theoretical and empirical—that always have characterized, and continue to affect, the "doing of anthropology." The intellectual heterogeneity of anthropology in both the past and the present has centered on several key domains that, like hydra's head, cannot be laid to rest. Issues of space, time, voice, and power have been carried forward from our intellectual past into our present. These issues continually emerged during discussions at our conference, they underlie the papers in the volume, and they thread through the growing body of literature in historical anthropology.

SILVERMAN: What . . . is the anthropological approach to the past? Why are we different?

GULLIVER: We do field work. We engage the present with the past.

TAYLOR: We use case material to build theory.

BIRDWELL-PHEASANT: We look at the past to explain the present.

SMYTH: You do ethnographies of the past.

VINCENT: Through ethnohistory, we have a long tradition of historical anthropology. Yet today, it's different; what is it in 1989? Perhaps it's a strategy for the past.

RUANE: But what is the strategy?

A Compelling Sense of Place

That "anthropologists have a compelling sense of place" is very much reflected in the historical case studies in this volume. For anthropologists now working in Ireland, this "sense of place" probably derived from the imperatives that resulted when they confronted a settled agriculture and fixed property matrix in both the past and present; from an earlier anthropology in Ireland with its notions of tradition and community; and, most important, from a more general and contemporary tendency within anthropology, and historical anthropology, to use locality as a central fulcrum: the "local is interesting precisely because it offers a *locus* for observing relations."[95] At the same time, the essays in this volume illustrate that the ways in which local space may be conceptualized and organized can differ dramatically.

Currently in anthropology there are ethnographers who have been concerned with the cultural construction of particular places, of particular localities. Works from the margins of Western Europe (e.g., Cohen [in Whalsay, Shetland Islands],[96] Parman [in a "Gaelic-speaking crofting community . . . on the island of Lewis in the Scottish Outer Hebrides"],[97] and Ennew [also in Lewis][98]) are examples of recent efforts in the British Isles to construct the historical bases for a "culture of community." What is interesting about these studies, and what makes them similar, is that the localities were designated as peripheral—defined as such by the people living there, by emigrants and outsiders, and by the tenets of certain dependency theories. In such studies, the local place was seen as a "community of the periphery" and the anthropologists were concerned to explore the cultural responses to marginality and to learn, therefore, how meanings about

the present were constructed using notions about the past. These studies fall into the genre we have called the anthropology of history. According to Parman, the "position taken in this book is that history and myth should be compared not for their factuality but for their meaning. That is, history should be intepreted not as a recording of what 'really' happened but as a cultural construction that is meaningful in the present to the people interpreting the past."[99]

This kind of approach contrasts sharply with that taken by the authors of the case studies in the present volume and therefore with their conceptualizations and uses of locality. Peripherality, in the ethnographic cases here, is never taken as a fixed condition but as an aspect that alters in time. As a result, cultural constructions of locality are treated as temporally specific, partial, and variable. Ethnographers in the present volume therefore refute the idea that there is a structurally based inevitability about the seeming peripherality of little localities. Instead, they treat peripherality as dependent on the historical trajectory of the so-called geography of domination.[100] Clearly, then, the case studies presented here lean more toward the historical genre that we have called "historical ethnography."

Interestingly, several contributors to this volume explicitly chose fieldwork sites in order to confront conventional assumptions about place. At the conference, Birdwell-Pheasant explained how she purposefully went to the west of Ireland—to a prosperous farming area—to provide an alternate view to the dominant stereotype of the west as poor and marginal. Silverman and Gulliver chose the southeast for similar reasons: to show "another Ireland"—without its stereotypical accoutrements of poor people and so-called traditional culture.

None of the papers in this volume, then, is concerned with the marginality of local places or with the historical construction of a culture of fixed marginality. Instead, the meanings associated with place are seen as changing over time; perhaps more important, a central feature of the cases is that they all look to a wider spatial or analytical context within which local meanings were generated.

Vincent's essay is concerned with the morality of famine behavior and the culture of dearth as these were rooted in a "culture of neighborhood" in County Fermanagh. However, Vincent makes clear that this culture was hierarchically organized and that "the neighborhood" did not conform to a fixed space. Instead, the culture of neighborhood was linked to the structure of landholding and to the changing microeconomies of two different regions on either side of Lough Erne.

Vincent's inductive construction of localness, which she builds

from archival materials, contrasts with those contributors who selected their place and then defined an analytical problem. Gulliver and Birdwell-Pheasant both are concerned with testing "received wisdoms" and the findings of other researchers. Gulliver confronts sociological and historical conclusions about the relationships between shopkeepers and farmers and about the political roles that shopkeepers played in agrarian and nationalist protest. Birdwell-Pheasant confronts interpretations about the "stem family" concept in the Irish farming context. Gulliver uses data from the parish of Thomastown, County Kilkenny; Birdwell-Pheasant uses data from twenty-six contiguous townlands in County Kerry. In both cases, particular localities form the context within which models and hypotheses are tested and sociopolitical and economic processes are investigated.

The two other anthropological essays in the volume provide additional ways of integrating locality in the context of historical ethnography. Taylor analyzes the relationship between meaning and power using three kinds of competing religious discourse found in southwest County Donegal in the middle to late nineteenth century. His concern is to shed light on "popular Catholicism" and on the "devotional revolution" that was occurring at the time throughout Ireland. For Taylor, locality is a broad and general "region" within which his theoretical interests can be pursued. Yet he remains very much rooted in locality. For the nineteenth-century organizational context (of new market towns and associated class structure) and the devotional bases of local society (focused on holy wells) were very much part of a northwest Irish context. Taylor's description would not fit most of southeastern Ireland at the time.

In contrast with Taylor, Silverman begins with a smaller place—the town and hinterland of Thomastown on the river Nore. She focuses on the various classes that were represented in order to describe the political process—of encroachment and protest—by which the burgeoning rights of private property gradually criminalized the customary, common right to fish during the nineteenth century. She follows the action, spatially, as it waxed and waned over the century, to incorporate larger and smaller spaces at different times and for different reasons.[101]

Overall, then, each historical ethnographic case is rooted in a locality, but what is done with place—and how it is defined—varies. No one, however, reifies a particular locality by making it synonymous with community. Indeed, in a conference session, participants agreed that a community is not a place but an ideological construct. It therefore is not surprising that no one takes a place or locality as

the object of analysis. Instead, locality is a flexible context for the analysis of historical problems, while peripherality, when addressed, is treated as a historical question requiring investigation and not as an inevitable state of being a small place.

Locality, Holism, and Narrative Sequences in Past Time

A corollary of locality in anthropology has been the notion of holism: if the place was small enough, then it seemed self-evident to many anthropologists that everything that was relevant should be included and that nothing should be omitted, even if its significance was not immediately obvious. When this anthropological view is associated with a time sequence, an inherent problem emerges: historical ethnography may get lost in its own density.

In studying the past, the importance of presenting a sequence of events over time—a narrative in consecutive order, a chronology—can be of central importance. This is different from the frequently used anthropological method of delineating a series of consecutive time periods and presenting an ethnographic snapshot of each. Such a strategy stopped the chronology at different times, so that a description, however complex or analytical, could be inserted.[102] Another contemporary textual method has been to subordinate chronology to the multifaceted aspects of a particular cultural or social form (e.g., a ritual, political movement, resistance, system of oppression). With such a strategy, historical and chronological transformations in the form have been traced by focusing, separately, on the various aspects that comprised it (e.g., symbol, meaning, structure, agency, social relations). Then the conjunctures—the various transformations in the various aspects—have been brought together and described.[103] A third textual strategy has been to specify a period in the past and to show the various sociopolitical and cultural strands that comprised or contributed to the chronology.[104] What happens, though, when the ethnography itself is a series of chronological events and not a description of relations within one or several time periods or a description of events/time within a sociocultural form? In other words, how can the historian's use of narrative be combined with the anthropological notion of holism?

In the essays by Vincent and Silverman in the present volume, the combination of chronology and holistic ethnography threatens to become overwhelming. Silverman originally set out to describe the nature of protest among salmon fishers against the privatization of

the inland fisheries along the river Nore at Thomastown in the nineteenth century. Her concern was to link Thomastown to social history, to the literature on peasant and working-class protest. However, in organizing her data, what slowly began to emerge was a narrative—a long, complicated story of a process that had both local and nonlocal origins. This process generated varying responses from among local class segments that in turn affected the patterning of privatization while simultaneously stimulating new local responses, alliances, and meanings. To tell this story, Silverman has to present overlapping chronologies across several axes simultaneously: sociologically, as individuals acted as parts of particular groups or class segments at varying times; spatially, as the action moved back and forth between different places and localities in the river system; and institutionally, as the process moved through and into different arenas—land law and policy, fisheries policy and administration, and the various layers of the legal system. Telling this complicated story, presenting the chronology, creates a paper that several conference participants described as "dense."

Rogers had earlier mentioned, in relation to Vincent's paper, the problems in trying "to layer the analysis," for Vincent too has to manage multiple strands of data as she links changes in the culture of dearth and locality with several simultaneous and overlapping sequences of events—in a local meeting, in the economy of the region, and in the politics of colonial and capitalist domination. She also has to move among actors located—sometimes simultaneously in terms of their interests and roles—in rural townlands, Enniskillen town, Dublin, and London.

Both Vincent and Silverman are able to grasp the complexity of process because both are rooted in a locality and committed to holism. Yet these same features strain the narrative mode as both grapple with the logistical problems of presenting numerous and simultaneous events, agents, and meanings within a linear tale. Silverman explained at the conference that she still had left out important things, such as the microsociology of the fishers and the wider political economy of the working class of which they were part. Vincent explained that she had barely touched upon the complexity of the political process among the varying groups—gentry, bourgeoisie, peasantry, and laboring poor. Samuel Clark, a historical sociologist, insisted that the omissions were serious: Vincent should have included the politics of the landlords and the British administration, and Silverman was reminded that much of the impetus underlying privatization was "not local" and should have been pursued.

Thus, the commitment to anthropological holism in the context of locality and in association with the need for a sequential ordering strains the narrative mode at the same time that the anthropologist must leave out what others regard as the "important things." There is, therefore, an inherent tension between three key features—locality, holism, and narrative sequence—which together comprise one way of doing historical ethnography.

"Being There": Engaging the Present with the Past

Taylor's essay in this volume is concerned with the "processes by which a text was responded to by listeners" and the fields of meaning that were generated in the process. He is concerned not with the production and transmission of culture but with its reception. Taylor described at the conference how fieldwork alerted him to this process. While in Donegal, he heard people telling stories over tea. Being there allowed him to juxtapose the teller with the story itself: the story was a lived experience for the narrator, and Taylor was able to learn what the editing was, to hear different interpretations, and to begin to understand that in southwest Donegal, stories and language had become a way by which people supported varying religious views. They did this by sustaining multiple, and often contradictory, religious discourses. In addition, being there allowed Taylor to recognize the importance of many of the elements that comprised the content of the narratives: holy wells, for example, which were an important part of many stories, were peripheral in space and in formal Catholic discourse; however, they were central places and key narrative components in the local life that Taylor encountered.

Contemporary fieldwork thus underlay Taylor's approach to the past: the experience alerted him to the importance of particular historical texts; it made him query how these were connected to the social processes within the church at the time; and it led him to explore the more general and theoretical link between meaning and power in past time. In other words, it was fieldwork that led Taylor to historical texts, a research question, and a theoretical problem.

Although Taylor's field experience enabled him to recognize the importance of discourses in the past, the more common anthropological experience of "being there" has been that the anthropologist was inspired or impelled to move back in time in the effort to make sense of the present. All the anthropologists writing for this volume had done "fieldwork" in their areas, and all were concerned to link

the past with the present—if not in the case studies in the volume, then certainly in the long-term development of their projects. Indeed, in some cases, the fact of being there allowed anthropologists to approach the past in the first instance. This was not simply because anthropologists used or were interested in oral accounts of the past; it was also because many documents on and from the past were "purely local"—buried in an old cupboard of a village office in Guyana,[105] in the home of the current president of the village council in Spain,[106] or in the bottom drawer of a sideboard in a farmer's living room in Ireland.[107]

More generally, this linking of the past with the present seems to be distinctly anthropological.[108] It marks an essential difference between anthropology and social history; it also contains certain dangers. Social historians, said Rogers at the conference, do not use the present and, apart from oral historians, they do not interview the subject. He expressed a historian's dismay at the way anthropologists accept oral testimony about the past. He recalled an incident at the conference when an anthropologist, in answer to a question as to how he knew a piece of information, said, more or less, "Mrs. Murphy told me." What astounded him, said Rogers, was not just the statement but that all the anthropologists around the table simply nodded in acceptance! He argued that for a historian dealing with a document, this was unacceptable. "There must be a critical interrogation of the sources," he said; information cannot simply be used. Rogers was assured by the anthropologists that it was known who Mrs. Murphy was, what her interests and concerns were, the nature of her social and cultural milieu, and so on. Therefore, the anthropologists claimed that what she had said had a material context that gave her information as much validity as a document properly interrogated and contextualized. Rogers remained unconvinced, and he remained uncomfortable with oral materials. "I would feel better if I could hear the tapes," he said, "or if your field notes were in a public archive instead of in your personal filing cabinet, so that they could be rechecked." "Maybe," said Gulliver, "but when that archive is at the other end of the world, perhaps requiring months to obtain research permission simply to enter the archive, it surely is only tokenism amongst historians that the document is presumed to be available to others."

Rogers did concede, however, that anthropologists are "ahead of historians" because they are concerned with how things were told them and not simply with the truth or falsity of information.[109] At the same time, he pointed to a potential danger. Because anthropologists often do history in order to project backward, he suggested

that they may unduly emphasize continuities and cultural resilience instead of ruptures. We would add that it may lead anthropologists to ignore the historical trajectory caused by the dialectic between the two.

In fact, backward projections in historical ethnography already have yielded studies of continuity. For example, Behar responded to earlier studies of social change in Spain by focusing on history and continuity. She wrote: "For the most part, anthropological accounts of rural life in Spain have been studies of contemporary social change rather than studies of long-term cultural continuity."[110] She attributed the concern with change to conditions within the village itself during the 1960s and 1970s—emigration, new agricultural technology, decline of formal religion. However, when she arrived in the 1980s, "things had changed to a point where one could take a longer perspective on village life." She therefore "set [her]self the task of seeking out those aspects of the old rural culture that had endured, that had not been lost in the midst of change."[111] Behar thus studied "long-term persistence" and how the people "forged an adaptation to the profound social, economic and political changes that are so often assumed to have destroyed the old agrarian regime."[112]

For Behar and others, linking the past with the present while doing history became a study in continuity.[113] For some, such continuity also was linked to the ever-present cultural anthropological idea of adaptation. There is, of course, nothing inherently inappropriate about seeking out persistence. The danger lies in positing dichotomies and in studying one side of them (e.g., persistence as opposed to change; continuity as opposed to discontinuity). It also lies in assuming that the present, because it exists, must be linked to a continuous past. Moreover, the danger lies in seeing "social change" (discontinuity) as the opposite of "history" (continuity) and in challenging old studies of sociocultural change by doing new studies of historical continuity. As Sahlins most usefully noted, it is unecessary and erroneous to set up a dichotomy of change and continuity as if there had to be either one or the other, but not both. Clearly, what results is a complex amalgam: the more things change, the more they stay the same; and the more they stay the same, the more they change. Moreover, the subtle synthesis in any particular period and context calls for careful scrutiny, but we must always remember that this is not the end of the story (it might be thought of as only a chapter), for the story continues and requires persisting attention.[114]

A related danger when anthropologists link the present with the past is that they do a kind of Whig history—with its interest in

survivals and with its projection of contemporary structures and ideas backward into time. Said Lamphere:

> It is important . . . not to project present-day analysis into the past in some straightforward manner. In other words, the strategies of resistance that I isolated in a contemporary apparel plant may lead us to look for similar strategies during the 1920s and 1930s, but it would be inappropriate to suggest that these particular strategies were used. . . . Continuity occurs at a more abstract level.[115]

In addition, E. P. Thompson's review of Macfarlane's study *The Family Life of Ralph Josselin* is an example of the kind of critique that anthropological methods may engender among historians for what may be a peculiarly anthropological type of so-called Whig history. For anthropologists not only work with the present of their own particular place and therefore run the danger of extrapolating present meanings onto the past but they also work with cross-cultural understandings—that is, with the present of other places, which has been derived, perhaps from their own experiences, but more likely from the ethnographies of colleagues. Thus, Thompson wrote of Macfarlane's study: "It is by no means self-evident that studies of *Nupe Religion* and of *The Sherpas of Nepal* can serve as 'models' for understanding funeral rites in seventeenth-century Essex."[116] He added:

> The discipline of history is, above all, the discipline of context; each fact can be given meaning only within an ensemble of other meanings; . . . it is most unlikely that any "sociological concept" can be taken, raw, from twentieth century suburbia (or from Melanesia) to seventeenth-century England, since the concept itself must be modified and refined before it will be appropriate to the ensemble of 17th-century meanings.[117]

Clearly, then, as anthropologists approach the past, they would do well to leave behind their well-worn dichotomy of continuity as against discontinuity, and they must tread carefully when carrying concepts from place to place, cross-culturally. Yet most anthropologists would disagree with Thompson's embargo on cross-cultural applications. For he ignored the extent to which anthropologists are self-critical in their application of concepts cross-culturally. He also ignored the fact that there are different levels of abstraction among different concepts and different degrees of precision in their use. The anthropological notion of "lineage" is of a different order from a

"political game" and both are very different kinds of concepts from that of, say, "power."[118]

What all this means is that a historical anthropology must be based on a critical awareness of how the past is being conceptualized—that there are problems inherent in endeavors to link the present with the past, from efforts to use experiences of "being there" as a historical tool, and from anthropology's essential cross-cultural outlook, experience, and assumptions.

The Dearth of Data from the Past Time: Temporal and Analytical Boundaries

Although the experience of being there was central for the anthropologists in this volume, both Taylor and Gulliver expressed great frustration about the limitations of data when approaching the past. Of the three historical texts that he used, Taylor admitted to having different degrees of success in describing their effect in past time. Similarly, in his analysis of shopkeepers, Gulliver bemoaned the paucity of data prior to the late nineteenth century. He commented wryly at one point: "Do you realise that no one today even knows what the inside of a shop looked like in Thomastown in 1900!"[119]

The dearth of ethnographic data is clearly exacerbated by the anthropological rootedness in locality. Indeed, this can directly affect the definition of locality. For example, in Peletz's historical ethnography of Malay kinship, he necessarily focused—given the availability of data on the past time—on the district for the nineteenth and early twentieth centuries, but he concentrated on "his village" for the period after independence in 1957.[120] This kind of changeover may be inevitable, even though it may distort the analysis. In addition, because there is more contextualized data for a smaller locality in the recent past, social and cultural structures in the more distant past may look more homogeneous and more normative, whereas those of the more recent past and present may appear more heterogeneous, more optative.

At the conference, Rogers expressed concern at the kinds of temporal boundaries drawn by the five anthropological case studies: all are located in the nineteenth century and later. Was this, he asked, because they felt they had to engage the present with the past and this temporal boundary represented the absolute outer limit of human or cultural memory? Was it because the anthropologists wished to give a diachronic dimension to a contemporary analysis and therefore only went as far back (about four generations) as necesary? Was it because in Ireland, the documents that survived were from that period?

To all these questions, the answers were yes.[121] Ruane also picked up this question when commenting on Taylor's paper. If "fields of religious experience" were differentiating in the late nineteenth century, was it not relevant to seek out the temporal roots of this process? How far back did the anthropologist need to go?

Part of the answer to this question is linked to how far the anthropologist can go back, given the dearth of data from the past. For a French Alpine village, Rosenberg was able to construct a detailed family history from the mid-seventeenth century;[122] Netting, with his interest in historical demography and ecology, had systematic population data from 1700 onward for his Swiss Alpine village;[123] and for Belmonte de los Caballeros (Spain, population 1,300), Lison-Tolosana noted that "after 1550 the history of the town can be followed satisfactorily in the manuscripts of the parochial archives."[124]

In contrast, in his study of ideology in the circumcision ritual of the Merina (Madagascar), Bloch was able to look at the "history of the ritual, which, with difficulty, can be traced back almost two hundred years." To do this, Bloch did not focus on a "little locality," and he used colonial sources. Rosaldo, in attempting to do Ilongot history using small residential groups and native "stories," pushed back to the late 1880s, although

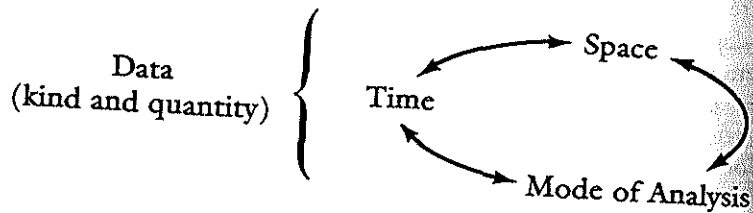
> my reconstitution of the . . . past before 1905 is based on what Lakay and his age-mates remembered of what their parents or grandparents had told them long ago. Usually these received memories were lists of place names where people had lived, stories about the sources of kinship . . . and episodes from feuds. Like Lakay and the others, I have no direct access to the early . . . past.[125]

The differences between these studies may be related to two features: the former ethnographies were about "peasantries"; the latter were about "tribal" peoples. The former were located where there were in-depth archives, in Europe; the latter were not. These features also seem to have affected the kind of study that was done. While Rosenberg produced a narrative history grounded in political economy and Netting gave an ecological and kinship analysis over three centuries, Bloch and Rosaldo dealt with ideology and the cultural construction of ritual and warfare, respectively, over a far shorter time.

Thus, a series of interconnected features seems to affect directly the kinds and quantity of data and therefore to condition our boundaries in time. Very schematically, this can be represented by co-occurring continua.

data sources	in depth archives		no archives
society type	state peasants		stateless (tribes)
place	Europe[126]	early colonized	late colonized
temporal boundaries	early time		recent time

Moreover, while the availability of data constrains the temporal boundaries that we can use, it simultaneously conditions our use of space and affects our mode of analysis. For as we decide on the time period, so we concomitantly either constrain the size of the locality or fix on the particular place that can be analyzed. As these constraints on temporal and spatial boundaries are experienced, so constraints are exerted on the kind of analysis that may be undertaken. Generally speaking, whether a study can rest on socioeconomic relations as opposed to cultural forms and whether it can move toward narrative history-demography-kinship, into political economy, or into culture transformation will depend on the constraints created by the availability of data on the past and on the temporal and spatial boundaries we set. Conversely, of course, it is important to realize that preexisting anthropological interests—in the context of the kind of data that are accessible—will affect how we bound both time and space. For example, if the anthropologist's concern is, say, demographic analysis, then this may constrain the temporal and/or spatial boundaries according to the availability of adequate data. If the concern is for a longer range of time, in a locale without census materials or their equivalent, this will preclude demography altogether, although it may allow a more thorough study of cultural meanings.[127] Thus, we have a schema that postulates causal relationships among data sources, spatial-temporal boundaries, and theoretical-empirical interests. In suggesting this, we are aware that we underemphasize the effects of personal histories and theoretical predilections on anthropological choices.[128] The schema therefore shows only what may be possible in particular contexts; it does not define what anthropologists choose to do. However, it does make explicit the fact that some of our theoretical predilections may be less a product of choice and a search for—or ignorance of—"the other" than a product of happenstance and necessity. If so, it may make us less critical of those anthropologists who choose to do history somewhat differently.



The cases in the present volume, which reflect Irish historical ethnography today, incorporate the preceding constraints, possibilities, and boundaries. Irish historical materials are heavily weighted for the nineteenth century and later.[129] Moreover, for the period after 1850, it becomes possible to interconnect different data sources. For example, parish records begin to intersect with land records, and both overlap with the 1901 and 1911 household (census) returns. In addition, much can be linked to newspaper reports that increasingly became more concerned with local news and events after 1850.[130] Finally, in the 1980s, elderly people could remember back to the turn of the century. All this probably has conspired to locate historical anthropology in Ireland, and perhaps elsewhere, in the more recent past—a past that anthropologists have explicitly connected with the present, with "being there."

To explore this more fully, it is useful to look at how the dearth of data and its relation to the themes of continuity and discontinuity have been manifested in adjacent disciplines. In this volume, Smyth, a historical geographer, is concerned with reconstructing society and settlement patterns in seventeenth-century, pre-Cromwellian Ireland and with describing the processes of transformation that occurred as a result of the subsequent conquest. At the conference Smyth bemoaned the dearth of data: he could never obtain the ethnographic detail that an anthropologist would want and, apart from contemporary folklore, he had had to use the "documents of conquest"—produced by the conquerors as they went about displacing and reorganizing local society. From their property surveys and tax records, however, Smyth argued, a geographer could analyze "areal distributions" as the "key to understanding the process of change." The documents of conquest therefore could be made to speak to historical anthropologists and to deal with the pre- and postconquest periods and with the present.

Smyth chooses two counties (Tipperary and Kilkenny), largely because they were the most completely documented, and he builds a series of typologies—of ecological, cultural, and settlement varia-

tion—that capture regional differences within the two counties during the preconquest period.[131] Most generally, the preconquest property matrix contained three types: 1) landowning areas held by those of old English descent, linked to each other in a wide-ranging kin net and containing a hierarchy of towns, villages, and hamlets as part of a "commercialized world" (south and east Tipperary, County Kilkenny); 2) areas organized by a hierarchy of Gaelic land units shared by kin and partners (north and west Tipperary); and 3) buffer areas held by assimilating and modernizing Gaelic families.

This typological strategy allows Smyth to move into the postconquest period to show the process of social change—how the "material bases for the old order were disrupted and, along with it, the associated settlement infrastructure." Tracing the distribution of Cromwellian surnames in both town and country after the conquest, Smyth pinpoints areas of continuity and discontinuity within the two counties: places where villages disappeared counterpose areas where the "residual power of the older society remained" and where today "villages persist from the seventeenth century."

In analyzing places and patterns of continuity and discontinuity, Smyth is clearly aware of the limitations of the typological approach imposed by the kind of data he has. Using a "time 1" and a "time 2" contrast is not the best way to understand change. He also is aware that the dearth of data limits his ability to address the "local level" as distinct from more regional ones. Yet Smyth maintains a careful balance in assessing continuities and discontinuities, thereby illustrating that a dearth of data does not have to lead to a homogenization of social and cultural structures in the more distant past.

Moreover, if the documents of conquest can be made to speak about the seventeenth century, what can they say to the present and to historical anthropologists working with materials more than a century older? If the rootedness of certain economic formations is accepted, can Smyth's analysis provide a base for anthropological studies in the nineteenth century and later? For example, can the apparently unusual patterns that Gulliver traces among Thomastown's shopkeepers be linked to the town's role as a node in the old English trade network from the thirteenth century onward, a function that survived—and indeed was enhanced by—the increased commercialization wrought by the Cromwellian conquest? Conference participants agreed that a link could be hypothesized if a very general notion of continuity were accepted. Yet they also were uneasy at the conceptual and temporal leaps required—from the seventeenth to the nineteenth centuries, from macropatterns to microdata, from spatial distributions to social relations. The anthropologists felt dislocated

from place and time; interestingly, Smyth and Clark—the historical geographer and the historical sociologist—did not. Vincent argued that it was more important for anthropologists to work from the present backward. Birdwell-Pheasant agreed: "We do history to find out what really happened," she added.[132] It also was Birdwell-Pheasant who most strongly supported the plausibility of the conjectural linkages to the Gaelic past in her own analysis of early twentieth-century household and kinship patterns.

This issue of continuity over extensive time periods will probably become a point of great contention among historical anthropologists. An early example, in 1976, concerned the study by Schneider and Schneider who—to the discomfort of many—traced cultural codes from Roman times to the contemporary mafia. Yet cultural constructs—more particularly, reputed "cultural survivals" and Herskovits's idea of "old forms, new meanings"—are especially amenable to analysis in the long term;[133] social relations, culture practices, and the material conditions of life are less so, given the dearth of data.

Inside and Outside Voices: Contextualized Understandings and Aggregated Data

Birdwell-Pheasant's essay is a response to what others have written about kinship in rural Ireland. She looks at the stem family "model" for which Irish farmers have served as a classic ethnographic example in Western European studies, and she tests its applicability in Ballyduff using data from the 1901 and 1911 census returns, parochial records, land registry records, and interviews. To do this she divides the model into its "ideal" component parts—how it has been said to work ideally. She then compares this to what Ballyduff people have done in relation to premortem property transmission, male primogeniture and impartible inheritance, marriage patterns, three-generational residence patterns, and sibling dispersal. She finds what she calls a "flexible" condition in Ballyduff—a flexibility linked to a hierarchy of values held by Ballyduff people. With this hierarchy, and in the face of economic and demographic realities, farmers have managed the transmission process as best they could—trying to hold firm to their primary goal of maintaining the family on the farm while aiming, secondarily, for ensuring father-son inheritance, and finally, for giving a start to as many of the children as possible.

The range of patterns that Birdwell-Pheasant finds clearly raises questions about the use of rural Ireland as an archetype of the so-called stem family. Perhaps most important, it suggests the impor-

tance for anthropologists, when studying the past, of maintaining two key distinctions with which they generally are careful when using nonhistorical materials—that between (outsiders') models and (insiders') norms and that between insiders' norms (ideal) and insiders' (real) behavior. Indeed, it has been the confusion among models, norms, and behavior that probably has created the archetype in the first instance: "We don't really know what we mean by the so-called stem family," said Birdwell-Pheasant. "Is it a model? A norm? An idea? And whose?"

The issue of "whose" that Birdwell-Pheasant raised parallels Gulliver's questions when he explores two generalizations about rural shopkeeping that, like the archetype of the Irish stem family, have been made by historical sociologists and historians and that have been accepted as "true." The generalizations are the following: shopkeepers were (are) recruited from, and had (have) key links to, farm families; and shopkeepers always were (are) key activists and leaders in nationalist and agrarian politics.

These "truths" about shopkeepers have been put forward in numerous studies. Virtually all of these used data aggregated for large units (county, province, and/or state); they used data from diverse sources (e.g., police reports of arrests, lists of county councillors); and they used data that pertained to very different time periods and events or contexts after the mid-nineteenth century. Most important, the data were derived from documents that did not, and could not, have a common definition or understanding of what a shopkeeper was.

In contrast to these studies, Gulliver works with locally based documents and recollections from Thomastown. He looks at actual decision-making preferences, real social networks, and recorded political participation. He also aims, despite the difficulties, for a coherent use of the concept of shopkeeper. For *shopkeeper* was an insider's category in Thomastown, and its application and usage were complex—depending on situation, personal histories, and historical context.[134] If Thomastown is typical in this respect, it is unclear whom and what the historical sociologists and historians have caught in their net of aggegated data—derived not only from numerous locales and times but also from conflating inside and outside categories and from combining such diverse terms as *merchant, publican, trader, shopkeeper, commercial sector, townsman, employer, manager, contractor, small business class,* and *gombeenman.*[135] Not surprisingly, Gulliver's findings, with only a few exceptions, depart radically from the established truths.

Gulliver does not want to suggest that Thomastown was either typical or atypical. Instead, he argues for more local-level or contextualized studies associated with a greater sophistication in the use of categories and categorizations[136]—all in order to test properly the conflicting conclusions derived from historical anthropology, from so-called national and regional trends, and from a historiography that has been rooted in large regions and in the seemingly major political events and institutions.

Studying, and knowing about, particular contexts in order to use documentation coherently are necessary for making generalizations that can form the basis for cross-cultural comparison; and such a comparison was an important goal for all conference participants. It also is one that has long been associated with anthropology. However, applying the cross-cultural method to the past is more complicated than applying it to the present.

In the discussion on Gulliver's paper, participants were intrigued by his seemingly unusual findings, despite the problems of comparison. They began to explore possible explanations. It was suggested that class structure and relations in the southeast were different from those in western regions and thus constituted a different context for shopkeeping. Farming was too "important" to be left to shopkeepers (and vice versa), said Smyth; Silverman added that the large numbers of laborers may have provided the main clientele of the shops. It was then suggested that discussion ought to move away from interregional comparison—that the key factors were not rooted in the east-west distinction that has permeated the comparative method in Irish studies. Instead, it was suggested that the key factors were located in the particular histories of particular localities—that is, in precisely those relations that were lost from view when aggregated data from regions were compared. Participants then proposed criteria that might have intersected in different ways—in particular localities and at particular times—to create the peculiar histories within which variations in shopkeeping might have occurred. These included the presence or absence of a gentry class and of local industry (milling, tanning, and so on); the nature of the agricultural hinterland (farm sizes, extent of commercial farming, cropping patterns, and so on); the availability of money, capital, and credit; the pattern of exchange within and between classes; demographic patterns, such as population size and structure, the proportion of shopkeeers to other occupational groups and its implications for recruitment, and so on.

If all these variables have changed over time in their relation to each other, then clearly the potential complexity of local histories is immense. Yet the resulting variability clearly constituted, for participants, an argument in favor of more contextualized, local studies. On the other hand, some participants became uncomfortable with the

implications, for cross-cultural comparison, of dealing so intensively with particular localities. Some suggested that the absence of a sufficiently large, quantitative data base with which to test hypotheses would constrain comparison. Others added that the possibility of "unique" findings for each locality would preclude comparison altogether.

This issue was not simply a division between quantitative and qualitative approaches, nor was it the issue of typicality. Rather, it was how the comparative method could be used in the past time. Should it be based on the comparison of contextualized histories or on the comparison of variables? The essays by both Gulliver and Birdwell-Pheasant provide a combination of strategies, but there also have been examples in historical anthropology that have aimed at comparison using multiple contextualized places[137] or time periods.[138] In addition, there have been those, largely kinship and household, studies, that have focused more on the controlled comparison of variables.[139] What all this suggests is that the strategy for cross-cultural comparison in the past time can vary. However, to allow for the possibility of future comparisons among studies carried out by different anthropologists, there must be a firm application of the distinction between inside and outside categories and a contextualization—in time and place—of all data, even those that are aggregated.

Locality and the Wider Analytical Context: Time, Space, and Power in Cross-Cultural Perspective

Ruane's essay in the present volume can be located in the preceding anthropological litany that the local level is rooted in a context that must be integrated into any analysis.[140] For one model of this wider context, colonialism, is explored by Ruane as he reviews the literature on Ireland's past and assesses the divergent opinions on, and varying usages of, the colonial theme. At the conference, Ruane argued that a colonial model could provide a "middle-range conceptualization or level" in historical analyses. Such a level, he argued, was missing from contemporary studies of Irish historical processes even though it may have set key parameters as to how Irish society in general, and particular localities, developed over time. Ruane also argued that such a model might provide a unifying theme for all disciplines that study the Irish past while providing a point at which the varying interests of different disciplines could meet and articulate. At a minimum, said

Ruane, if historical anthropologists were to use their comparative method and materials, they had to decide if the Irish past had parallels with the non-Western, colonized contexts in which they have tended to do most of their work.

However, after exploring in his essay how the colonial model has been used to study the Irish past by practitioners in several disciplines, Ruane concludes that we cannot yet assess the relevance and applicability of the colonial model to Irish historical processes. This is because it has been used too uncritically and inconsistently by both anthropologists and others. At the same time, though, Ruane's review outlines the elements of an approach—which combines theoretical analysis, ideological critique, and a style of empirical research (holistic, contextual, and comparative)—that might allow the question of colonialism in the Irish past to be addressed and resolved. Moreover, such elements apply to any effort to characterize and analyze the wider context, however defined. On a more general level, of course, Ruane's essay addresses the debate between historians and social scientists over the use of theoretical models to interpret historical processes. Given the reluctance of historians to import concepts and models, Ruane's essay—and his concerns—illustrate that anthropologists are not only sensitive to the reasons underlying such reticence by historians but they also are acutely conscious of the problems that ensue when concepts and models are, in turn, imported into historical anthropology.

However, in questioning the relevance of a wider context that may be conceptualized as colonial, Ruane raises an important issue in historical anthropology. For colonialism, in its fundamental guise, is about power. Yet the case studies in this volume, and indeed in historical anthropology generally, fail to address power in any direct, explicit, or systematic way. For example, the vast majority of monographs in historical anthropology that we have cited so far in this essay did not list *power* in their indexes.[141] This does not mean that it was absent from the analyses; rather, it has been hidden under other rubrics, which have changed over time as the vocabulary of the discipline has changed along with its main paradigms. Power, therefore, has often been subsumed as an attribute within particular institutions or of certain individuals—as evidenced by such terms as *political power, symbolic power, spiritual power,* and *discursive power.* It often has been implied by such notions as ideology, oppression, and control. Most recently, it has been included in such concepts as domination and hegemony.

At the conference, Rogers made a similar observation about an-

thropology and power. He said that anthropologists have tended to use the concept, albeit often skilfully, in a "noninstrumental way"—in contrast with political scientists and historians, who have been more likely to see power as instrumental.[142] In other words, anthropologists have embedded their ideas about power in other concepts and domains (e.g., in networks, notions of authority, and symbols). Yet when they did this, Rogers added, they often lost sight of its centrality and were unable to define its role and its boundaries explicitly. So, for example, some anthropologists have used the concept of class, but its key constituent element, power, has not been made clear.[143] As a result, class has often been used as a static category rather than as, following Thompson, a "coming into being"—as a dynamic formation in process.

In fact, four of the six cases in this volume are about the dramatic workings of power. Smyth describes a conquest; Vincent describes the destruction wrought by the state through the imposition of the Poor Law; Silverman describes the victory of private property in one arena; and Taylor describes the discourse, and its source, which competed with hegemonic religious power. Although these cases focus either on the local effects of power or on the narrative behind its imposition as experienced from below, none focuses on the history of that power itself.

This fact constitutes the central theme in Clark's essay. For him, how Ireland entered the so-called modern world is the key contextual issue, and the problem that must underlie any historical analysis. He argues that anthropologists have failed to study this. Interestingly, the anthropologists at the conference in turn criticized the modernization model that centered Clark's own concerns. Echoing a debate from the anthropological past and present,[144] they argued that a colonial interpretation would be more congenial were the anthropologists to approach the so-called making of contemporary (modern) Ireland.

Clark then reiterated his more general point: the case studies had failed to locate themselves in an explicitly wider context (e.g., colonialism, modernization, and so on). In response, Taylor argued—and the other anthropologists agreed—that so-called models of process and of Irish history were embedded in the anthropological case studies and could be found if anyone wanted to do so. Said Rogers in support: "A local study can be both particular and general. The building of theoretical paradigms can be implicit."

From this perspective, it can be argued that the essays in this volume do address the processes that contributed to, and the issues underlying, the making of contemporary Ireland. Moreover, when

linked together, the case studies comprise a cumulative, lineal, and historical picture of much of that making; and they do so using an implicit colonial model. Thus:

Centuries	*Essays and Issues*
Seventeenth	Conquest (Smyth)
Early to mid-nineteenth	Expansion of the state (Vincent)
Mid to late nineteeeth	Extension of private property (Silverman)
	Catholic hegemony: contested meanings (Taylor)
Nineteenth and twentieth	Fracturing of local classes: farmers distinguished from shopkeepers (Gulliver)
	Hierarchy of values and property transmission (Birdwell-Pheasant)

What is missing, of course, and what makes this effort different from historical sociology, is that the cases remain discrete and the linkages between them remain implicit, awaiting a synthesizer. Also missing is an explicit use of the concept of colonialism and its associated notion, the concept of power.

Yet such an avoidance may have advantages. It may preclude seeing the so-called wider context as the cause of local histories, and it may prevent making colonialism or "capitalism too determinative."[145] Indeed, a "new kind of functionalist reasoning"[146] and a "new kind of global functionalism"[147] have often been associated with macro concepts. Thus, in the same way that the anthropologists at the conference were uncomfortable about conjectural leaps required to bring the distant past forward to the present, they also were wary of using power as part of the kind of explanation that may ensue when overarching concepts, such as world system or colonialism, are used to elucidate local places. The concern was with the ways in which both the prior and later histories, and historicities, of localities and real people are homogenized or lost within such overarching constructs. At the same time, no one denied the relevance of such contexts. Rather, the problem was to handle them so that they could be made appropriate to the histories and historicities of localities.

Wolf made a recent attempt to do this. He distinguished four levels of power: power as potency, power in social interaction, tactical or organizational power, and structural power. He argued that history, process, and signification all involve "considerations of power," particularly the latter two types of power.[148] This focus on power—in a

single context where history, process, and meaning are treated together—may provide a middle-range conceptualization that integrates the "local" and "wider" context. For although the distinction between *local* and *wider* has been pronounced untenable, the absence of workable, alternative conceptualizations remains a key problem.[149] We agree with Rosenhaft that the "adequate comprehension of social developments on a larger scale and a higher level of discourse and its integration with the explication of everyday life . . . represents the greatest challenge to anthropology."[150] Rosenhaft saw the concept of hegemony as possibly performing this function; others have agreed.[151] But as Rosenhaft noted, although the concept "directs us more clearly than any other term in current use to examine 'culture' as an arena of class domination and negotiation, . . . it does not instruct us as to what we will find or even how to go about it."[152] There is also the danger that in the context of doing history, the term may become description, cause, and effect all at the same time and thus of limited analytical use.

From another perspective, the discussion about power and the wider context at the conference, illustrated the difference between social history and historical sociology. Said Rogers, a social historian:

> Thompson and Tilly for example are explicit about theory building. Others emphasize texture and empiricism. Social historians usually begin with an historiographic problem to be tested, with an hypothesis. So although some historians are theoretical, most prefer their working paradigms to be implicit. But in any case, they do not work with a high level of theory; they work instead with "conceptual clarities."

In contrast, Clark said, "sociologists do not begin with a clear notion of history. In fact, their notion changes as they work with the data. And their hypotheses are post facto." Moreover, said Clark, they prefer so-called jumbo history—macrobased in both space and time—because "it juxtaposes different types or varying levels of data for the purpose of elucidating general processes and explicit paradigms." Rogers disagreed with such an approach. He pointed out how historians choose a group, a location, or a period in order to deal with the complexity of the past and to explore the issues that have been raised from a broader intepretative level. He called these "middling issues"—less grand than Tilly's, he said, but interesting all the same. Moreover, Rogers argued that local studies can be used in comparative perspective to control for certain variables that have been designated by these middling issues.

More generally, it was Clark, the historical sociologist, who had

the greatest difficulty with what he saw as the anthropological propensity to ignore the wider context. He found problems with the smallness of scale in the anthropological case study approach. He was intrigued by our "up-down" metaphor—how we saw ourselves as moving "up" from our local areas to wider levels of analysis and how we saw ourselves as moving "down" again. He wondered if anthropologists still assumed that their local-level studies were typical, representative, or unique. "How is an area chosen," he asked, "because it's unusual, or because it's typical? How do you generalize? By replicating local studies? What else can be done with them?"

Clark's questions crystallized, for the anthropologists, the way in which the discipline still was perceived by many outsiders, much in terms of discredited viewpoints. Yet as anthropologists approach the past time, they inevitably and often will meet such opinions—from those in other disciplines who are working in the same archives, using the same data. During much of our past, we were off somewhere else, in a locality (exotic) and time period (now) that only occasionally overlapped with the interests of other disciplines. As the disciplines now meet (e.g., in the National Library or Public Record Office in Dublin), anthropologists may often find themselves on the defensive—accused by others of "messing around with detail" in places where nothing much ever happened. Yet, given the problem with conventional wisdoms that both Birdwell-Pheasant and Gulliver found and given the silence of the historical record on the rural, nonagricultural laborers and fishers, on the complexity of life and death during the famine in Enniskillen, and on the competition among religious discourses and beliefs in Donegal local life, the anthropologists were inclined to agree with Rogers's more sympathetic and understanding viewpoint. However, this interchange signaled two features: first, the communication difficulties that necesarily will ensue as anthropologists continue to approach the same past as other social scientists; and second, a growing rapport between historical anthropology and a certain kind of history.

A Distinctive Anthropological Tradition

The problematic nature of such rapport, however, becomes manifest when an apparent convergence, or a division of labor, among disciplines is mooted. For example, in 1980, Hobsbawm wrote, with reference to LeRoy Ladurie's work: "There is nothing new in choosing to see the world via a microscope rather than a telescope. So long

as we accept that we are studying the same cosmos, the choice between microcosm and macrocosm is a matter of selecting the appropriate technique."[153] From the historian's point of view, insofar as anthropology often is located in small contexts and frequently provides "a worm's eye view," historical anthropology may emerge as a handmaid to, or a segment of, history.[154] From an anthropological perspective, it has been suggested that "to juxtapose historians and anthropologists . . . is simply to reify an artificial boundary and to negate the 'blurred genres' of the contemporary academic scene (Geertz 1983)."[155]

Yet, wrote Tilly, the "discipline of anthropology is far broader than ethnography" and the kind of work done by Le Roy Ladurie.[156] Moreover, many historians have been skeptical of rapprochement.[157] In any case, and from the other side, we maintain that sociocultural anthropology contains more than a series of discrete conceptual items that can be consumed at will by historians, that it is more than a type of textual or discursive analysis that converges with other like efforts from other disciplines,[158] and that it is more than a "genre." Therefore, historical anthropology cannot be a small-scale variation of history, a useful repository of useful concepts, or just a technique. Rather, it approaches the past with a coherence that is derived from its own histories, the persistence of (and rebellions against) its own traditions, and the long-term conflicts (some old, some new) among its component parts.

Looked at from the perspective of the historian's past and present, fundamental differences between anthropology and history are clear. For example, there remains today an important division between narrative history and a history that uses social concepts around which to frame a case study.[159] In anthropology, such a distinction has little meaning. Any anthropological ethnography, whether historical or not, has invariably contained conceptual categories (both insider and outsider ones) while a conceptually based study always has had some narrative—to delineate a genealogy, to describe microevents, to construct a life history, to set the stage in the past about the present, to explicate developmental cycles, or to deal with sociocultural change and continuity in the present.

This inevitable overlap between narrative and concept has had many strands in anthropology, most of which have had a lengthy history. The use of situational analysis (or social drama) is an example, and it is compellingly used by Vincent in her case study here.[160] Another example is the continuing anthropological struggle to link the actions of individuals to the formation of groups—an effort

found in the case studies by both Silverman and Gulliver. A third example is the century-old concern with kinship systems and how they operate, a theme that Birdwell-Pheasant pursues. A fourth example is the ongoing anthropological concern with change. Early efforts were Fortes's notion of developmental cycles, Firth's distinction between social structure and social organization, and the studies of culture contact and acculturation in the 1950s. The concern continued, leading to Sahlins's question in 1981 ("How does the reproduction of a structure become its transformation?")[161] and to Wolf's question in 1990 ("How do we get from viewing organization as a product or outcome to understanding organization as process?").[162] In this volume, the essays by Smyth, Vincent, and Silverman provide case studies on the theme of change; Ruane explores a general theoretical context within which historical anthropology might locate change. A final example of the overlap between narrative and concept in anthropology is the endless discussion on the relation between culture and social relations. This has found its way into historical anthropology, resulting, inter alia, in the two genres noted here—historical ethnography and the anthropology of history.

More generally, as historical anthropologists work through their case materials—from archives and from participant observation—the various issues that have been peculiar to the anthropological past are combined in various ways, which reflect the distinctiveness of anthropology and give a particular hue, direction, and content to historical anthropology. Thus, in this volume, Vincent integrates the narrative mode, situational analysis, and the production of culture at a critical juncture. Her effort complements, but is different from, Taylor's use of textual and discourse analysis in a particular historical context. Similarly, yet somewhat differently, Silverman moves between social relations and individual actions, on the one hand, and class action, on the other. She uses concepts from political anthropology as well as the idea of culture as a mediating factor in the context of a complex narrative over a long period of time. Gulliver and Birdwell-Pheasant address variations in kinship and political patterns, using concepts of stratification, values, and interests.

As Vincent pointed out at the conference, and as we do here, all these ideas, concepts, approaches, techniques, and so on, derive from anthropological work in other places and times and for other purposes. Moreover, "we do not all do the same anthropology," said Vincent tellingly. Thus, the elements that comprise the history of the discipline, and the personal histories of anthropologists, all provide distinctive perspectives on, and approaches to, the past. Yet, although

such variations occur, it is important to recognize that they do so with a coherence derived from a shared, albeit often internally contested, intellectual tradition.

In addition, when anthropologists bring their conceptual baggage to the past, they combine it with a distinctive use of source materials. Oral testimony, and the linking of present with past, are of course the obvious examples.[163]

VINCENT: Anthropologists see sources differently. For example, a debate recorded in Hansard. An anthropologist looks not only at the context, as does the historian, but at the interactional and situational dimensions of the context—who's speaking? who's listening?

GULLIVER: . . . and who's there?

SMYTH: Anthropologists also ask different questions of the sources.

ROGERS: That's true. Anthropologists tend to ask *how*, not simply *why*, unlike a good many historians.

GULLIVER: Then there is the interface between archival materials and ethnographic data about the present. Historians do not make this connection.

CLARK: Which do you do first?

VINCENT: We do the "archive in the field"—they go both together, hand in hand. Taylor and Gulliver did this systematically.

BIRDWELL-PHEASANT: And if the present doesn't link directly with the past . . .

SMYTH: . . . stories, songs survive.

VINCENT: There is a layering of the past, and a transformation of the past, as in Taylor's paper; and you can use this to come up to the present. In other words, there is a present which incorporates the past and yet, also, there remains the possibility of isolating the past.

SILVERMAN: But that creates difficulties with the idea that there are always "different histories."

RUANE: In the north, for example, there are Protestant and Catholic histories.

TAYLOR: Yes. This raises issues about narrative and knowledge . . .

ROGERS: . . . and whose knowledge? I think an ethnographer is more likely to respect "other knowledge" for itself than is a historian.

VINCENT: At the same time, today, no anthropologist can or would ignore the archives. Malinowski's view that "the past is in the minds of the informants" is simply no longer tenable.

Toward a Historical Anthropology

In approaching the past, then, anthropologists bring with them a distinctive tradition rooted in the academic history of the discipline and in the professional histories of its practitioners. A corollary is that they also bring their theoretical and intellectual problems, ethical dilemmas, and interpretative arguments that have both plagued and intrigued them for decades. As Ortner noted, "Insofar as history is being amalgamated with virtually every kind of anthropological work, it offers a pseudointegration of the field that fails to address some of the deeper problems."[164]

That, we would argue, is only part of the issue. For many anthropologists who "do history," the past has become just another "foreign country," yet another society or culture. Indeed, this place called "Thepast" has been added to the long list of exotic places in which anthropologists may do fieldwork. However, it is important to recognize that we not only arrive there with our "deeper problems" but also inevitably encounter new kinds of analytical issues precisely because "Thepast" is unlike other anthropological fieldwork sites. This means that historical anthropology cannot be used simply as a means of avoiding or, more likely, of intensifying old conflicts.

For example, what we would call the "misuse" of the past and the reification of history is apparent from the following exchange. Ortner cautioned, in her concern for culture "practice," that, "History is not simply something that happens to people, but something they make within, of course, the very powerful constraints of the system within which they are operating."[165] Roseberry countered that political economists place "anthropological subjects at the intersections of local and global histories." In so doing, "they offer a fundamental challenge to those who discuss culture, history, and practice without sufficient consideration of class, capitalism, and power."[166] Clearly, the divisions are intense, but they are not helped by using "history" as an epithet or by opposing gross concepts (e.g., *people, system, class, power*) that may symbolize but not really address, or allow us to address, the complexities, subtleties, and new problems of doing history and approaching the past.

Thus, we see historical anthropology both as a strategy for the past and as an opportunity to expand the way in which we do anthropology—with stimulus from the new problems raised by approaching the past, with ideas from those who bring different anthropological understandings to the task, and with encouragement from those of other disciplines who have been visiting it before us. Said Smyth, the historical geographer at the conference, with the optimism of a sympathetic outsider: "Anthropologists should not be concerned with doing history. They are approaching the past and that's what's important. They should do anthropology." We agree. For we too see anthropology as a "cumulative undertaking, as well as a collective quest."[167]

NOTES

1. Most of the time, anthropologists have not described how experiences of various kinds combined to produce their finished product. Comaroff wrote: "The connection between text and context is largely one of 'silent development,' and the conventional acknowledgements of authors tells us little about the actual formation of any intellectual product" (1985:xi). Indeed, even in the few cases where nods in the direction of describing their pasts have been made, anthropologists still provided far too little background—as did Comaroff—about the underlying "material and conceptual relations" (Comaroff 1985:xi). This certainly has been true for historical anthropology. We therefore feel that we need not apologize for including our own odyssey here. Instead, we expect that it may be useful for some who are on a similar route. We certainly could have used, many times over, the experiences of others.

2. For example, *The Bulletin of the Irish Committee of Historical Sciences* was first issued in 1939. "Its purpose was to keep members and associates of the Irish Historical Society and Ulster Society for Irish Historical Studies informed of work in progress. It provided notes and news of Irish historical interest and abstracts of papers read before both societies" (McGuire 1981:225). In 1971, a "survey of Irish historiography sponsored by the Irish Committee of Historical Sciences, the body representing Irish Historical interests on the Comité International des Sciences Historiques," reviewed works published between 1936 and 1970 (Lee 1981a:vii). In 1981, historian Joseph Lee edited a similar book of essays that surveyed historical works published between 1970 and 1979.

3. The idea of economic dualism—of a "dual economy"—in Ireland came out of the work of economic historians (e.g., Lynch and Vaizey 1960), but it was soon replaced by a more complex notion of regionally based, economic variation (e.g., Cullen 1972). Geographers were already using the latter idea

(e.g., Hughes 1963; Smyth 1975), although they were mainly concerned with agriculture (e.g., Gillmor 1967; O'Carroll 1978; Ross 1969). However, it was the idea of cultural "dualism"—of the south/southeast versus the west of Ireland—that entered into analyses of social and community relations (e.g., Hannan 1979), and it certainly underlay the community studies done by anthropologists at the time.

4. A review of this approach was provided by Roseberry (1988). For Silverman it was a continuation of her earlier work in agrarian systems and class formation in Guyana (1979) and Ecuador (1987) and in archival research (1980). For Gulliver it provided the opportunity to research the past—as compared with what had been possible in much of his earlier field research in East Africa (1955b, 1963); to deal more concretely with issues of social change (1955a, 1958, 1969); and to follow up an emerging theoretical interest in linking the present to the past (1971).

5. This of course typified anthropological analyses at the time. It began with Arensberg (1937) (republished in 1988) and included such works as Cresswell (1969), Messenger (1969), Harris (1972), Symes (1972), Brody (1973), Bax (1976), Leyton (1975), Kane (1977), Fox (1978), and Scheper-Hughes (1979a, 1979b). An early critique of this community study approach was made by Gibbon (1973) in reviewing Brody. A later critique, from the perspective of dependency theory, was made by Ruane (1978).

6. Apart from the Harris and Leyton studies listed in n. 5, the anthropological community studies at the time had been done almost entirely in the west of Ireland.

7. The variable and complex meanings of the term *Thomastown* were described in Gulliver and Silverman (1990).

8. These administrative units ("district electoral divisions," or DEDS) did not correspond to the parish, although they were contained within it, together with other parts of other DEDs. In any case, Thomastown parish itself did not correspond to any administrative or legal unit, either in the present or past time. In 1981, the population of the two DEDs was 1,932, and the parish contained 2,670 people, or 716 households. This population was about one-third what it had been in 1841 (Silverman and Gulliver 1986: 18–27).

9. At the time, the archival sources that we knew about were the parish records, parliamentary papers, and census materials. As time passed, our archival arsenal additionally came to include local business and farm records, minutes and correspondence from local organizations, local school records, county and national newspapers, poor law union records, land valuation records, memorialized deeds, encumbered estate court papers, wills and probate papers, land registry materials, and so on. For more detail, see Gulliver (1989).

10. Canon Dr. Michael O'Carroll, P.P., was an unstinting supporter of our work. We owe *him* a great deal—not simply because of the parochial records but because his early and immediate acceptance of our presence in

Thomastown, and of our research, allowed us to approach more easily other Thomastown people for their help.

11. For example, Ó Tuathaigh (1972). Although other historians denied that it had been a watershed (such as Crotty 1966:46–51), the fact that there was academic dispute inclined us to think that, at the very least, it was a "key event." See Daly (1986:117–24) and Ó Gráda (1988) for discussions of its variable, and debatable, impact.

12. It is curious that we had such difficulties with temporal boundaries as compared with spatial ones. In fact, neither one was ever drawn to our complete satisfaction. In relation to our spatial boundaries, we found—midway through our 1980–81 sabbatical year—that our two DEDs were heavily concentrated on "lowland" areas and that this had implications for farming and settlement patterns. So we began to include other parts (townlands) of the parish that contained "upland" areas (and two farming villages; see Smyth, this volume). This meant that we had failed to collect some data for these (such as newspaper reports). However, in most cases (such as parish records, probate papers, etc.), we actually went back to the sources to collect the materials. Similarly, our temporal boundaries were once again pushed back—to 1800—largely because we found the parochial records for 1798 and beyond, Tighe's 1802 publication, newspapers, and 1833 tithe lists. We then found memorials of deeds and county newspapers for the late eighteenth century. Essentially, we have never firmly fixed our temporal boundaries, although, clearly, different kinds of data apply to different periods. Our spatial boundaries have remained far firmer, although as we go back in time, we occasionally have had to take other geographical units—such as baronies—that were used in the records; and we did collect surviving estate papers for areas adjacent to Thomastown parish. This has meant that we have spent the summers subsequent to 1980–81 reviewing old archives for new areas and times. "Doing history"—as Price (1990:xix) and others have noted, is time-consuming and tedious. For more detail see Gulliver (1989).

13. For example, not only did the Land Wars pass by with only two or three meetings of a land committee in Thomastown, but neither battles nor skirmishes occurred in the War of Independence (1919–21) or the Civil War (1922–23). As a reflection of this, few older people had any personal recollections at all of anything happening—things they saw or heard, about people whom they knew or knew of—in connection with those events.

14. Although this seems self-evident, it is a hard fact to accept about the past when there are so few documents that the researcher wants each one to be of some use. The historians' idea of "interrogating the sources" is useful, but it does not solve the problems of unknown bias and, of course, omission.

15. See n. 5. In fact, by the 1980s, the focus of anthropological research had shifted somewhat to Northern Ireland, largely because of anthropology departments there. However, the western bias remained. In a 1989 collection of seventeen essays by anthropologists on Ireland (edited by Curtin and Wilson), six were located in the west (including Donegal), five were in the north, three were in the east-southeast (including one by Silverman and one by Gulliver), two were located in Dublin, and one was in Kilkenny city. The urban focus was, of course, new.

16. A recent example of this kind of study—of customs and tradition—was that by Shanklin (1985). For an analysis of the confusion between *history* and *tradition*, see Herzfeld's discussion of the "aboriginal European" in Greek ethnography (1987:56–61).

17. Part of our recognition of this position came from our Canadian roots and from dependency theory that pertained particularly to Canada and the work of Harold Innis (e.g., Melody 1981). As citizens of what we believe to be the "richest underdeveloped country in the world," we found a great many structural and historical parallels in the Irish context. Therefore, that Ireland might be similar to a Third World country (and to Canada) did not require any great conceptual leap on our part. It was, however, very dismaying to most Irish people with whom we discussed this. (See Ruane's essay in this volume.)

18. We are very sure that these were not cases in which "Thomastown people" engaged in a community-based, collective process of historico-cultural amnesia/recall. This has been suggested for other places (e.g., Collard 1989). First, Thomastown people—assuming one can ascertain what that means in the first instance (see Gulliver and Silverman 1990)—did nothing collectively and never have done. Second, we found that different people—depending on age, sex, class, locality, personal history, social networks, and personal predilections—remembered or forgot different things and different categories of things (e.g., Silverman 1989). Third, dramatic and traumatic events that actually had been witnessed by living people, and that were linked to events in the national chronology (such as the funeral, during the War of Independence, of a Thomastown lad who joined the IRA and was killed in action in north Kilkenny) were very clearly remembered and spoken about by eyewitnesses. In general, the process of remembering *events that were actually witnessed* very much depended on the person's interests and personal involvement and on us, the anthropologists, being able to elicit memories by asking the right questions. The process of remembering events prior to living recall was very undeveloped in the Thomastown area. We learned very early on that people's knowledge of events scarcely predated their birth. In contrast, informants could go a little further back in time if *facts about people* were being elicited by us. This was because informants had heard about individuals who had been old when the informants were young. We suggest that the limited time depth in the recall of events and in the recall about people might have been related to the fact that national, and hegemonic, chronologies were so deeply entrenched.

19. This was explored in more detail in Gulliver and Silverman (1990).

20. In fact, with only minor variations in meaning and, occasionally, with additional categories appended, this class-based social map was of very long standing and fairly broadly distributed. We have found it in early nineteenth-

century documents for both southeastern Ireland and England. Interestingly, the structure was used in nineteenth-century sources to describe the past as well as the present!

21. We had "made history" in two senses: not only had we created a new version of the past, but we had also become a part of, and an element in, Thomastown's past after 1979.

22. The conference was held April 4–9, 1989, at Seneca College, King City Campus, Ontario. It was funded by the Social Science and Humanities Research Council of Canada (SSHRC); by the Wenner-Gren Foundation for Anthropological Research, New York; and by contributions from the universities of the participants. Two other participants, not included in this volume, were Chris Curtin (University College Galway) and Thomas Wilson (United Nations International School, New York).

23. Recently, the cause has been identified differently. "In hindsight, anthropologists' previous failure to tackle history seriously was due to their colonial *mentalité*" (Ohnuki-Tierney 1990a:2). We argue, however, that any single-cause explanation is inadequate.

24. Use of the "ethnographic present" has been both a symptom and a partial cause of anthropological failures to consider processes through time, change, and history. Indeed, its use in ethnographic writings has been amply criticized. However, the ways in which academic language and discourse perpetrates the ethnographic present has seldom been discussed. Thus, it is virtually an unquestioned convention to state: "Dr. X writes that . . ." — even though Dr. X wrote in, say, 1972. This use of the academic, ethnographic present is misplaced, misleading, and indicative of ahistorical bias. It also has probably been at least as responsible for ill-considered slides into the easy and convenient ignoring of the dynamics of social life, as have been the more frequently cited causes—ethnocentrism and paternalism among anthropologists in relation to those they have studied. In this volume, because our concern is with history, we have made a careful and deliberate effort to respect temporality. Things that happened in the past are described in the past tense—whether that was yesterday, last year, or last century and whether that was in the ethnographic case studies or in reference to scholarly works. Therefore, when we write about the present volume, we use the present tense, but when we refer to past work(s), events, fieldwork, and so on, we use the past tense. We write, for example, that s/he "described," "explained," or "put forward the view." It is interesting that the conceptual difficulties that we had, as editors, in expunging the present volume of all misleading tenses suggested to us how deeply ingrained is the academic, ethnographic present. It is also interesting, as a telling anthropological footnote, that our severest editorial difficulties came in editing tenses with reference to scholarly works (i.e., not to "the other" but to "ourselves") and to folklore in Taylor's essay (i.e., so-called tradition).

25. Evans-Pritchard (1949).

26. Evans-Pritchard (1962:24ff).

27. Evans-Pritchard (1961:20). It is worth noting that so many critics of an earlier anthropology chose to take Evans-Pritchard's 1940 monograph on the Nuer as an archetype (e.g., Rosaldo 1990) and as a prime example of the absence of concern for diachrony and history. Those critics then ignored both the historical ethnography of the Cyrenaican Bedouin (1949) and the study of the dynamics of local-level processes among the Nuer (1951).

28. Some examples were Barnes (1951, 1954) on the Ngoni of northern Rhodesia (Zambia), Fallers (1956) on the Soga of Uganda, Stenning (1959) on the Fulani, Smith (1960) on political developments in the Hausa chiefdoms of northern Nigeria, Bailey (1960, 1963) in Bihar (India), Mintz (1974) on the Caribbean, Redfield (1962) on a village in Yucatan, Cohen (1965) on Arab villages in Palestine and Israel, Béteille (1965) on an Indian village in Tanjore, Lison-Tolosana (1966) on a Spanish town, Friedrich (1970) on a Mexican village, Wallace (1970) on the Seneca. An exhaustive listing would be rather longer than this and much longer than stereotypical assumptions have allowed. In the same genre, although somewhat different, were the studies in culture history. For example, Mintz and Wolf (1950) first described the historical antecedents of *compadrazgo* in Latin America and then proceeded to a functional analysis of the contemporary institution. See also Wolf (1957).

29. Here and elsewhere in this essay, we do not attempt an exhaustive review of all published works that might fall under the rubric of historical anthropology. The particular studies cited are intended only to be illustrative.

30. Early examples were Gulliver (1958) and the studies in Southall (1962). Later examples were Gough (1981) and Kottak (1980).

31. Scholars in other disciplines (political scientists, sociologists, economists, etc.) and nonacademics—such as aid and development planners and administrators, politicians, and educators, and, of course, local residents.

32. This approach was conditioned by an adherence to functional theory; see Wilson (1945).

33. Peace (1989:106).

34. See the references in n. 28.

35. See Lewis (1968), Ruane (1978), and Peace (1989).

36. For example, political processes analyzed by Leach (1954), Turner (1957), and Swartz et al. (1966); processes in domestic and kinship fields described in Goody (1958) and Gray and Gulliver (1964); jural and dispute management processes in Gulliver (1963).

37. For example, Silverman (1980) and Schryer (1980).

38. See, for example, Redfield (1930, 1955), and Arensberg and Kimball (1940).

39. This was begun by a number of French anthropologists—for example, Meillassoux (1964), Godelier (1966, 1967), Terray (1969)—and became widespread in the 1970s.

40. Some examples have been Nash (1979), Vincent (1982), Stoler (1985), Sider (1986), Bloch (1986), Silverblatt (1987), Rosenberg (1988), G. Smith (1989), and Donham (1990).

41. Mintz (1985); Wolf (1982).
42. Cole and Wolf (1974:3).
43. Cole and Wolf (1974) demonstrated this by contrasting two adjacent villages in northern Italy, one German-speaking and one Italian-speaking. A comparable demonstration was given by Carol Smith (1984) using data from Guatemala.
44. Laslett (1965).
45. For example, Plakans (1984) and Casey (1989).
46. For example, Goody (1983).
47. Laslett et al. (1972); Wachter et al. (1978); Netting et al. (1984).
48. Cohn (1987a, 1987b, 1987c).
49. For example, Samuel (1981) and Medick (1987).
50. Kertzer (1984).
51. From Gough (1968) and Asad (1973) to Marcus and Fischer (1986).
52. Rather more limited categories were suggested by Chapman et al. (1989:1) when they asked two questions: "How did the past lead to the present?" and "How does the present create the past?"
53. Hastrup (1990).
54. Vincent (1984); Silverblatt (1987); Dening (1980).
55. Le Roy Ladurie (1978, 1979).
56. Donham (1990:206, his italics).
57. Sharp and Hanks (1978:31).
58. Parmentier (1987:5).
59. Parmentier (1987:7).
60. Trevor-Roper (1983).
61. Some recent examples have been Rappaport (1985), Farriss (1987), Feinup-Riordan (1988), Bowen (1989), Buckley (1989), Davis (1989), and Silverblatt (1989).
62. See, for a useful review, Hobsbawn (1983).
63. Hoskins (1987:619).
64. It may be added that the envisioned past of a particular people as seen by outsiders can also be re-created, as with Westerners' views of the past in Asia and Africa.
65. Sahlins (1985:vii).
66. Sahlins (1985:144, 152).
67. In recent time, historical anthropologists have carried out general thematic analyses over very broadly defined spaces. For example, Wolf (1982) and Mintz (1985) studied commodities and world systems; Goody (1983) and Segalen (1986) explored principles of kinship in Europe; Macfarlane (1987) produced a series of essays on the "culture of capitalism" largely in Europe. There also have been historical anthropological works that were regionally based: for example, Comaroff (1985), Smith (1985), Bloch (1986), and Silverblatt (1989). However, the majority of historical anthropologists have tended to concentrate their data collection on places that corresponded more with what we are calling "little localities."

68. Whether peasantries should be analyzed within the context of a state as against a city, and whether they should be seen as linked primarily to a dominant political regime as against a wider culture, became crucial points of difference in the analysis of peasants (e.g., Wolf [1966] as compared with Foster [1967]). More Marxist-oriented approaches added their own opinions (e.g., Shanin 1971). Regardless of viewpoint, though, no one questioned the centrality of locality for analyzing settled agriculturalists who were incorporated within a complex society.
69. Certainly this criticism could be made of an earlier anthropology. We ourselves make it in relation to Irish ethnography in the first section of this essay. We do not feel, however, that such a critique is warranted in relation to most contemporary anthropological endeavors. However, the possibility of misunderstanding, criticism, and dismissal remains. For example, Simmons wrote that "historically minded anthropologists, like their structural-functional predecessors, run the risk of pursuing theoretical questions in times and places that may seem to be of little or no importance to historians and others. . . . This is to be expected. Historians concern themselves with a different kind of problem," with "larger scale orders of data" and with "different and larger contexts" (Simmons 1985:182).
70. These examples are taken from our own experience in Thomastown. The document was a copy of the household returns from the 1831 census for the Tighe estate, Inistioge (eight miles from Thomastown). There may be only three such documents that have survived for the entire Republic; none survives from County Kilkenny. We made a copy of the returns, thanks to a local network of local historians that works to keep everyone informed of all new "finds" before they "disappear," as local people would say, irretrievably, into the hegemonic bowels of the Public Record Office in Dublin. However, as we note later, such documentation—from outside one's intensively studied locality—is of limited use to the anthropologist.
71. O'Neill purposefully chose Killashandra parish because the household returns for the 1841 census survived, thus providing the main source for a "unique data base" (1984:25). Rogers pointed out (in a personal communication, February 1991) that historians generally tend to accept the validity of using documents from so-called comparable areas. He added, however, that some historians—such as Hoskins and his students (the Leicester school) and the *Annales* historians—"have a better sense of place and are more sensitive to locality than are other historians."
72. See the essay by Clark, this volume. In political science, historical analyses have followed a similar tack. For an Irish example, see Walker (1983).
73. More specifically, this is because the data in such documents, however rich and scarce, lack a socioeconomic and cultural context in both time and space: they are unlinked to other information, such as births and marriages, property conveyances, and so on. Using such documentation on its own can lead to a timeless snapshot of structure, which is precisely what most anthro-

pologists today are trying to avoid. For a more detailed discussion of this, see Gulliver (1989).

74. For example, Rosenhaft stated that "ethnography, like all forms of structural analysis, is better at describing than explaining, and better at accounting for a situation than at predicting its outcome" (1987:102).

75. Cole and Wolf (1974); Schneider and Schneider (1976); Stoler (1985).

76. For example, Blok (1974), Kottak (1980), Kertzer (1984), Lan (1985), and Donham (1990). In addition, some historical anthropologists have used only a state or regional level (e.g., Tambiah 1976; Sider 1986; Hastrup 1990). Others, in the context of a so-called tribal society, have used a "people" (e.g., Vincent 1982; Comaroff 1985; Bloch 1986).

77. See n. 12 for our experience with boundaries in Thomastown.

78. Although there have been exceptions, these usually had an explicit reason or goal. For example, in his study of a Portuguese village, Brian O'Neill stated: "I have worked with the community-study framework despite the methodological problems that choosing a bounded 'community' raises. Macfarlane has pointed to the utility of a revised definition of the community as an object of analysis (1977); I have followed his tack by attempting to expand the community-study framework diachronically" (1987:19). O'Neill was addressing a very precise ethnographic problem as he tried to deconstruct a dominant idea in Iberian ethnography—that villages had been and were egalitarian. The "most essential task at this moment is to provide solid empirical data which will serve to banish once and for all the myth of the egalitarian hamlet community" (p. 2). The fact that O'Neill had a particular ethnographic purpose in bounding a so-called community and the fact that he regarded it as necessary to explain why he was dealing with "community" suggests that the current trend in anthropology is to avoid what we have here called closed systems and esoteric findings.

79. Wolf (1971:51).

80. Wolf, in the previously quoted article, illustrated his point by providing a historical and political description "of the ways in which social groups arranged and rearranged themselves in conflict and accommodation along the major economic and political axes of Mexican society. Each rearrangement produced a changed configuration in the relationship of community-oriented and nation-oriented groups" (1971:62). However, Wolf's more complicated ideas about a "web of group relations," "local termini," and "intermediate levels" remained unsatisfactorily explored.

81. Smith (1985:194).

82. For example, Sydel Silverman (1965), Blok (1969), and Marilyn Silverman (1979).

83. This was Carol Smith's point as well. She argued that "capitalism [is] a social and cultural phenomenon as much as an economic one . . . that can be and is affected by class struggle and human agency all along" (1985:225).

We would add here not only that the dimensions of class struggle and agency have local manifestations but that they may be apparent only from a local perspective.

84. The extent of theoretical and cross-cultural understanding on this issue, which has come largely from the political economy turn in anthropology, is apparent from the following examples: Hedley (1979 [Canada]), Kahn (1980 [Indonesia]), Vincent (1982 [Uganda]), Holmes (1983 [Italy]), Roseberry (1983 [Venezuela]), Trouillot (1987 [Dominica]), Gavin Smith (1990 [Peru]); and, of course, the lengthy discussions on domestic commodity production in the *Journal of Peasant Society*. For an analytical, critical, and historical review of this material, see Roseberry (1988).

85. Akenson (1972:2).

86. There are long-standing agencies through which this dominant history has been transmitted: the schools, the church, the media, and political organizations. These not only existed at the time the history was being formulated but they also contributed to its formulation. Moreover, although these institutional interpretations often articulated with some local experience, they also were made to articulate with it—and to interpret it—by powerful, local agents who represented these interests—teachers, priests, intelligentsia, and political leaders. Shanklin presumably came across the essence of this dominant history when she encountered the "COBO response"—the fact that the notion of "centuries of British oppression" was used continually by people in Donegal to explain what was disliked, disapproved of, or not completely understood (1985:24).

87. This distinction between insider and outsider has been expressed in numerous ways by anthropologists at different times. The dichotomies of emic-etic, other-self, native-anthropologist are examples. More recently, the separation of "voices" or the use of "history" as against "historicity" has reflected *this* distinction.

88. Yet there have been numerous studies that analyzed the processes of Irish agrarian reform using nationally based, aggregated statistics, economic data, and legal frameworks. For examples, see Solow (1971) and Kolbert and O'Brien (1975).

89. For example, Crotty's excellent, deconstructionist analysis of the history of Irish agriculture (1966) could tell us little about what might have happpened in Thomastown; nor could it do more than suggest very broad issues that might help us in our efforts to analyze agrarian history as it was experienced in Thomastown. Mainly it was a study of national trends—using aggregated (statistical) national patterns—and of official policies and politics in agriculture. As such, it provided us with the fact of a particular state policy, for example, and we could then try to ascertain if it had had any impact in the Thomastown area. Beyond that, this "history of Irish agriculture" had little meaning or applicability to our own efforts to do agrarian history.

90. Lyons (1973:219).

91. For Irish examples of this approach, see Connell (1962), Gibbon and Curtin (1978), and Breen (1982a).

92. For local analyses of marriage patterns in Ireland, see Birdwell-Pheasant (this volume), Symes (1972), Smyth (1975), and Breen (1984b).

93. In a 1981 review of Irish historiography, Ó Tuathaigh noted: "In Irish political history the most encouraging development of the past decade has been the shift in emphasis from, in Theo Hoppen's phrase, 'national politics to local realities.' " He cited Hoppen's work on electoral history as an excellent example of the genre (1981:88, 90). In the preface to his book published several years later, Hoppen indeed noted that he had been struck by the fact that the "more the detailed workings of individual political communities in Ireland were examined, the more striking and important seemed the gap between local realities and the rhetoric of national politics. Such communities, whether individual in the geographical or the social sense, often maintained a style of politics only intermittently in step with the stated aims and methods of the movement generally held to have dominated Irish history in the nineteenth century" (1984:vii–viii).

That said, Hoppen's analysis departed dramatically from what a historical anthropologist would call a local focus. He used aggregated data for counties (e.g., pp. 348–49 on agrarian outrages) or the nation (e.g., pp. 412 and 413 on the occupational and ethnic backgrounds of policemen and British soldiers; or p. 436 on rates of urbanization). His analysis moved over the entire country, both north and south. His analytical categories aggregated large numbers of "landlords," "laborers," and "farmers" from diverse areas and there was no in-depth analysis of a "localized place."

These observations are not to detract from Hoppen's excellent study. They simply illustrate the very different notion of "locality" that has characterized contemporary Irish historiography (e.g., L. Kennedy 1983; O'Shea 1983; Donnelly 1975; Feingold 1984; Boyle 1988) as compared with historical anthropology.

94. A telling example of the difficulties that this can raise occurred at an Irish Studies Conference in 1988. Gulliver presented some of his "unusual" findings concerning Thomastown's shopkeepers (see Gulliver's essay, this volume). A historian in the audience argued that it was because he had gone to the "wrong" place: if Gulliver had chosen a larger place or a "better" place, he would have found what historians had been telling him was there! A more promising outlook is Ó Gráda's discussion on the "incidence and ideology" of the famine. He stated: "Shattering dangerous myths about the past is the historian's social responsibility. In Ireland, where popular history is an odd brew of myth and reality, there is plenty for him to do" (1988:79). For historical anthropologists, of course, "odd brews" themselves constitute fertile ground for research.

95. Sabean (1990:10).

96. Cohen (1982, 1987).

97. Parman (1990).

98. Ennew (1980).

99. Parman (1990:13).

100. Donham (1990:141).

101. Silverman's analysis includes, at different times, the Thomastown area, the nontidal portion of the Nore River, the entire length (tidal and nontidal) of the river, all the nontidal regions of the watershed to incorporate the rivers Barrow and Suir, and the entire watershed system—both tidal and nontidal.

102. Examples of this textual strategy from historical ethnography have included Blok (1974), Schneider and Schneider (1976), Silverman (1980), Verdery (1983), Lamphere (1987), Peletz (1988), and Rosenberg (1988).

103. Examples have included Comaroff (1985), Bloch (1986), Sider (1986), and Newbury (1988).

104. For example, Inden (1976), Netting (1981), and Sabean (1990).

105. These formed the basis for Silverman's 1980 study of micropolitics between 1902 and 1970 in a Guyanese East Indian, rice-farming village.

106. This was the case in Santa María del Monte, a village studied by Behar (1986) using, for the most part, village-based records.

107. We found numerous documents—generated both in the private (e.g., shop accounts, farm records) and the public domains (e.g., estate maps, minutes of local organizations, correspondence)—in the hands of Thomastown people. It was our continued residence there and our ongoing assurances that we were interested in any and all "old papers" (and that we would always return them!) that often induced people to bring these documents to us or to to tell us about them.

108. Gulliver (1989) described both the fruitfulness of, and the methods for, linking archival research with fieldwork. Rosenberg noted of her work in France: "This system of moving back and forth between the village and the archives was both a source of inspiration and a check on flights of fancy. Grounding ethnography in history and history in ethnography provided me with a sense of what was plausible and what far-fetched in both fields" (1988:xiv).

109. Rogers added that there have been recent examples of historians interested in how (e.g., Davis 1987) and that this was a new approach. However, Rogers believed that the general distinction between anthropology and history remained: that the former has tended to ask how, whereas the latter has tended to ask why.

110. Behar (1986:12).

111. Behar (1986:13).

112. Behar (1986:13, 14).

113. Some other examples have included Cole and Wolf (1974), Schneider and Schneider (1976), Kottak (1980) and Frykman and Löfgren (1987).

114. Sahlins (1985:144).

115. Lamphere (1987:329).

116. Thompson (1972:43).

117. Thompson (1972:45–46).

118. The complex relation between meaning, abstractness, and applicability is apparent in these three examples. *Lineage* has a precise meaning and has generated great debate as to its applicability to any context whatsoever. The concept of a "political game" has been accepted almost as a metaphor, and apart from questions as to its paradigmatic implications, it has been used—without stimulating debate—in numerous cross-cultural instances. As to *power*, it has often been used, but there has been no agreement about what it means and it has been among the most problemmatic concepts that anthropologists have dared to use. Rogers recently suggested (personal communication, February 1991) that Thompson was against "crude" conceptual applications, not all applications. Nevertheless, it seems to us that not only have historians been wary of such applications (Cohn 1987c:66) but the problem also remains as to what constitutes "crude."

119. Cohn noted a similar phenomemon: "In working with judicial records for a local region in India, I felt hampered by not knowing the people I was dealing with, except from what was in the record. Simple questions with regard to the litigants, the lawyers, the judge and the witnesses, about their ages, their social and economic statuses, and their formal and informal relations to each other, could be answered only if the same individuals appeared in other cases or if other materials were available which gave information about them. Such questions can often be quickly answered in the field, but not in the library" (1987a:7).

120. Peletz (1988).

121. Rogers also asked if the temporal boundaries were the result of major breaks in Irish history that had caused the anthropologists to begin in the nineteenth century. This suggestion seemed less plausible to the anthropologists—both from their reading of Irish historical materials and from their experiences in their local places. Much depended on what they had been studying. For example, both Birdwell-Pheasant (given her concern with kinship) and Vincent (with her interest in the Poor Law and state building) had found that the famine of 1845–49 was an important marker. For others, this had not been the case and there was no agreement on the "major breaks." See our earlier discussion on temporality in Thomastown at pages 5–6.

122. Rosenberg (1988:58–71).

123. Netting (1981, chapter 4).

124. Lison-Tolosana (1983:7—originally published 1966).

125. Rosaldo continued: "Thus the early period of Rumyad history can be understood only through close attention to the observation of Ilongot historical consciousness" (1980:247).

126. We use Europe here because it contains the archival centers of the empires from which anthropologists have mainly come, in which they have mainly worked and whose documents they have mainly used. It is likely, however, that the archives from the centers of non-European empires—such as those in China, Japan, Russia, Turkey, and India—also contain data of excellent quality. However, apart from Inden (1976) on Bengal, and Robert Smith (1972) and Nakane (1972) on Japanese family history, we are unaware of historical anthropologists who have dug deeply into these. Presumably we have simply missed them. Ohnuki-Tierney's (1990b) material on Japan appears to have been based on secondary sources, although this is not entirely clear.

127. An example was Silverblatt's (1987) study of the transformations in Andean culture and women's roles in the light of the Spanish conquest. This was an *early period* and an *early conquest*. Therefore, there were *archival remains*, albeit somewhat sparse and largely from the conquerors. However, using a *broad spatial area*, Silverblatt was able to move into the realm of *social relations* as well as into the *cultural domain* to try for an analysis that was located simultaneously in *political economy* and *culture change*.

128. For example, Fernandez (1990) used archival materials from rural Spain for a symbolic analysis of the "contest" over enclosure and change over two centuries. The available documentation might have allowed him to do more of a narrative or political economy type of analysis. Presumably he chose not to do so.

129. Primary documents, held by public bodies in Ireland, are simultaneously rich and patchy. Much was destroyed when the archives were burned during the Civil War in 1922. This has led to the oft-cited rationalization that "it was better to have a nation without an archive than an archive without a nation." Perhaps as compensation, Irish land records are far superior to any in England. Given the popular interest in local history, there have been several guides to holdings, such as Nolan (1982).

130. Surprisingly perhaps, many nineteenth-century materials are richer than twentieth-century ones. To take an important example: there were no government commissions after 1922 that published testimentary evidence from local witnesses as there were before Independence.

131. Ecologically, areas of intensive tillage counterposed areas of pastoral economies. Culturally, areas varied in the extent to which old Gaelic naming patterns had declined. Settlement patterns also varied—nucleated settlements had grown up around churches, castles, and mills; agglomerated settlements were farm based and kin based, usually around a head tenant farmer, his partners, and their laborers; and dispersed settlements were a third type, more difficult to find because of a shortage of data.

132. Birdwell-Pheasant elsewhere explained why she, and perhaps other anthropologists, "got into history. We do it in order to do better anthropology, because many of the socio-cultural-politico-economic processes that we are so intrigued with describing and explaining are processes that do not occur merely within a lifetime or over a generation, or even between two generations. They are (often stochastic) processes of multiple generations, the collective and collaborative product of long sequences of lifetimes. There are, indeed, cycles and patterns that can be discerned *only* within historical time

that are just as real as individual life cycles and patterns. . . . Unless we do history, then, we are like biologists who study flowers and leaves and seeds and pollen as distinct entities rather than as parts of a complex . . . ecosystem" (personal communication, March 14, 1991).

133. For example, Ohnuki-Tierney (1990b) and Fernandez (1990). A recent example of the controversy that may be engendered can be found in *Current Anthropology* (June 1990). It concerned Spencer's critique of Kapferer's analysis of cultural continuity and nationalist ideology in Sri Lanka.

134. The category of so-called shops in Thomastown has faded into artisanal enterprises in which tradespeople have sold their goods over a counter (e.g., shoemaker, tailor). The boundary also has been blurred by persons who have sold their services without a fixed place—for example, masons, insurance agents, electrical contractors. Thus, the term *shopkeeper* has been used in Thomastown, sometimes but not always, for some tailors and shoemakers; for a particular electrical contractor but not others; and occasionally, for the insurance agent but never for the mason. The term *shop* also has been used, by some people but not all, to cover the premises of so-called hucksters. Thus, insiders' usages have been extremely complex. They have varied according to the particular context and the person speaking.

135. The problems of applying concepts to the past have of course emerged in the most statistical of all historical endeavors—family history. For example, distinguishing *household* from *family* and from *domestic group* has proved difficult, as has distinguishing *kin* from *servant* (e.g., Netting et al. 1984; Sieder and Mitterauer 1983). Insider categories have proved as difficult to apply as ousider ones. Such difficulties have prompted Hammel, for example, to an extreme position of emphasizing outsider usage alone: "The more strictly that analyses of different data bases adhere to a particular scheme of analytic categories, the more likely those analyses are to be comparable with one another and the less likely they are to adhere closely either to the appropriate folk categories or to actual behaviour in the societies concerned" (1984:30). Many anthropologists would not accept that procedure. However, it does point to the problems that certainly have been ignored in Irish historical studies.

136. In Irish studies, it is only the stem family concept that has prompted efforts at concise definition. What Gulliver's study shows is that such an exercise is required in other areas as well.

137. For example, Kottak (1983) and Heiberg (1989).

138. For example, Silverman (1980) and Hansen (1989).

139. For example, Hammel (1978).

140. The term *litany* is appropriate here, for it is easier said than done. Yet how or whether to do it constitutes the essential anthropological problem today. See n. 149.

141. Even Comaroff (1985), for example—with *power* in her title—did not list it in her index.

142. Rogers added that, since Foucault ("language as power"), there has been a tendency among historians to pull in "noninstrumental usages" as well.

143. Of course there have been exceptions, particularly among Marxist anthropologists for whom the nature and cross-cultural applicability of "class" has been a central theoretical issue (e.g., Donham 1990; Smith 1990). However, the centrality of *power* was often not the main concern while the shortfall between sophisticated, theoretical constructs and messier empirical data has remained a problem.

144. For example, Vincent pointed to the American culture history school—of Mintz and Wolf, and their concern with "actual history"—in contrast with Steward's concern with "national integration" and modernization. Other examples of the confrontation between the two models were also given: Fallers (1967) as against Saul and Woods (1971) and applied anthropology as against action anthropology.

145. Roseberry (1988:170).

146. The existence and persistence of noncapitalist features have often been explained in terms of the functions that they performed for an overarching capitalism (Roseberry 1988:170).

147. Carol Smith (1985:194).

148. Wolf (1990).

149. Roseberry (1988:173). Roseberry stated: "A logical and historical separation of the 'local' and the 'larger context' . . . is no longer tenable." Instead, "anthropological subjects should be situated at the intersections of local and global histories." In saying this, Roseberry probably echoed a general feeling in the discipline today. Fortunately, though, he added that "this is a statement of a problem rather than a conclusion." Indeed, we would argue that it is *the* problem. It is an aspect of the "dilemma of levels, layers, and local termini" that we discuss earlier in this essay (pp. 25). However, there is a danger that Roseberry's statement about what is untenable will become a litany with which to criticize efforts in historical anthropology. For if our tribulations in Thomastown have done one thing, it has been to teach us that it is far easier to talk about history than to do it. It also has taught us that the previously mentioned dilemma is central.

150. Rosenhaft (1987:103).

151. For example, Sider (1986).

152. Rosenhaft (1987:105).

153. Hobsbawm (1980:7).

154. For example, in a somewhat negative review of Price's book, *Alabi's World* (1990), Hobsbawm contrasted "Richard Price's views about how history should be written and those of more traditional historians and anthropologists" (1990:46). That is, a historian divided anthropology—part went with history and part went awry—all without reference to the ways in which anthropologists themselves have carried on their internal dialogues and conflicts.

155. Ohnuki-Tierny (1990:2).

156. Tilly (1978:213). He added: "The portion of anthropology with which French and francophile historians have worked most effectively is only a small part of the field, and in some regards a backwater. Furthermore, the influence of historical work—including that of the *Annales*—on anthropological practice has been slight."

157. Cohn noted that "doubts are raised by eminent historians about the fruitfulness of closer working relationships with anthropologists." He cited Stone, Thompson, and Le Roy Ladurie. "What has been questioned is the appropriateness for the study of the European past of the theories, models, and methods which were developed by anthropologists in order to understand and interpret the non-European worlds" (1987c:66).

158. Indeed, textual analysis, as represented by Clifford and Marcus, for example (1984), is ahistorical.

159. For example, see Worden (1991).

160. As used by Turner (1957) and discussed by Van Velsen (1967).

161. Sahlins (1981:5).

162. Wolf (1990:591).

163. That the occasional historian has done fieldwork (Ohnuki-Tierney 1990a:2) is largely irrelevant. Most have not, nor have they been expected to. In contrast, almost always, anthropologists have been required to do so—both as a rite of passage and to collect/produce data. The different kinds of data and "comprehension" that have resulted were examined by Cohn (1987b:47–49).

164. Ortner (1984:159).

165. Ortner (1984:159).

166. Roseberry (1988:179).

167. Wolf (1990:594).