



Ethnography in Post-Franco Spain: the View of an Outsider

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The usual difficulties of doing fieldwork in a new site were compounded by two factors: the nature of the kind of anthropological information about rural Spain available outside the country; and the very limited amount of intercommunication in the public sphere concerning political issues in the early years of the Transition. The article describes the discussions taking place in anthropology at the time of the fieldwork, and proposes that a long prior period of selective repression explains the differing ways in which informants spoke of the historical past and the political present.

[SPANISH TRANSITION,
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article

I did one year of ethnographic fieldwork in the extreme south of the Province of Alicante between the autumn of 1978 and August 1979 and I continued doing fieldwork in the region, returning frequently for extended periods up to 1996, when I was joined by Susana Narotzky. The book we wrote together, *Luchas Inmediatas* (2010), covers the period of the Transition in great detail, and I have also devoted a chapter in *Intellectuals and (Counter-) Politics* to “History’s absent presence in the everyday politics of contemporary rural Spain” (Smith, 2014: 129-149). So in this article I will restrict my account to just the one first year of that study. This allows me to emphasise the distinct *foreign-ness* of my perspective, and it also serves to highlight some of the issues that face anthropological fieldworkers in similar settings.

In a small rural town around that period, just three years after the death of the *caudillo*, people did not have the habit of talking about public issues, politics or local and national history. I arrived as an outsider who had only visited Spain once before and whose particular approach in social anthropology was strongly marxist. These three factors –the extremely limited familiarity local people had with intercommunicative practice in the public sphere, the ignorance about rural Spain of the newly-arrived foreigner, and the persecution in Spain of anything even remotely to do with Marx– combined to influence my initial fieldwork experience. I will speak of all of these

in what follows, but I would like to make the reader aware of the chronology of my experience by describing the *immediate impressions* as I began my fieldwork and then move to my more *analytic interpretations* of what I was seeing.

1. POLITICAL ECONOMY IN ANTHROPOLOGY BEYOND SPAIN

But to begin with I would like to describe to the reader the baggage of anthropological reading that I brought with me as I entered Spain, because it is important for the Spanish reader to know what the broader setting of Left anthropology was like at that time.

In Anglophone and francophone anthropology at that time the move towards a more marxist-influenced anthropology that had begun in the mid-sixties was still quite strong. In France figures like Claude Meillassoux and Maurice Godelier were considered among the more important anthropologists and even Claude Lévi-Strauss (1955) admitted that he could not begin a day of writing without first reading a few pages of Marx to get him in the right frame of mind. In the United States criticism of the war on Southeast Asia produced people who referred to themselves as ‘radical anthropologists’ and a major rift occurred in the American professional association when they exposed other anthropologists who were aiding the military in that war. Even so many of these anthropologists did not take Marx as an inspiration for their work. It must be remembered that, although not as violent as in Spain, the repression of ‘marxism’ (very broadly defined) was pervasive in the US even in the late sixties. As a result anthropologists employed self-censorship by using the term ‘political economy’ as a code word for an approach that was strongly influenced of by Marx¹. But except for a few of the older generation like Eleanor Leacock, Stanley Diamond, Eric Wolf and June Nash, and a

younger generation like Carol Smith, Donald Donham and William Roseberry, careful engagement with Marx’s epistemology was not a feature of most American ‘political economy’.

In Britain the two major universities – Oxford and Cambridge – followed their long-standing belief that what was happening in the rest of the academic world was of little importance to them and a quite traditional kind of anthropology held dominance. The two other universities where there was a strong tradition of anthropology, London School of Economics and Manchester, were considerably more open and their departments more heterogeneous. At Manchester the major figure, Max Gluckman, had long been sympathetic to marxian approaches and Ronald Frankenburg was openly sympathetic to a strongly marxist kind of anthropology. At the LSE young graduate students were greatly influenced by the marxist approach of Maurice Bloch (1983) himself carrying with him the French aura.²

Of course in Mexico, where the discipline of anthropology was much more generally influential both in government and in critical political debates than in these other countries, marxian anthropology had a long history. Indeed major figures in Mexican anthropology, some of them exiles from Franco’s Spain, very significantly influenced American anthropologists like Eric Wolf and June Nash. In Andean south America the role of a radical kind of anthropology (marxist or not) often depended upon whether or not the department was to be found in an elite (often Catholic) university or in a university with greater popular access. Thus in Peru, where I did my first fieldwork, the established anthropologists at *La Pontificia Universidad Católica del Perú* followed almost rigidly the old anthropology traditions of the past, especially structural-functionalism but also an almost mystical kind of Catholic anthropology. Meanwhile, at *La Universidad Nacional de San Marcos*, anthropology as political economy

1 Political economy’ in anthropology is still frequently a coded mystification of the influence of Marx. It allows writers to take authority from Marx at one moment and distance themselves from him at another, as they choose.

2 Ignasi Terradas took his PhD from Manchester. Josep Llobera was a major influence among graduate students at the LSE and was one of the founders of the journal, radical at the time, *Critique of Anthropology*.



was entirely pervasive, students being encouraged to read Mariátegui (1928) and Arguedas (1968), just as much as the more mainstream anthropologists.

2. ANGLOPHONE ANTHROPOLOGY OF SPAIN

I provide this background partly because it explains the frame of mind with which I arrived in Spain in the Fall of 1978, and partly to place Spanish anthropology in a more global setting. To put it bluntly I doubt if any of these anthropologists would have been able to teach in Spain at that time. The only two figures associated with anthropology who were read beyond the Pyrenees were Caro Baroja, who was more of an ethnologist than a social anthropologist, and Lisón Tolosana who had been trained at Oxford and wrote in a dismal structural functionalist manner that managed to describe the pseudonymous town of *Belmonte de los Caballeros* (1966) as if nobody had suffered repression nor anybody practised the kind of miserable selective terror that was the daily life of Franco's Spain at least for the period that the monograph covered.

Two books of major importance had extraordinary influence on the image anthropologists held of Spain. These were *Mediterranean Countrymen* edited by Julian Pitt-Rivers and Ahmad Mustafa Abu Zaid, which had an extensive essay by Caro Baroja on the culture of the Spanish understood as 'folk'; and *Honour and Shame in the Mediterranean* edited by Jean Peristiany. We learned from these books and the monographs that followed, that there were essentially only two matters of interest for anthropologists in Spain. These were 'patron-client relations' and 'honour and shame'. Despite the glaring facts of its geography we were told that Spain was a 'Mediterranean country'. In this way it came within the field of study of traditional anthropology, that is the study of societies perhaps not always themselves *exotic*, but at least with strange practices and beliefs that distinguish them from 'us'.

So it is important to convey the effect this had on an anthropologist arriving in Spain with a view to speaking

with colleagues before deciding where and how to begin ethnographic research. On the one hand my experience in South America had given me an exaggerated sense of the degree to which anthropologists with greater or lesser influence from Marx were engaged in what we might term 'the national project'; on the other hand the knowledge I had gained of Spain through my reading of English-language ethnographies of the country were so far from my own interests that they seemed to provide a kind of fantasy world not so far from what we might read today by Ruiz Zafón. 'Class' was confined to status and anyway was obscured by the supposedly pervasive practice of patron-client relations. 'Ethnicity' as a category distinct from the rationality of urban Man (sic) was either something common to all Spaniards ('beyond the Pyrenees'), or was understood in terms of the 'folklore' of remote regions, such as Galicia, the Basque country, or rural Andalucía. And gender relations had only one form of expression: that between the honour of men and the shame of women.

3. INTRODUCTION TO THE FIELDSITE

On my arrival in Spain, in September 1978, I was extremely fortunate in having met Joan Martínez Alier while I was working in Peru and I had read everything I could find that he had written (both about Spain and elsewhere). I had also met –very briefly and on an earlier visit– a young but immensely knowledgeable graduate student called Ignasi Terradas. Martínez Alier soon introduced me to Joan Frigolé. There were of course very few anthropologists in Spain at that time and none of them, as far as I know, in dedicated anthropology departments. The 'liberal sciences' not to mention the social sciences did not sit well with the militaristic and Catholic world view of Spain during the dictatorship. But I had the sense that Frigolé was among the few anthropologists in Spain, who was doing extensive fieldwork over quite a long period: that is, in the tradition that I was used to as a British-trained anthropologist. So he was able to give me excellent guidance and suggested that I begin by speaking with

various professors at the University of Valencia. While Josepa Cucó was then conducting fieldwork in Valencia, generally the people I spoke to were geographers and economic historians.

Knowledge of the material aspects of the physical body of Spain was integral to the *caudillo's* vision of governance and, as a result, an especially thorough, detailed geography thrived, although its empiricism was minimally shaped by any theoretical ambitions. These geographers were extraordinarily helpful to me in selecting a fieldsite in the extreme south of Alicante. But I was struck by an interesting distinction between these scholars who tended to be quite familiar with details of the current regional situation, and others working in the social and human sciences who were just beginning to engage in work driven by *theoretical* debates across Europe. Economic historians, many of them at last able to work openly on the specific characteristics of the Valencian economy, were at an especially exciting moment in those days, testing out new theories and ideas and critiquing the biased theories of capitalist development coming from northern Europe. But their knowledge of the present did not match that of their geography colleagues. So, given the sparsity of research funds and the lack of recognition of anthropology as an independent discipline (unlike either sociology on the one side or folklore on the other) there was very little long-term fieldwork-based ethnography happening in Spain. Theory was exuberant and exciting as it began to break out of the Francoist straight-jacket, but initially it tended to occur without the support of grounded empirical research (except that produced for the effective functioning of the declining regime³).

This then was the Spain I entered as I began my fieldwork in the Bajo Segura in 1978. Let me begin with first impressions, before moving to my attempts at analysis and its limitations.

The first thing to note, in comparison with other articles in this collection, is my relative ignorance. This was of course my own fault but it was also the result of

3 I use the term 'declining regime' because Francoism did not die with Franco and the Transition was by no means a 'new start'.

the literature that had emerged from Spain to be read abroad during the previous twenty years, and this included anthropology⁴, but I want to stress that the problem of this ignorance –perhaps just as great as many anthropologist find as they arrive in the field– was compounded by the severe restrictions on free and open discussion among people in the Bajo Segura at that time. A central part of my research plan was to collect life histories by means of a framework I had developed while in Peru. There I had collected over a hundred such abbreviated histories and I followed these up with in-depth discussions with especially interesting cases. Now I expected that, with the *caudillo* dead for three years, people would be delighted to talk with me about their life stories and I fully intended to use the same method as I had in Peru. It did not take long for me to discover that most people were simply unwilling to participate in the interviews; some expressed enthusiasm *in principle* but became vague and impatient if I actually tried to do the interview; others –the elite of the town– were quite willing but there were vast gaps in the stories they told me.

But what, precisely, was it that I wanted to talk to people about? No ethnographer arrives in the field without a particular focus or a set of questions to be answered and I had decided to come to this part of Spain for a particular reason. In my earlier fieldwork in Peru (Smith, 1989) I had been surprised by the way in which working people with both agricultural and non-agricultural occupations and in both the country and the city had joined together in a political struggle against a dominant class. I had expected that a real proletariat had been freed from the means of production (usually through being driven off the land) and as a result were forced to sell their labour to mostly urban capitalists.

4 On the other hand three major books on Spain by English historians were available in Spain by the time I arrived. These were *the Spanish Civil War* by Hugh Thomas 1961, Ronald Fraser, *Recuérdalo tú y recuérdalo a otros* (1979) and a translation of a much earlier book, *The Spanish Labyrinth: an Account of the Social and Political Background of the Spanish Civil War* by Gerald Brennan, published in 1941.



But this was not what I found. Instead I found that rural agriculturalists and pastoralists combined with their migrant colleagues working in the cities and together they undertook a successful struggle against land-owners.

So I had chosen to come to the País Valencià because there had been a long history of often quite intensive agriculture (aided by irrigation) combined with small rural manufacturing. And this pattern continued to the present (i.e. the 1970s). So my central concern in my fieldwork was to study the nature of working class relations *across* the agriculture/industry divide. To do so I would have to talk to many different kinds of workers and I would have to find ways of studying the relations between them and their employers. And I would have to place what I found in the broader social and political setting of Spain –not just in the present, but as things had unfolded over the past years.

This all seemed obvious to me. It was simply a matter of getting down to work and recording what I found. By this time in my career I had rejected both British and French versions of ‘structuralism’ and, influenced by anthropologists like Sidney Mintz and Eric Wolf, and historians like Eric Hobsbawm and Edward Thompson, my idea of ethnography was that it should be a *historical* study of the present in terms of *political economy*: in other words, asking questions that would help me to understand reality in marxian terms (for greater detail, see Narotzky and Smith, 2006). This will no doubt strike the Spanish reader familiar with those times as a point-of-view very far removed from the ordinary discourse prevailing in rural Spain then. But, as I have said, my naivety at that time was both partly a weakness of my own but, more importantly for this issue of the journal, it was a result of what little was known about Spain. The possibility of basing my ethnography on discussions of the past fifty years of history was, of course, going to be deeply problematic as the shadow of Franco’s rule stretched darkly across the present, especially I would say, in the countryside. And then to add to this my assumption that I would record both the history and the present by means of marxist terms –the tensions of class, exploitation, merchant capital and so on– was simply absurd.

The absurdity, or at least the obstacles in my way, did not initially surprise me. I had grown up in a small village in the centre of England and, while questions about history would have troubled nobody, questions about class and exploitation, even if veiled in other terms, would have been quite offensive to people. So I was not initially discouraged by what I found. It was only as I began to produce a richer and more profound picture of the town and the region that first the contradictions emerged, and then the difficulties of acquiring the information I needed became manifest.

In fact in a superficial way it was much easier to ‘see’ the class differences in the Bajo Segura than it would have been in my own village in England. In my own village very few people would actually speak of real class (or, more accurately, status) differences, even where they obviously existed. With a number of Labour governments behind us, since the Second World War, ordinary people tended to be quite dismissive of the difference between the gentry (*los caballeros, los señoritos*) and themselves. By contrast, it quickly became clear in the Bajo Segura that there were an old class of *señoritos* who had been well-established as quite large landowners or tenants since the end of the Civil War. They were joined by those who more recently had acquired land or had been successfully running one of the many small manufactories in the region, or made money as agricultural wholesalers and transporters. These were quite clearly the elite of the village all of whom had benefited from the years of Franco, some more from the earlier years, some more from the later years. Then there were the shop-keepers, minor professionals, middle-sized land-owners or tenants, and those who owned small workshops, who constituted a locally acknowledged ‘middle-class’. Finally, anybody who earned a wage was working class, although there was a clear qualitative difference between those who worked in small factories and workshops and the *jornaleros* in agriculture.

This was my initial impression and it included issues of gender. The people I first spoke to tended to tie the class or status of an adult woman to the occupation of their husband, though this changed along a spectrum

as one moved from the top to the bottom of the social scale. Among shop-keepers and others like them, husband and wife tended to work together in the family enterprise, while for 'working class' people, women both took in homework from the local factories, often thereby becoming the main income earners, and also worked as *jornaleras* though usually on a more seasonal basis than their husbands and depending on the demand for their home-work. In fact, despite the Anglophone literature on 'honour and shame', I was comforted by the fact that women in the Bajo Segura appeared to me to have far more control over the domestic sphere than I think I would have found in my own village in England, and this increased as one moved from the petit-bourgeois small shopkeepers to the various kinds of proletariat.

4. FROM FIRST IMPRESSIONS TO PRELIMINARY ANALYSIS

As the months passed, this initial framework became more complex and barriers I had not anticipated at the beginning began to arise. Perhaps what illustrates this best is to speak of my discovery of the spatial topography of the town. I had initially settled on Catral as the base for my study of the area, as opposed to a number of neighbouring towns, because it was the only one where I found a house to rent for me and my family. Yet the location of this house immediately directed my initial fieldwork interactions. It was a street in the centre of the town made up of a number of shops, the homes of work-distributors for the shoe industry, as well as a variety of others. Crossing this street at right-angles and running the length of the main part of the town was a street made up of the somewhat more splendid houses of the larger land-owners and tenants as well as professionals such as the doctor and the mayor who had a senior management job in an agricultural supply firm in Murcia (and had been appointed to his position during the Franco years).

It was of course easy for me to talk to my neighbours and get an impression of the town through their eyes. Generally speaking, Franco himself was almost never mentioned but the impression people wished to convey to the foreigner was that

Spain was now a 'modern' society much like any in Europe, and in any event southern Valencia was an especially dynamic and advanced part of Spain. As I walked through the town in search of people to talk to, the better off land-owners enjoyed taking the time to educate me about the particular features of the town and the region. In this case Franco was quite often mentioned and a frequent anxiety was expressed that there would now be a decline in social order with less 'responsible' people trying to run things, though it was never made clear what things they might run. It is interesting to note that the most obvious was the entry of new people into local political office – which had always been entirely dominated by the elite –, but the actual entry of openly competitive elections had to wait until March of 1979 and, up to a few weeks before that time, it was as though nobody really thought the politics of *elections* would actually disturb the order these people so valued and were so afraid of losing.

There were of course divisions even in this part of town. The old *Casino*, for example, was still there and only the better off shopkeepers would have thought of taking coffee there. But the really glaring division was between this central part of the town and a long stretch of small houses running down a single street to the east. Here every house was occupied by a *jornalero* family, or families whose members worked in a small workshop, or a mix of both.

There is no question that the existence of this topography was a result of the way the local political economy had been organized during Franco's government. Moreover it conformed also to the kind of talk that occurred in each space: ordinary chatter about daily and local business issues among people on my own street; a desire to impart to me a coherent and quite well-planned narrative of local society by the elite; and for the first few months of my fieldwork *no discussion at all* between me and the *jornaleros/as* in the barrio where they lived, much as I tried. For some this may have resulted from a real suspicion of what I was doing, both as an outsider and also as somebody who lived among a certain group of people in the town. But for most, it was simply the fact that they were not used to anybody listening to their opinions or taking what they said seriously.

I want to convey this sense of the first months in rural Spain in the early years of the Transition because the emergence of many years of repression that began with extreme



and quite selective violence and then moved into a routine of ordinary taken-for-granted fear and the care that this generated, produced this kind of *superficial* normality. As I have said, of course, what appeared normal was expressed differently by the chattering shop-keepers, the authoritative language of the elite, and the near-silence of the *jornaleros/as*. And the way the Transition was managed from above, the compacts made among the leaders of the political parties, the items on the news, the continued respect for the very people and institutions that had not just colluded but had managed and benefitted from the dictatorship –the church, the military, even the education system as it reached a town such as this one– these all served to legitimate these different arenas as normal public discourse.

The atmosphere this created for all but the *jornaleros/as* was in some ways rather contradictory. On the one hand there was an assurance that Spain –in this instance Catral and the region around it– was not going to relapse into the old rigidities and dogged grey obstinacies of Francoism. On the other hand there was a parallel assurance that change would be so gentle that perhaps it would hardly be noticed at all.

The one place where the separated spaces of the town's topography came together was the bar where labour was hired early each morning. This was a large room filled with tables and chairs and with a bar running down the length of one wall. And yet even in the bar there was a spatial configuration. The elite stood at the end of the bar nearest to the entrance. Various people leant along the middle areas and of course people had to come up to the bar to get their coffees and cognacs. At the far end were the older men, no longer working. And the room itself was similarly divided. The area most easily reached from the entrance was taken up by a variety of small farmers. Beyond them, along the wall opposite the bar the *jornaleros* sat at tables playing dominoes, waiting for the possibility of being chosen for a day's work. People moved from one table to another to take up conversations but nobody moved from the space of the small farmers into the space of the *jornaleros*. And in fact, while I did eventually spend a great deal of my time with these people and socialized with them in their *barrio*, throughout that first year of my fieldwork I never crossed the space in the bar that was theirs.

Although the upcoming elections in March 1979 were spoken of, it was not until just the month before the elections that

party lists began to be circulated and candidates began to make themselves known. It was assumed by virtually everybody I spoke to that the UCD under Suárez would win. The argument for voting for UCD candidates had nothing to do with Suárez's political platform. Rather people felt that it was important to be represented by somebody who could tap the shoulder of the people in power and get something done. By contrast, one could vote for the PSOE because one was sympathetic to the Socialists, *but it would be a wasted vote*, because they would not get in and so would have no influence on government. In other words people found it hard to have faith that even the limited 'democracy' of a government-and-opposition would actually work. There was in fact very little discussion, either at the political meetings or in the street, about the different policies the candidates would pursue. Those who ran for the UCD presented themselves as the most technically skilled agents in the region –people who knew about agricultural prices, or about recent developments in the shoe industry– and their message seemed to imply that 'politics' was unnecessary given their superior skills and knowledge of the world. Not surprisingly in the later municipal elections the UCD dominated the council.

I want to stress two points. First there is the question of the absence of what Habermas would call intercommunicative practice in the public sphere. Second there are the distortions that resulted from the extreme ignorance of the foreign fieldworker. Obviously the possibility of solving the second problem was greatly restricted by the existence of the first problem.

I think it is very important to convey to people who did not live in rural Spain at that time –and I include Spanish people too– the extent to which people did not speak across the lines I have drawn here. I have proposed two lines –topographical and class-based– and I have suggested that the one reinforced the other. Of course people *did* speak across these lines and also among themselves in the public sphere –on the street, in the bars, and at official meetings of various kinds– but what most struck me was how the minutiae of everyday affairs were used *as a means to push out the possibility of any kind of talk that might raise social or political issues*. Among people of much the same occupational background a habit had grown up that was not immediately modified by the introduction of a so-called

‘democracy’. As elections approached I might find myself in a shop, buying groceries. Somebody might begin to talk about the possibility that agricultural prices might be improved with a new government. Immediately others would shift the conversation to the state of this year’s orange crop, or the behaviour of somebody’s daughter and so on. And *across* these lines the habits of life under the dictatorship accounted for a complete absence of such talk, especially in the case of the *jornaleros/as*.

If you think about the extremes of the well-established land-owners on the one hand and the *jornaleros/as* on the other, then it is clear that the former were accustomed to speak openly of their political views and to assume that the person they spoken to would agree with them –at least superficially. But the *jornaleros/as* by contrast were accustomed to never voicing their political opinions often even among themselves. For many this was not simply a question of being cautious, it was also the result of a long period in which this was simply not what one spoke about: almost as though the physical ability to express one’s views had been unexercised for so long that it did not really work any more.

During the Civil War Socialists and Anarchists had dominated political life in Catral. As a result, the repression that followed was extreme though and –importantly– *selective*, both of particular families and particular individuals (see Narotzky and Smith, 2010; Smith, 2014: 124-149). As a result, while some families were able to avoid castigation and even manipulate themselves so as to benefit from the new regime, others were more savagely dealt with and had no room for manoeuvre. As the years unfolded, this produced different kinds of *habitus* for different groups within the town. In the area of the town in which I lived this resulted in a kind of *banality* of ordinary life, rather than a generalized sense of on-going oppression. But in the barrio of the *jornaleros/as* this was not the case. There the UGT had been operating clandestinely through the later years of the dictatorship (and even before that) so for many people there it was not that quite radical socialist political views were absent, but rather that they were exchanged within a well-protected and closely guarded cohort.

Obviously this situation distorted the image I produced in my early months of fieldwork. It took a long time before I was able to relax with people from that part of the

town and talk to them –not just about politics, but about their life-histories. In fact, the key moment was when my research assistant, a socialist, rented a room in the barrio: at that time people began to understand the kind of work I was doing, and felt comfortable to talk to me.

5. AFTER THE FIRST YEAR OF FIELDWORK

Given the fact that I was collecting life-histories, inevitably once this fence had been crossed, a wide variety of different people’s activities during and following the Civil War emerged, from the opportunism of some to the ever-worsening conditions of others. But an odd thing happened, as though the silencing of histories was a kind of epidemic that even the foreigner might catch. By the end of my first year of fieldwork, I myself –the foreigner– had begun to repress what little knowledge I had gained about the effects of repression on people’s lives. So when Susana Narotzky first went through my fieldnotes and survey material perhaps a decade later, she found my recordings of people’s references to the dictatorship which I had entirely silenced in my own mind. Even as I read and re-read my fieldnotes, I had slipped past my informants’ own references to the awkward moments of their histories.

I hope I have conveyed some sense of what my experience in that first year (Sept, 1978 to Aug, 1979) of fieldwork was like. As the years passed, many things changed. The PSOE came to power and the compromises they accepted were a disappointment to those who had worked clandestinely on the left to help workers defend the conditions of the lives. For them the struggle that they and their parents had undertaken for a real socialist society should now come to fruition, and they felt disappointed and deserted by the regional and national political elites of the PSOE. And then I myself found a Register of those killed by Franco’s regime during and after the Civil War and, as a result, discovered that the major prison camp of Albaterra was just a few kilometres from where I lived (Smith, *op cit*). In raising these kinds of issues, people began to feel able to talk more easily. But, as I said at the outset, that is another story and one that can be read elsewhere.



BIBLIOGRAPHY

- ARGUEDAS, J.M. (1968) *Las comunidades de España y del Perú*. Lima: UNAM.
- BLOCH, M. (1983) *Marxism and anthropology*. Oxford: Oxford University Press.
- LÉVI STRAUSS, C. (1955) *Tristes Tropiques*. Paris: Plon.
- LISÓN TOLOSANA, C. (1966) *Belmonte de los Caballeros: a sociological study of a Spanish town*. Oxford: Clarendon.
- MARIÁTEGUI, J.C. (1928) *Siete ensayos de interpretación de la realidad Peruana*. Lima: Amauta.
- NAROTZKY, S.; SMITH, G. (2010) *Luchas inmediatas: gente, poder y espacio en la España rural*. Valencia: P.U.V.
- PITT RIVERS, J.; ABU ZAID, A.M. (eds) (1963) *Mediterranean countrymen: essays in the social anthropology of the Mediterranean*. The Hague: Mouton.
- PERISTIANY, J.G. (ed) (1965) *Honour and shame: the values of Mediterranean society*. London: Weidenfeld & Nicholson.
- SMITH, G. (1989) *Livelihood and resistance: peasants and the politics of land in Peru*. Berkeley: University of California Press.
- SMITH, G. (2014) *Intellectuals and (Counter-) Politics: essays in historical realism*. Oxford: Berghahn.

Resum

Las habituales dificultades de hacer trabajo de campo en un nuevo lugar se debían a dos factores: la naturaleza de la información antropológica sobre la España rural disponible fuera del país y la limitadísima intercomunicación sobre cuestiones políticas en la esfera pública durante los primeros años de la Transición. El artículo describe las discusiones antropológicas que se producían en la época del trabajo de campo y propone que un largo período previo de represión selectiva explica los distintos modos en que los informantes hablaban del pasado histórico y del presente político.

Resumen

Les habituals dificultats de fer treball de camp en un lloc nou es devien a dos factors: la natura de la informació antropològica sobre l'Espanya rural disponible fora del país i la molt limitada intercomunicació sobre qüestions polítiques en l'esfera pública durant els primers anys de la Transició. L'article descriu les discussions antropològiques que es produïen en l'època del treball de camp i proposa que un llarg període previ de repressió selectiva explica les diferents maneres com els informants parlaven del passat històric i del present polític.

Palabras clave

Transición española, método etnográfico, política

Paraules clau

Transició espanyola, mètode etnogràfic, política