Approaches to Roma and Gypsies from within social anthropology with particular reference to the Anglo-Saxon traditions

Michael Stewart
UCL/CEU

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For most of the past fifty years, anthropological approaches in the British and American schools of social anthropology have by and large been concerned with explaining the survival of the Gypsy way of life and of the population associated with this. As we shall see, three types of explanation have been offered: historical explanations which focus on the distinct origins of Gypsy populations and treat them in effect as a 'foreign' ethnic group with a distinct ethos; structural explanations which locate the persistence of Gypsy populations in the way they have occupied particular niches within the changing European division of labour; a subset of these structuralist approaches are the Foucault-inspired positions that focus on labelling strategies by state institutions; and, finally, we find culturalist explanations which look at the internal coherence of Gypsy or Romany value systems in a self-declared 'holistic' approach.

In recent years, however, a shift away from this perspective – under the influence of a French school of research – has become noticeable and with this new research questions concerning the internal logic of Romany and Gypsy social organisation have come to the fore.

Although the field of Romany studies within anthropology is now respectably represented by full time academics in various departments across Europe, one of the most curious aspects of this field is the way that for most of the history of the discipline the study of Roma and Gypsies was totally missing from the discipline and ignored by its practitioners. Judith Okely notes in her path-breaking (1983) study ….

Indeed, in the early 1950s Frederik Barth's thesis on the Tattare of Sweden was failed at his Norwegian defence, in part it seems because of the peculiarity of the topic (as well as the fact that he had not completed a full twelve months field research and his committee, following E. Evans-Pritchard's instructions from Oxford, insisted on this as the minimum necessary in this new field).

The are several reasons for this total neglect of a subject that in so many ways *today* just seems to present itself to our students and colleagues alike as obvious material for anthropological enquiry. The first of these tells us something important both about the history of anthropology and about the way that Romany populations have subsequently been approached.

Part of the reason for this puzzling lacuna in the field of anthropology throughout the first century of the discipline, lies in the origin of the field and the peculiar place within it of the study of so-called 'hunters and gatherers.'

Anthropology emerged in the late 19th century at least in its British formulation to answer what was in effect a new problem in social and historical investigation. After the success of On the Origin of Species (1859) and then The Descent of Man (1871) it the assertion of humanity's animality could, at least for proto-anthropologists, being taken for granted. Although the mechanisms of evolution were still a matter of debate, the field of biology was clearly going to be able to account for how humans emerged from the broader monkey and primate populations. At the same time there were a series of explanatory schemes for taking humanity from the dawn of civilisation in ancient Greece to the modern epoch. Developing models and descriptive accounts of this process belonged to the sphere of historical investigation. But what of the period between the last ape and first Greek, between Neanderthal man (first skull named as such in 1863 – and taken as a progenitor of early Europeans at that time) and Homer? What happened in this great swathe of time to shift shuffling, grunting ape-man into the eloquent artist and manufacturer of modern humanity?

And of course, the fortunate coincidence that large parts of the planet had been opened up to investigation by processes of colonial 'pacification' and development of rapid (steam) transport provided a ready laboratory for a discipline whose task would

be to fill this gap between biology and history. In a fashion that Herzfeld (1982) and Fabion (1983) have critically identified of using the savage other of the present to stand in for our own past, the 'Savages' of the colonies would provide the laboratory in which data could be gathered for answering these great historical questions. And if we think of the great early works of either British or French anthropology we can see the influence of this model. To take just one work that is still widely read and taught to all undergraduates the world over, in 'The Gift (on the form and logic of exchange in archaic societies)' Marcel Mauss attempts to construct an evolutionary account of exchange in which Roman law represents the beginning of the modern/civilised and various savage societies around the world provide evolutionary stages on the way there (with India standing in lieu of Greece in this case as the pre-Roman, literate civilisation).

Now, for the most part, this model of doing anthropology disappeared, for better or worse, as the discipline was institutionalised in the first decades of the twentieth century. Anthropologist became Africanists, or Melanesianists or Americanists, or even Europeanists, thus becoming identified with the study of regions of the world and their current inhabitants rather than with periods of human history they were trying to account for. With one notable exception. While most colleagues of mine attend an annual regional conference, those who study small bands of people who currently or have recently provided part of their means of subsistence through foraging and hunting, prefer to meet in conferences of 'Hunters and Gatherers.' This is an oddity as we do not find general anthropological meetings devoted to say 'swidden farmers' or 'industrial farmers' or 'manufacturers.' Of course we find economic or development-oriented conferences organised around such technologies, but the notion that technology determines a whole social form captures no one's imagination in anthropology today. And the same, in fact, applies to Hunters and gatherers. As Woodburn long ago pointed out, there is in fact little sociological unity to Hunter and Gatherer societies – with enormous contrasts between the value orientation of African Savannah or jungle dwellers and, say, the Australian aboriginal communities. The only explanation, then of this peculiar continuity of the Hunter-gatherer field lies in the residual trace of the original evolutionary framework into which anthropological data was conceived as contributing. And, of course, Hunter-gatherer studies are not some marginal speciality of our discipline. To take just two

examples, James Woodburn's accounts of the Hadza of Tanzania were central to Ernest Gellner's expansive, long-duree accounts of human diversity and history (e.g. 1989) and collective studies on property and political regimes emerging from H-G conferences have had an influence far beyond their limited field (e.g. 1997).

It is, or so it seems to me, because of this central role of the Hunter-Gatherer field and model in anthropology that studies of Gypsies took so long to emerge. For one of the most striking things about the dominant Gypsy 'model,' if one can call it that, in Europe is its similarity to that of some of the Hunter and gatherer populations. Indeed, at the outset of my doctoral work, James Woodburn, who was then a senior colleague at LSE, advised me that, in his view, there were important similarities between the Gypsies and the hunter and gatherer way of life. This is a point that several of the early ethnographers also made (e.g. Okely, 1983; but see also, in a very different context, Vekerdi (Nyelveszeti adalekok…1981). The problem for anthropology of that period though was the total lack of fit between the role allocated to Hunters and gatherers within the discipline (as time-warped representatives of early stone age civilisation) and the urbanised, motorised and somewhat domesticated way of life of the European Gypsies.

One other more obvious 'problem' was posed by Gypsies that also made them hard to think as ethnographically interesting to anthropologists of the mid twentieth century. Ever since Radcliffe-Brown had articulated his notion of anthropology as a natural science of society at least one dominant school of British anthropology had assumed the object of study was 'societies' with a more or less clearly bounded socio-cultural system. On the zoological model of comparison of species, anthropology would provide a comparative sociological anatomy. Of course, this model of social structure relied on an image taken from the world of nation-states with people a on territory a and people b on territory b. As early as Evans Pritchard's (1940) study, The Nuer, it was becoming clear that this was a naively positivist model of social structure, but despite work on urbanisation in Africa (where culture-contact and melting pot models reinstated neatly bounded groups in the rural outside) and Leach's total reworking of the paradigm of the meaning of ethnicity (1954) it carried on powerfully within anthropology.

Gypsies, fairly obviously, did not fit this model at all. Here was a population that always spoke the mother tongue of the state they lived within (even if they sometimes spoke another language) and who in many respects resembled the populations amongst whom they lived, but who were nonetheless seen and saw themselves as a distinct population. They were, as Judith Okely later put it in a memorable phrase, people who lived in the interstices, behind the gaps in the hedges…. And so, again, hard to place.

There was I think a third reason that made anthropologists wary of approaching fieldwork in this area – and this emerges from the story of the one study from the 1960s that was carried out; a somewhat unconventional study and one that would certainly not have passed muster with Evans-Pritchard. But then it was executed by an anthropologist who had already won his spurs and more with notable fieldwork in central Africa.

In the summer of 1961, Luc de Heusch, an already prominent Belgian anthropologist and protégé of Levi-Strauss, took part in a journey across Europe that formed the basis of a remarkable if short study, or rather set of commentaries on various Romany families he was introduced to, (later published as, 1966). The point of my story lies less in de Heusch's important if miniature observations than in the background narrative. De Heusch was more than competent in several central African languages by this stage of his career but he had no central European tongue and no Romany. His guide, intermediary and translator was another Belgian of his own age who had spent several years of his adolescence living with a Lovara Romany family in the low countries (see Yoors, 1967 and 1971). De Heusch's own account of the journey (that was in part funded as a recce for a proposed documentary film) has Yoors taking him on a journey hunting for the family who had befriended Yoors in the 1930s and during World War Two and the head of that family, a man called Yanko. Setting off in Brussels the pair of explorers move down the Danube, into then Yugoslavia and as far east as Istanbul, without picking up the trail of the elusive Yanko. Now, the odd thing about this journey is that Yanko was, until his death in the 1980s, one of the most famous Rom in northern Europe. It would have taken Yoors no more than two phone calls from one of the café's frequented by Rom, and where phone numbers are left of important persons in the community, to have found his former protector. The

assumption has to be that Yoors, confronted by the challenge of introducing the eminent but perhaps rather formal professor of anthropology, armed with notebook and scientific gaze, Yoors decided that discretion being the better part of valour, he would lead his learned friend on something of wild goose chase down the Danube.

The point being that traditional ethnographic method and the study of a politically marginalised and persecuted minority in our midst do not sit so easily together. It took a new, younger generation of anthropologists, who had an inherited confidence in their method – thanks to the very rigour of their predecessors work – to adopt a more informal and soft-pedalled approach to ethnography.

The result of the more or less systematic abandonment of the field by professional social scientists was that until the 1980s Romany studies in Britain, France and America was a field overwhelmingly dominated by the approach of folklore and, moreover, its amateur incarnations. The only journal in the field (then only appearing occasionally as its editors aged, unreplenished by a younger generation with the exception of the British civil servant, Sir Angus Fraser). It is for this reason that Judith Okely spent a fair part of her early work clearing what must have felt like the Augean stables.

For, just as Michael Herzfeld demonstrated with reference to Greek ethnology (Laotika), this was a discipline which had never got beyond the use of peoples of the modern world as props with which to stage the nation's past. Interestingly, for the folklorists, the hybrid, composite nature of Romany culture posed no problem: unlike those who cleaved to the anthropological model of closed and bounded cultures, folklorists were happy to accept that a mixing of 'old' and 'new' elements had taken place – but, and here they reinsert the myth of original, autochthonous cultures, they insisted that they were capable of distinguishing and therefore able to define the pure and the impure elements and weed out the hybrid from their data and analyses. It was this perspective that led to the constant search for 'real Romanies' who speak Romany and the rejection of the 'mixed breed', or 'didicoi' as they were contemptuously known in English at least. In consequence, and here echoes of anthropological primitivism are all too obvious again, the more isolated the Gypsy population the

more pure they were imagined to be. Industrialism was thought to be the enemy of their way of life as it would corrupt their pure nomadic world.

One can see that it took some courage for a junior doctoral student at Oxford in the 1970s to declare that this was going to be her field of study and set off 60 kilometres up the newly opened motorway from London to a parking lot on the edge of a main road. Neither academic field nor physical location promised much.

Anthropological Approaches – three traditions and their overlapping zones

The current efflorescence of anthropological work on Romany communities has basically one intellectual root – the work done in the UK in the 1970s by Judith Okely in the first instance and one practical root provided by a professor of sociology and the first director of a degree carrying the title, Romany studies, Thomas Acton. At more or less the same time as these two coeval scholars were at work in the UK there were two works published either in or about American Roma at least one of which remains one of the best ethnographies ever written about Rom but neither of which had the intellectual impact of Okely's work. Nor did either author have the long term practical commitment to field-building and seminar work that Thomas Acton has heroically demonstrated from his offshore base in the University of Greenwich.

From the point of view of this essay, focussing on the anthropology of Romany communities, Acton's work is important above all for the way he provoked Okely to take an opposing and contrastive stance. It is also, however, important for legitimising in the field of sociology (as Okely was later to do within Anthropology) the study of Romany communities and their political organisation. If you were a young scholar interested in Gypsies in the 1970s in the UK it would have been to Acton or Okely you would have turned and never to the by then dank and moribund world of the Gypsy Lore Society.

So, from the anthropological point of view, Acton's two great contributions were to launch the study of Gypsy politics as those of an ethnic minority like any other, a particular case of ethnic mobilisation within the British polity. Though statistically

almost insignificant (travellers numbered roughly 0.01% of the total population) conflicts over access to land use were intense and publicly prominent and Acton was able to provide a focus for all concerned with these issues. Secondly, Acton laid siege to and utterly demolished the castle of the Gypsy Lorists with their motte and bailey constructed from the pure Romani and Didicoi distinction. After Acton, it was clear that the pure Romani were the Gypsies over the hill one saw on holiday and the dirty family down the road in the lay-by on the way to work were always, inevitably, the Didicoi – so one man's Roma were another man's bastard hybrids. In other words the distinction had no sociological value whatsoever, whatever its sociological interest as an object of analysis. This was serious scholarship combined with engaged politics. Acton was an early campaigner for new legislation to provide powers to enforce local authorities to offer land to Gypsies to stay on. And if the social-democratic model of a generous state provisioning its people has been proven to be rather less successful than the privatised version more popular today among English travellers of private site provision, this was more a matter of the times than anything else.

Acton's work came straight out of what were the beginnings of ethnic and racial studies in Britain. Okely, by contrast, developed in what was the British department with both the strongest unbroken tradition of being at the forefront of anthropological reasoning (before this was driven underground by the antics of its own leading members) and the department most open to innovative approaches and fields of study at this time. Indeed, though the dominant intellectual figure in Oxford had moved in his later career towards claiming that the main goal of anthropology was to be restricted to the rather limited task of translating concepts from one culture to another, in his earlier work Evans-Pritchard had demonstrated a profound understanding of what a Durkheim-inspired sociology might look like with his demonstration that Nuer social structure was better understood as two conceptions of what constituted the social in Nuerland - the ideas of the lineage and segmentary opposition – rather than an empirical phenomenon like the anatomical structure of the human skeleton. On the ground, E-P observed, the empirical pattern of human settlement was extraordinarily diverse and complex. Nuer and Dinka, to take but one example, lived and worked side by side. But conceptually, Nuer operated as if the Dinka did not exist and conceived their social structure in the terms he described.

In British anthropology the intellectual sophistication of this stance was, in some respects at least, unique. Its influence on Judith Okely can only have been indirect, mediated by her teachers like Shirley Ardener and the Lienhardt brothers, but from where I write it seems that this notion, that a social system exists fundamentally in the conceptions of it actors and could never be reduced to the mere expression of some putatively more real facts on the ground must have liberated the field and made it seem possible to do fieldwork in some parking bay on side of a motorway. Because, from this perspective the lay-by outside Coventry is no different from the swamp in the Wadi – it is just the physical location where humans, with similar imaginative capacities, set about trying to construct meaningful and enduring social relations among themselves.

Judith Okely's most renowned contribution was, of course, the hypothesis that English traveller Gypsy culture might have an indigenous origin at least as significant as the role of any foreign, imported culture. I am not sure how Judith would see this argument now, but 25 years later it seems to me that at least part of the inspiration of this stance was to reject and effectively bin the obsessive and totally paralysing concern of the folklorists with 'origins' and, in particular, exotic origins. Okely's implicit value judgement was surely that it makes no difference whatsoever whether a way of being in the world comes out of a distant or a local history – the origins of a way of life should have no bearing on one's assessment of its value. As she pointed out, the wretched notion of the bastard or hybrid in the lorists work was the direct descendant of ideas that the Nazi racial scientists latched onto. In their stead, Okely demonstrated she had found a coherent cultural pattern articulated around a series of tightly interrelated values, most importantly the value placed on personal and political autonomy. Autonomy in work, meaning a great value placed on self-employment, autonomy in politics – the absence of leaders and reliance on tests of physical prowess to resolve disputes – and autonomy in residential and kinship arrangements, which she saw as the basis of Traveller mobility lay at the heart of social arrangements, much as lineality and segmentary opposition lay at the heart of Evans-Pritchard's account of Nuer social arrangements.

But Okely also drew on two related models of ethnic relations to provide a radically new way of conceiving relations beyond 'the tribe.' In 1954 Leach in trying "to

understand why Kachins should be different from Shans” (1954: 288) introduced a whole new way of thinking about ‘ethnic’ or ‘tribal’ relations. in which the protestations of members of 'ethnic groups' (e.g. to common origin or common 'culture') are viewed in a sceptical, irreverent light through being placed in a wider historical context (1954: 12). Though one of Leach's aims was "to try to understand why Kachins should be different from Shans" [1954: 288] he did not wish "to represent the variations of Kachin culture as characteristics of 'tribal' entities of any scale" (1954: 292). Rather than hunting for tangible, mappable ethnic groups Leach saw that the nature of communities people live in "is a question, in part at any rate, of the attitudes and ideas of particular individuals at a particular time" (1954: 286), and that the ethnographer’s job was to describe the ins and outs of an ideology, and through that, show how life on the ground reflected "differing forms of a compromise between two conflicting systems of ethics" (1954: 292) rather than the working out of some integrated ethno-cultural logic. In Leach's work, the features of the culture of a people (language, local organisation, religion) that become important in a people's discourse depend on the political context in which they are acting (1954: 290). "The significance of language group solidarity is not something that can be determined from first principles...." (1954: 46). But the precise idiom of unity used in people's discourse is highly important since "the transition from Kachin-type organisation to Shan-type organisation involves the substitution of a relationship based either on common lineage or affinal dependence" for one conceived as landlord-tenant [1954: 288], that is a change in the ideology of the relations that link people together in communities [1954: 13].
As Frederik Barth has noted, it took a while for Leach’s iconoclastic stance, the ‘politics and ecology model’ as he labelled it, to seep into the discipline and this occurred in part through Barth’s own work (see, http://www.alanmacfarlane.com/ancestors/barth.htm), which, together with Mary Douglas’ account of symbolic boundary maintenance, was an explicit influence on Okely’s approach.

Barth’s initial recognition related directly to the field of Gypsy studies (and surely not by accident since his own first fieldwork had been with Tattare [1975]). He argued that since we could no longer assume 'that cultural variation is discontinuous', loose uses of the term 'culture' to describe the object of anthropological study were no

longer acceptable. The term 'culture' did not describe any discrete phenomena on the ground. Nonetheless it would be possible to 'operationalise' the concept of Culture, in the sense of shared patterns of behaviour. This could be done by studying groups on the ground which used features of their cultural behaviour to mark themselves off from other like groups. Such groups would use "a limited set of cultural features" to define themselves so that most of the "cultural matter" associated with a population is not linked to the group's boundary (1969: 38). Cultural items (language, religion, clothing) can change, or disappear so long as a set of boundary markers is maintained. Individuals and groups can cross ethnic boundaries, leaving the boundaries, the groups, intact. Only if changes are made to the select markers of group identity does the 'ethnic group' itself change. Like Leach, Barth talked of ethnic categories as being "an organizational vessel that may be given varying amounts and forms of content in different socio-cultural systems" (1969: 14). He suggested that "tribe, caste, language group, region or state all have features that make them a potentially adequate primary ethnic identity for group reference" (1969: 34).

Now, the combination of Barth's focus on boundary maintenance and Mary Douglas' insight that notions of cleanliness and dirt are moral classifiers for the social environment and come to symbolise important social boundaries, allowed a revolution in the way Romany cultures could be described. One of the old saws of the folklorists had been to stress the superstitious beliefs of the 'pure' Romany speakers in notions of pollution and ritual cleanliness (ref JGLS 1926 articles). Thomas Acton himself had felt obliged to deal with this question, arguing that such customs were better understood as a pragmatic response to conditions of life on the road, without the hygienic facilities of settled folk. Okely saw, however, that if you put together Barth and Douglas you could dispose of the distasteful orientalism of 'magical beliefs' and provide in its stead an account of English Gypsy cultural distinctiveness that made not just symbolic but a kind of plausible psychological sense.

The Traveller woman's body became the symbolic medium through which an ethnic distinction between insider (pure) and outsider (impure non-Gypsy or Gorgio) could be lived. Animal symbolism reinforced this sense of living in a Manichean world with non-Gypsy pets like the cat treated as polluting and Gypsy-life creatures that lived in the shadows in between civilisation and the wild (the hedgehog) acting as a kind of

totemic animal. This was a brilliant synthesis of theory and data and one that had a marked influence on authors, like myself, from the next generation down.

Although widely respected in the field as the pioneer who struck out first into what had been ethnographic terra nullis, Okely's socio-structural approach had less direct influence on what one might call the Franco-Italian school and in particular the work of Leonardo Piasere and Patrick Williams than on her British colleagues like myself. Aware that the British tend to relegate 'culture' to a by-product of social structure (pollution beliefs as expressive of social boundaries, for instance), these two authors set out in a slightly different direction. The overarching and dominant influence on their work was Louis Dumont's notion of holism and hierarchy. Dumont himself had, of course, been inspired by his reading of Evans-Pritchard's early work (1968) but not in the way the English followers had understood it (see, Dumont 1968, republished in English, in Beattie and Lienhardt, 1975). Dumont saw in *The Nuer* an early attempt to grasp the cultural logic of a holistic system of value. And his French disciples (Piasere did his PhD in Paris at more or less the same time as Williams) in our field likewise tried to grasp what Williams called 'le système Tsigane.' As Dumont had argued any holistic approach inevitably involves a hierarchy of values in their orientation towards and reproduction of 'the whole system.'

This is why Williams spends considerable time in his doctoral work, published as *Marriage Tsigane* (1984) attempting to define just what to include and exclude from what he calls 'le système Tsigane', noting that no coherence and no system can be found trying to integrate the notions of *Tsigane* and *Rom*. They belong in effect to two parallel but coterminous universes of meaning. The only système of classification that the ethnographer of the Rom can concern himself with is that which articulates a whole around the opposition of Rom to Gadzo and which allows the Rom to construct a closed world of their own in the midst of the Gaze, as Williams demonstrates with his two tour de force in his first and last chapter where the request for a bride (the 'marriage' of the title) is first described and celebrated as a moment when the whole value scheme of Romany social life is brought into view and then analysed as such.

Williams was, one can infer, influenced by Gropper's great ethnography of New York Romany families – probably the deepest and most accurate single general ethnography written about the Roma by someone who had years of experience working with her informants. But otherwise he has carved a path of his own. At the heart of his work, it seems to me, lies an ethnographic insight encapsulated in an article contrasting economic strategies in Paris and New York: the Rom tend to seek invisibility, to find a curtain behind which the Gaze cannot observe them and will never know the wonderful secret and pleasure of the true way of life.

Williams wrote his doctoral work aware of Okely's but it was only at the 1986 conference of Etudes Tsigane in Paris that Okely, Piasere and Williams met up. And it was only with the publication of Williams second work that he articulates in an entirely indirect fashion his dissatisfaction with the British socio-structural approach. The moment arises in a chapter where he turns to consider the 'manus among the Gadzos.' Williams notes that in this context, 'there is a temptation to see specific traits in terms of adaptation. After all, it is hard to overlook the fact that Manus affirmation has to occur I the midst of another society, and thus there cannot fail to be some correlations between the nature of this society and the nature of this affirmation, or, more precisely, between the nature of the latter and the fact that it is expressed within a world defined by others. But I don't think that these correlations can be limited to a deterministic interpretation.'

In the gentlest possible way, then Williams reject the 'deterministic' explanations of those who would derive ore reduce the Gypsy or Romany way of life to a response to their economic or political niche and their relations with the non-Gypsy world. And here he makes a crucial, if subtle distinction. It is absolutely true he says, that the Manus live 'in the world of the Gadzos' and not 'in the same world as the Gadzos.' So they relate to the whole world outside their own, nature included, through their relations with the Gadzos. But, while co-existing with the Gadzos the Manus detach 'themselves form them,… put[…] themselves at a distance, which precisely cause them to become Manus and the Gadzos to become Gadzo.' (all above p.29). All of which reminds me – though I have no reason to think he knew this tiny remark in a fifty year old text still much read in British Anthropology - of Leach's assertion that Kachins and Shan though both forming 'groups' in some loose sense were not really

the same sort of thing, and that to call both these identities 'ethnic' would miss a profoundly important aspect of the change involved (1954: 288). It is meaningless, at least for the Manus, to talk of them as an ethnic group like any other in France, for that is not how they conceive themselves. As if they were the structural equivalent of the Beurs or the Italians of Savoie, or the Catalans of south-central France.

The challenge this poses to traditional theories of Roma as an ethnic minority like any other (as in Acton's account but more recently say Vermeersch and above all Barany) should not be underestimated. Indeed there is a strange difference in the relationship between the position of ethnographers of the Roma in general and Roma activists and, say, anthropologists of the 1950's and 1960's who were able to align themselves much more easily with the decolonisation movement. While fully aware that the Kenyattas and X were members of a new elite, anthropologists could see that the national projects being articulated offered an inspiring vista for the post-colonial future. I can think of no anthropologist working with Roma who has any romantic feelings at all about the self-declared 'Roma Rights' movements. Indeed on numerous occasions anthropologists have had to point out the dramatic difference between the huge and transformative effects of neo-protestant churches and the total irrelevance of the 'ethno-political' movement.

Williams' work and his stance has begun to attract an ever wider range of scholars who while in no sense imitating him, are adopting this Dumontian approach to grasping a cultural logic and viewing Romany value systems as phenomena sui generis rather than determined by the relationship between the Gypsies and the non-Gypsies. Of those professional anthropologists who still work on Romany issues (and publish in English, French or German) Paloma Gay y Blasco is perhaps the senior figure with Elizabeth Tauber bringing young talent to this approach.

Gay Blasco's work on Spanish Gitanos is noteworthy for a number of reasons. First and foremost is her rigorous demonstration of the construction of the Gitano notion of the person around a set of sui generis values, and in particular understandings of male and female gender. Conceptions of the righteousness of male behaviour and the modesty of female behaviour ground and constitute Gitano practice – they are not a result of practice. Blasco gives numerous demonstrations of this theoretical stance but

her examination of ideas and practices around conception and the construction of a specifically Gitano female body provide a splendid point of entry to those unfamiliar with this work (1997).

Blasco's work also began to formalise an insight, less developed than perhaps it might have been to which, perhaps, Blasco will again return: the absence of any sense of 'society' among the Gitanos Whereas, I for instance, had lazily talked in my thesis title about the 'preservation of Romany community' under socialism, while knowing full well that there was really no coherent definition of community, Blasco confronted the lacuna head on. For her the Gitano world is constituted by the construction of the male and the female. This is a point that Tauber also picks up with her observation that the widely used term in politically correct talk, Roma & *Sinte* means in English, Roma and *relatives* since the term Sinte is used in German Romany to mean related persons as in the common phrase '*amare Sinte*' (our family). There are a number of things which follow from the lack of any social order over and above the network of relatives a person constructs, but of most immediate importance is the enormous challenge posed to any person or movement that wants or claims to represent 'the Sinte' or 'the Gitanos' – since the very notion is incoherent.

Blasco notes in her examination of the manipulation of biology by the Gitanos that the 'emphasis on proper sexual behaviour gains much of its strength through comparison with the Payo life-style. To the eyes of the Gitanos the Payos break all the moral rules and particularly those that have to do with relations between men and women…'Evils' such as pre-marital sex and divorce are thus rampant among Payos because the women lack self-control and the men fail to control them' (525). As such this is a familiar point in the literature. As Blasco notes, Okely pointed out this kind of disdain to the non-Gypsy in an early article. What I would like to point to here, however, is another familiar feature of this type of claim. In post-Franco Spain, the Gitano stance is, in a certain way, a claim to Spanishness, or at least a conservative version of that. Like the Rudari in Romania, who claim to descendants of the Dacians (and thus more Romanian than the Romanians) and the Hungarian musicians who take such pride in having been 'ambassadors for their country' under socialism (Jaroka, nd.), to my eyes this looks distinctly like a claim not just to moral superiority in general, but moral superiority in terms which the non-Gypsy population recognises.

This takes me back to what I see as one of my contributions to this field – even if it this is hard to render compatible with Williams' and Blasco's stance. In part perhaps because of my initial research in the socialist period in Hungary, I have always found it hard to see the Rom I know controlling the range and forms of involvement with (dominant) others, or the terms of their relationship with the Gadzos. Or to put this more poetically. I am constantly struck by the contradiction among many of the families I have known for over twenty years now between their relative poverty and their proud self-understanding as princes of their world. I recognise, of course, that the social *is* our understanding and imagination of it – a nation only exists if it is imagined and having imagined people as co-nationals one behaves to them according to understandings that only arise through their cultivation in institutional settings that are grounded in the notion of 'nation.' And yet, I would insist, that what we might call these mental models, are not the sole reality that we as humans live with. And so, I see in gestures like the claim to Dacian ancestry or to be the truest Spaniards a claim to belong to the world in a different mode than that of Rom or Gadzo, a recognition of another way of seeing the world.

There is, however, a delicate balance to be captured here. Are the extraordinary palatial houses of the Cortorari in Romania – each new one built higher than its predecessor so it may be better visible to the Romanians in the centre of the village – a kind of camouflage for the real source of social worth and value in these communities – the family heirlooms, that is the pewter beakers that contain a kind of *mana* embodying the virility, fertility and luck of the patriline. And while the houses are visible to all, but one suspects of little interest to the Rom, the cups are invisible almost all the time, often hidden among the non-Gypsies who barely suspect the true nature of their importance. Working one's way interpretively through these fields requires the kind of ethnographic skill at uncovering modes of hidden cultural intimacy that Patrick Williams' work exemplifies.

<u>An alternative model</u>

I think it probably true to say that all the scholars I have discussed would concur, though for different reasons, in their admiration of and gratitude to the radically new approach to our field offered by the three Dutch scholars, Wim Willems and Leo

Lucassen and Anne-Marie Cottar. I say this even though I think none of us anthropologists would be convinced that their post-foucaultian stance can provide the whole story of the formation of Roma social formations across Europe in the last five or six hundred years.

Here I will focus particularly on the contribution of Lucassen and Willems since Cottar's work is more socio-ethnographic on an indigenous traveller community in the Netherlands (the *wohnwagenbewhohner*). Lucassen and Willems actually come at this field from rather diverse stances. Whereas Lucassen approaches the field from an interest in the official treatment of migrant labourers and the mobile self-employed, Willems mixes literary and intellectual history to account for the emergence of the field of study known as Gypsy Lore and, in our time, Gypsy or Romany studies. Lucassen's approach could be summarised as a radical re-statement of Okely's iconoclastic hypothesis that the indigenous origins of English Traveller-Gypsy identity were as important as any foreign or exotic ancestry. Lucassen whose doctoral work was on the development of the police category of '*Zigeuner*' looked at the development of a whole system of wanted notices and police circulars in order to deal with the mobile population of criminals and travellers who tended to evade identification through being able to alter identity from one jurisdiction to another. Lucassen saw in the archival sources that only some of the people whom the police came into contact with because of their status as migrant labourers ended up being classified as Zigeuner in books like Dillman's synthetic 'encyclopaedia' of Gypsy families published by the Bavarian ministry of the interior (1905). Lucassen is very struck by the fact that there is nothing to distinguish these people in socio-economic terms from others who do not appear with the *Z* letter by their name. The same occupations (xxxx) provide a livelihood, the same routes are taken, the same conflicts arise with the local authorities when migrants are reduced to begging when there is not enough work on offer in the place they have ended up. The only significant distinguishing feature is that those people who travel in families rather than as single men (or, occasionally, women) tend to be labelled *Zigeuner*. And why should this be? The history of poor law and welfare arrangements make abundantly clear that since the care of the poor was the responsibility of their own locality, local authorities had a significant interest in finding ways to exclude categories of people who might otherwise fall on their charity. And while it might be easy enough to encourage an

individual labourer to move on – and not cost so much to provide temporarily for them if they were incapable of so doing – the problem of families with small children, who could call on Christian sentiments of pity for the destitute, was altogether more intractable.

It was, in brief, such migrant labourers and their families who fell foul of the way welfare support was organised and who formed part of that 'hard-to-identify' mobile mass whom the early police forces targeted for identification, who became the ancestors of the *Zigeuner* of the German lands (and, by extension, the Gypsies of Europe). Since they are treated as a pariah group, soon enough they acquire the sense of having something in common with others like them. Identity, in this case at least, is little more than an effect of identification.

There are limits, however, to how far you can take this sort of analysis. Thomas Fricke, in his doctoral work – that is sadly almost unavailable due to the small print run of his publisher – provides an extraordinarily rich picture of the lives of all kinds of migrants whose lives appear in the official archives. Precisely because his chronicle covers a range of the vagrants, nomads and travellers of the late 18th Century, some of them stand out. A letter, written from a prison cell in xxx in a personally invented script tells the author's wife, in their native tongue, Romany, of his misery, hunger and fear. Languages, as I have said, are handed on in families and the notion that the author of this letter shared nothing but his official label with the speakers of German Romany today simply beggars belief.

Roma and anthropology in eastern Europe today

As Csaba Pronai recognised many years ago, one of the bridges which any Hungarian (or indeed east European) anthropology has to build is one that crosses the field of Romany studies. To this end, in an unparalleled series of translations and intellectual contextualisations Pronai has attempted to bring the best as well as the range of western anthropology to his local audience.

With that work in mind, and with Pronai's protégés and students like Kata Horvath and Cili Kovai now entering productive careers of their own, alongside more senior

scholars like Peter Berta – as well, of course, as a host of extremely talented, qualititative sociologically trained researchers - where should a socio-anthropology of the Roma be headed and what might anthropology have to gain from a fuller engagement with this diverse field?

Some final notes:

1. Hungarian anthropology will miss a trick if it continues to turn its back on an obvious source of both interesting theoretical issues and of public importance if it maintains its relatively restrained engagement with this field. Is it not strange that the best (admittedly unfinished) doctoral work on the Beas [at least that I am aware of] is by Gabor Fleck (trained at ELTE sociology) and that there is no ethnography of this fascinating population and their history?
2. The whole question of social scientific writing about Roma cries out for anthropological engagement. As we are all aware, one of the contributions anthropologists/ethnographers bring to a field is to ask how the subjects themselves view their predicaments and possibilities – with marginalised and partially impoverished populations like most of the Roma this contribution is all the more necessary.
3. Anthropology in general is at a bit of a cross-roads – and, at least in my view, this will become obvious in retrospect twenty years from hence. There are strong pressures towards what in reality are regional studies under any other name (and I particularly include those who focus on so-called glocalisation in this category); there are also strong pressures to rendering much anthropology policy oriented (and those of you who know me will realise that I have no objection to the cultivation of these links). What is also emerging or re-emerging are two main ways of thinking comparatively and in the long-term about human social formations. In both these directions, an anthropology engaged with Roma might make very significant contributions. Let me just point to three issues: the study of cultural creativity and social diversity might be powerfully re-charged through study of a population who – unlike most Europeans, swept along in the nationalist mythology – are not only uninterested in their own supposedly unique cultural genius but moreover positively celebrate their ability to adapt, adopt and mimetically assimilate practices they find among their neighbours. Nationalist ideology makes us see

this as trivial and uninteresting, but of course most of human history (including the spread of the modular ‘national idea’) illustrates the central role of mimesis in human history. Second, Romany populations provide a series of strange paradoxes to any social theory that wants to think in long term social, quasi-evolutionary, history - a point I will elaborate on in my presentation. Finally, the growing collaborations between psychological and anthropological research provides a whole new area in which the study of Roma might provide grounds for important advances in the field – as well as making a possibly important contribution to educational policy and challenging some of the nonsense that is spouted as wisdom by the current educational establishment.