Chapter 12

Mediterranean Equivoques at Oxford

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The notion of Mediterraneanist anthropology is traditionally associated to Oxford in the 1950s and to such figures as John G. Peristiany, Julian Pitt-Rivers, or John K. Campbell. Yet, in the course of its half-century of existence, anthropological Mediterraneanism was forced to respond to profound changes both in terms of new understandings of anthropological theory and of European geopolitics. In this paper I present a series of chained equivocations that I had to contend with over the past decades. My aim is to highlight, on the one hand, how the history of Oxford Mediterraneanism has been systematically misread and, on the other hand, how anthropology has failed to deal adequately with the matter of sociocultural comparison. Essays such as that which Chris Hann has recently published in *Current Anthropology* debating the category of Eurasia (2016), whether or not one is in agreement with the specificities of the argument, are very welcome, for they force anthropologists to address explicitly something that they normally prefer to leave unstated: the contexts of socioregional comparison implicit in their ethnographic analyses (see Pina-Cabral 1989a).

The first aspect that needs to be stressed is that the category 'Mediterranean' is not independent of the other regional categories that surround it: Europe, the West, Africa, Middle-East. Each of these carries with it a set of symbolic implications that were central to the dominant imperial projects over the past two centuries. It is hardly

just a matter of identifying a body of water around which were built some of the world's most important historical cities. A broad regional category such as Mediterranean is deeply relational; it cannot be defined in purely geographic terms, as it only makes sense by reference to a set of assumed alterities: the Orient, Africa, the Middle-East, the Eastern bloc, the Third World.

The definition of what is Europe or West has evolved, responding to the profound changes in political and economic conditions that occurred over the decades. Our recent history is full of such geopolitical shifts: e.g., the end of the Ottoman Empire, the creation of a Soviet bloc, the post-War recentering of Westerness in America and no longer in Europe, the process of decolonisation and the arrival of colonial populations in the European metropolitan cities, the advent of the southern European democracies and their entry into the European Union, the creation of the eastern European democracies, the more recent split of Europe into two as a result of the present recession (the 'austerity' regime), nationalism and the 'refugee crisis' ...

Today, the Mediterranean region silently qualifies Europe and its imperial successor, the West, by fudging its borders. The Mediterranean sits at the border of the 'Rest' (as in 'the West and the Rest'), playing the role of historical mediator to a civilizational heritage that 'the West' claims to own but whose origins it traces elsewhere (the Egyptians, the Chaldeans, and the Phoenicians come to mind as master metaphors). As these two categories evolved in the second half of the twentieth century, so the anthropological Mediterranean was forced to shift its place and alter its nature, being silently resituated a number of times.

The works of the Oxford Mediterraneanists (but also the early Algerian work of Pierre Bourdieu—1979 [orig. ed.s 1962 and 1973]—or Ernest Gellner's Moroccan

ethnography—1969) are part of a process of re-situating the Mediterranean that was taking place at mid-century, accompanying the shift in the meaning of the word 'West', when it came to signify primarily American civilization and no longer Western Europe. This was not, however, the sort of implications that the concept had carried half a century earlier, during the Belle Époque, when it was used by the founders of the social sciences in the early twentieth century. This deserves to be noted.

For example, when Durkheim and Lévy-Bruhl corresponded in 1909 concerning the title of the latter's book *Fonctions Mentales dans les Sociétés* … 'primitive', as Durkheim would have it or 'inferior', as Lévy-Bruhl finally decided,[1] the operative opposition they used was between 'the mentality of the primitive/inferior societies' and that of the 'societies originating in the Mediterranean civilization' (cf. Goldman 1994: 177-8). In short, the primitivizing re-reading of the Mediterranean that was going to occur after the Second World War was still foreign to them. It occurred as the result of a major change in the meaning of southern Europe that came about in the course of the American occupation of southern Italy. This process was brilliantly and poisonously described in Curzio Malaparte's masterpiece *La Pele* (1949) or in Norman Lewis's famous war diary (1978). Works of Mediterranean de-legitimation such as Banfield's elaborations on 'amoral familism' (1958)—and much that was written on clientelism, patrimonialism, etc.—were an integral part of that ideological shift. Over the past decade, however, such discriminatory type-castings have again resurfaced by the hands of 'social psychologists' as part of the neo-liberal attack on peripheral European societies that is once again splitting Europe apart (e.g. Cohen and Nesbitt 1998).

[1]See Lévy-Bruhl 1910; their correspondence in Davy 1973: 320.

Mediterranean equivocation in anthropology is the product of the fact that theory and geopolitics silently merge in social scientific thinking. This is due to the ideological centrality of the primitive/modern polarity during the past century. Our nineteenth-century anthropological ancestors developed the notion of primitive (that is, the association of 'otherness' with primordiality) as an intellectual tool in order to account for the unity of the human species in the face of its blatant diversity (see Kuper 1988: 1-36). Soon enough, however, it turned into one of the more powerful implements of European imperialism. Ernest Gellner (1974) used to call the boundary between primitive/ancient and modern 'the Great Divide'. He claimed that it was an indispensible theoretical tool of analysis (perhaps the single most important one in twentieth century social sciences, as Latour has argued—2006 [1991]). What is not so seldom observed is that the way the Great Divide is specifically drawn responds to geopolitical interests that shift every few years. Since the days of Durkheim, the Mediterranean has become an area of uncertainty, a terrain of negotiation, a favoured field for geopolitical mediation *and*, therefore, of analytical equivocation. It is a producer of 'remoteness' (see Ardener 2012).

Following my own personal history as a practitioner of the discipline, this paper argues that a series of equivoques have been heaped up that deserve to be unravelled since they seem to be persistent and remain fully contemporary. A few years ago, Lidia Sciama and myself were asked by the *Journal of Mediterranean Studies* to comment on the apparent persistence of many of these regional confusions and uncritical attributions of stereotypes (Sciama 2013, Pina-Cabral 2013). These perplexities are, after all, the terms of European schismogenesis, with immense long-term historical implications. We were questioning them in Oxford over twenty years ago, when they were evolving due to the enlargement of the European Union, and

then the Fall of the Wall of Berlin (see Pina-Cabral 1989a).[2] But they are again evolving today in a different direction in response to the collapse of the project of European unification.

Furthermore, this paper sustains that, due to the essential role of primitivism in the way Anthropology has defined itself as a discipline in the course of the twentieth century, the complex original drive that led to the study of the history-filled social contexts that surround the Mediterranean Sea by a number of anthropologists in the post-War period has remained hidden from our view for too long, both in terms of the geopolitical implications of studying this border zone and in terms of the theoretical implications of doing so.

The first equivoque

In early 1977, I had a long conversation with David Hammond-Tooke, my erstwhile supervisor at the University of Witwatersrand, Johannesburg, concerning where I should go on to do a doctoral thesis. At the time, we were both fascinated by Lévi-Straussian structuralism— after all, my earlier thesis had been a formal study of the canonical formula of myth using the example of a Portuguese folktale and he was carrying out a structuralist analysis of the Zulu folktales collected long ago by Bishop Calloway (1977, 1992).

Being a Portuguese national, at a time of decolonisation, I could not remain in South Africa and, in those days, Portuguese universities still had not recovered from the 1974 Democratic Revolution. Paris would have been an option. However, in spite of our fascination with French structuralism, it was clear to both of us that

[2] To understand how polemical this questioning was at the time and the way in which, then as still today, it casts a disturbing light in the way psychological determinism is being abused, it is perhaps worthwhile looking at Gilmore 1990 and Pina-Cabral 1991b.

postgraduate study in Paris was far too personalised and erratic an activity for me to be comfortable about it. So, England it had to be. I had a short list of people that inspired me. *Placing the Dead* had been a major inspiration (1971), but Maurice Bloch was on sabbatical leave in the United States, so I looked elsewhere. As it happens, Mary Douglas, who had also inspired Hammond-Tooke's early work, had just left for the States too, on a more permanent capacity. Therefore, one afternoon, over the telephone from South Africa, I spoke with the Director of Graduate Studies at UCL, and I asked him where I should go. All he knew was that I wanted to write a thesis on Portuguese rural society, so he said that the person for me to study with in the United Kingdom was definitely John Campbell at Oxford and that, if I liked structuralism, I could also be oriented by his friend Rodney Needham. So off I went. A few years later, when I met him in Berkeley, where he was an Invited Professor, we discovered that the person I had spoken to was none other than Michael Gilsenan. This constitutes my first Mediterranean equivoque.

In truth, Michael was not supposed to know that neither is it obvious that one should classify northern Portugal as a Mediterranean country from a sociohistorical point of view, nor was Needham in those days any more a structuralist thinker. After all, if you do not look closely enough, Portugal seems to be just on the other side of the Strait of Gibraltar, very close to Spain, which is the motherland of all Mediterraneanism. And then, Rodney was just about to launch his famous Wittgenstein Seminars at All Souls College, where he was going to relentlessly pull apart the sort of semiotic structuralism that had brought him to fame in the 1970s.

In my first encounter with Rodney he immediately corrected me concerning structuralism: the first book I ever read at Oxford, by his suggestion, was a spoof on Lévi-Straussian structuralism (Asger and Arnaud 1968). That convinced me that I

had no alternative but to follow the old empiricist route: the ethnographic data would drive my cogitations, not the testing of some predetermined hypothesis. But as to what was precisely at stake theoretically in this abandonment of structuralism, it took me over two decades to work it out. In fact, I do not blame myself for being obtuse, as Rodney too, just as he was writing his later books, was driven by a process of deconstruction the conclusions of which, I believe, he never really managed fully to come to terms with (1987). It was only by the early 2000s, long after his death, that much of what he was critically uncovering in the mid 1980s started to fall into place in terms of new approaches to cognition in anthropology.

Once at Oxford, upon doing the background reading, I quickly realised that, from the point of view of its rural institutions, northern Portugal was part of those societies in the Western fringe of Europe that were then called Celtic. The old 'honour and shame syndrome' as described for the Mediterranean heartlands simply did not fit that material. This may not have been very important, were it not for the fact that anthropological feminism (and feminist inspired psychoanalysis) was all the rage in anthropology in those days and I was deeply impressed by some of those ladies. I wanted to enquire into the relation between gender, value, and personhood. But the equivoques kept coming.

I remember a conversation with Ann Whitehead based on a total failure to communicate. I had claimed that gender relations in northern Portugal were far more egalitarian than in southern Iberia. She arrogantly put me down, accusing me of being a male chauvinist ignoramus. I concede that I might have been ignorant about many things in those days, but I was not a conscious male chauvinist and what I was reporting to her concerning the very, very long term nature of the relation between gender roles and social organisation in Northwestern Iberia, still seems fascinating to

me today (cf. Pina-Cabral 1983, 1991a, and Pina-Cabral and Silva 2013). Indeed, the people who have studied the matter in greater depth over the years have repeatedly manifested their agreement with me on that point (see Bestard 1998, Rowland 2011).

After my first fieldwork stint in 1978-9, it became absolutely clear that, much as I was very impressed by *People of the Sierra* (Pitt-Rivers 1954), by *Honour, Family, and Patronage* (Campbell 1964), or by *Portrait of a Greek Mountain Village* (Du Boulay 1974), the material I had in hand was most comparable with Martine Ségalen's ethnography of the Pays Bigoudin, in Britanny (1985), not with the Greek material my colleagues in Oxford were collecting at the time (Roger Just, Julie Makris, Michael Herzfeld). Edwin Ardener was leading a group of students working in what he dubbed 'the Celtic Fringe'—Brittany, Ireland, and Scotland. It seemed reasonable to check, so I did just that, even knowing that it would hurt a little my two supervisors, who were his sworn personal enemies. I found Edwin a learned and inspired thinker but a somewhat uncomfortable person to be with, who lacked Campbell's generous concern with his students and Needham's naughty capacity for intellectual provocation. Nothing came of that.

The second equivoque

I will now move on a few years to the second Mediterranean equivoque. The actors here were a different bunch. In those days, the small group of Europeanists working under the wing of John Campbell organised a reading group at St. Antony's College. Michael Herzfeld had finally finished his thesis and shortly after published *The Poetics of Manhood* (1985). We read it just as it was coming out and, on the whole, we were disquieted by it. *Ours Once More* (1982) had seemed a really visionary book, since it was in fact working actively at the European project in terms of

anthropological cross-fertilisation; it was opening up a way of rethinking the relation between the old strands of pre-Malinowskian nationalist ethnography with the cosmopolitan brand of theoretically informed anthropology that we aimed to practice. But the ethnography of Crete written from the perspective of men's talk in a local village café, seemed to us to lack some of the more important aspects of what we believed ethnography should convey.

Our response to the culturalist interpretivism coming from America in those days was double pronged. Those of us, such as myself, for whom Marxist thought had been an early influence, found the 'semiotic turn' an unfortunate development, particularly in light of our reading of Pierre Bourdieu's, Michel Foucault's, and E.P. Thompson's work. At the same time, we were fascinated by the potentialities that were opened up in terms of new rhetoric modes of writing ethnography (cf. Rabinow 1977).

Back in Lisbon, in 1986, I started writing *Aromas de Urze e de Lama* (2008 [1993]), an exercise in the rhetorics of fieldwork reporting (that is, ethnopoetics). When he heard of it, Rodney sent me Raymond Queneau's *Exercises de Style* (1979). But my intentions were hardly those of the surrealist poet. They mostly had to do with the need to explode the old formats of ethnographic writing which, by then, felt so restrictive and unresponsive. In those days, I evaluated my own recently published *Sons of Adam, Daughters of Eve* (1986) as a good study written in a very boring style. The truth is, I was never really at peace with Evans-Pritchard's ethnographic empiricism and the way it sneakily hid away its own sophisticated but complex theoretical underpinnings.

In 1986, John Campbell and I decided to organise a workshop at the Congress of the European Association of Rural Sociology to debate the changing conditions for

ethnographic fieldwork in Europe and we invited colleagues from Portugal, Spain, the United States, and Britain.[3] In the end, John could not come for the usual health reasons, but in the introduction to the book that we eventually published, *Europe Observed* (Pina-Cabral and Campbell 1992), we noted that:

> The British and American postwar fieldworkers did not think of themselves as Europeanists, preferring the designation of Mediterraneanist. They stressed the similarities and continuities between the southern and the northern shores of the Mediterranean Sea at the expense of those existing between Mediterranean and Atlantic Europe. To a younger generation of fieldworkers studying Atlantic Spain, France or Portugal, this view appears increasingly problematic (...). Some contributors to this volume would support the latter point of view, but even they would not regard the option for abandoning the Mediterraneanist label as a radical criticism of it, but rather as a reasoned challenge to the long established practice in Anglophone anthropology of dividing Western Europe in two for the purposes of ethnographic comparison. (1992: xi-xii)

As it turns out, the Braga meeting was awash with equivoques. Those who were present will never forget the deep embarrassment of Stanley Brandes who had kindly accepted to translate Isidoro Moreno Navarro's paper into English without previously enquiring from him what it was about. Stanley was the perfect gentleman and a superb translator. Isidoro, on the other hand, delivered a heavily emotional diatribe in which he stated that, since they never quote their Spanish colleagues' work, American anthropologists were stealing ideas and, therefore, they should be

[3] Workshop: *Methodological Problems of Participant Observation in a European Context*, April 1986, Braga, Portugal.

made to pay a tax to the Spanish government for being allowed to do fieldwork in Spain.

Similarly, when the time to speak came for Josep Ramon Llobera, the Anglo-Catalan anthropologist informed us that he no longer wanted to deliver his paper. To everyone's surprise, two months later, *Critique of Anthropology* (Llobera 1986) printed a vicious attack on the workshop that Josep Ramon had prepared and evidently sent for publication *before* he even went to Braga. He had a different axe to grind: as myself and John Campbell were associated with Oxford, Josep Ramon had interpreted our concern with discussing participant observation as a manifestation of 'mindless empiricism', unmitigated structural-functionalist obscurantism. He called our need to rethink the bases of participant observation, 'an epistemological straightjacket'. Many of us, with Peter Loizos in the forefront, responded (Loizos 1987, Pina-Cabral 1986), but it all fell on deaf ears and the equivocation survived.

John Campbell's position in all of this does deserve comment. In those days, he was not only a Mediterraneanist but *the* Mediterraneanist, and he was not about to abdicate from that position. He was, on the other hand, a deeply informed person and a sophisticated political historian who was quite conscious of the divers geopolitical implications of using such a category at a time when southern Europe was becoming closer to northern Europe, eastern Europe was entering in dialogue with western Europe, and the Middle-East and the Maghreb were entering rather dark and disturbing moments. On the other hand, in theoretical terms, having a historian's imagination, he was in fact an Oxford empiricist, in the sense that he had no trust in rootless theory. But he was deeply engaged in bringing about a relation between historical and anthropological thinking (both methodologically and theoretically) and, much like his friend Pitt-Rivers, he had much sympathy for phenomenological

insights, on condition they were grounded in actual ethnographic interpretation (see Mazower ed. 2008).

Initially, and in view of the strangeness of the workshop in Braga, John and I decided to drop the idea of publishing the papers as a book. But those last few years of the 1980s were years of great upheaval in Anthropology; US colleagues were aggressively rewriting the history of the discipline and they were (often not deliberately) obscuring much of the best work that had been carried out by anthropologists elsewhere. As it turns out, Isidoro did have a point. The younger generation of European anthropologists were being educated mostly in the recent trends in cultural anthropology and they were prone to be rather ignorant of the rich and varied history of social anthropology as a whole. They were taking on board wholesale a series of misinformed accusations about their own anthropological predecessors. It took a while for it to become apparent how deleterious this kind of amnesia was for our discipline. John Campbell and I felt the need, on the one hand, to give an historically more sophisticated account of the history of Europeanist ethnography than those that were coming out in the disciplinary press at the time and, on the other hand, we were keen to signal that we were not simply continuing with the theoretically blind positivism that Llobera assumed we served.

Europe Observed (Pina-Cabral and Campbell 1992) is a response to all this. The book addresses essentially three concerns: (a) the need to overcome Evans-Pritchard's interpretivist view of ethnography as translation (see Pina-Cabral 2014b), by setting it within a phenomenologically inspired discussion of fieldwork intersubjectivity and its implications; (b) the role of gender, nationality, and class in fieldwork and the way in which the ethnographer's own personal characteristics affect the nature of the exchange, thus questioning the implicit association of 'us' = modern

v. ‘other’ = primitive; (c) the way in which the intense interchange with history that had been developing ever since the late 1970s had led to a series of innovative fieldwork techniques and research dispositions.

Finally, the book gathers two unique documents: John Campbell’s own account of his field experience in post-War Greece and Julian Pitt-Rivers’ reencounter with his personal fieldwork record in the light of his own father’s (deeply problematic) engagement with the discipline (see Hart 2015).

The greatest equivoque with this book is that, if we had called it *Mediterranean Observed*, it would certainly have had a much wider readership. As it happens, neither my own preoccupation with rethinking the nature of the ethnographic method in the light of phenomenological thought, nor John Campbell’s message concerning the interaction between history and anthropology ever came to be noticed by our colleagues. In particular, in light of his lifetime’s devotion to integrate history and anthropology, and in light of the sophisticated methodological discussions we gathered in this book, the repeated claims that continue to emerge today that anthropology has not interacted with history, can only be interpreted as culpable ignorance.

The third Mediterranean equivoque

The next equivoque happened just before the fall of the Berlin Wall and as Adam Kuper and a group of us decided to launch a European Association of Social Anthropologists. With hindsight I can now see how courageous it was of Adam, then editor of *Current Anthropology*, to accept to publish a paper I presented called ‘The Mediterranean as a Category of Regional Comparison: A Critical View’ (1989a). The paper had been written as a response on my part to the blatant misunderstandings that

had emerged in the *Critique of Anthropology* debate (1986/1987). I felt that we could not accept any longer to be limited to two types of anthropological comparativism, both of which seemed equally unsatisfactory: it was either a 'one-village-one-vote' type comparison (see Hammel 1984) or a cultural area type comparison based on the blank attribution of stereotyped psychological traits to whole regions. We needed some kind of 'controlled comparison', but this meant working at developing a far more systematic approach to the nature of long-term sociocultural differentiation, something that anthropologists mostly shy away from doing.

Llobera had accused us of not working with history, of ignoring class and rural-urban differentiation in our studies, of being more concerned with the literary value of our ethnographies than with the way they contributed towards the broader scientific undertaking. And yet, avoiding those failings had been precisely our aim ever since the early 1980s! In particular, a whole generation of which I was part had been dialoguing intensely with the demographic historians of the Cambridge School (e.g. Joan Bestard Camps, Paolo Viazzo, Robert Rowland, etc.), in an attempt to develop more sophisticated, long-term approaches to the nature of regional differentiation in marriage patterns, household formation patterns, and community organization (see Pina-Cabral 1989b). Curiously, none of that work seemed to have been registered either by the people that engaged in the *Critique of Anthropology* debate or those who responded to the *Current Anthropology* debate.

To this day very few anthropologists even contemplate debating critically how regional attributions are made in our discipline. Methodological nationalism continues to be *de rigueur* to this day: an anthropologist of Greece stops his comparison at the borders of Greece, an anthropologist of southern France does not even know the name of his colleagues working in Barcelona, and an anthropologist of

Galicia is more likely to compare his findings with those of his colleagues working in Valencia than with those working in Portugal a few kilometers away. My feeling, twenty years after the *Current Anthropology* debate, is that ultimately we have not managed to dispel the equivoques that we aimed to clarify. In fact, as financial deflationism ('austerity') wreaked havoc across the countries of peripheral Europe, financial oppression started again to be explained by the 'primitive' characteristics of those who were being subjected to it.

Two instances of the systemic nature of this Mediterranean equivocation come to mind, as they had a great impact on me. The first was a passing, but very incisive comment by Sydel Silverman, then president of the Wenner-Gren Foundation, at the meeting in Castelgandolfo (Italy) where we prepared the launch of the European Association of Social Anthropologists.[4] Upon being introduced to me, Sydel made a passing comment congratulating me for the smartness of the publicity stunt I had pulled off at the expense of my masters. I forget her precise wording, but I was absolutely stunned, particularly as she did not seem to be criticising me. She could not imagine what really had happened, that the paper had been read beforehand by John Campbell, who was mostly in agreement with its central points. It was her kind of Mediterraneanism that was being attacked, not his, but she could not see this. She simply assumed otherwise.

The second was a detailed and aggressive response published in *Current Anthropology* by David Gilmore accusing me of being a Durkheimian anti-reductionist and of stubbornly failing to see how useful psychoanalytical insights can be to anthropological theorisation (1990). He failed to even start to understand why I was not satisfied with the psychologistic stereotypes that he applied to a region that,

[4] I turned out to be chosen as the organiser of the conference in 1990 (Coimbra, Portugal) where the association was formally instituted.

according to him, went from somewhere in Asia all the way to Portugal and that included both sides of the Mediterranean Sea in their vast historical and social differentiation. Why he had been incapable of seeing that I was asking for a more systematic and responsible form of engagement with sociocultural differentiation, and that this had nothing to do with the use of psychoanalytical insights, left me utterly puzzled (Pina-Cabral 1991b).[5]

Again, in light of what happened after that, I should not have been surprised. To this day, most ethnographers of Europe fail to grasp that they are responsible for explaining why they choose some regional comparisons and avoid others; that they are responsible for the way in which they cope with long-term sociocultural differentiation, and they have to validate their options empirically and theoretically. Every so often, a study emerges where this is done, like the notable case of the study of Peter Rivière about the Amerindians of the Guyana region (1984), but it is the exception rather than the rule. Historians have addressed this challenge much more effectively than anthropologists. And those of us who, in the 1980s and early 1990s, worked at it systematically attempting to experiment with new and more theoretically validated forms of regional comparison inspired by historical research have had to be frustrated by the fact that the later generations of Europeanist anthropologists are as unkeen to validate analytically the bases of their comparisons as were their own mentors.

For this reason, the equivoque survived. In 2000, Horden and Purcell published their comprehensive historical revisitation of the unity of the Mediterranean. There, they again fail to see that my central argument is NOT against the evidence that there are some socio-cultural features that are generally found in the

[5] As it happens, I was right then researching a paper on genital symbolism where I precisely explore the relevance of psychoanalytical insights for the anthropological analysis of gender (the 1992 Malinowski Lecture, Pina-Cabral 1998).

regions that border on the Mediterranean Sea (see 2000: 487-8). My argument is another one (and an old one, by the way, see Eggan 1954): when we place our ethnographic evidence in comparative terms, we have to be careful that the parameters of our comparison are historically and geographically relevant. Honour and shame, for example (see Horten and Purcell 2000: 522), is a good example of a category that might easily be observable in one or another manifestation in most civilizations across the globe. When he was writing *The People of the Sierra* (1954), Pitt-Rivers first encountered these notions in the work of Georg Simmel, and they are not presented there as features specific to "the Mediterranean", but as broad features of all human experience to which Pitt-Rivers found a specific local cast that he meant to analyse.

The last equivoque

This leads me to the last equivoque that I will detail here. I only found about it as the result of a conversation I had in Recife's coastal walkway one evening in 2004 with Richard Fardon. We were there as representatives of the various anthropological associations that Gustavo Lins Ribeiro (then President of the Brazilian Association of Anthropology) had brought together to plan the founding of what eventually became the World Council of Anthropological Associations. I told him that I had my doubts concerning the nature of the Mediterraneanist label and that I felt that the theoretical foundations of that school had never been properly researched. For one, I was sure that neither Pitt-Rivers nor Campbell agreed with the culture-trait type of interpretation of the 'honour and shame syndrome' that had become the standard American form of Mediterraneanism (e.g. J. Schneider 1971). I have never forgotten Pitt-Rivers' comment in the introduction to the second edition of *People of the Sierra*

that the notions of honour/shame he used in his 1954 monograph were directly influenced by German *verstehen* theories of personhood and value.

In fact, when we look at the founding texts of Oxford Mediterraneanism, we soon discover that the aspiration to define the distinctive features of the Mediterranean as a 'culture area' was simply not there. For example, in his introduction to *Honour and Shame: The Values of Mediterranean Society,* Peristiany never even tries to explain what is specifically Mediterranean about the values he identifies. In the beginning of his exposé, the matter is presented in a thoroughly universalist fashion:

> All societies sanction their rule of conduct, rewarding those who conform and punishing those who disobey. Honour and shame are social evaluations and thus participate of the nature of social sanctions; the more monolithic the jury, the more trenchant the judgement. Honour and shame are two poles of an evaluation. They are the reflection of the social personality in the mirror of social ideals. What is particular to these evaluations is that they use as a standard of measurement the type of personality considered as representative and exemplary of a certain society. Whoever is measured by its standards and is not found wanting may, without falling from grace, break a number of rules considered minor in relation to those of honour. (1966: 9-10)

Then he goes on to detail the specific papers gathered in the book only to conclude with a second assessment that is even more universalistic: 'For any one man to embody the ideal, to pursue a rigid and uncompromising course, generates social reactions from which most truly outstanding men have suffered.' (1966: 17)

In my conversation with Richard Fardon, I argued that, in the 1950s, the discourse of honour and shame in Oxford had had little to do with the generalised

attribution of broad psychological propensities, but was in fact a theoretical reassessment of the notions of person, gender, and class; a questioning of the Durkheimian paradigm that had ruled anthropology during the Classical Period by a group of people that were themselves, like the Mediterranean itself, uncomfortably seated on the borders of Westerness: Peristiany, Abu-Reid, Caro Baroja, Gellner (a Czech), Bourdieu (of southern French, peasant extraction), and even Pitt-Rivers who, for all that he was an English aristocrat, was married to an Andalusian countess when he wrote his book.

Richard's response to me was that he was not at all surprised by this, since he had just collaborated with Jeremy Adler in the production of a two volume edition of the unpublished works of Julian Pitt-Rivers' DPhil supervisor: Franz Baermann Steiner, a Czech Jew. A few weeks later, he actually sent me the volumes (Steiner 1999a and 1999b). I have to say that reading them was a veritable epiphany. Steiner, who had studied and then taught in Oxford in the immediate post-War years, had died suddenly in 1952 leaving most of his work unpublished. He had turned out to be a major influence in practically all of the young people that carried out postgraduate work in Oxford in the immediate post-War period and who were to shape the discipline internationally from thence to the 1970s (Mary Douglas, Laura and Paul Bohannan, M. Srinivas, Louis Dumont, Julian Pitt-Rivers, etc.—all of these have made explicit comments about Steiner's decisive intellectual influence). When he died, Steiner was halfway through a series of very insightful lectures on Simmel's theory of value. Furthermore, it must be noted that Steiner was not a pure theorist, he had carried out fieldwork before the War in the rural areas of Carpathian Ruthenia (present day Slovakia). At that time, he had been a student of Anthropology in Vienna, but he then went on to study in Palestine. So, Pitt-Rivers' option for

fieldwork in Spain must have seemed very different to Steiner from what it seemed to Evans-Pritchard, whose primitivist leanings led him to sustain a marked disdain for fieldwork in Europe (which he felt to be less demanding and less valuable) to the end of his days.[6]

Yet Steiner's influence is not the only instance of a kind of intellectual association that anthropologists have surprisingly failed to explore; *a silenced legacy*. For example, there is much work still waiting to be done in digging out the roots of Campbell's theology—a central aspect of his study of the Greeks. Many of us, working with him, from Du Boulay to Charles Stewart, Roger Just, Renée Hirschon, and myself, have had quite a lot to say about southern European and Middle-Eastern Christianity. He guided us that way, as this was explicitly an interest of his. Campbell's influences—mediated by Onians' famous text that he always timidly suggested we might like to read (1954)—are those of the sort of Anglican neo-Platonian theology that was practiced at Oxford going back to the days of Ruskin, Collingwood's patron. The latter wrote an essay on the Devil that Campbell might have had in mind whilst writing his own contribution to Peristiany's book (1916). This matter surely does deserve further attention.

Suddenly, I became aware of the greatest of the Mediterranean equivoques: I had not known and no one had ever bothered to tell me anything about the intellectual roots of Oxford Mediterraneanism! It turns out that it did not fit into the general mould that the chief historians of our discipline—such as Adam Kuper or George Stocking Jr.—had written about. Why had we not been told of Collingwood, of Simmel, of Franz Steiner's folklorist training, of neo-Platonist theology? No one had

[6] John Campbell's personal information. See MacClancy's contribution to this volume where previously unknown documents are presented that show that the breaking point in the Institute of Social Anthropology in the late 1970s was the attempt by the new Professor to bring John Campbell, who was associated to St. Antony's College, into the Institute.

ever even suggested *verstehen* sociology had been the central source of inspiration in rethinking personhood that Pitt-Rivers and his colleagues had undertaken and that, later on, was an important step in the development of a constructivist view of personhood taken up in the 1980s by such feminist theorists as Marilyn Strathern. No one seemed to know that the supervisor of the founder of Mediterranean ethnography in Britain had been a specialist on the peasantry of the Carpathian Mountains trained in Vienna!

Read from that perspective, 'honour and shame' was something totally different from the vaguely culpable 'culture trait' that our American colleagues were trying to map out and explain away in a reifying fashion along neo-Freudian moulds. As a discourse on value and personal constitution, honour and shame is one thing; as a reifiable item, a mapable culture-trait, it is food for the geopolitical trade of attribution of psychological causes for underdevelopment, in line with such ideological equipment as 'amoral familism' or 'patrimonialism'. Seen in the light of Simmel's sociology, Pitt-Rivers' work acquires a new phenomenological significance that would otherwise remain totally unfathomable. His paper in *Europe Observed* leaves no doubt that those were his intentions. So, it was with great pleasure that I recently saw his work being reedited by HAU Books, in line with a far better informed revisitation of the intellectual roots of anthropological thinking (Pitt-Rivers 2015).

Conclusion

I would now like to make a more general comment concerning equivocation. The Mediterranean equivocations I have identified above are not only in the reception. An equivoque is an act of communication where what is to some person one thing

appears to their interlocutor, albeit under the same word, as being something else. As we have seen, equivoques imply a relation between shared meaning and sociality as a field of power. This is largely due to the fact that the two partners to the communication act assume different contextual backgrounds without necessarily realising it, for they believe they are communicating. And there is no doubt that they do communicate, only that the indeterminacy between their two meanings is higher than it would be under a linguistic context of fuller communication. It should not surprise us, therefore, if someone such as myself, who holds onto a minimalist realist position and has an investment in the anthropology of action, should be so interested in equivocation as an analytical tool (cf. Pina-Cabral 2002: 105-126).

Thus, I feel that it is worthwhile emphasizing that an act of communication is held to be an instance of equivocation when what is at stake is not merely a matter of indeterminacy but where the nature of the different receptions suggests the common presence of two (or more) different implicit structurings of the world, different 'worldviews' (even when these are present within one single person—see Pina-Cabral 2010). There is politics in all equivocation, therefore, for there is the assumption of a contextual placing that implies the common presence of different collectively shared traditions of meaning contextualisation, that is, the implicit acceptance of different hegemonic orders. This is the case, for example, when Banfield or Gilmore speak of the Mediterranean assuming geopolitical implications that are deeply divergent from those that, for example, Pitt-Rivers or myself would carry.

But in the case of the Mediterranean equivoques detailed above, as we moved from instance to instance, we saw that the failures in communication associated to Mediterraneanism hailed from two different fields of meaning: on the one hand, there were geopolitical equivocations, but on the other there were theoretical equivocations.

The really surprising thing is that these two aspects should interact due to the centrality of the primitive/modern trope.

Over the Mediterraneanist half-century, the geopolitical meaning of the Mediterranean Sea changed significantly, as Europe underwent major political restructuration. This sea, that in the immediate post-War period seemed to the Anglo-American anthropologists to divide Europe whilst integrating in their common 'backwardness' the countries around its borders, became today one of the bloodiest and most sinister reminders of the deep unfairness of the present world order, separating those who can hope to receive a salary for their work from those who are simply not remunerated for their daily efforts. Lampedusa, of all places, came ironically to embody the most nightmarish objectifications of the Duke of Salinas' lonesome cogitations. In April 2015, only in a week, more than 600 people died trying to cross the Mediterranean Sea, and the bloodshed goes on unabated to this day.

So, the equivocal character of the Mediterranean label turned out not to be valid only synchronically, but also diachronically. This equivocation came to arise not only between different takes of what that geopolitical order of Europe, the Maghreb and the Middle-East is supposed to be at any one time, but also across time, between anthropologists and their disciples and their future readers. After all, in the wake of the Peninsular Wars, Pitt-Rivers' Grazalema (a.k.a. Alcalá de la Sierra) had been a famous bandit hideaway, trading on the contraband of the nearby port of Cadiz; later on, during the Spanish Civil War, when Pitt-Rivers got to know it, it represented one of the most notorious instances of anarchist popular revolt—the villagers even burnt their own village church! Today, the only thing that reminds us of Pitt-Rivers in Grazalema are the ancient stone faces in the fountain of the central

square, appearing from behind the large buses that bring German tourists every day to sample the 'traditional' food and enjoy the mountain views.

Similarly, theoretically speaking, assumptions have changed throughout the Mediterraneanist half-century, to the point where the original theoretical drive in the attempt to reform Durkheimian anthropology by an input of German phenomenologically inspired sociology was practically lost and had to be analytically unpicked by such persons as myself and Richard Fardon through historical research in the face of the new theoretical hegemonies that imposed themselves in anthropology from the 1980s onwards. These were so forceful that they managed to silence the earlier theoretical constructions, imposing a 'culture area' mode of comparison that was totally foreign to people like Peristiany, Campbell or Pitt-Rivers. I might not even have been led to search for this, had it not been for the deep-seated distaste of these men for the Mediterraneanism that was all the fashion in the 1980s and early 1990s. Oddly enough, to the surprise of those who interpreted my Mediterraneanist defection as an act of betrayal, John and Julian of all people had no difficulty in coping with my own youthful arguments concerning the unsuitability of the 'honour and shame syndrome' for the Alto Minho, a region where, historically, gender relations where structured very differently from those that were characteristic of the Southern European coasts of the Mediterranean Sea.

Yet the complexity of the Mediterranean equivocation is still greater, for there is a crossover between theoretically-based equivocation and geopolitically-based equivocation caused by the primitivist assumption. 'Amoral familism', patronage and corruption, feud and the vendetta, 'the honour and shame syndrome', the 'veil' and other forms of female dress code, all of these have been at one time or another instruments of ideological dispute that had profound effects in moulding 'the West'.

They are all instruments that validate such acts of oppression as the so-called 'economics of austerity' which we suffer from today or, in my youth, the dictatorships that were financially and diplomatically supported by Anglo-American interests all around the rim of the Mediterranean Sea and the need for which was also explained by the very same 'culture traits' that a more phenomenologically inspired anthropology would have readily debunked.

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