

Construction and Consumption of the Past From "Montaillou" to "The Name of the Rose"

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Christiansen, Palle Ove 1988: Construction and Consumption of the Past. From "Montaillou" to "The Name of the Rose". *Ethnologia Europaea* XVIII: 5-24

This paper deals with problems in the type of cultural research that is often called the (new) "little history". One of the things that characterizes this genre is an interest in the close description of ordinary people's everyday life and conceptual world, and dissociation from the so-called "grand" old theories.

The article takes its point of departure in central parts of the work of Emmanuel Le Roy Ladurie, but works by Carlo Ginzburg and Natalie Zemon Davis are also considered. It is argued that there is not necessarily any conflict between theoretical structural studies and cultural studies. This problem is discussed in the light of Umberto Eco's treatment of theoretical issues in relation to empirical forms in the novel *The Name of the Rose*. Here the two dimensions are "united" in the course of practice by the very fact that the author separates them conceptually. He makes them refer to two different levels, and looks at one by means of the other.

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There is much to suggest that the intra-disciplinary sectoralization that many old university subjects have experienced in the last half-generation is not only connected with greater thematic specialization, but is also directly related to the ever-clearer theoretical and methodological *interdisciplinary* tendencies that have become part of our lives. After a period of – often extremely abstract – debates and work on large, often world-encompassing syntheses (systems and transitions) we have in recent years seen a new interdisciplinary interest in so-called "little history", where people's life, culture and forms of expression – including feelings – are in focus. Depending on disciplinary orientation, this type of study is called new cultural history, research on mentalities or the history of consciousness.

One book that is often mentioned in this context is Emmanuel Le Roy Ladurie's *Montaillou* from 1975, which has been translated into many languages and gained enormous currency outside the circle of specialists. Ladurie presents here quite fantastic material about the internal relations, daily social con-

tacts and cast of thought of 14th-century French villagers. Despite different interests and disciplinary traditions, there is a fairly clear connection between *Montaillou* and the American historian Natalie Zemon Davis's book about the disappearing act of a peasant in 16th-century France (Davis 1985) and the Italian Carlo Ginzburg's treatise on the worldview of a miller around 1600 (Ginzburg 1979). One can say that these studies are more or less becoming classic frames of reference for a new type of research on the conceptual universe of so-called ordinary people in the past. Among general readers they have very understandably been consumed as easy-to-read historical and cultural studies, and have, along with the resurgence of biography, helped to create a wave of reflective identification with the past. In specialist circles the genre is often cited as an example of a new form of close or "thick" social history.

Yet it is strange that most international reviewers and commentators have in their enthusiasm primarily focused on the often exotic material from the past that is presented in

books like these, whereas interest in analysing what it actually is that these researchers are doing, in the way they represent the past, has been very limited. With *Montaillou* as my point of departure, I will focus on how the new phenomena of interest are explained, how they are communicated and consumed by the reader in a quite particular (new) way, and discuss whether a showdown with structural history is really necessary in principle – as is often claimed – to be able to write about culture and everyday life.

Despite the fact that *Montaillou* is no longer hot off the presses, it still deserves a more detailed presentation in this perspective. At the same time Ladurie's book touches on what is by now an old, but still unsolved problem: what could be called the sociology of place. This sounds easy, but is hard. Up until now all attempts to explain "local milieu" have been subjected to such merciless theoretical criticism that most researchers have turned their interest in other directions. But this has meant that the problem has been left unsolved!

The Locality

Ladurie's local environment is a village of the past at the foot of the Pyrenees, 1300 metres above sea level, which at the beginning of the 14th century consisted of 200-250 people. Here Bishop Jacques Fournier made persistent efforts to wipe out Christian heresies. Yet this was nothing new. The persecution had gone on for over a hundred years, and the whole adult population of the village had as early as 1308 been arrested by the Inquisition. The later interrogations also revealed how people's whole way of life, topics of conversation and modes of expression, despite fear and uncertainty, were stamped by Catholic/heretical issues and the direct and indirect presence of the Inquisition.

The book is about some of the things these people told the Inquisition. The statements were translated from Occitanian into Latin, written down, and preserved.

Several people have used this material, and in 1965 Jean Duvernoy published the Inquisition cases in three volumes. The indices to this source publication must have been indispens-

able for Ladurie's way of thematicizing the huge amounts of scattered statements. His book juxtaposes the various passages in the records about – for example – shepherds, sheep routes, marital relations, love, friendships, communal eating, social contacts, emotions, religion, magic, gossip etc. The reader apparently gets to know various households and their members so well that he is almost able to identify the character traits of individuals.

The fantastic thing about this material – conveyed by the source edition and Ladurie's sifting of it – is that it seems to be possible to get as close to certain people in the middle ages, their thoughts and life in their village, as is the case here.

At first this closing of a 700-year gap has an almost intoxicating effect. Yet for many it has been replaced through reading by a longing to know the actual contexts of the many quotations in the book from Fournier's interrogations. Ladurie only gives us, cutting across the scribe's records, selected fragments of answers, whose function is usually to illuminate other aspects of life than what the inquisitor was then looking for. This is of course quite legitimate in terms of the author's intentions. As a professional researcher Ladurie on the one hand controls his material thematically and empirically with an iron hand. But on the other hand the material (what it tells, compared with what it says nothing about) has in fact also controlled *him*: yet not so much because he concentrates so fixedly on his primary material as because in reality he controls it loosely in terms of the overall concept. This brings us to the issue of how the author juxtaposes themes and passages. For the point is that this is not done in a void, but always on the basis of something other than the material itself. If we are not attentive to this other which controls the new organization of the material – and which is precisely what makes *Montaillou* into *Ladurie's* book – then we in fact only see part of his work. However, it is rare for the *Annales* historians to say anything explicit about such circumstances. It is all the more remarkable that specialized readers have to such a great extent disregarded this aspect of the treatise, when Ladurie in fact *in this case* clearly draws atten-

tion to his theoretical/methodological assumptions.

The “Golden Rule”

At several points throughout the book Ladorie mentions that he has chosen to mould his presentation around what he calls the village monograph in a peasant society. According to the author this form of presentation has become a golden rule within this topic. Here, it looks as though he is in particular thinking of Redfield's study of the Mexican village of Tepoztlán from 1930 and Wylie's book on the Southern French village pseudonymically called “Vaucluse” from 1957 (Redfield 1968; Wylie 1964). Even if the intentions of the two studies were different, they are both – like *Montaillou* – characterized by a very small degree of analytical elaboration. Wylie was a Professor of French Civilization, and Redfield one of the American pioneers who tried to graft ethnographical small-scale studies in kinship societies on to community studies in so-called complex societies. These local community studies were developed methodologically until around 1960 (especially in the 1950s) and in their best form became material-packed, but generally comprehensible “thick descriptions” of everyday human life in small localities. The theoretical ideas behind the studies followed ever more clearly the multi-faceted development and establishment of functionalism and structural functionalism in the USA, and in particular in England.

Functionalism tries to see society as an integrated system whose components enter into a functional relationship with one another and thus with the totality. Ideally, the components relating to society as a whole are identical to the so-called institutions (e.g. marriage, legal norms or religion) – that is, the *milieu social*, as Durkheim (mentioned in Ladorie's bibliography) put it. Seen in relation to the institutions, regarded as wholes, however, the individuals themselves become parts over whom society, through its institutions, exercises power. Through this (concealed) social structure, people are kept in their place in society and within the socially/culturally created in-

stitutions, which in return – if integration and continuity are achieved, that is – satisfy the social and cultural requirements (needs) of the individuals. Through a searching, non-ethnocentric method of working, moving from the part to the whole, the researcher pieces together the holistic picture of life – often in a small locality or some other delimited environment. Let us look at *Montaillou* once more in terms of these elementary concepts from the functionalist monograph.

Ladorie in fact places special emphasis on *Montaillou* as a self-contained and rather isolated village. Peasants, and especially shepherds, though, often range far, and the village is also visited by outsiders – yet there is still a kind of wholeness. *Within* this local community the author emphasizes again and again the central institution, the *domus*. Even though there appears to be a myriad of strong kinship ties in the statements of the villagers, Ladorie focuses on the significance of the “house”. This is where people belong, here they are socialized, this is what the members want to protect and to be loyal to. The *domus* is more than the sum of its perishable *individuals*. He emphasizes that the *domuses* are the basic *cells*, which multiplied a few dozen times go to make up the “village”.

The other important institution is the *cabane* of the shepherds and goatherds. A *cabane* is the herdsmen's group, eating and living collectivity in the mountains. Even though the herdsmen could take certain liberties in which they could not indulge in the village of *Montaillou*, the *cabanes* were still, through the economic relations with the owners of the sheep – the peasants – and through the herdsmen's relations with their *domus* and kin, always (despite the often great physical distance) part of the social organization of *Montaillou*.

Here we are clearly presented with the village as an integrated system of continuous reproduction and production, where *domus* and *cabane* are the primary institutions. They are the socially/culturally developed components (cells) compared with the village as a totality. And for the villagers these institutions are the normative wholes in relation to themselves as subjects. It is the law of the household and the

collectivity that governs acceptable social behaviour. True, the institutions have been created by people in their relations with one another, but at second hand they also exert pressure (collective expectations) on the individuals.

All this fits the *functionalist*, locality-oriented peasant community monograph like a glove; but it is far from certain that the author was thinking of this theoretical model construct when he wrote his book. But it had at any rate been indirectly incorporated via Ladorie's sources of inspiration for what he may have hoped (?) was simply a thematic ordering of the material.

The conjecture that in *Montaillou* Ladorie is *unconsciously* becoming a structural functionalist – although more in the spirit of Malinowski than that of Radcliffe-Brown – is strengthened by the fact that he does not pursue this organicist train of thought very systematically. For all the inhabitants' many interesting utterances on beliefs and their connection with their actions are strangely enough not directly and explanatorily related to the other institutions. Religion floats oddly over aspects like ecology, production, the household, communal village life, etc. *while at the same time* the quoted utterances show that the villagers cannot have viewed their existence piecemeal in this way. The integrated overall mode of thought precisely invited the functionalist anthropologist to find the relationship – perhaps hidden under the surface – between “mental”, meaningful and material institutions.

Ladorie emphasizes the way heresy formed a bond between people, and how *Montaillou*, compared with lowland villages, was to a very great extent a kind of remote heretical sanctuary. At the same time he says that *Montaillou* was also split between believers (i.e. heretics) and unbelievers (more orthodox Catholics). This graduated relationship with a strongly-controlling (religious) norm is a classic, delicate problem. But when heresy is presented as such a dominant force underlying everything, one's interest is forced towards the non-heretics. Both categories appear to have lived side by side! How could this work? The believers and unbelievers appear to have shared a common

culture and common activities apart from religion. But given the prominent position held by religion as a provider of norms for countless tasks, it hardly seems likely that this difference could be maintained without precisely becoming evident in a (different) relationship with other aspects of everyday activity.

To put it in another way: if belief – which was evidently so important that people died for it – split the village and the parish, this split is difficult to reconcile with the fact that life is otherwise depicted as homogenous. From a functionalist point of view one could wish as much, or more, justification ask whether *Montaillou* and its environs did not represent two forms of social organization, which may well have included certain common institutions (from pre-heretical times) but which defined their content differently and thus also lived out different practices. Ladorie often mentions conflicts between different houses and kinship groups. But, oddly, he does not pursue these differences. We read much of the heretics, but one thirsts after closer knowledge of the life of their opposites, the non-heretics. Only then can we grasp the village in its *entirety*; but it is true that it is a whole consisting of opposites that we are now moving towards.

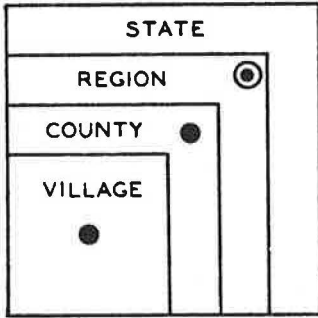
However, let us stop here before we over-interpret an already twice-interpreted text. That the book *Montaillou* is closely related to an early, and often-criticized, functionalist tradition, is nevertheless clear.¹

The foreign environment

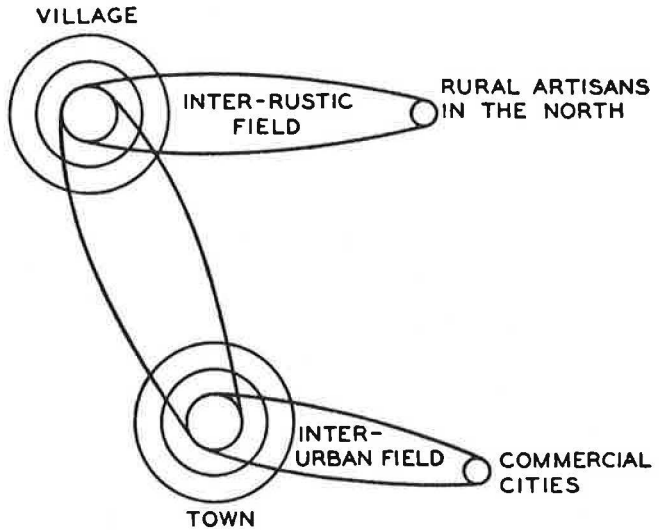
Besides many who have expressed enthusiasm over Ladorie's way of translating the world that emerges from Fournier's records, some have also viewed with concern the rather heavy-handed, direct way he interprets the interviews and translates the scribe's already once-translated Latin text.

In terms of research methodology, one can just as reasonably wonder about Ladorie's way of reading his sources of inspiration and comparative references. Redfield's earlier books are mentioned (and more could be said here about what Ladorie overlooks in the latter's interest in the relation between town and

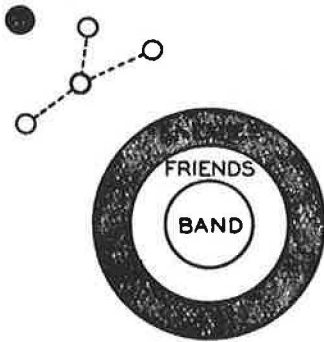
**LOS TUXTLAS
(Starr)**



SCANIA (Hanssen)



SIRIONO



NUER (E-P)



In a series of lectures delivered at the University of Uppsala, Sweden, in 1953 the late Robert Redfield raised the question: How are we able to describe a locality as a little community – as something in itself – when at the same time this community is part of a much larger and more complicated system?

With the purpose of drawing attention to our own as well as the villagers' thinking of community/society he compiled four illustrations: Allan Holmberg's drawing from the Siriono indians in Bolivia, E. Evans-Pritchard's sketch of the Nuer people of Sudan, Betty Starr's diagram from the region of Los Tuxtlas in Mexico and Börje Hanssen's figure showing levels and interaction in Scania, Sweden. The last two illustrations are intended to show differentiations *and* relationships/dependency (From Redfield 1967 a, 117).

Today Redfield's perceptions can appear rather mechanical. Anyhow, he managed to put his finger on the problem how we at the same time conceive and describe the indivisibility and the internal differentiation of a region.

In *Montaillou* Ladurie presents to us the village as a *whole*. This was what Redfield called the *first* chapter of his book. The later parts were more advanced.

country); but it is remarkable that Ladorie does not see in the books (also mentioned) *The Little Community/Peasant Society and Culture* from the 1950s that Redfield in fact in one of the chapters completely explodes the idea of the functional description of the peasant society, which never exists in isolation.² He says that the peasant village, in its relations with people and institutions outside itself (manors, towns and cities) is perhaps so incomplete a system that it cannot well be described as a self-contained social structure (Redfield 1967a, 128; 1967b, 24). Redfield predicted that anthropologists would have to get out of the village and look at bigger regions instead. Yet this was hard as long as the method developed by functionalism reigned wellnigh supreme in anthropology.

Here we are confronted with a direct paradox in understanding Ladorie. He knows Redfield's books – both the earlier and later ones – but only uses the inspirations from the arguments in the (early) treatises, which were greatly revised and put in perspective later by the same author. This is all the more remarkable when one reflects that the relevant wishes and hopes expressed by Redfield in his later work (directed at the local community functionalists) have to a great extent been brilliantly fulfilled precisely by regional French historians like Braudel and his disciples – including Ladorie himself in his thesis on the Languedoc peasants. Logically, this is only comprehensible if Ladorie regards Redfield's later arguments as irrelevant. But when he elsewhere rightly pleads the merits of his colleagues, it is hard to believe that in this area he does not in principle share Redfield's later view of the study of peasant society.

When Ladorie took up Redfield for investigation he was apparently faced with the task of uniting two traditions, each fruitful in its field.³ Worked together by him, they could have led to a third, quite different result. Yet this did not happen, nor is it certain that he so reflected. But if he had read his sources in other disciplines more closely, he would at least have had the opportunity of doing so. At any rate it is odd to see that in *Montaillou* he did in fact to a very great extent put aside his own

Annales tradition of the 50s and 60s to write instead within a now rather antiquated anthropological school. However, interdisciplinary experience from recent years shows that the use of neighbouring disciplines' concepts (themselves strongly criticized) can easily force admiration in one's own discipline (among those who are not familiar with the criticism). This phenomenon is probably a part of any incipient interdisciplinary effort (Christiansen 1984).

Yet I believe that it is both wrong, and too easy, to accuse Ladorie of opportunism, even though he sometimes appears rather nonchalant. The phenomenon, or the disciplinary misunderstanding, apparently also has something to do with the way one specialized discipline reads the other's books.

In Ladorie's otherwise everyday-style and rather normatively-written work there are here and there small specialized debates or comparisons. These mostly concern how things "really were" or are, not modes of explanation or other theoretical problems. In these discussions the author operates within his own disciplinary universe, and many of the references are understandably enough to other historians. Besides the anthropologists already mentioned, though, there are also in this context people as different as Radcliffe-Brown, Lévi-Strauss, Leach, Pitt-Rivers, Goody, Wolf, Godelier, Sahlin and Bourdieu. But it never comes to real specialized debate with these scholars. Ladorie allows himself for the most part to be inspired by this or that (often small) feature in their books which has turned his thoughts to similar or opposite factors in his own material. This is fine in itself, but none of these anthropologists have written their books for the sake of the small details. Authors who have wished to do something different or something more must find it disheartening that Ladorie does not delve deeper. For most of these people could in fact indirectly help Ladorie to avoid some of the most classic problems of the village monograph. But Ladorie apparently reads them not out of interest in what they do in their way of reconstructing a possible "reality", but rather for the concrete details they are writing about. This is completely in accordance

with the references to the sociologists Tönnies, Chayanov and Shanin (as well as Marx). It is precisely their more explicitly proposed theories about *factual situations* that are taken up to a far greater extent by Ladurie.

But when one considers how difficult it is for outsiders really to get to grips with the French historians' own well-concealed concepts, it is perhaps not so remarkable that Ladurie has difficulties in seeing what some anthropologists' books are (also) about.

Regional variation

For readers not well up in French medieval history and regional variation it may come as something of a shock to realize that Ladurie has represented Montaillou ahistorically. Those who think they can benefit from knowing M. Bloch's and G. Duby's classics on French feudalism can think again. In the first chapter Ladurie devotes a few pages to an account of the power that he later shows to have been of little significance in the village: the feudal overlord, the Comte de Foix, the land steward/bailiff, the diocesan council, the Crown and the Inquisition. Only the last of these is made much of, although we never really experience the clash between the Church and the village. The others "are there", but become only distant stage props. This way Ladurie avoids completely telling the reader about French feudal *society*, and – more fatefully – he avoids telling how Montaillou and its surroundings were a different variant of medieval society than the one Bloch wrote about in *La Société Féodale* (1939-40). Some elaboration of this difference and connection would in fact have been far more clarificatory than the vague references to Montaillou's marginal placing in relation to Foix, the ecclesiastical centres and the focuses of trade. It is as if Ladurie unfortunately, in the best village monograph style, more or less makes a virtue of representing Montaillou as an isolate.

He is undoubtedly right that the mountain village was politically marginal to the centres of power and that a different situation prevailed in other places, but one hears almost nothing about the differences. We get only spo-

radic information that Montaillou had no corvée, no serfs, no common-field agriculture, that there was as good as no hierarchization (although the conflicts were strong), that class boundaries were fairly fluid, and that nobility – if not combined with wealth – had little significance. Ladurie may be quite right about these things, but it is hard to grasp the contrasts with Montaillou of which he gives no account and which are precisely what makes the condition of the village studied into something more than exotic localism.

As Ladurie knows better than most, the peasants of Europe were not just peasants. Very roughly, one can distinguish between the following coeval forms: first the demesne/peasant tenure (arable farming) system best known as the classic rural feudalism of the core area between the Loire and the Rhine, but also existing in England, the German Kingdom, Poland and the northern regions of Spain and Portugal. Then there was the Mediterranean system (with denser populations) which could however also extend across the Alps. Here there were mixed forms of freehold and tenancy systems (with fixed crops, pasturage and cattle-breeding), but in both versions based on a fairly high production of goods for sale in the towns that had developed here. Finally there was a form of animal husbandry and marginal pasturage system along Atlantic coasts (and probably also in Eastern Central Europe) where the manorial production system had not spread or was only found on a small scale (cf. Norway, the northern Low Countries and to some extent West Jutland).

Montaillou and its surroundings were a variant of the middle type, with a mixture of freehold land and tenancy with certain regular crops and a high degree of transhumance with very stable supplies of goods and relatively well-developed money circulation. One is barking up the wrong tree if one believes, as a reader, that one can simply regard Montaillou as a good example of how peasant agriculture in general structured life in the medieval village in France or in Europe. Medieval society *could* so to speak be built up over the same structure as agricultural production, but was not necessarily so. It was not the practice of

agriculture in an ideal medieval version, or as a remote relic, that determined the general way of life in Montaignou; on the contrary it was the structure of property and production as it prevailed in agriculture in this region, and then special local circumstances (from the ecology to the Inquisition). Only against that background are we able to see how interesting Montaignou actually is.

This context is of course familiar to French historians and a few others; but Ladurie either thinks that all others also have such knowledge, or his model, the functional *community* study, has made it less relevant for him to consider such “external” social historical factors. This is why his in many respects fantastic account has difficulty in adding new dimensions to historical research. He grafts as it were on another branch than his own. Many would say that his interest in the vital life of the village, in people’s conceptual worlds, in the exotic (the peculiar) etc. is something new. These aspects are perhaps new as specific fields of interest, but Ladurie does not unite them in any integrated collection of the knowledge and analytical advances of his own colleagues’ acknowledged research. *Montaignou* is at any rate written as if its author wishes to put part of his own disciplinary tradition behind him in order to do something radically different. It is laudable to want to press on, but in this case the advances – and among these one must not count the old anthropological “golden rule” – have also had their costs.

What is science?

After having discussed some features of what Ladurie was looking around for in 1975 (but which he has later replaced with other things) it seems appropriate to give a brief account of what it was he turned away from with one fell stroke in *Montaignou*.

In 1966, when Ladurie published his justifiably highly-praised thesis on crisis and long-term change in material life among the peasants of Languedoc, he was clearly building on traditions within quantitative social history and demographic history in the *Annales* version. He had himself previously been greatly

occupied with the influence of the climate on production in the peasant society before the Revolution. Unlike so many other historians, he attempted to develop a holistic history which, besides explaining change, also aspired to deal with various aspects of life as integrated dimensions of a total society.⁴ The book attempted to show changes in the relationship between on the one hand biological, climatic and ecological phenomena and on the other crises of survival and social conflicts. Compared with so many of his contemporary colleagues Ladurie was here not just a kind of social statistician with the past as his field; he was much more, and it is probably this that has meant that the Languedoc book will undoubtedly for a long time to come remain as an example of a sound work, rich in perspectives, by a professional historian. Later, in fact, the book gained, for example in the American edition,⁵ from having some of the passages specifically concerned with economic history cut out – passages which on the other hand had been necessary at the time of writing in order to live up to the norms of Ladurie’s French milieu.

The thesis appeared during the years when a neopositivist wave was making itself felt – at the same time as other sectors of the intellectual world were rediscovering Marx! In the context of these new departures Ladurie abandoned the totalizing view that the thesis had also championed to concentrate on in-depth quantitative studies of more isolated factors, among which invariable causal relations were to be established.

The basic idea of this type of research is that *when* we have acquired the new, fundamental knowledge about price fluctuations, climatic changes, area productivity, population, mortality, marriage rates, fertility etc., we will then have created the real background for writing a new – and for the first time(!) – *scientific* history. Such notions of objective history were not new in French research circles, but were now given much stronger emphasis. To this end Ladurie and his colleagues wanted to borrow methods from what they regarded as the advanced social sciences: that is, from positivist sociology, economics and demographic theory.

In the years from the thesis more or less up to the appearance of *Montaillou*, Ladurie wrote several treatises on factual topics and a number of informative and summarizing articles within this new "incipient science".

Against the background of these works he published in 1973 – two years before *Montaillou* – a collection of articles (translated into English in 1979) which gained wide currency. This was a unique mixture of intelligent inventiveness, innovative enthusiasm and merciless criticism. Here Ladurie lashed not only his contemporary colleagues but also historians of the past, who were all measured with an unproblematized future yardstick for what good research should be. Ladurie himself had no doubt. The *Annales* tradition had been good, but the only things worth mentioning were the cases where the grand old men (including Lucien Febvre) had quantified. Economic history would (as a number of American historians were already doing) find its scientific content in "the new theories of economic growth" (take-off) to the advantage of the discipline's old unifying concepts like technological or industrial revolution. The last remains of the history of events and persons were doomed in favour of the "mathematical resurrection of a total past" in the form of historical demography. Techniques and theories from economics and demography were both borrowed to make history scientific. Ladurie directly asserted that "history that is not quantifiable has no claim to be scientific".⁶ This was a defence of a history that built on a widespread, but actually misconceived, view of the processes of natural science. The computer was made the problem-solver, and the work on concepts that Braudel had begun was now apparently a thing of the past.

It is an irony of fate that just a few years after this sally Ladurie would in quick succession write both a book about persons (*Montaillou*) and one about an event (*Carnival in Romans*).

There is nothing wrong with changing one's point of view as such; with thinking that counting has to be done (which it sometimes *has*); with being inspired by what neighbouring disciplines are doing; with enthusing over the fact

that one can, from endless rows of parish registers, through family reconstitution, piece together demographic patterns of the past; or – for that matter – with being a neo-positivist.

In my view, however, it is wrong when a specialist propagandizes authoritatively in favour of something without showing how the new approach can help to solve old problems, how it can gather up established results in the drive towards further horizons, and/or how the old interests and problems have become irrelevant in the new light. And Ladurie as well as many others did not make much of this.

This way one in fact confuses one's surroundings, and others are left, in a peculiarly academic tradition, to find their own bearings in a field that is officially represented as scientifically clear and precise, but in reality is made impenetrable. Those with the best nose for the way currents of thought are flowing become the "front line" – within the bounds of culture; the others "lag behind".

In addition, a man with Ladurie's authority inevitably provides an indirect impulse to much research. When a highly esteemed scholar expresses himself as strongly as he does, many will take him at his word alone. This is perhaps a little naive, yet understandable. Then, when they have launched their new "front-line" project, they find – as was the case with historical demography – that what was "certain knowledge", "the future", etc. just a few years ago is perhaps regarded today as a complete dead end.

It has been a source of much frustration that Ladurie has taken up a posture and pointed in one direction, then turned around and done something else himself. This has happened with almost every one of his books. I would like to emphasize that it is important that one is able to revise one's own opinions and seek out new fields of research. But the *way* Ladurie does so is, after *Languedoc*, not particularly convincing or fair to his students.

No one would deny him learning, intelligence, energy, a talent for combining problems in a new way and an eye for so-called angles. Yet at the same time he also appears to be a kind of scholarly freebooter, restlessly consuming the working methodologies of neighbouring

disciplines, and apparently demanding that his own consumers, the readers, should be enthusiastic about his ever-new ways of making conquests. One must admit that until now he has been quite successful. And when he presented *Montaillou*, even the majority of the specialist establishment had apparently forgotten what he had written in letters of fire just two years before.⁷

The novelty effect and the extremely fascinating material seem to have overshadowed the rather paradoxical features of the situation. Unlike before, Ladurie did not now say explicitly what was wrong with the old approach, that community studies should not be modelled on the work of the historical demographer L. Henry's *Crulai* (1958), but à la Redfield's *Tepoztlán*; or that the serial, quantitative studies should give way to an interest in cultural value norms, attitudes and daily life. But indirectly he said with *Montaillou* that he apparently now wished to put the past ten years' "science" behind him, while still – justifiably – regarding himself as a historical specialist.

Mentality or "just" culture

Since the appearance of *Montaillou*, the book has in my view been more overinterpreted than digested – perhaps because it has reasonably been considered by so many as a major publication. The interesting English interdisciplinary scholar Keith Thomas immediately saw in it a refflorescence of the historical interest in the beliefs, myths, attitudes and modes of thought of ordinary people that L. Febvre represented, and which Thomas himself had been almost alone in practising for between ten and twenty years (Thomas 1978). Others have recognized the theme from literary presentations of former times, and a few have thought they could see Saussurean and Lévi-Straussian tendencies in the book. Some claim it is an element in the "green wave" quest for a good life and a good (?) past; some that it expresses an identification perspective by contrasting our own age with another. But

mostly it has in the last few years been seen as the beginning of the new – and difficult to define – interest in the history of mentalities.

It is understandable and necessary that we think and reflect when we read texts. It may also be true that Ladurie intended all this – and much more – when he wrote his book. Yet if the author writes what was at least his primary ambition with the work, it seems reasonable to me that one should first judge his book on that basis before attempting to interpret it as a re-presentation of something else, and something more.

As far as history of mentalities is concerned, the claim is, in the case of *this* book, rather rash and perhaps more an expression of the reader's own priorities than the author's intention. *Montaillou* contains no real analyses at all of what is understood by *mentalité* in French, and the extensive bibliography also reveals little interest in this. On the contrary the book is full of statements on subjects that the *later* history of mentalities, or popular history of ideas (understandably enough) is interested in. It must be underscored that in *Montaillou* Ladurie is not a psychological historian, literary theorist or semiotician. He is an anthropologist and historian – which is much the same thing here. And precisely the anthropological tradition he has here made his framework for investigation and presentation gives him (ideally) the possibility of integrating people's so-called material life with their norms and attitudes to that life. It is in this way that these aspects calmly and quietly, if in a fairly simple form, play their role in *Montaillou*. Within anthropology and ethnology they are normally called "culture".

It is only in his later books that the history of mentalities genre becomes visible, but in Ladurie's case in much closer association with disciplines like (modern) anthropology, folklore and semiotics than with an actual new science or "prefixed" history of the collective mentality or unconscious.

In the book about the insurgency and conflicts in Romans in 1580, which appeared in French in 1979, the actual way of viewing the occasion of the revolt, the carnival (the event), is taken from the American anthropologists

Turner and Geertz, and the focusing on the indirect language of the symbols from people like Barthes, Foucault, Greimas, Lévi-Strauss and Douglas.

The next year came his treatise on the analysis of a French legend or migrant tale as a reflection of forms of consciousness in 18th century villages (Le Roy Ladurie 1979, 1984). The angle of approach through structural semantics (Greimas) was even clearer here.

These two studies can probably be considered as contributions to the broad debate on the history of mentalities. But they can also be seen, respectively, as an attempt, with the help of a new type of analysis (the "social drama") to reintroduce the event into the French historical tradition; and to combine history with an explicit conceptual treatment of cultural expression/content in folkloristic guise. One could, perhaps a little paradoxically, say that Ladurie in these two works uses structuralism to settle accounts with some of the doctrines of the *Annales* school concerning structural history (especially that of Braudel). He begins with the latter and ends up with the former, with *Montaillou* as a (primarily structural-functional) interlude. All three phases have to do with structure in different versions, and it is understandable if outsiders find the distinctions confusing. They may well all have the same philosophical roots, but as scholarly disciplines they have in the course of practice taken on very different contents. Within and among these disciplinary groups there is no simple developmental connection; there is only difference. As a writing subject Ladurie alone has established *his* connection. The ways in which he has written his books are *his* realization through his specialized search for a mode in which to comprehend and describe existence. It is fairly easy to convince oneself of this if one on the one hand compares his always ample bibliographies from one book to the next, and if one on the other hand, within the individual work, sets off the theoretical and methodological references against the way in which they are read (and used) in the treatises. Some would presumably describe Ladurie's disciplinary loans negatively as overenthusiastic and hectic; others more positively as

questing and vitalizing. The eclecticism has existed throughout his writing life, but after *Montaillou* – where it probably had the least fortunate results – it is clear at least to me that it has been better-founded and has produced more consistent results. That Ladurie then in his last three books in fact interprets sensationally more than he argues calmly to convince the reader with analysis is another matter.

From the specialist point of view – that is from the generic disciplinary point of view, not the philosophical one – this has meant that Ladurie's historiography has changed very strikingly.

Neoromanticism and the "little" history

Ladurie's last three books have, in the light of the very variegated phenomena that are called postmodernism, been seen as examples of a (*hi*)story of the non-concrete, which, during the "collapse of the old paradigm of truth and reason" is just as real, relevant and true as all other narratives, including the source itself. Interestingly enough, there is special emphasis here on the analytically least-elaborated *Montaillou*, which is often mentioned in the same breath as Carlo Ginzburg's above-mentioned book on the conceptual world of an Italian miller around 1600 and Natalie Zemon Davis' treatise on the disappearance and later reappearance of an ordinary French peasant around 1550 (Ginzburg 1979; Davis 1985). All of these take their point of departure in certain peculiar phenomena (the apparently unique). Besides showing that the world too is unique, they also want, however, by means of a "thick" and often indirect treatment of the material, to place other factors in the time of their personages in perspective. This "other" is seldom specified in more detail (and is probably deliberately not stated clearly), and it is attempted to reflect it through the narrative and strongly personally-centred representation of the past events. Thus the modern reader can also to some extent read *his* or *her* reflections into the histories. Now, this word *history* means both one thing and two things: on the one hand the

old Ranke paradigm – what actually happened (then); on the other 1) how we experience this other and 2) how we use the past in our understanding, relativization and clarification of our lives in the present.⁸

In retrospect these books have rightly been made the flagships of the masses of articles that have appeared in recent years on dimensions and aspects that are perhaps small, but have been overlooked. This “culturalist” wave is to be found in many disciplines.

The genre is based more on the classic humanities and hermeneutics than on the so-called big theories from rationalism on. Its practitioners usually present their results without major explicit theoretical/methodological deliberations and write mostly in an essay-like and often directly literary style. The reader is to *experience* more than be convinced.

As I see it, this concept builds especially on a dissociation from the systematizing thinking of the 1970s, where individuals and cultural variations were disregarded. The reaction is understandable and was probably to be expected after the slating, by some sectors of neopositivism and neo-Marxism, of more or less everyone who did not toe the line. Perhaps culturalism – although in a friendlier way – is about to do the same thing, only against the background of the opposite content?

One cannot help thinking whether some strange misunderstandings are not involved in the rejection of the theories of the past at the same time as the enthusiasm for the empirical basis of the same predecessors has grown so great. Old social scientists are in these years almost bathing in exotic (traditionally humanist) empirical waters.⁹ But if theory belongs to the world of the past, how or with what does one gather up and grasp this empirical material? If one has no need of tools in the form of concepts, one must almost have superhuman qualities. How can one know that something is white if one does not know or construct its opposite, which we call black? How can one categorize if one does not know what one is categorizing in relation to? And if one can only categorize by contrasting various cultural phenomena, does one not end up (again) with a

kind of dichotomized empiricism or double cultural ethnocentrism?

If one goes to the attack on the “theories” on the basis of the 1970s’ big constructs involving imperialism, centres and peripheries, feudalism and capitalism etc., one unfortunately often forgets that (apart from the clarification they provided, despite everything) they were largely great empirical hypotheses, whose negation does not “debilitate” the theories as concepts. To this, it is true, one could answer that the concept of a “concept” is in itself a cultural and thus a relative phenomenon. If one takes the argument that far, we should really close down the universities. Interestingly enough, no one has suggested that. Instead there is apparently a movement back to Romanticism where one lets the person *see*, the subject *speak*; research is to provide *experiences* and *impressions* and satisfy collective – formerly buried – *needs*. But unfortunately what we have to see, speak and experience with has just been abolished!

I must confess that I do not believe that this new subjectivism, despite great current interest, will enrich us significantly. The ambitions are both too great (as far as liberating research is concerned) and too small (in terms of theoretical potential). No narrative presentation can be written without explicit or *implicit* theoretical concepts, without explanations, without means – whatever they may be called – to persuade the reader (the cultural subject). However much one wants to liberate oneself, one is consciously or unconsciously engaged in a dialogue with contemporary and past writers (and their arguments) and thus also, for better or worse, dependent on them. It is rather similar to the case of parents, whom one cannot choose for oneself either.

Where does the new type of history or historical anthropology stand among these new departures? Here it would be dangerous to generalize too much. The spectrum is very broad, and there are constant shifts within this field. Yet let us, against the background of the readers’ consumption of *Montaillou* and Ginzburg’s and Davis’s by now almost classic books, consider a few differences, but more particularly some similarities.

Ginzburg is, like Ladorie, interested in the common people's own utterances about their lives and their understanding of the surrounding world. But where Ladorie focuses far more widely on the practice of daily life, Ginzburg is concerned with what one in older cultural-historical terms would call the spiritual dimension. Through the miller, the peasant and the craftsman Menocchio's statements he wants to bring out information about the thoughts, understanding and entire frame of reference of the commonality concerning the order of things. Ginzburg's overall picture of this popular cultural mode of understanding (the "reconstructed universe"), pieced together from parts, is constantly related to the ideational picture of God and the world of the ecclesiastical élite. Thus he shows how the common people read a different content into the official (ecclesiastical) culture's concepts. The corollary of this is that in the eyes of the Inquisition they become heretics. Ginzburg is a pathfinder among the archives, and very well read in theological and classical literature. His theoretical ideal here, however, is a kind of continuation of 19th-century historiography, with the individual, the individual's thought and conceptual world as its concrete concern. This fits well with the new culturalism. But in a world where ever-fewer people see any theoretical difference – although they do see an external one – between the modes of perception of the natural sciences and the humanities, Ginzburg very consciously maintains this distinction. Oddly enough, he believes that a reintroduction of some of the methods of empiricism from the end of the 19th century can help to create a new humanist probability model.¹⁰

Ginzburg emphasizes the importance of observing – like a Sherlock Holmes – the detail before the totality. It is also here he has his strength. How one, *through* the study of revealing details, signs and traces (the parts of something organic, cf. his "medical semiology"), forms some picture of the whole, is however something he is – for good reasons – silent about. Ginzburg does not engage with the fact that his rediscovered methodology can presumably first and foremost be used on events. What clues reveal the painter of the picture?

Who committed the crime? What illness are the symptoms an indicator of? But then Ginzburg claims as a historian to be primarily interested in the reconstruction of ordinary anonymous people's lives and conceptual worlds!

Natalie Z. Davis is also interested in depicting the conditions of former times in a new way, i.e. society seen with everyday eyes. On the basis of minutely detailed archive work, trial records and previous accounts, she has renarrated the course of a peasant's disappearing act. One day Martin Guerre, for apparently no reason, leaves his family and no one hears from him for many years. One day much later a man comes to the village and claims to be the vanished Martin. He can speak in detail of many of the features of Martin's former life, and he moves in with his "old" family again. After a few years, though, he is accused by "his" wife and her family of imposture. During the trial, however, where he comes close to convincing the judge that he *is* who he claims to be, the real Martin steps into the courtroom and the swindle is exposed.

The story is compelling, but actually much more banal than the ones Ginzburg, and in particular Ladorie, tell. Yet Davis reveals in the telling more feeling for the society in which the story is played out than Ladorie, and in particular than Ginzburg. She places the event in its proper relief, and this relief, 16th-century peasant society versus the world of the legal apparatus, is also one facet of the book's explicit aim. The course of events, Martin Guerre's life, is the loom she uses to weave her tale around, to give it direction (time) and to provide drama. This art, or this methodology if one wills, is not without effect.

Seen in this light, the other facet of her aim is strange, though. Here she wants, among other things, to answer the question of why Martin left his village, why his substitute became an impostor, and why he failed to cheat Martin's wife. Compared with the otherwise well-conducted study and her good reflections on the possibilities of the historian as opposed to truths, however, she gets into trouble here. And things could hardly have gone otherwise, given the way she formulates these questions.

Davis's revolt consists of her interest in so-called "little history" and her sense of the significance of cultural dimensions. Here a phenomenon like Martin Guerre fits well. But in the problems she also formulates as controlling her study, she is actually deeply-rooted in a kind of German historicism (in a later American vintage). In her quest for the interrelationships of *events* she focuses on the *motives* and *intentions* of the *agents*. In the history of the little man, however, it is now not the individual's role in diplomacy, politics or war that is the central theme, but quite parallel issues of *why* Martin did this or that. Here all the dilemmas of simple *causal explanations* (without a necessary relation between cause and event) and the motives of *single individuals* as objects of social analysis come rolling in over her. Given the way she asks, her answers must necessarily begin with a *because* – and so they do. Martin's disappearance is explained by such reasons as that he was (perhaps?) harassed because of his Basque origins; that he was (perhaps?) impotent; and that this impotence was (perhaps?) caused by his having been brought up exclusively in the company of women and girls! And thus the reasoning continues, and is thereafter summated as the explanation of which Davis would like to convince the reader.

Here her otherwise meticulous work comes to stand in glaring contrast to whole and half suppositions and intuitive "explanations" at the popular magazine level. The most interesting thing, however, is that she in fact had no need at all to pose these theoretically problematical and empirically unnecessary, person-fixated "why" questions. Martin did as he did, no matter whether he was impotent, tired of family life or maybe just half-witted. It is hard to use the course of a life as a guiding principle in a treatise with wider perspectives; but it can indeed be done.¹¹ Yet it looks as though Davis let the means, Martin's life story, become the central thing, so that it was no longer value-judgements and views in the agrarian society Martin *lived in* that were to be reflected, but more Martin himself. What she very honestly calls the made-up story is apparently not necessary to the fulfilment of her intention as

outlined at the beginning of the book. Hitherto she has wanted to write collective history seen "from below", but is here on her way over into a form of individual history.

The postscript to Davis's book in its Italian and Swedish editions was written by Ginzburg. He describes, among other things, how the writing of history a couple of centuries ago helped to develop a new kind of romance – the novel. Ginzburg does seem to be aware of the dilemma in Davis's book, but contents himself with saying that the contrast between being a historian and being a novelist has gradually been eradicated. He discusses this in the light of the claim that the dogma of one historical truth can no longer be maintained. Probably everyone today would agree with him on the last point. But at the same time he elsewhere associates the discipline of history with the idea of a "claim to truth" which does not exist in fiction. Where Ginzburg apparently on the one hand wants the historians to breach the traditional discipline-defined boundaries – which is a positive thing – he is still unclarified in his own seemingly self-contradictory relativism. In the postscript he reveals how he is in fact incapable of speaking in principle about the historical problem that both he and Davis are concerned with. He has clearly, against the background of his (Italian) philological and interpretative tradition, no language with which to speak of phenomena lying outside the text itself (which is also evident from his generally very close adherence to archive material and examples). Instead he speaks with and through countless examples from literature, where he is to a much greater extent on home ground. Even though he too wants to get from the event to "the deeper historical tendencies",¹² he very understandably gets no further than vague statements that both the historian and the author want to represent the past and the actions of human beings. But he does not discuss where the difference between the two forms of representation lies (strangely enough, he persists in the distinction), or how one is to do this. However, it is clear that Ginzburg has difficulty in seeing the difference between what I will call a disciplinary conceptual discussion which may in the final analysis result

in a doubting "perhaps" (for example on an issue of interrelationships or dependence) and the wholly subjective/ fictive utterance. As a specialist he must analytically adopt an almost indifferent attitude to Davis's experiment, and apparently thinks that if we cannot profess the great old "claim to truth", then we only make up our own imaginary pictures. At the moment he is not alone in thinking this.

Here it is worth recalling that Ladurie despite everything did not, as his wife suggested,¹³ write a purely fictional novel based on the Montaignou material!

Structure and/or real life

Ginzburg is however right in saying that the books mentioned here supply food for thought to several "camps" and that they may be experienced differently by the various camps.

That they have achieved popularity with a broader reading world is possibly more related to the subjects they deal with than the more academic arguments to which they directly – and especially indirectly – address themselves. The books quite simply arouse attention because they describe what villagers spoke about in the middle ages while they sat on their doorsteps delousing one another; or how a miller in the 1580s explained that it was people who (ideologically) created God and not the other way round; or how a man in the 1540s left his family only to appear many years later in the courtroom while his "replacement" was being accused of posing as him.

We are not used to getting this close to the past. Many reviews bear witness to the fact that the books are consumed in the same way as classic, and now also completely modern, historical novels: the reader is fascinated by the strange (here past) milieu and identifies himself or herself by means of the characters or fates of the persons in the books.

In specialist circles interest has probably primarily been aroused because the stories are about ordinary people's daily life and conceptual worlds, about the conflict between the people and the élite, and implicitly about the potential of the coherent narrative. Here it is presumably relevant to ask what actually con-

stitutes the new in this "new history" and whether future history should be modelled on these examples. It is interesting that Braudel's mammoth work *Civilization & Capitalism* appeared in the English-speaking world at about the same time as the three books mentioned. If Braudel represents the best of what should now perhaps be called the old history, I am rather inclined to believe that it is the geriatric who will survive – despite his prejudices and febleness. Can one really imagine a history only concerned with individuals, not with society (whatever we understand by this); with parts, not with the whole?¹⁴

What is new for me is not the alternative view of scholarship claimed by some, for here very little has changed in principle. The renewed interest of historians in culture, however, is a kind of novelty that may have interesting consequences. Probably more far-reaching, though, is the realization – new at least for some people – that the simple combination of the sources and the historian as subject do not recreate the past or the other. If the historian does not want to be a novelist, concepts of how one constructs the past are necessary. And here Ladurie's *Montaignou* in principle represents a greater innovation than Davis's and especially Ginzburg's book.

The new history, or at least the culturalist part of it, does probably offer its greatest innovation in the form of its consistent argument that structural studies and cultural studies are incompatible entities. Here it takes sides in favour of the so-called living human beings. Yet these hard words are a postulate which has never been argued convincingly. In my view this dogma is predicated on a misunderstanding which will take a good deal of space to explain. Yet we are fortunate that in recent years a book has gained currency in wide circles that demonstrates *practically* how structure, event, drama and culture are not irreconcilable entities (if indeed they are different entities at all). I am thinking here of the Italian semiotician Umberto Eco's novel about the traces of the past, *The Name of the Rose*.

But it is important to emphasize here that Eco in his principal work on semiotics *La Struttura Assente* (1968) [cf. *A Theory of Semi-*

otics (1976)], which is indirectly present in many of the arguments in *The Name of the Rose*, “solves” the controversial problem of structure and event by separating them analytically so as to see (“penetrate”) later the events (i.e. the empirical forms) by means of the structure (the concepts/model). Structure for Eco and his linguistic sources of inspiration is on the one hand not something that exists out there in some reality. It is a “model” or set of conceptual relationships that we, the scholars, construct: something by means of which we see (analyse) what can be seen (analysed) with the given structure. The argument is in fact present in the Italian title, which against the background of the arguments in the book can be translated as “The structure absent (from “reality”)”. For we only know reality through the concepts with which we can penetrate some of “it”!

At the same time Eco also works indirectly with another approach to the phenomenon of structure with parallels in some French historical research. In the (empirical) *course* of events of which he gives an account in *The Name of the Rose*, he shows that “the characters are obliged to act according to the laws of the world in which they live” (Eco 1985, 28). This could also have been said by Braudel. It is the same thing as the so-called prison of Braudel’s persons, which some of his critics later turned into his own theoretical imprisonment.

In the literary (double) narrative framework that Eco chose for his novel, he succeeded however in confronting the discussion of various conceptual schools’ ways of constructing reality (especially in the dialogues between William and Adso) with several different cultural practices, while filling his book to the limit with events and symbols ranging from murder to architectural ornamentation. If we imagine *The Name of the Rose* not written as a novel but as a work of history, where the author only speaks through deliberations and descriptions, the book, because of its constant alternation between theoretical reasoning and subsequent empirical events, would probably be described as uneven. Yet this appears to me to be a moderate price to pay for being able to write a treatise embracing both structure and culture,

discussions of perception and events, concepts and living life.

But it would be too optimistic to believe that we would solve all the dilemmas by following Eco on this point. He is one of those scholars who even in the act of achieving the sublime keeps teasingly warning the reader against believing that he has now grasped how we can understand and describe society, culture, the monastery, faith, laughter, or whatever we think we can call “reality”. Scholarship has always only been able to deal with the things it can say about what in everyday speech we *call* reality; not what it in fact is. This is why all our accounts – while telling of experiences of things – also become experiments with ways of saying things. It is perhaps the learning of this humbling and yet uplifting lesson that makes the writing subject into a scholar. Whether the medium is then the “strictly scholarly” treatise, narrative, essay or novel is in itself secondary.¹⁵

Therefore *Montaillou* can also be read in several ways. Ladurie has not written *the history* of life in the village. He has told a story or *his story* against the background of quite particular conditions. His premisses were primarily some books about other peasant villages at other times and in other parts of the world. It is not unlikely that other scholars on the basis of the same material will one day write a new book about Montaillou, either also in an attempt to describe the place, its life and conceptual world or to solve other problems. This will not make Ladurie’s work less valuable. Such a study will directly or indirectly engage Ladurie’s treatise just as the latter, through Duvernoy’s source edition, has established a discussion with the philologists, and through the functionalist inspiration has brought about a dialogue with the disciplines interested in the sociology of place.

Epilogue

Ladurie, Ginzburg and Davis can, as shown, be read with interest as well as doubt. Their works help to make the spectrum of our subject richer, but we must constantly discuss whether

we are willing to tread the paths along which they wish to lead us. How fruitful are their approaches, and where are they going?

All three books close in on themselves in strange ways. This is certainly connected with the genre they represent. The three historians (now) wish to dissociate themselves clearly from describing the society or societies of the past as the economist or the positivist sociologist usually would. Just like their long-dead predecessors they want to be allowed again to narrate their history. If this narration is reintroduced as an experiential framework vis-à-vis the public, though, it requires much more extensive, if indirect, structuring. Otherwise it will end up just like the greatly-scorned naive realism which the new theorizing rightly turned against only a generation ago. And then we are in my view back at square one.

But belief in the great theories and the ambitious system histories is at the same being rejected during these years. This postmodernist scepticism towards research as a precondition of knowledge and action, towards general statements of truth, and towards any striving for a hegemony of meaning (encompassing language itself) is perhaps comprehensible as a social phenomenon, but can easily, if pushed too hard, lead to a chaotic entropy or indifference.

In the examples discussed here we have seen that it has led, in the reaction against so-called "big" history, to a concentration on extremely small histories,¹⁶ presented as narrative and often even devoid of information that could place the events and people described in a local or societal context. It is interesting to observe, though, that since the authors as subjects are hardly able to divest themselves totally of their past, the historical thirst for truth has still accompanied them; now it has just been scaled down to the completely small level (concerned with ordinary people's everyday motives). But is society thereby not once again made into the sum of individuals, and do the small community descriptions not once more become atomistic subcultural examples? Despite the show-down we are close to being back in the scholarly tradition where the totality is to be built up of infinitely small parts, no matter whether

these are warlords or anonymous peasants, national histories or village studies.

Now, there is in my view nothing wrong either with writing in the narrative mode (which can have great communicative, experiential value) or writing about individual lives or unknown localities. It all depends on what one's intentions are with one's writing.

If one thinks that all of our so-called disciplinary concepts are a kind of self-delusion, one should, as far as I can see, simply write pure fiction, and thus allow oneself to relate with *complete* freedom to the traces that the past or our own age have left us. Here there is no reason for limitations. The break must be made radically if one does not simply want to use literature as a hiding-place to avoid disciplinary debate or criticism. If one instead simply tries to write "little history", to write "close to reality" or to write about the non-material (culture), one solves no real (epistemological) problem; one simply shifts the problem, and in some respects one perhaps even makes it bigger. Dealing with both culture and structure only becomes quite hopeless when one regards culture as *Geist* or the history of ideas without explanatory relations with practice. And these relations are, as outlined above, never something that directly emerges in even the most "honest" realistic account, or something one simply finds in the traces themselves (the sources). They are something that we as scholars establish, construct, conceptualize or whatever one wants to call it. Here we are all forced to theorize, and then we are inevitably forced back into interplay and counterplay with the theories of former times.

Because many today have had to face the fact that there is no given "true" theory that can penetrate everything, there is no reason to lapse into theoretical scepticism. The sense of doom that can be felt in many scholarly environments almost recalls the many times the patriarchs thought the coming of Antichrist was at hand.

The solution to this issue does not have to be a relativist – and thus actually resigned – attitude to theoretical work in general, an ultraliberal conceptual eclecticism or a return to existentialist/phenomenological philosophy. The

heritage of the great theoretical constructs of the past and their minor branches (which are those that have been most applied in the specialist disciplines) surely does not have to weigh so heavily that one cannot learn to live with it. There may also be a strength in learning to acknowledge doubt, and in attempting in the dialogue with others to explain the nature of the scepticism, while in one's own practice one is dependent on some of what one doubts. No theory has ever been able to explain "everything", whatever one might mean by that. But some theories have been able to explain something; and others, something else. And simple addition is for logical reasons not always possible.

When we find ourselves in the midst of these processes, we can presumably as "historical subjects" only exploit the knowledge of the past by – in the areas where we can trace non-sequiturs, dead ends, etc. – going back through the arguments to pinpoint where things went wrong, and then attempt to build further in another direction. We can also try to formulate quite different theoretical approaches. However, if we still want to acknowledge some of earlier research's worthwhile issues, the requirements of such attempts must also be that the new theory can gather up these issues, even if it is in another form.

It is here that works like those discussed here in their approach to history have a tendency to cut themselves off from the dialogue with the research of the past and much of the research of the present. The (external) form has become more important than the content (as far as the development of the disciplinary environment is concerned). They are even very difficult to build further on,¹⁷ which is probably not the point anyway. Thus they become a kind of exclusive one-off performance. However, if one completely abandons the belief that it is worth trying to roll the stone up the mountain again, there is no reason for such deliberations at all.

In *The Name of the Rose* Eco teaches the reader to laugh at *the* truth and at belief in certainties about what the world (reality) is (i.e. one truth or the truth about the universe as defined by the prevailing ideology). At the

same time he never gives up constantly setting up and arguing for new possible possibilities in the quest to attain what *perhaps* is unattainable. The ideologue does not need to try any more; he does not doubt, for he thinks he knows how things are to be explained. For the researcher it is probably rather the opposite that is true.

Notes

I thank Jens Rahbek Rasmussen, Bjarne Stoklund and Uffe Østergaard for their comments on the manuscript for this article.

1. Community studies have their historical roots as research in functionalism's organicist view of "society", which was more or less spatially defined. Even though we are able to see the problems in functionalism, despite this heritage we will still have to study any specific life and consciousness in localities like villages, for example, or urban areas; that is, if we need to, and if we do not wish to disregard this because of the analytical problems.
2. The chapter "A Community within Communities" in Redfield 1967a.
3. I am here considering the author on his own premisses alone. At this juncture Ladurie was in a situation where he had a total overview of the best of the French historical tradition, while at the same time he had familiarized himself with anthropological studies of social relations and culture in small localities.
4. Le Roy Ladurie 1966. The book has later been described as pure Malthusian reductionism. I would like to draw attention to the fact that it is much more than that.
5. Le Roy Ladurie 1974. The same can be said of the abridged American and English edition of *Montaillou*, even though the background here is different. On the face of it, one would expect that a holistic monograph like *Montaillou* would completely fall apart by having several chapters at the end removed. That this is not the case is precisely because the aspects dealt with in these sections are not placed in the necessary relationship of dependence to other central aspects of the book. They appear only as empirical themes presented in succession.
6. Quotations and discussions from Le Roy Ladurie 1979, 10, 11, 15, 18, 223, 233.
7. Not true of all, however. See e.g. Stone 1979.
8. On the psychological use of the past, see Ariès 1978.
9. Friedman (1984) has similar thoughts.

10. Ginzburg 1980. He regards his interest in the small features of society as a form of semiology (rather a big word) which he characteristically has belonging to philology, not to linguistics.
11. The works that, so to speak, manage to deal with the problem, seem to be those that allow the social/cultural circumstances to explain the individual life process, not the other way round. The course of a life can open our eyes to dimensions that have to be explained, or reflect particular ideologies and patterns of organization. But if the explanation is sought in the individual as such, we are indulging in psychology in the end. If it is sought in the society the result may in the event be called history, ethnology or sociology.
12. Using the study of the unique or the event as a means towards the study of the more universal is mentioned by both Ginzburg and Davis. But both in fact do not make very much of it, and do not discuss at all how one does it. This makes the statement seem like a postulate or just a pious (Utopian) wish. Ladurie's *Romans* is a much better example here.
13. Personal communication from Ladurie.
14. It has been attempted to solve these problems in very different ways. The narration of history can, when it is best, be implicitly made to include both dimensions. It was presumably this Braudel was working towards in various ways. In Denmark Niels Steensgaard is probably also a good example of the same kind of effort. But one should be aware that the analysis of such factors that is implicit, but deliberate, in the narrative process, is probably a rather greater challenge than the type that is more explicit (but also less friendly to the reader!)
15. For several reasons we cannot ask the question why the semiotician Eco chose to write a novel, when he wrote what one could call his first empirical study. What he did do, on the other hand, was to write within a medieval literary framework (a narrator who tells of his experiences and dialogues with others concerning a course of events) which among other things allowed him to show that the world is full of signs, and that one only understands them if one is able to connect them. One can say that Eco lets his William (Sherlock Holmes) follow through the analytical consequences of Ginzburg's focusing on the signs overlooked by others. These relations among signs however require constructions (which lie "outside" the signs themselves) – or analyses if one prefers. It is this that for me also makes *The Name of the Rose* a specialist book. Even if all the novel's empirical descriptions of events were pure fiction (which they are far from being), this does not make the epistemological passages in which Eco has his characters experience them into individual subjectivism. Ginzburg does not seem to be aware of this distinction in his discussion of fiction and history.

- Ginzburg no longer believes in a historical truth (i.e. one true description of reality). As he apparently cannot get to grips with the issue of how we construct relations between signs, he seems to have chosen a relative and subjective researcher-centred attitude. Eco chooses to set up various (alternative) constructions and then see what comes out of this. While he doubts, he always understands how to be explicitly aware of the operations he is carrying out.
16. However, there is in my view no reason to turn up one's nose at "little" history just because it occupies itself with specific problems or localities. As Uffe Østergaard (1986, 40) has pointed out, the small narrative can in reality be more synthesizing than large but thin overview syntheses. But if the small narrative can *only*, be perceived intuitively, there is not much to be said about its contribution to any joint theoretical progress.
 17. At a research seminar in historical anthropology at Schæffergården in 1980 two of the questioners touched on this situation during the discussion after Ladurie's lecture. Ladurie was asked about the aim of the Montaillou study, about the omission of the context and the change aspect, as well as about the lack of interest in dialogue with living anthropologists' work within the field at issue. In his answer Ladurie more or less chose not to answer. Instead he spoke of representativity, of the uniquely fascinating material and of "little" history. The audience more or less got the impression that Ladurie did not wish to see his research as an element in any collective effort. (Cf. *Dansk Folkemindesamling* files, *Historisk antropologi*, 1/9 1980, Tape 1).

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