

# THE PRODUCTION OF KNOWLEDGE AND THE PRODUCTION OF HEGEMONY: ANTHROPOLOGICAL THEORY AND POLITICAL STRUGGLES IN SPAIN

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“Communication is the process of making unique experience into common experience, and it is, above all, the claim to live. For what we basically say, in any kind of communication is: 'I am living in this way because this is my experience'... Since our way of seeing things is literally our way of living, the process of communication is in fact the process of community: the sharing of common meanings, and thence common activities and purposes; *the offering, reception and comparison of new meanings leading to the tensions and achievements of growth and change*” (emphasis added, Williams 1984:55)

The project of a World Anthropologies Network (WAN) challenges anthropologists to engage in world wide communication not only among scholars but also with the knowledge produced in non-academic contexts and in non-scientific realms of experience. The desire to create a new form of communication stems from the will to be alive, to form a community that will allow us growth and change in unexpected directions. This ecumenical objective however, has to deal with the awareness that all knowledge is produced in, and seeks to create, particular fields of power and we are not exempted from it ourselves. The tension in the WAN project is one between 'epistemological tolerance'<sup>1</sup> with its paradoxical liberal, modernist taint, and the setting of an epistemological program which has a definite grounding in various emancipatory political projects. It is this tension I want to address in the paper.

My attempt in this paper is to explore the positionalities of three probably incompatible discourses that represent, however, serious attempts to go beyond the easy disqualification of particular non-hegemonic forms of knowledge as 'epistemological nativism'. The first is the discourse of singularity and autonomous consciousness that raises the issue of the unavoidable opacities of translation we should contend with if we choose to recognize the 'heterotemporality' of plural histories and forms of knowledge (Chakrabarty 2000: 72-96). The second is the discourse of active participation in a local political project as part of the production of knowledge, where commitment to a collective struggle against (or for) some form of domination and injustice requires both concrete experience and a certain urgency in the necessity to identify historical forces that are seen to be substantial and material (this would be exemplified by the work of Moreno or Sarkar below). The third is the discourse of ethnographic realism (Terradas 1993), representing an attempt to revive anthropology as a comparative endeavor by developing the explicitness embedded in the realist methodology of early ethnographies. By engaging with these three methodological perspectives my aim is to raise some issues about how a World Anthropologies Network could provide a real communicative space for fostering growth within anthropological knowledge.

### Introduction: Andalusia and the Rest

Let me start with a piece of local history of the awareness of a particular form of anthropological knowledge in Spain. In 1973 in the *Primera reunión de antropólogos españoles* (Sevilla), Isidoro Moreno (University of Sevilla) in a paper about "anthropological research in Spain" addressed the production of anthropological knowledge in Spain, and in particular in Andalusia. He described it as a double colonization: 1) spatial --Spain conceived exclusively as a territory full of informants, as an object of study-- by foreign anthropologists (mainly North-American) who had offered nothing valuable "to the knowledge of Spain, the progress of Spanish anthropology or the development of anthropological theory"; and 2) theoretical --through the mechanic application by local anthropologists of concepts and theories developed by Anglo-Saxon scholars to deal with other realities (1975:325-6). In a paper written ten years later Moreno (1984) develops this early insight more thoroughly and tries to show how two very different ethnographies of Andalusia, one by a British structural-functionalist (Pitt-Rivers), the other by an American Marxist anthropologist (Gilmore), both suffer from blatant forms of ignorance that stem from their pragmatic and superficial involvement with the local history, economic realities, political conflicts and symbolic expressions of Andalusia.

In both community studies, once again, Andalusia *provides only the field*, and the excuse, for useless academic polemics that take place in other countries and for obtaining degrees and status for professionals of anthropology that have little interest really in the present and the future of the Andalusian people. And this has only one name, that of *anthropologic colonialism* (original emphasis, Moreno 1984:73).

Let me highlight here "useless academic polemics", "obtaining degrees and status for professionals of anthropology" and "little interest really in the present and the future of the Andalusian people". Although these remarks echo some of the contemporary critiques of the production of anthropological knowledge (Asad 1973, Fabian 1983) as well as such path breaking papers as F.H. Cardoso's critique of the a-political "consumption" of dependency theory by US scholars (1977) and the earlier phase of subaltern studies (cf. Pouchepadass 2000), they were produced without knowledge of them, that is without the sense of participating in a wider polemic about anthropological knowledge. Rather they were conceived as part of a personal experience as an anthropologist *and* as an Andalusian nationalist of a marxian background, strongly engaged in political participation. Moreno's critique stems from the felt inadequacies of 1) separation of theory from practice, 2) the reproduction of a structure that validates what *counts* as anthropological knowledge, that is, the patterns for acquiring professional status at the center and 3) the lack of engagement --personal and political (i.e. "present and future of the Andalusian people")-- of the researcher, that is, the objectification of the anthropological subject. I will come back to this later on. The story, however, has more developments to it. In 1997 in his contribution to "Provocations of European Ethnology" Michael Herzfeld points to the

"various responses of Europeans to the, sometimes startling, discovery that they are already under the dissecting gaze of anthropologists. This is *both* an intellectual refinement of a **covert racism** (of the 'we are not savages' variety), at one level, and

at another, paradoxically, a **late** version of the colonialist critique of anthropology. These are not necessarily mutually incompatible stances. Taken together, however, they indicate how powerful and pervasive is the model of **occidental superiority** and the idea that rational scholars are somehow free of cultural constraints or the messy vagueness of symbolism (see especially Connor 1993; Huntington 1993). Moreover, they reflect the **perpetuation of colonialist assumptions** even, or especially, within the optimistically named 'new Europe'. This appears with notable force in the **epistemological nativism** of certain Spanish anthropologists (e.g., Llobera 1986; Moreno Navarro 1984), although rarely those in the national capital [no names or references given], a contrast that shows how easily subnational hierarchies may **reproduce international inequalities** (see Fernandez 1983)". (Original emphasis, bold added, Herzfeld in Asad et al. 1997:714).

What I find revealing is the way in which Herzfeld's discourse is an unforgiving disavowal of European [Southern European] anthropologists as such, that is, as colleagues who could share polemic anthropological ground. I find it particularly revealing as well that he cites a Catalan --Llobera-- and an Andalusian --Moreno--, both overtly peripheral nationalists and not particularly Spanish in their 'self-presentation'. Herzfeld creates a breach between himself and the likes of him (reflexive anthropological scholars at the centers) and the 'nativist' intellectuals, through *conflating* their position as anthropological objects of foreign (Anglo-Saxon) anthropologists' gaze and their arrogance and "occidental superiority as rational scholars". As a consequence, he invalidates their scholarly but *obsolete* methodological critique of knowledge production in anthropology on the grounds that it is "covert racism" and "epistemological nativism" and in fact negates the possibility to communicate with these scholars on common professional ground. Indeed, the problem that Herzfeld seems to have with the epistemological nativism of peripheral Spanish anthropologists stems from his view of them as akin to nationalistic folklorists and therefore subsumable under his critique of the methodological 'distancing' and conceptual 'fixedness' of nineteenth century folklorists (Herzfeld 1987). As a corollary, it is then based on his epistemological rejection of the blatant and explicit political intent of their intellectual project. European contemporaneous 'anthropologists' who are peripheral nationalists are placed in the field as objects of study and significantly precluded from entering the 'a-nativist' (scientific?) epistemological debate in anthropology.

Why is Herzfeld unwilling to engage in a serious epistemological discussion with these anthropologists? Why the very arrogant and dismissive tone of his critique? Why a form of 'colonialist critique' that has been voiced before and after by scholars inside and outside of the US and the UK since at least the late 1960's (Gough 1968, Berreman 1968, Asad 1973, Fabian 1983) is considered unacceptable in a Southern European location? Because it comes from "European" scholars? Because it comes from "Spanish" scholars? Because it comes from (peripheral) nationalist scholars in Europe? Or because he feels that there is a competition for field and knowledge production from these local scholars,<sup>2</sup> something that could undermine Anglo-Saxon authority in that 'area' of study, much in the same way as the subaltern studies school has managed in a very short time to substitute in the centers of knowledge production about South Asia most non-South-Asian origin scholars?<sup>3</sup> Is it an appeal to openness or a practice of closure? We will follow these themes latter in the paper.

### Passionate epistemologies and the 'dissimulation of dissimulation in the north'

I want to present another strand of the story of the polemical 1984 article by Moreno. In his critique of Pitt-Rivers' classic of Mediterraneanist anthropology, *The People of the Sierra* (1971[1954]) he writes:

"When Pitt-Rivers in the [second] preface to his book --that, by the way, was not published in the Spanish version-- declares that his objective has been to explain, through an ethnographic example, Georg Simmel's essay about secrecy and the lie, he congratulates himself at not possibly having a better example than Grazalema to prove it, given that --and this is a literal citation-- 'Andalusians are the most accomplished liars I have ever encountered ... one never knows what Andalusians think'. / What evidence did Pitt-Rivers have to assert this? His two year experience in Grazalema. How would we qualify the assertion? (...) it shows a total ignorance of the meaning of popular Andalusian culture as a *culture of oppression*, where a series of traits --such as, for example, mistrust disguised as sympathy with strangers, English anthropologist included-- are a mechanism of defense, fruit of centuries old collective experience, in face of that which is external and unknown, which is always something potentially aggressive and source of possible misfortunes (...). In any case, this quality of great liars that the aristocracy of British anthropology attributes to us, he should have looked into it in depth in order to explain it, instead of presenting it, as he does, as if it was a cultural explanation". (Moreno 1984:73).

The interest of this critique in the context of our paper appears if we compare it with a recent development in US anthropology that has a methodological objective and uses (among other material) precisely this preface to the second edition of Pitt-Rivers' *The People of the Sierra*. I am speaking of Michael Taussig's *Defacement. Public Secrecy and the Labor of the Negative* (1999).

In this rather obscure and philosophical work, Taussig presents a critique of historical *origins* and social *functions* as methods to approach an understanding of social reality. In his characterization of reality passion and empathy seem to be better ways into cultural understanding than rational analysis. But the position of the anthropologist or historian in communicating this reality, or even the need for doing so, remains obscure in his account. Taussig uses Pitt-Rivers' preface (as well as the entire ethnography) to prove a methodological pitfall predicated on the dialectics of secrecy, revelation and public secrecy. The variable geometries and tensions between reality, performance and authorial narrative involve 1) anthropological subjects' dealings amongst themselves (as observed and interpreted by the British anthropologist) 2) the interaction between the anthropologist and his subjects of study, and 3) between the anthropologist and the reading public (both scholar --i.e. his confrontation via footnotes with E. Hobsbawm-- and non-scholar, but English speaking). Taussig points thus to the unavoidable selective processes of categorization and analysis in the Enlightenment tradition of the social sciences that obscures and silences real life practices as they enshrine others with central explanatory powers.<sup>4</sup> Moreover he stresses the deceptive methodological pretense that description and explanation are possible at all because they are based in the "concealment of ideology" (74), the "charade of scientific detachment" (75) and the repression of passion (76), while adding the concrete grounding of this in a North/South power relationship:

“For what is surely referenced here in this epiphanous encounter between north and south, between the cultivated man of letters from the north and the sun-drenched tillers of the southern soil of untruth, is an uneasy acknowledgement as to a certain secret of the secret in which the south has long had the function of mirroring, in its dishonesty, the dissimulation of dissimulation in the north?” (1999:76-77).

Two things seem to me worth highlighting. First, Taussig's comfortable unawareness of local scholars' (anthropologists and historians) critiques of Pitt-Rivers' ethnography (Moreno 1984, 1993, Frigolé 1980, 1989, Serrán Pagán 1980, Martínez-Alier 1968). This would have given him some insight about important aspects of knowledge production, about what the 'cultivated men of letters from the south' thought of the encounter and about the real politics of the production of truth in Mediterranean anthropology through various forms of concealment. For example, what happens with local scholars' multiple and diverse [even contradictory i.e. Frigolé 1980 and 1989] critiques of Pitt-Rivers' work? How would he deal with their methodological perspectives, some more passionate than others, but all of them with a 'scientific' pretense, therefore within the social science Enlightenment tradition? What happens when social scientists, while aiming at description and explanation do not pretend to be passionless, ideologically neutral or detached? How does their work speak to the work of those that base their knowledge and its authority on the pretense of detachment? How is it part of a political engagement locally, nationally? Also, reading local scholars would have given him an additional information about public secrecy: the fact that the preface to the second edition was not published in the first Spanish edition (1971) although it was contemporary with the English second edition (1971). Why? Was it bad consciousness at calling Andalusians as a whole --as a 'culture'-- liars? Was it self-protection against possible critiques by local 'native' anthropologists such as Moreno? In any case it provided the turn of the screw for the "dissimulation of dissimulation in the north".

However, I am more interested in a second aspect of the comparison between Moreno and Taussig's scholarly exploitation of Pitt-Rivers' secrecy and lie perspective in his ethnography. Taussig makes it a critique of modernist realist pretenses of revealing truth through rational analysis (of functions or origins, i.e. Dunk 2000) and proposes impassioned characterization instead, but from a distance. Moreno, while also making a critique of detachment --"professionals of anthropology that have little interest really in the present and the future of the Andalusian people..."-- proposes going into the historical depths of the production of a 'culture of oppression' and of a meaningful national Andalusian identity that does not eschew class, gender and race fields of force (Moreno 1981a, 1981b, 1981c, 1990, 1991, 1992, 1993). In Moreno's critique we see that passionate practice is not simply a self-centered search for pleasure through an abstracted idea of concrete participation that becomes an end in itself for the anthropologist's self. Rather it is actively and outspokenly a political project, a desire for change, an emotional engagement aimed at transforming Andalusian lived reality in a particular direction, through producing particularly useful knowledge towards this aim. As part of a political project then, it is necessarily part of an abstraction, a process of 'fixating' concepts that design and enable particular forms of collective action.

The above can be seen in a recent contribution to a volume on *La identidad del pueblo andaluz* (The identity of the Andalusian people) where Moreno (2001) explicitly states his intellectual and political program as the participation in the production of an Andalusian

identity in historical, cultural and political terms in order to empower Andalusian people in the context of increasingly globalized market forces and multi-layered structures of governance:

“In no place in the world does *national sovereignty* exist any longer, as it has been understood up to now: our age is now one of 'shared sovereignties', where a web of knots of different sizes and importance is being woven. These knots will define the structure of future relationships between peoples. If Andalusia does not become one of these knots, it will be excluded. If, on the contrary, it manages to occupy one of these positions, this will mean emerging from the present-day periphery and subalternity. And this is not only a problem of juridical definitions, rather of an everyday cultural and political leading position. There is no other form, presently, of guaranteeing the survival of a people, in our case the Andalusian people, other than through asserting and developing the triple dimension of identity: historical, cultural and political” (2001:160)

And he adds:

“But the *identidad-resistencia* (identity-resistance) that can be generated today by Andalusian culture should not be understood as an end in itself, but as a means, a necessary preliminary stage, towards the construction of an '*identidad-proyecto*' (identity-project) aimed at making possible a less unequal and unjust society than the present one, through a deep transformation of the internal social structure and the termination of the external dependency and subalternity.” (2001:170).

To a 'scientifically detached' intellectual, this program can be read as an attempt at providing a clear conceptual category "Andalusian identity" in order to create something similar to what Gramsci called an historical bloc capable of producing an alternative hegemony for revolutionary purposes. Or, alternatively it could be read as akin to the nationalistic folklorists search for 'origins' (Herzfeld 1987).<sup>5</sup> For the local scholar involved in the production of this quasi homogenizing concept of Andalusian cultural values to be reshaped into tools of struggle against market totalizing values (2001:162-4) it is much more than that. Are we prepared to deal with this sort of politically engaged, local knowledge production without displacing it away from epistemological coevalness? And how would we do it if we eschew all of the unitary frameworks produced by modernity?

### **The production of knowledge and forms of political engagement**

As the above stories highlight the issue of communicability within fields of knowledge is tied both to the institutionalization of particular regimes of truth and to the involvement of these processes with real life issues of dominance and exploitation, with the reproduction of particular structures of inequality, or alternatively the substitution of those structures by other ones. I am well aware that my own discourse is well entrenched in modernist assumptions about history as a continuous and connected (therefore unique) process tying past and present realities with imaginings of possible futures. It is not one, however, that incorporates as its foundational corpus an idea of a particular teleology of transitions into a particular social, political or economic future. My perspective remains critical of Foucault's notion of

'genealogy' as opposed to that of 'history' for I try to place the concrete analyses of local or regional historical processes in the wider movement of a global, connected history. And I try to follow the threads that create feelings of community and coherence, the production of multiple histories enabling political agency, from the raw materials of heterogeneous and contradictory situated experiences.

If the process of 'genealogy' oriented against the power effects of 'scientific' discourse has been a major epistemological breakthrough for the social sciences, it seems to me nevertheless that it has also produced a paradoxical catatonic effect that was not intended by Foucault. The ultimate goal of the genealogy process was, for Foucault, an attempt to free historical local knowledges from their subjugation in order to enable them to counter the coercion of a fixed, unitary, scientific theoretical discourse, and this with the explicit aim of empowering them for struggle. Archeology as 'method', genealogy as 'tactic' (Foucault 1979 [1976a]:131). Unlike what has happened to many of his followers in the social sciences, Foucault *had* a political project explicitly concerned with specific, local struggles. He was deeply engaged in the active transformation of social reality as he experienced it, he was definitely not only interested in the unveiling or exposure of multiple discourse/ knowledge processes, but also wanted to "exercise power through the production of truth" (Foucault 1979[1976b]:140).

However, and this is the unresolvable tension in Foucauldian epistemology, the 'production of truth' from where we may 'exercise power' in our struggles against diverse forms of domination will have to be eventually inscribed in some kind of 'fixed' hierarchies, referring for example to a particular concept of 'justice'. It will also have to be inscribed in a geometry of objectives for change and thus get a set course, an 'orientation', a determined 'sense' of/ for action. Yet it is obvious that the literal definition of 'genealogy' presents an absolute arbitrariness of being, a permanent fluidity of everything: time, space, people, concepts, relations, knowledge (Foucault 1979[1971]:13). It is difficult then, from this epistemological position to engage with reality in an attempt to transform it because there is a break --instead of a dialectical tension-- "between 'real history' on the one hand and the historical commentaries and texts of social actors and intellectuals on the other" (Roseberry and O'Brien 1991:12).

We are then left with the question of how to render politically productive the tension between the production of multiple situated knowledges and concrete political engagements. In an interesting debate on the historiography of racisms Ann Laura Stoler raises the crucial problem of 'the politics of epistemologies'. Her analysis of *anti-racist* histories of racisms leads her to underscore: 1) "the pursuit of origins that constitutes 'traditional history' is a moral pursuit that is fundamentally ahistoric" (Stoler 1997b:248) and 2) "a search for racisms' origins both shapes and is shaped by how we think about race in the present and what we imagine is effective anti-racist scholarship today" (Stoler 1997b:249). Her point is that the focus on "fixity, permanency, somatics and biology" (Stoler 1997b:249) as the 'original' visible physical form of racism hides the fundamental ambiguity always present in racisms between 'ocular epistemologies' of somatic taxonomies and the fluid plasticity of the intangible qualities that are social and cultural elements of racial political practices. And her conclusion is that "if racisms have never been based on somatics alone nor on a notion of fixed essence, then progressive scholarship committed to showing the protean features of racial taxonomies does little to subvert the logic of racisms

since that logic itself takes the plasticity and substitutability of racial essences as a defining feature of it" (Stoler 1997b:252). In sum, that both a) epistemological error deflects anti-racist struggle from the real issues and b) that contemporary anti-racist political agendas inform histories of racisms' origins (see also Stoler 1997a:201). If we think of 'political rationalities' as an important part of political economy as Stoler suggests (1997b:250) we can better situate our knowledge-making as part of our own political agendas (anti-, pro-), but also, as we attempt to analyze or communicate with other forms of knowledge we need to be able to gauge the weight of power struggles in the theoretical structures (and teleological histories) they produce.

Consciousness then --and coherent consciousness as 'knowledge'-- is a material expression of experience --giving meaning to social relations in real life-- and is also a material force, exerting pressures leading to change (Thompson 1978:97, 171, 175-6; Williams 1977:75-82). It is in light of these discursive practices or political rationalities that I want to approach the issue of knowledge production and political engagement. Fabian (1983:152-165) has developed the concepts of 'allochronic distancing' and 'coevalness' in his effort to historicize anthropological practice and find a way out of dominating forms of producing knowledge. His insight stresses the unavoidable coevalness of communication in the field encounter but also in the encounter with other forms of produced knowledge through polemic. That is there is a recognition of conceptual co-presence, therefore summoning us to engage with it as a present reality (and a political one) and not as something definitely enclosed in a past that is no more, or is in a realm of other-than-knowledge cultural production (cf. also Amselle 2000:211).

In my opinion, we need to know more about the global and local histories that shape a particular order of domination, its material processes and discursive frameworks, and the micropolitical and macropolitical fields of power. Categories that shape local knowledges should be treated as part and parcel of a historically formed discursive framework during conflictive nationalist, colonial, post-colonial, etc. historical times and spaces. We should engage with the fact that these categories take form as part of tensions between different social and political agents at different times, agents who get involved in multiple and heterogeneous relationships in the course of trying to secure differential access to resources and power, while forwarding and resisting claims over land, work, symbols, etc. through the production of different discourses that all have a pretense of coherence (Roseberry 1989, 1994).

### **Exploring epistemological barriers to real engagement**

At this point I would like to develop further 1) the issue of comparison vs. incommensurability, and the possibility of thinking of any 'knowledge' as beyond communication, but also the tension between distancing and participation; 2) the issue of 'project' and the break from the epistemologies of 'modernity', that is, the capacity to foster change of post-modern fragmentary epistemologies where categories and knowledge are 'multi-sited', endlessly self-reflexive and permanently unstable; 3) the issue of markets for knowledge products in relation to the reproduction of power structures, setting the field of forces for 'authority' in knowledge, within the academy as well as without it, in local, national and international arenas. In this regard I would want to show that political engagement and



'project' development is not an exclusive prerogative of the Left, on the contrary, and that 'peripheral' intellectuals are not a homogeneous body of 'counter-hegemonic' knowledge producers either.

In order to develop these points in a comparative dimension I will briefly explore the case of post-colonial South Asian scholars with the aim to address the obstacles for real engagement produced by an abandonment of 'realism' and of modernist unitary epistemological frameworks. The post-colonial critique of knowledge production is based on 1) power and 2) discursive regimes. The relationship between these two concepts is posed in such a way that their articulation constructs both the object of study and the paradigm (in a Khunian sense) or authorial narrative (in a post-modern sense) in which social relationships are explored in search of an explanation. There is no fixity (no essentialism?) to the object of study, but constant displacement as the power relations enclosed in the teleological and unitary histories of modernity (colonialist, nationalist, marxist) seek to produce a particular knowledge in order to serve the perpetuation (or subversion) of the existing order (Guha 1983). Following a trend that had its origins in the late 1960s Euro-American historiography, first among the French Annales group with the *'Histoire de la vie Privée'* and feminist historians, but almost simultaneously among Italian historians doing micro-history, British Social Historians such as Raphael Samuel and the History Workshop group, and German Social Historians such as Lüdtke with the Alltageschiste group, the aim of the Subaltern Studies group was to give voice to the 'subaltern', meaning by this a large and heterogeneous category of people. The originality of the group developed as it drove closer to post-modernist or Foucauldian assumptions and away from Marxist social history. That is, when it abandoned realism. When discourse became the only referent of reality, and knowledge production became self-referential. As Prakash (1990) defines the new post-Orientalist scholarship "First, it posits that we can proliferate histories, cultures, and identities arrested by previous essentializations. Second, to the extent that those made visible by proliferation are also provisional, it refuses the erection of new foundations in history, culture, and knowledge." (1990:406). Moreover, this project is situated as a political one "an issue of engaging the relations of domination" (1990:407) where "the power attributed to the knowledge about the past makes historical writing into a political practice and turns the recent post-Orientalists historical accounts into contestatory acts." (:407).

Following other critiques of post-colonial perspectives (Dirlik 2000, Subrahmanyam 2000, Sarkar 1997, 1999, Pouchepadass 2000, O'Hanlon & Washbrook 1992) I want to underline the difficulty that this epistemological vision poses to *real* political engagement. Critiques have pointed to the fact that post-modernist perspectives are themselves a 'grand narrative' and are inserted in present-day economic and political fields of force (Subrahmanyam 2000:95, Dirlik 2000:77), they have pointed out that those producing this post-colonial knowledge are themselves fully a part of the centers of knowledge production, mainly in US universities (Bénéï 2000, Friedmann 2000) and therefore occupied in academic power struggles within it, rather than in subversive action in 'subaltern' locations. Some have pointed to the danger that post-colonialist essentialization of local culture can pose for justifying the nationalist Right politics locally in India (Bénéï 2000, Pouchepadass 2000:179).

As a matter of fact it is interesting to compare the work of 'subalternist' historians in the US, with the rest (in Europe and India). Kaviraj [SOAS, London] (2000:75) for example, points to the dependency of post-colonial theory in regard to Western knowledge, and seems

to propose a detachment from it linguistically (2000:79) and theoretically (2000:84-5). Not so much a closure of a different realm of knowledge but a vindication of unawareness of Western theory, and Western debates (cf. also Ramanujan and Narayana Rao in Subrahmanyam 2000:92). On the other hand, a historian such as Sumit Sarkar [University of New Dehli], originally a part of the Subaltern Studies group, and actively involved in the public critique of the fascistization of the Hindu nationalist movement (through his contributions in daily newspapers, teaching, and writing in Bengali and English), was not prepared to forgo a Marxian idea of differentiation *within* the framework of a unique history and, significantly, of a realist history. In Sarkar's view then, 1) different struggles or localized 'histories' are perceived as part of a unique although differentiated process, 2) that there is a distinction between description of a past reality as gathered through documentary information and the attempts by government to construct a particular narrative as well as a particular discourse.

What is striking in his presentation about a particular struggle over historical knowledge production is its *grounding in reality*, that is in concrete, present-day political struggles in India. <sup>6</sup> Indeed, it is *real* that Sarkar has had serious censorship problems with his account of the history of the anti-colonial movements in India because he stated through documentary evidence that right-wing Hindu nationalist movements such as those in power until April 2004 were conspicuously absent from the fight for freedom. What is also striking as opposed to the ecumenical discourse of proliferation of discursive realities and shifting perspectives of post-colonial theory is his outright disqualification of 'old fashioned, discredited' discourses about history, as well as the clear 'foundational' and 'modernist' paradigm at work both in the presentation of the past and in the struggle to spread a particular knowledge of the past in the present and *for* present-day struggles, namely the struggle against the rise of a totalitarian state (Sarkar 1993, Sarkar in Frontline, V.7, 2000).

To my mind, Kaviraj and Sarkar's intellectual positions represent two different politically engaged possibilities in the struggle against hegemonic control of knowledge production and their effective subversive power. The former says that we do not have to convince those in power that the particular knowledge we produce is valuable, we just have to give it value on our own terms and ignore the center's (Western) unawareness of it. As a corollary we will be empowered to value non-scientific, non-rational, non-modern (non-secular, literary, ritual etc.) forms of knowledge if we so decide. The result of this project is the absolute incommensurability of forms of knowledge, the 'autonomy' of the subaltern consciousness (Pouchepadass 2000:177-82). Although this sounds extremely radical, it is in my opinion an expression of the very liberal willful notion that one is free to make history as one wishes. But what does incommensurability entail? It entails the impossibility of comparison and generalization therefore impairing the construction of 'grand narratives' including the possibility of producing new emancipatory narratives to replace discredited 'modernist' ones (cf. Pouchepadass 2000:181; Dirlík 2000). The impossibility of abstraction across localized and extremely diverse and often contradictory experiences of knowledge production. The impossibility of a common 'project' (is this exclusively a 'modernist' concept as well?) of any sort because of the endless dynamics of fragmentation (in time and space). How do we deal with these sorts of propositions that eschew comparison as a 'foundational' principle? Of propositions that preclude a unified language of some sort that would make communication possible and hence collective action? What are the concrete political realities of such projects? Theoretically anthropologists have dealt with the issue of comparison and

incommensurability for a long time, and have produced such concepts as 'emic' and 'etic' which, as we all know are tricky (because of the objectification of anthropological subjects that they entail) but useful (because it is an attempt to bridge the incommensurability of radically different forms of knowledge production and the need to enable communication). The case of Sarkar, on the other hand, proposes the struggle for control of the locations of knowledge production in a concrete situation. But, although he underlines, in his view of history, the multiplicity of struggles that converge in the anti-British struggle, he defends a 'modern' unitary conception of social history, where different forms of struggle predicated on different experiences of reality and different (but not autonomous) forms of consciousness, are linked to a unitary movement of history through the reference to a real reality. It is also significant that his particular 'modernist' version of history is related to a 'modernist' political emancipatory project and is actively involved in trying to counter the right-wing nationalist government's control of knowledge production.

### Responsibility and communication in a World Network

The cases presented above serve as comparative stances of the involvement of social scientists in transformative projects of reality. I am going to go back, now to my initial story of the Andalusian anthropologist Isidoro Moreno and his particular way of knowledge production and how one might deal with it in the event of a World Anthropologies Network. Is it possible for a worldwide 'scientific' community of anthropologists to be open enough to others' passion as to be able to communicate with scholars who are working in their own places? To achieve their goal such anthropologists are partly renouncing the distancing and 'objectivity' of the conventional professional anthropologist, a distancing that is belied by the 'cultural shock' experience that they claim is evidence to their proximity and immersion? Is it possible for anthropologists to be open to concepts and paradigms of knowledge that are alien from the one (whatever it is) they have, or is hegemonic in the academia? <sup>7</sup> How is the participation in the struggles and debates of their own society of anthropologists such as Moreno and historians such as Sarkar, different from the methodological oxymoron of anthropology: 'participant observation'? How is the fact of having a project about the transformation of the society we live in and we observe and study different from 'social engineering'? What makes it different (we should keep in mind that often an 'emancipatory' intention or discourse is an important part of both right and left political agendas)? Who decides?

The first thing that we have to deal with is the fact that anthropologists (and other social scientists) who are engaged in a political project use *stable* concepts and *unitary* laws of movement (whatever they be). If their objective is to transform reality, they have to have a realist (not just discursive) sense of reality. They will need categories adequate to the political projects they want to engage in (this has been the practice always among those holding power as well as among those wanting to become empowered) and they will need a structure of meaning that makes explicit the relationship between those categories and the relationship between categories, analysis of reality and transformation of reality, that is, a link, through experience that connects consciousness with practice (O'Hanlon & Washbrook 1992, Dirlik 2000). Moreover, they will aim not so much at fragmenting realities but at producing a collective will (Gramsci 1987:185).

The knowledge produced by and for political engagement on the ground, eliminates distancing and tends to create stable, unitary and directional frameworks and concepts of the 'modernist' type. This type of knowledge is based on a sense of responsibility, that sets very clearly 1) the relationship between the anthropologist and those he observes 2) the issues that have to be explored to get a better knowledge and set forth some kind of organized, transformative project, 3) the concepts and models that should be developed for it. Engaged anthropologists in the 1960s such as Gough (1968) represent already an expression of this.

But outlining the framework of mutual responsibility between those participating in a coeval reality that is meant to be crystallized as knowledge of some kind is, I contend, the only way that we can create a real space for communication. Responsibility is what links knowledge production to reality, real people, real suffering, real power. Responsibility is what makes knowledge into a project. 'Participant observation' on the contrary, is what creates distancing out of what is irremediably an experience that creates responsibilities while it lasts, the ethnographic fieldwork. But is distancing necessary in order to create some grounds for comparison, some shared discourse across places and types of knowledge? My feeling is that some distancing is necessary if what we aim at is communication and as Williams said, through tensions, growth.

But we have other obstacles to deal with if we aim at creating this planetary space of anthropological encounter. Obviously one is language: To be forced to use International English in order to communicate more widely is in itself an aspect of oppression and dependency (Kaviraj 2000; Comelles 2002), but I won't expand this question. What I am more preoccupied about has to do with local political struggles and knowledge legitimization processes at the peripheries and how, from the distance of being in another place and coming from a different disciplinary history, we are to approach them without having a background, as we say in Spanish, '*sin conocimiento de causa*'. I have always been surprised by the 'errors of judgement' that some of my foreign colleagues make about the work and political positioning of some of my Spanish colleagues. I myself make a judgment from my particular academic experience, linked to a particular history of the discipline within Spain, and from my political positioning regarding present-day issues in Spain (and the world). However biased my appreciation of the knowledge produced by my anthropologist colleagues is, I can situate it in concrete practices and particular histories that help me 'understand' what they are *really* saying under the often indistinguishable use of the latest conceptual jargon borrowed from abroad (Narotzky 2002). Foreign friends and colleagues of mine with similar political positions and biases are incapable of reading between the lines, until they become aware of the local histories and struggles. My question is how do we build the criteria necessary to the 'understanding' of the work of colleagues whose disciplinary histories and political positioning with respect to real world issues we ignore? Is it an unnecessary preoccupation? Is blind openness a 'good' per-se? Can knowledge, through exchange, appear as detached from its process of production? What sort of vision do we have about knowledge flows as different from the actual 'market' of knowledge, one of generalized reciprocity?

Let me recount another story that will clarify this in relation to Spanish anthropology. In February of 2000 there was a pogrom-like event against North African immigrants in the town of El Ejido, Almería (Andalusia), a town governed by the Partido Popular (Right). Under the eyes of a non-intervening and complacent local police, Spanish residents attacked immigrants, in an organized way, destroying their property, desecrating

their cult space and driving them to the mountains to seek refuge. For the then governing party Partido Popular (PP) the events had been a spontaneous burst of anger from the Spanish residents at the non-integrating practices of the immigrants and their criminal behavior (the alleged cause of the pogrom was the assassination of a local girl by a Moroccan immigrant). But what I am interested in exposing here is the participation as 'experts' in the production of knowledge about this event and more generally issues around immigration policies and multiculturalism by Spanish anthropologists. Before the events took place, several anthropologists such as E. Martín (Universidad de Sevilla) and F. Checa (Universidad de Granada) from Andalusia had been doing fieldwork in the area of the Poniente Almeriense where El Ejido is located, trying to evaluate the realities of immigrant workers' life in intensive plastic agriculture. The work of Martín *et al.* (2001) was being co-financed by the European Union and the Junta de Andalucía (Autonomous government in the hands of the Social-Democratic PSOE). Their work was mainly empirical but clearly related to a loosely 'political economic' framework, highlighting the impact of economic transnational processes and national exclusionary policies. An anthropologist from Madrid, Ubaldo Martínez Veiga (Universidad Autónoma Madrid) was also doing fieldwork in the area (Martínez Veiga 2001). Close to the union Comisiones Obreras (Left) he was moved to respond to the role of the unions in organizing and defending immigrant workers' rights. The work he produced was very theoretical and framed in a strong political economy conceptual structure. After the events, these anthropologists began being very vocal about the knowledge they had produced, participating in many local forums such as union meetings, immigrants associations, etc. <sup>8</sup> All of them tell how they experienced forms of intimidation, from censorship to life threats. <sup>9</sup> A few months after the events of El Ejido, in December 15<sup>th</sup> 2000, the new Ley de Extranjería, excluding illegal immigrants from basic civil rights such as association, public meeting, union membership, health and education was approved by the Senate (PP majority). In January 4<sup>th</sup> 2001, 12 immigrants die in Lorca, Murcia. In application of the new Ley de Extranjería repression hits the victims: illegal immigrants in the area are expelled from the country in order to apply for legal admission. Local agricultural entrepreneurs in Lorca are exonerated from their exploitive and inhuman practices regarding immigrants. While immigrants explicitly point to the new law as causing the increase in the death toll.

In this context the PP government creates an institution 'Foro de la Integración de los Inmigrantes' and places as its president an anthropologist, Mikel Azurmendi (an ex-member of the Basque nationalist group ETA, he is presently in 'exile' from the Basque country after allegedly receiving death threats, and is now close to the PP's views and policies against peripheral nationalism and immigration). Soon Azurmendi produces a book *Estampas del Ejido* (2001), and numerous newspaper contributions (Azurmendi 2002a, 2002b) where he explains the events as a conflict between cultures, where immigrants are responsible for upsetting local ways and lacking a proper 'work culture' and 'democratic tradition' and justifies the government's repressive policies towards immigrants. After the public presentation of these views by the anthropologist 'expert' Azurmendi, a strong reaction took place among the many anthropologists that did not share his views. Following an initiative of Isidoro Moreno (then president of the Anthropological Associations of the Spanish State, FAAEE) a document of protest about Azurmendi's positions and questioning his professional capacity, was written by Martín and Moreno and sent to be signed by all other academic anthropologists. <sup>10</sup> Of a total of 129 anthropologists [tenured in universities] 63 signed [some non-tenured], that is approximately 50%. It is difficult to assert the 'reasons' for not supporting such a 'corporative' move, defending the anthropological profession against an alien body

(Azurmendi). Many who signed the document would not have written it in the same way, but felt that it was a move towards positioning anthropology in a particular framework of responsibility. Many who did not sign did so also for 'corporative' reasons: not voicing public critiques against a 'colleague'. Others probably did it for pragmatic reasons: there is a lot of funding from government agencies for doing research on immigration. Finally others plainly supported Azurmendi's views.

So where does this leave us for gauging the value of knowledge produced in peripheral locations, when we ignore the practices and realities that create a context for 'understanding' the complex power relationships and contradictory projects involved in its production? The post-colonial critique has often 'essentialized' non-hegemonic locations in the production of knowledge, as it has essentialized Euroamerican locations. But as the case I have just presented shows, the production of anthropological knowledge in the peripheries is diverse and attached to multiple political agendas spanning from the Right to the Left, from the justification and support of governmental policies (whatever those be) to their critique, from 'institutional' forms of agitation to 'alternative' forms of agitation. I am not driving toward a proposal of censorship: I may learn a lot from reading Azurmendi (one has to know the arguments of one's opponents), and I want to know what he has to say *as an anthropologist*. However in order to be able to appreciate his knowledge and make something of it, that is, in order for the communication of that knowledge to be really possible I need to know where he stands on real life issues. 'Scientific' detachment has made us believe that knowledge can flow and communicate without being grounded, not only with respect to the author but also to the historical context of its production. And it is partly so: that is how we read most of what we read in our Western hegemonic knowledge production context. But we always have some clues, precisely because the hegemony has produced a 'small' world: we know what sort of journals publish what, we read the acknowledgements and get an idea of the personal context of the author, we know about the political positioning in the US academy because it is well covered, discussed and publicized in the many forums open to it. But we do not have the same sort of knowledge about India, or China, or Morocco, or Russia to name but a few. Would a world anthropology network provide a space not only to access detached forms of knowledge but also to situate them in their production processes?

### **World Anthropologies: a Realist Proposal**

We get back now to the need to communicate our knowledge and to create growth out of communication. How would this be possible within this very 'politicized' environment that I have described, within this lack of distancing, this unavoidable and obvious participation of anthropologists who work 'at home' in the very real issues and debates that produce the present? Should we, following Herzfeld, discard 'epistemological nativism' as un-scientific and engage with it only as an object of study? Should we, following Taussig, opt for self-contained characterizations of passionate experience? Should we, following the lead of Kaviraj, ignore what we do not know? The problem is difficult to solve and is a classical anthropological problem after all, spiced with history and politics and the Damocles sword of post-colonial and Foucauldian critique. It is the problem of incommensurability and comparison, of detachment and participation, of the degree to which knowledge production is political from the outset and the need for communication.

As I see it, there is no way out of political positioning in the production of anthropological knowledge. In this, the post-modern critique made us aware of the profound political implications of seemingly objective forms of knowledge, that were nevertheless enmeshed in a particular regime of truth. But do we throw away all the knowledge that we know was produced from a particular (intended or unintended) political position? Do we learn nothing from Aristotle, Thomas Aquinas, Hegel, Arguedas, Rivers, or Malinowski, for example? We undoubtedly do. So how do we proceed? We situate them and their concepts in a historical context, a reality in the past that gives a particular meaning to what they said. Then we proceed analogically bringing those descriptions of reality, the concepts and structural frameworks, to work with our own present reality that we want to explain; we stretch the concepts; we confront them; we create new ones out of the creative synthesis with other concepts from other times and authors; we produce a new framework or modify an older one, to give meaning to the relation between those concepts we use in reference to the reality we want to understand and change (or support).

In anthropology, moreover, we use the ethnographic descriptions (however critical we are about how they were produced) as material for comparison, we adopt in this regard something similar to the 'suspension of disbelief' that realist fiction entails, we have to trust that there is some reference to 'real' reality in the description. Because we need to proceed in this way through communication of other works of very different kinds to think and grow in our thinking about reality. And, to be able to do that in a creative way we need explicitness, that is, we need to be told (or to know or learn) what the political project of the author is. This will empower us to 'understand' better his or her work, and to relocate knowledge in reference to a concrete reality. We can only learn something from discourses that are alien to our concrete reality and to our theoretical framework if responsibility of the author in relation to his/her reality is clearly outlined, if an effort is made towards explicitness. Then we will be able to place that knowledge or its critique in our theoretical framework and proceed towards our own project.

Something similar is what Ignasi Terradas (Universitat de Barcelona) proposed with his reappraisal of the methodology of 'ethnographic realism' (1993):

"To the extent that the ethnography exists, it exists as a *thing* in the Durkheimian sense and in the Marxist sense. Objectivity and alienation are the stereotypes of the failed ethnography. Subjectivity and metonymy are those of a pseudo-ethnography. The realist ethnography moves in between a reality that always exceeds it and a theorization that is an approximation. (...) Judgements about ethnographies must come from their mutual confrontation. If not we won't get ethnographic knowledge but psychological, stylistic, moral, political, etc. [knowledge]. (...) The inter-ethnographic dialogue is what realizes the appropriate and wide meaning of the ethnography. That is its real meaning, that which appears in the context of the flow of ethnographies itself." (1993:120)

For Terradas it is the relationship between ethnographies that constitutes anthropology as "a scientific (analytic) and artistic (evocative) reality" (1993:120) and it is the possibility to apprehend the distinction between description and interpretation in ethnographic writing that constituted the epistemological break in anthropology:

“Our point of departure now is that realist ethnographic composition and anthropological theory have to come from the total confrontation of several ethnographies. (...) the pioneer effort of anthropologists at the beginning of this [20<sup>th</sup>] century stems in their forecast of such a confrontation. That is the reason why they made the effort to provide an explicitness without precedents in their descriptions, methods and theories. The ethnographic text that gets to us after this epistemologic break that we can easily characterize as the revolution of explicitness in anthropology, is what distinguishes ethnographic realism and marks a very important stage in the history of the discipline.” (Terradas 1993:121)

What Terradas' paper points to is the importance of ethnography in our discipline, but an ethnography whose aim is to transcend a particular experience through its aprioristic will to communicate with other ethnographies and through this unending parallax exercise try to better understand reality. If it is true that the production of ethnographic knowledge has to be historicized it is nevertheless our particular link to reality as a social science and we should re-learn to deal with it. It is from within our ethnographic experience, thinking and writing, that communication with other anthropologists' work can proceed but also, unappealingly, the will to communicate has to be part of the life and growth of anthropology. It is from our reference to lived reality, a unitary, contemporaneous and shared reality, that modes of responsibility can be made explicit. I contend that both distance and participation are necessary for communication to take place and that political projects are an unavoidable reality of the products of social scientists. Only hegemonic forms of knowledge present themselves as apolitical.

## Endnotes

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<sup>1</sup> I am indebted to Marisol de la Cadena for the concept and for making me think about this issue.

<sup>2</sup> Although Llobera is one of the few early 'cosmopolitan' anthropologists in Spain.

<sup>3</sup> The attempt at controlling particular fields for ethnographic research is an accusation that has been thrown at some anthropologists from within Spanish anthropology. There is a legend that recounts that somewhere in the early 1970s' there was a meeting of three anthropologists (Esteva Fabregat, Lisón Tolosana and Isidoro Moreno) who partitioned Spain into closed areas where they would have control over who was allowed to do field work in them, referring basically to the young generations of Spanish anthropologists. Esteva controlled Catalonia, Lisón controlled Galicia and Moreno controlled Andalusia. However,



exclusion has not been confirmed in actual practice although early students of them when doing field work in Spain tended to work in these areas.

<sup>4</sup> We may recall here Appadurai's 'gatekeeper concepts' in area studies (1986) and Herzfeld's analysis of the creation of 'fixed' concepts in Mediterranean anthropology (1987).

<sup>5</sup> The project that Moreno presents is a clear search for origins and history and oblivious of foucauldian emphases on genealogies (1979[1971]).

<sup>6</sup> During the seminar, Shiv Vishvanathan remarked to me that Sarkar had been involved in 'official' politics and been pampered by previous governments. He had then supported and excluded particular histories and historians and his present day harassment situation had to be understood in the context of these long-term and complex processes in Indian politics and academia. In short, Shiv's point to me is that Sarkar was not an 'innocent victim' in a dichotomic good-guys bad-guys scenario. In my view his remark lends support to my argument (below) that grounding in the political practices of scholars is a crucial element for knowledge communication.

<sup>7</sup> Indeed, it is amazing how we are prepared to accept, even if it is to oppose it, the hegemonic discourse, and how in this process we often resort to adopting and adapting a large part of its conceptual tools and driving narratives, while we tend to dismiss without second thoughts or more often to ignore non-hegemonic discourses about society.

<sup>8</sup> Another anthropologist from the Universidad Autónoma de Madrid, Carlos Giménez, who works on immigration but had not done fieldwork directly in that area was also very vocal during a first moment.

<sup>9</sup> Martínez Veiga and Martín, personal communications.

<sup>10</sup> The document and the signatories appeared in *Página Abierta*, n.128, año 12, pp.46-47

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