

The Danger of Knowing What You Are Looking For On Routinizing Research By Orvar Löfgren

The Academic Division of Labour

We are sometimes misled into believing that there is some system behind the division of labour among the various disciplines in the humanities and social sciences. Yet most of these subjects were created by chance conditions and momentary situations in the past. There are interesting differences from country to country and from university to university. The disciplines have emerged through the splitting of old subjects or the creation of new ones. The form they have taken is often due to attempts to distinguish themselves from nearby subjects or from the older disciplines of which they are offshoots. This tendency survives so doggedly in most disciplines that it is easier to define what is not ethnology, sociology, or history.

The element of chance in the rise of academic disciplines is evident if you look at the growth or absence of ethnology in different parts of the world. How significant is it that we in the Nordic countries and central Europe have a division of labour between a general anthropological perspective and a regional specialization (with a historical perspective) in the form of European ethnology? From the end of the nineteenth century and onwards, a new discipline has staked out its territory in these countries, thereby shaping not only its own identity but also the orientation and aims of neighbouring subjects. In countries without this tradition, the field of cultural history has been divided up in different ways.¹

In the discussion of the traditions and boundaries of scholarly disciplines, there is a tendency to overemphasize what might be called "the Sunday best": high-flown goals, theoretical programmes, and normative declarations about what the subject ought to

be. There is, however, a gap between these goals and what people actually do. Little thought is given to the way everyday scholarly praxis moulds the identity and evolution of a subject. Everyday research routines have a greater effect than scholarly ideals. New adepts are schooled in the hidden curriculum by learning the implicit knowledge and unwritten rules. Sundaybest rhetoric belongs in the prefaces of undergraduate textbooks, inaugural lectures, and other programmatic statements; everyday imprinting occurs in coffee-break discussions, at examinations, in the many tacit agreements about what good or bad research

The Importance of the Material

The presence of such an important controlling mechanism in scientific praxis is partly due to the material studied by the discipline in question. The most obvious example of this is archaeology, where digging and collecting have created such enormous mountains of material that they sometimes have a paralysing effect on the ambition to theorize or synthesize.

I experienced the confrontation between different attitudes to the material during my undergraduate years. I began by reading history in the 1960s, and I remember how much the question of what was considered researchable was determined by a firmly established concept of sources: certain types of historical material constituted sources (usually locked away in institutions known as archives) and the first question we learned to ask then - in the rather positivistic spirit of the sixties - was not whether the problem was interesting but whether there were any legitimating sour-

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ces. Much toil was devoted to the craft of reading, interpreting, and handling the various classical types of sources and, perhaps more importantly, learning to exclude categories of material which were not considered serious or even defined as sources in the first place .2

When I then came into contact with anthropology, I was almost shocked by the difference in the approach to empirical material: the researcher himself had to acquire it by going out into the field. It was simply a matter of finding interesting problems, after which the material would take care of itself. The assembled material had no special value - the interviews and field notebooks were the property of the anthropologist, who had little thought of preserving them for posterity. Like sociology, then, anthropology became a subject without archives: the material was mostly kept in the researcher's drawer and perhaps thrown away when the study was finished. Behind this practice there is a different scientific outlook from that in the historical disciplines.

My next schooling was in European ethnology, where I found myself in a discipline which had been mostly shaped by the anthropological tradition of gathering one's own research material, but which also had the ambition of documenting for the future in the archives of tradition. Research praxis in ethnology, especially after World War II, thus developed in the intersection between the traditions of cultural history and the social sciences.

European ethnology as a subject was born as a national rescue action - an ambition to salvage the remains of what was then thought to be a dying, "classical" peasant culture. This very special birth de-

fining the subject as a *collecting* science, which created close links between teaching, research, collecting, and archive-building. This origin distinguished the subject from other humanistic disciplines like literature or art. It gave us archives, but it also shaped our research praxis in an interesting way: even though ethnology has undergone several paradigm shifts since the beginning of the century, we still have this link between cultural documentation and analysis in our everyday work.

The Institutionalization of Science

When a new science seeks admission to the scholarly community, its pioneers are often intoxicated with the thought of all the possibilities for developing the subject. Looking in the rear-view mirror, we often see only the main road that was followed, not the side-tracks and alternatives that once existed but are now perhaps overgrown. It is therefore particularly important to examine the way a young discipline becomes an institution with its fixed routines.

In the same way, it is all too easy to over-emphasize the consensus within a discipline, precisely because researchers who deviate from the accepted course are informed (often indirectly) at an early stage that they do not belong, that they should try another subject, or devote their energies to something other than serious scholarship. The appearance of a high ceiling may be due to the way people bow their heads submissively.

All disciplines in the making go through a course from utopia to routine, or what the human geographer Anne Buttmer (n.d.) calls the transformation from Phoenix to Faust.

The Phoenix represents the pioneer spirit, the fantastic moment when a new subject or paradigm is born out of the ashes and sets off to conquer the world, bursting with ideas and research programmes. The world is still young, everything is possible - this is a time to break away from traditional academic conventions and doctrines. Some of the Phoenix's visions are short-lived, since they would need to be brought down to earth if they are to be implemented in practice - and it is here Faust takes the stage. The pioneer era gradually gives way to the concrete phase of establishing, ordering, institutionalizing, and anchoring the visions. Faust sets to work building fences, creating a fixed boundary between his science and all the others, enclosing research fields, writing curricula and textbooks, establishing principles for teaching and filing. Faust's work may not be as heroic as Phoenix's flight, but it is just as essential. The problem with his project is that it creates stereotyped routines: there is more reproduction than innovation.

In time tension may arise between the original ambitions of the Phoenix and the institution built by Faust. Out of this tension is born Narcissus. He is bored with Faust's diligent labour and asks himself how ethnology ended up here, what purpose it has. Narcissus starts to dig in search of his roots, wondering about the identity of his subject. The result may be that he ends **up** contemplating his navel, but he can also obtain a clearer picture of the subject's possibilities and future. This can lead to a new Phoenix rising from the ashes, as Anne Buttimer shows in her discussion of the transformation of human geography.

The point of these three characters is that they do not have to be seen as parts in a

Cyclic evolutionary drama. They represent different aspects of scholarly work, the roles that can be united in a single research career. I have discussed this life cycle elsewhere (Löfgren 1988b). Here I shall content myself with looking at some lines of development in ethnology.

A subject whose *raison d'être* was originally a rescue operation, whose representatives rushed out into the countryside to collect the remains of a supposedly dying traditional culture, lent itself to becoming an archive and a museum. Much toil was expended on figuring out how to handle all the material thus collected: how to construct archives, how to classify material. Scholars sorted what was important from what was less important or what could be ignored, either because it did not come under the heading of valuable folk culture or because people thought that it did not belong in the field of the new ethnological science.

Several generations of ethnologists have undertaken a critical ideological scrutiny of the collecting work of the pioneers, showing how they constructed rather than reconstructed "the old peasant culture", how contemporary middle-class values, dreams, and unease were projected into the creation of a national or regional cultural heritage (cf. Frykman 1979; Löfgren 1989b). What we less frequently discuss is how this selection process continues to be a living reality even for today's researchers.

Constructing Culture

The work of collecting this archaic peasant culture established some of the cornerstones of ethnological research and ideas about what culture was. Ethnologists learnt

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how to select, to look and overlook, to decide priorities and values. They divided cultural phenomena into fixed categories, itemizing and fragmenting, turning a real world into elements that could be sorted under convenient headings in the archives: customs, traditions, tools, genres, beliefs, and so forth. Then came the work of reorganizing this selection, of joining the elements together in a new whole: a culture pattern, a local culture, a world view, a synthesis which can then be presented in a monograph like "Folk Customs among the X", "Village Life in Y", "Regional Culture in Z". This process of selecting, naming, classifying, and sorting into new wholes was a basis for the archive-building science of ethnology, just as it shaped the ethnological work of the museums.

It is easy to be ironical about the lack of self-criticism with which this construction work was done, but when we look back we should not be blind to the fact that all research is based on similar microprocesses and more or less conscious selection principles. What is interesting is to consider to what extent we continue this tradition in our present-day research. Similar processes take place every time we begin to transform a piece of social life into a "culture" - youth culture, immigrant culture, workingclass culture - that allows itself to be described or filed away in an archive.

Perhaps a more radical way in which the archival and collecting interest has been able to control the everyday praxis of the subject is the established ethnological taxonomy - the division of the subject into areas of realia. This is so easily taken for granted today that we do not see how it permeates not only the filing system in the archives but also the lists of set reading for

undergraduates, the choice of research topics, the arrangement of a local study, a monograph, or a textbook. We have learnt to think of ethnology in terms of its subsections, whether the traditional division (food, costume, housing, customs, social organization, belief, work, folk poetry) or later rearrangements (ethnicity, maritime culture, teenage culture, etc.).

Imagine for the sake of experiment that we had chosen other principles for the division of the subject, using the epochs of the historians, the institutions of the sociologists, or the regions of the geographer. What would our archives and textbooks look like then?

I have discussed elsewhere (Löfgren 1988b) how the guiding effect of the traditional divisions has increased with the use of a magical conjunction. There is an ethnological fondness for linking areas of realia, not least in the curricula. The first decades of the subject saw the establishment of classic combinations like "belief *and* custom", "settlement *and* social life", "annual *and* life-cycle festivals", "costume *and* textiles". When these combinations proved less serviceable in the 1960s and 1970s, new ones were created, such as "family patterns *and* socialization", "community types *and* life-modes".

These divisions and connections have influenced the form taken by research tasks, as is reflected in the choice of student essay topics. If people in the fifties and sixties wrote about social conditions it was chiefly in relation to settlement; the study of social organization was often synonymous with the study of village regulations. In the same way, the study of customs concentrated on either annual or life-cycle festivals. Researchers were often labelled according to the

things they studied: they were food researchers, costume researchers, or custom researchers.

We find similar weaknesses for coupling in other disciplines, as in anthropology's classic combinations of "family and kinship", "ecology and economy", "ritual and religion".

The point here is that there is little room for topics which lie in the seams or the vacuums between the established categories. They tend to remain unseen.

Basic Concepts and Key Symbols

There is a similar controlling mechanism in the basic concepts used to define the field of study and the choice of problems. They become an important part of the canon of ethnology. For the first generation of ethnologists the three concepts of folk culture, tradition, and custom were a self-evident foundation. These were terms that needed no definition, and they were in turn used to define what ethnology should study, ignoring everything else or leaving it for other disciplines. Ulla Brück (1983) has shown how the concept of custom has been used to define certain forms of social behaviour. We find the same selection process in the concept of tradition - not every form of action showing continuity was labelled tradition and thus defined as a legitimate object for ethnological study.

It was the customs and traditions of folk culture which were to be studied, yet these concepts gradually became more problematic. A science which is born as a rescue operation must undergo a painful metamorphosis when the things to be rescued are no longer rescuable. Ethnologists faced the choice of either regarding the collecting

work as complete and turning the archive into a reliquary, or rejuvenating the collecting field. There have been many attempts to solve this problem. One way is to bring forward the time for "the extinction of traditional folk culture"; this tendency to maintain the role of ethnologists as a rescue squad leads to an unfortunate devolutionist perspective, where change is seen as cultural disintegration. The portrayal of ethnology as a rescue operation was also a rhetoric meant for the ears of the fund-granting bodies: the ethnologists were always working at the eleventh hour.

Another way to broaden the field was to try to expand the traditional framework offered by the fundamental concept of folk culture. Researchers tentatively explored the towns in search of "urban traditions", or started studying "working-class traditions", and even began cautiously talking of "contemporary traditions". Although these were all attempts to expand the field of study away from the foundation of the peasantry, the same matrix was used to incorporate new fields. The first studies of working-class culture began with agricultural labourers, who were closest to the traditional peasant culture, and some craft occupations with a clear set of customs and traditions.' In the same way, the appending of the term "tradition" determined what types of patterns of action were of interest to an ethnologist in the shape of "contemporary traditions" or "urban traditions".

In the end these extensions and adaptations could go no further. We may speak of a gradual undermining of concepts like folk culture, tradition, or custom, until they collapsed and became virtually unusable for new generations of ethnologists in the 1960s.

In Search of the Little Community

In the wake of this conceptual collapse, we can then follow the way alternative organizational principles and concepts were developed in European ethnology.

In the 1950s Robert Redfield had visited Sweden, charismatically pleading for the study of "the little community". Inspired by him, numerous ethnologists went out in quest of this microcosm. One result was a new interest in island communities with their (illusory) isolation and demarcation from the outside world. This produced several model studies of islands, where the little community was turned into an ethnological laboratory, where general forms of cultural change could be studied with particular clarity.^o

In the 1960s this interest in local communities grew in strength to become a dominant mode of thought. We who received our education then learned to see Sweden in terms of local communities. If we look at the choice of essay and dissertation topics in this period, we see the emergence of views of which communities were more community-like than others. This created a new selection principle, which was influenced in large measure by contemporary anthropological theory - both the functionalist and the interactionist variety.

This interest focused on the periphery of society rather than the mainstream. It is in this light that we should see the great interest in fishing hamlets; for many of us they represented the perfect cultural form of the little community - isolated, homogeneous, well-integrated, self-sufficient, and so on. (On closer examination, these coastal communities revealed a different reality.) The disproportional number of studies of such marginal settings was a quest for commun-

ities that were as "exotic" or "anthropological" as possible. With this search profile, for instance, the study of working-class settings was chiefly concentrated on small factory towns.

My own doctoral dissertation (Löfgren 1977) was a result of this interest in little communities. I did my fieldwork in a little fishing hamlet on the west coast, and my search profile was influenced by the fixed framework of the countless functionalist monographs I had read. A local study like this had to begin with "The Setting", possibly followed by a chapter on "The Historical Background". The author would then treat "Ecology and Economy", "Social Organization", "Enculturation", "Ritual Life and World View", and so on. This classical model for dividing up a community and then putting the pieces together again in a book exerted an obvious control over the way fieldwork was carried out. I was programmed to look for particular institutions and social forms. "Kinship systems", "household structure", "authority patterns", and "resource management" were the kind of terms I had ringing in my ears. Luckily, reality out there sometimes protested against being squeezed into this mould. When I reread the interviews and texts I can now see many connections and phenomena which were hard to capture with the analytical tools that I had with me then.

The interest in local communities also aspired to let the little community reflect society at large. The English anthropologist Ronald Frankenberg's classic study *Communities in Britain* (1966) was based on this idea, where a necklace of community types, from the little agricultural village to the city suburb, was threaded together to illustrate English society. The macrocosm became

the sum of a number of microcosms. Many of us in Sweden were influenced by this more model, and it was in this spirit that John

Granlund (1967) developed the idea of a number of ethnological research stations, where cultural and social change was to be investigated at regular intervals - taking Sweden's temperature.

Digging for Subcultures

The interest in local communities was to dominate ethnological research and collecting in the 1960s and part of the 1970s, but by the end of the 1970s it had lost its leading position to the concept of subculture. Interactionist theory had already directed interest towards cultural scenes and social interplay; for the fieldworking ethnologist this was "where the action was". This approach also brought the concept of cultural communication as a crucial selection principle. Certain phenomena and relations were found more "communicative" than others, and thus more interesting research topics.⁵

The interest in subculture grew out of this interest in interaction and communication, but also from an interest in breaking down stereotypes of Sweden as homogeneous (or local communities as well-integrated). This concept was used to capture other social units and cultural systems than the local study, but here too the natural result was that some groups and milieux were considered more "subcultural" than others: teenagers, children, women, immigrants. (Middle-aged, middle-class men were consequently the least subcultural category that could be imagined.)

The study of subculture began in an interactionist tradition but went on to follow

a semiotic path: from roles and scenes to codes and messages. It began to focus on the expressive: style, taste, codes, identity markers, and the like.

Another line of development with a stronger historical bent linked class with subcultures, but here too we can see a drive to find class cultures with clear contours. Of primary interest were some forms of bourgeoisie and working class, while the diffuse strata in between were not so attractive, partly because they lacked a distinct profile, and perhaps partly because they represented a life that was too close to everyday ethnological reality. Also important was the interest in cultural hegemony, which sent ethnologists in search of clearly dominating or dominated groups.

In the 1980s, partly as a reaction to the subculture perspective, there has arisen a longing for generalizations on a different level: the national. This ethnological interest in Swedish mentality and identity also reflected a public interest. Sweden's rapid change to a multiethnic immigrant state helped to problematize Swedish ways of living and thinking.

The Quest for the Everyday

In the 1980s the concept of the everyday became an important guiding factor in some historical and sociological research (cf. Gullestad 1989). When this became an element in our ethnological rhetoric, it was a way of distinguishing ourselves from other disciplines. Where earlier generations had studied "traditions" or "the folk": ethnologists now spoke of "everyday life", "ordinary people", "the little person". Such diffuse concepts functioned as landmarks, or what Billy Ehn has called "the knowing

winks" of ethnology. They existed to show that the ethnological interest in society was not concerned with the institutions and structures favoured by macrosociologists or historians. In reality the concept was not about the contrast between everyday and non-everyday, but a way of indicating a research policy interested in overlooked groups, activities, and spheres, often with a qualitative approach. The problem was, however, that some activities and people could easily be seen as more everyday than others. Women, for example, were sometimes presented as closer to everyday reality than men. Another danger of this perspective was that everyday life was too easily seen as something "down there", in a way that seldom reflected over the problem of what the opposite of the everyday was supposed to be. It was forgotten that the everyday existed not only in pensioners' flats but also in the corridors of power, that there was an everyday praxis not only in immigrant suburbs but also in scholarly research projects. Ethnologists who sometimes said that they wanted to "get out into everyday reality" missed the point that what is interesting about the everyday is that it is ever-present.

We can continue the list of research trends and ethnological modes of thought up to the post-modernist breakthrough and the metaphor of culture as "text", inscribed in the body. This borrowing, chiefly from literary criticism, has created a new selective focus of interest: a specific way of thinking culture.

What is a Historical Perspective?

In pace with the shifts in ethnological isms, the concept of culture has been subjected to debate and redefinition. Turning to the

other element in cultural history, the historical perspective, it is interesting that this has been the subject of relatively little critical reflection and deconstruction (not even among historians in Scandinavia). In ethnology the use of a historical perspective has greatly changed in form and meaning. In the classic ethnography of the peasantry, research was guided by the dichotomy of traditional-modern, past-present. The aim was often to move back in time to the classic, stable peasant culture, which represented a timeless world. Opinions could differ about how far back in time this quest should go: the start of this century or to the sixteenth century. As I have pointed out, there was a fairly fixed devolutionist narrative structure, often including some critique of civilization. The past represented continuity, integration, and harmony; the present stood for dissolution, rootlessness, and fragmentation.

A longer temporal perspective was applied in diffusionist studies of individual elements, where the aim was to find original forms and channels of dispersion, but this historical perspective was temporal - it was ahistorical in the sense that the researcher was usually not interested in relating the studied cultural element to the various social contexts through which it passed. The focus was on the migration of the custom or the artefact through time, not on the transformation of society.

Then came a time when ethnologists, fired with a functionalist enthusiasm for fieldwork, reacted with suspicion and lack of interest towards historical reconstruction, but they were gradually to realize that the link between present-day and historical perspectives was in fact an asset to ethnology.

This insight led to a heightened consciousness, not about the justification for the historical perspective, nor about its length, but about its purpose. How was it to be used as an analytical tool? In cultural history there are such widely differing research strategies that the concept of "historical perspective" is in grave need of deconstruction. Let me point out some very different ways of using a historical dimension as an analytical technique:

In the first place we have the selection of an *epoch* with a beginning and an end as the framework for a study. This concept has scarcely been critically examined. We must examine the ways in which a period is demarcated with the aid of arguments about continuity and discontinuity, how the creation of an epoch is a way of carving out a culture in time (cf. Löfgren 1988a).

Secondly, there is the search for *origins*, which is basically a desire to go as far back in time as possible, to find the earliest evidence.

A third way could be called that of *structural history*. Synchronic sections at strategically selected points in time are used to analyse change - a method which can hardly be considered unproblematic.

The fourth is *contrapuntal*, with a historical "then" used primarily as a contrast to a "now" which the researcher wishes to problematize. This is an analytical technique which has been frequently used by both ethnologists and sociologists in the last two decades. The aim is to use the past to create an exotic otherness as a reference point, just as anthropologists can use other places.

The fifth could be called a *formative* perspective, which is not a matter of the classic search for origins in a diffusionist or evol-

utionist spirit, but an interest in examining how a phenomenon is established, institutionalized, cemented, routinized, and eroded. The interest in the actual formative stage lies in the fact that it is here that the contours of a new phenomenon are seen distinctly, before they have been trivialized, mystified, or naturalized into a matter of course. An example of this type of historical perspective can be found in the anthropologist Sidney Mintz's study of the history of sugar production and consumption (1985).

The World Seen Through the Researcher's Spectacles

In his study of the Irish community of Ballymenone, Henry Glassie has shown how differently the world is organized by the local people and the researchers studying them:

Ballymenone's way to create order is not to demarcate a whole, then cut it into parts. Still rationalistic after all these years, we worry about consistency, exceptions, and contradictions, defining things by their surfaces and boundaries, and become happy when everything fits. We name culture a system, describe it as a closed entity, breaking it down into parts that interlock into a unit like a machine. They describe culture in epiphanies, by spotlighting key occurrences suffused with cultural essence. They craft centers with care, leave edges ragged, letting the whole take care of itself. We consider history a unit broken into periods, and geography a unit broken into regions. A year is an entity to be sliced ever more minutely into seasons, weeks, seconds. For them history is a cluster of powerful events, space is a collection of landmarks, the year is a set of Set Times marked by big meals and coming together (Glassie 1982:354).

My discussion has been about the way differing scientific praxis has guided ethno

logical research and collecting, from the early, preoccupation with atomization, an obsession reinforced by the need to chop up collected material into cultural items which can fit the boxes. The next generation was out to create a whole, to describe culture as a well-integrated system, making cultures out of communities, spinning a web of interconnections and dependencies between sets of institutions.

My aim with these examples has been to show that there is no simple, cumulative, or systematic strategy for collecting and research. In pointing out what previous generations of scholars have neglected, I am not out to deride. All research is based on economy and selection, and it is important that the searchlight is shifted from generation to generation. What I want to bring out is that key concepts like "custom", "little community", "subculture", or "the everyday" often assume a greater guiding force than was originally intended - they have a tendency to colonize the working praxis of research. Cultural phenomena other than local communities can be studied in terms of community studies, social life can be seen as a collection of subcultures or as a bundle of cultural texts.

We must accept the fact that research, collecting, and analysis have always been steered, consciously or unconsciously, by shifts in the social climate, by changing scientific outlooks, or by market mechanisms. At any point in time there is a view of what is conceivable, researchable, and marketable.

It is therefore utopian to believe that the archives of tradition can or should free themselves from this control. The examples we see of how people have tried to collect or research for the future should deter us.

We must accept that it is our interests today which control, and that collecting must always be linked to current research interests. We cannot foresee how future generations will react to what we have collected, whether they are fascinated by what we kept or exasperated at what we omitted. This does not mean that we should ignore tomorrow's needs in today's collecting, but we must beware of thinking that we have a mandate from future generations of researchers. Experience sometimes shows that even the dustiest old material can be given new life in an astonishing way.

This recycling of old ethnological material is interesting: how is traditional material redefined with the aid of new theoretical perspectives? The study of subcultures often went hand in hand with an interest in the present, but the interest in cultural codes and forms of communication, not least through structuralist and semiotic theory, brought a renewed interest in old archival material. A new generation returned to the archives to look at records which the functionalist generation had dismissed as unusable because they lacked the social context. With new theoretical spectacles it was possible to see new things, especially in folkloristic textual analysis.⁶

In the same way, I believe that the ethnologists of the 1960s who advocated fieldwork in the present actually revitalized the use of the archives, in that several generations of researchers have turned to history and the archival sources with the fresh, inquisitive outlook of the fieldworker.

Another important link between theoretical perspectives and archival material is found in the interest this decade's scholars have shown in the production of ethnography and the various ways in which culture

can be written. This approach has also made it possible to look again at the interviews preserved in the archives, seeing them as cultural texts with their own genre-based rules, stylistic conventions, and narrative structures.

Folklorists previously expended great energy on organizing their material in genres, a system which in turn helped to guide their collecting and research. We can learn from both the advantages and the dangers of this genre analysis. What is new is that other forms of material than folkloristic narrative traditions are now being examined from the perspective of genre. We can ask what folk taxonomies have been created, what genres occur in answers to questionnaires or in interviews. Let me take just one example from the production of life histories - a theme which has attracted the attention of many scholars in recent years.'

We need to explore further the ways in which genres or subgenres of life histories are created in different settings and periods. First of all, we may look at the general structure of a narrated life. How is life presented - as a journey or a standstill, as the climb up a career ladder, as a success story or a failure, as a comedy or a tragedy? Here we find genres like "From Rags to Riches" or "A Victim of Circumstances". Furthermore, we have to look at the extent to which the main character is presented or even present in the narrative: My life as a winner, as a martyr, as an active actor, or as manipulated subject.

Secondly, there is the temporalization of a life history into epochs, stages, transitions: the good and bad periods, the zenith, the turning-points, and so on.

These two aspects are often joined in the

presentation of a *life script* or scenario: what has been the purpose or result of my life, its main themes? Here you may find the basic structure focusing on missed chances, blocked alternatives, or lucky strokes. "If only I had . . ." or "My life really changed when... " The life script may also be linked to underlying didactic or moral messages in the narrative: how the past is put into a dialogue with the present.

These general principles of organizing a life history are of course heavily influenced by the age of the narrator (are we listening to the closing of the accounts or just a half-way report on life?) but they are also shaped by his or her social position. The same goes for the ways in which memories are singled out or revised in order to become symbolic statements of important themes or happenings in life. Here we are dealing with processes of crystallization and condensation, the transformation of events into guiding lights or milestones in a life journey.

The Need for Back Doors

In the discussion of how research is controlled and guided, we often speak of the significance of theoretical spectacles, how the changing isms - functionalism, structuralism, Marxism, post-modernism - have influenced the interests and perspectives of researchers. We less seldom scrutinize the everyday routines and matrices we use to plan and implement a study, a fieldwork programme, a questionnaire, a search through the archives. Here we often bear a heavier traditional inheritance than we realize, a continuity which can survive many changes of theoretical paradigm. As researchers we often learn how to sort out, forget,

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or ignore certain processes, relations, and phenomena. How do we capture drab everyday reality, the transient, the informal, the self-evident? There are areas of social life that are so grey and drab, elusive and shapeless, that they tend to escape our searching eye. Let me take just a few examples: Ethnologists have produced a fund of knowledge about village organization but little about contacts between neighbours. They have written reams about kin and family relations but little about the history of friendship or its changing social and cultural organization. Rites marking passages through time have been studied much more systematically than passage rites in the spatial dimension.

A way to avoid getting stuck in overused ruts would be to deroutinize our working methods at regular intervals. An important first step here would be to pause before rushing into the research task. The most interesting time is often just before research or collecting begins. So much can happen before we notice a thing. The phase when we try to identify and demarcate the object of our study, to formulate questions and to look for material, is often of decisive importance for the rest of the study. Once the course is set, it can be difficult to change direction.

There are too many magnificent main entrances to the study of culture: too many asphalt roads into a research field where we follow fairly unconscious routines for how a project is to be packaged, organized, and carried out. What I am urging is a greater use of back doors and tradesmen's entrances: we must test more ways into (and out of) a field of study. A strategy like this requires a rather anarchistic initial phase, where other questions, other material, other

connections are constantly tested. The routinization of the work will come soon enough. The choice of entrance concerns not just the theoretical spectacles we put on, but just as much the kinds of material and working tools we choose, and how we weigh up different approaches.

A prerequisite for a constructive division of labour between different cultural sciences is that we choose different entrances. During a period when I was working on a study of everyday Swedish life between the wars, I read of how some American archaeologists were excavating the twentieth century. They were conducting a traditional archaeological investigation of some Midwest farms which had been buried under the sandstorms of the thirties. The data they obtained through their painstaking scrutiny of refuse tips, houses, and outbuildings naturally differed from mine, but what fascinated me was their perspective. Just imagine for the sake of experiment that an ethnologist, an archaeologist, and a historian tackled the same setting from the 1930s, not just as three different disciplines but as three different crafts: the archaeologist digging up the past, slowly unearthing artefact after artefact, the historian leafing through dusty listings and government reports, the ethnologist letting the tape-recorder spin in the making of oral history, with local people laboriously trying to recreate their childhood of the thirties.

This picture conjures up the way scholars archaeologize, historicize, or ethnologize the world. This colonization constantly changes form and content from generation to generation. Boundaries are moved, traditional core areas are abandoned, new soil is cultivated, and sometimes people return to old fields with new tools.

Orvar Löfgren Docent, fil. dr.,
Dept. of European Ethnology,
Finnngatan 8,
S-223 62 Lund

Translation: Alan Crozier

Notes

- 1 Cf. the discussion in Löfgren 1987:8ff. and Löfgren 1989a.
- 2 If we want to understand how the academic discipline of history colonized various parts of history, it is particularly important to examine the relation of the subject to the making of archives: what exactly ends up in the archives, and which archives are most used by historians? In this respect we see interesting national differences.
- 3 See, for example Lindblom 1943-44, the first large ethnological study of working-class culture, where it was divided up according to the pattern used for peasant society, with chapters on house and home, clothes and food, everyday and special occasions.
- 4 Good examples of this genre are the two island studies by Gräslund (1952) and Granlund (1958). They both deal with the classic functionalist topic of "the disintegrating little community" and for several decades they were required reading for ethnology students as model studies of the breakdown of traditional peasant social organization and community spirit.
- 5 A good example of this focus is found in the papers from the 21st Conference on Nordic Ethnology in 1978 (published in *Ethnologia Scandinavica* 1979). The topics chosen were: symbolic language and mass communication, verbal transmission in enculturation, ritual as communication, dwellings as communication, symbolic values in clothing, the use and consumption of things, social and cultural integration in residential areas, the concept of identity in connection with out-groups and minorities. There is a strong linking of communication and identity here, as in many other studies from this period.
- 6 Jonas Frykman (1988) has analysed this recycling in his study of the way the responses to one questionnaire have been used in different ways by different generations of ethnologists.
- 7 Some examples of this interest are the discussion of life histories as a genre and the produc-

tion of ethnological descriptions (cf. Ehn & Klein 1989).

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