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What links are there between fieldwork sites and the specific authors, texts, and theories which inspired the ethnographer before and after the stint? Here are some retrospective meanderings on two terrains studied in the 1970s – the Caurel mountains in eastern Galicia and north-eastern Trás-os-Montes in Portugal. Neither of these places seemed to fit anywhere within the expansive British anthropology of the Mediterranean dominant throughout that decade. These hamlets certainly looked nothing like minuscule, egalitarian rural paradises. Dual family structures, complex household dynamics, asymmetrical labour exchanges, alternative marriages, and rampant bastardy required a radically different analysis. Inspiration came from Goody's comparative sociology of Eurasian inheritance strategies and Bourdieu's practice theory. My earlier undergraduate work on realist literature and descriptive dissection now heard – following the fieldwork itself – a loud echo. Both Goody's and Bourdieu's rebel and pervasive hyper-critical spirits provided the spark.

[PRACTICE THEORY, CRITICAL ANTHROPOLOGY, FIELDWORK, GALICIA, PORTUGAL]

article

Practice Theory in Practice: Critical Anthropology in Galicia and Portugal in the 1970s

Brian Juan O'NEILL

Senior Research Fellow, CRIA (Centre for Research in Anthropology) & Full Professor, Department of Anthropology, University Institute of Lisbon (ISCTE-IUL).
bbb.oneill@sapo.pt

1. INTRODUCTION

This paper deals exclusively with a *link* between fieldwork localities on the one hand, and on the other, the theoretical pillars chosen by the ethnographer to analyze and interpret those localities prior to and following the fieldwork stint. In other words, we propose to focus on the quality and intensity of the tie between (a) the field site itself, and (b) the authors, texts, and theoretical orientations invoked, firstly in the choice of those fieldwork sites, and secondly in the posterior analysis and interpretation of the field materials collected.

This means that our tone must be ineluctably double, by virtue of treating the *scientific/academic* dimension of this complex process, without forgetting the ways in which this level was affected by another angle, more *personal and biographic*. The interweaving of these two dimensions – which Edward Bruner (1993) has called “the personal and the scientific” – is itself a fertile topic for discussion, and must be kept constantly in mind as we re-examine, retrospectively, our own biographical and academic selections of inspirational authors and works. The latter deeply affected

two of our own ethnographies in Galician Spain and Northeast Portugal in the 1970s. This kind of retrospection is not in itself an easy task (see a slightly shorter text of mine on these 1970s experiences in the Portuguese anthropological journal *Etnográfica* in 2014), and therefore we will try to delimit our landscape drastically in order to keep this task from turning into a wider and less controllable narrative journey into the last three decades of the 1900s. We propose to deal, thus, only with the decade of 1970, bracketed by two years beginning in 1968, and extending two years later, up to 1982. We will explain this bracketing in due time, but must clarify now that the 1980s saw developments of a very different order, so that the 1970s provide us with our major stage. Clearly then, this is emphatically *not* a personal account or reflexive narrative of the fieldwork experience, but rather an attempt to link this experience to concrete texts, authors, and theories.

Four places are strategically invoked: two of them concern academic training (New York and London), while the other two constitute field sites (the Caurel mountain region in Eastern Galicia, and the province of Trás-os-Montes in Northern Portugal). We try to establish a dialectic between New York and Galicia, firstly, and secondly between London and Trás-os-Montes, because in each case our field sites were intimately defined by American anthropology and then by British Mediterranean anthropology. Our final synthesis, or hybrid conclusion, was found – curiously enough – in neither of the latter theoretical panoramas, but rather in the newly growing Practice Theory of the French ethnologist/sociologist Pierre Bourdieu and the “comparative sociology” of the British anthropologist Jack Goody. This only occurred due to readings between 1979 and 1982.

So, we will be looking in detail *not* at ethnographic materials per se, *nor* theoretical schools in and of themselves, but rather in this case at precisely *how*, *when*, and *why* a particular ethnographer chose specific articles and books as inspirations for analyzing complex field realities. In my own case, this did not happen during the fieldwork itself nor prior to the

field, but well afterwards. In the 1970s, practice theory – which we today associate not only with Bourdieu, but also with such authors as Sherry Ortner, Anthony Giddens, Marshall Sahlins, or James C. Scott¹ – provided a novel and stimulating instrument which incorporated Marxist, Weberian, and post-structuralist views, without denying the significance of individual, actor-oriented perspectives focused on families and informal social groups and networks. Another perspective, deriving from Goody's wide comparative scopes on European and Asian patterns of marriage and inheritance, provided a different angle on the kind of household dynamics and strategies which I had observed in the field in Galicia and Trás-os-Montes. Both of these angles – practice theory and a Eurasian frame of reference on kinship patterns – served to solve a profound problem I tumbled upon in my Portuguese fieldwork: how to insert a village characterized by high bastardy rates and non-marriage into a Mediterranean anthropological frame? Nothing seemed to fit.

2. NEW YORK, 1968

Let us start in New York in 1968, amidst the May student revolt that also hit Paris and Berkeley. I had gone to a secondary school – termed a “Prep School” in New York jargon – because of its “preparation” of students for entry into Ivy League Colleges such as Harvard, Yale, Princeton, or Columbia. I finished in June of 1968, two months after the student protests had begun in May, entering Columbia three months later, in September, when the atmosphere had not significantly changed. I had started in Psychology, but quickly realized that the Columbia Psychology Department was dominated by B. F. Skinner's reductionist behaviourism, and had a rat of my own during my first year, whose “bar-press responses” I had to

¹ Due to the simultaneously semi-biographical and semi-academic nature of this text, not all the names I mention will appear in the Bibliography at the end; see the brief note inserted in that Bibliography.

chart on a graph linked to the rat's food and drink habits in its cage. The Department's hallways were filled with pigeons in cages, which made me almost vomit. During this first year I shifted to English and Comparative Literature, which was much more to my taste. In the second year, I began an interest in anthropology, particularly influenced by Robert Murphy and Morton Fried, and by my reading of Lévi-Strauss' classic travelogue *Tristes Tropiques*.

In the English Department, I was immersed in readings of authors such as Melville², Poe, Hawthorne, Faulkner, Twain, Proust, Camus, Stendhal, Cervantes³, Shakespeare, Rabelais, Kafka, Tolstoy, and Dostoyevsky. For some reason, I developed a preference for analysis of extensive realist novels, with particular attention to narrative styles, descriptive detail, and the concept of biographical trajectories. What I had learned systematically in my secondary school concerning the necessity for maintaining a *critical spirit* towards almost everything was echoed in much of this realist literature. I began a course on "Modern British Fiction" given by Edward Said⁴ but found the novels of Joseph Conrad (at that time) very boring, so I gave up after about 6 lectures.

Shifting to a minor in anthropology intensified in my 2nd and 3rd years of this 4-year degree. At this time, some of the prevailing currents in New York anthropology included Harris' cultural materialism, Murphy's structuralism, Fried's political anthropology and Chinese ethnography, and Arensberg's applied

2 Later (O'Neill, 1992a), I published an article combining literary and anthropological perspectives on the novel *Moby Dick*: Um Approccio Antropologico al *Moby Dick* di Melville Aurélio Rigoli (ed.) *Uomini e Culture: Antropologia delle Americhe*. Vol. I, Parte III (Due Mondi a Confronto: Acculturazione e Sincretismi) Génova: Edizioni Colombo (Co-edición del Comitato Nazionale per le Celebrazioni del V Centenario della Scoperta dell'America); 219-230.

3 I had a one-semester course on Don Quixote, taught by Prof. Karl-Ludwig Selig, dedicated to a chapter-by-chapter dissection of the novel.

4 Note that this was in 1969, well prior to the publication of Said's classic *Orientalism* (1978) a decade later.

anthropology, all of which I had some contact with.⁵ I was most influenced by the links between structuralism and folklore, and found in this area an echo of the literature I had read in the English Department. One of structuralism's origins, as I learned, was located precisely within linguistics. In my 4th year, and at the moment I had to decide where I would continue my postgraduate studies (in the U.S. or in Great Britain), I read a monograph on Portugal written by José Cutileiro⁶ *A Portuguese Rural Society* (1971), although I had no inkling yet that I would later do fieldwork in Portugal.⁷ What happened, though, at the end of my *licenciatura*, was a decision not to continue within the field of literature. But a brief biographical note is here in order, with a view to clarification.

Both my father and mother were anthropologists,⁸ and in the year I concluded my B.A. in English (major) and anthropology (minor), that is, 1972, they pub-

5 At this time, Nina Glick-Schiller was preparing her doctoral thesis on Haiti in this Department, but I had not yet met her. Joyce Riegelhaupt (1964) had completed her thesis on a rural area just to the west of Lisbon, but I had also not yet met her, nor was I at the time interested in Portugal.

6 Cutileiro was later to become my doctoral degree supervisor in London in 1973, but this was not planned by myself at this time in New York.

7 What I liked most in this book was the author's intensely ironic and sardonic dissection of Portuguese fascism within the microcosm of a small rural world in the southern province of the Alentejo, itself evidencing many similarities with Andalusia. Many years later, I presented a paper on this ironic literary style which made reading of this monograph so hilarious (O'Neill, 2001): *Evocação de José Cutileiro: Ironia e Humor na Monografia 'A Portuguese Rural Society', Colóquio Interdisciplinar Culturas Populares em Portugal - Séculos XIX e XX*, Évora.

8 My mother had obtained her B.A. in anthropology at Barnard College of Columbia University in 1946, and my father his doctorate in archeology in the Department of Anthropology at Columbia in 1962. In his preface to this thesis (O'Neill, G. C., 1962), my father included in his acknowledgements Alfred Kroeber and Charles Wagley. Conrad Arensberg was one of the members of his doctoral defense committee.



lished a book entitled *Open Marriage: A New Life Style for Couples* (N. & G. O'Neill, 1972), which rapidly became a bestseller, later being translated into 14 foreign languages. The attention my parents obtained from the so-called *mass media* at the time cemented the book as a critical view of modern American marriage, and as well as a volume of “popularized” anthropology. This book itself was the fruit of that momentous year, 1968. Based on interviews with hundreds of American urban and rural couples in the mid- and late 1960's, it proposed a “radical” new model of marriage derived principally from anthropological models interlaced with the humanistic psychology of Abraham Maslow.⁹ It was written precisely following the 1968 protests, in 1970 and 1971, also infused with a highly critical spirit.

I vacillated over continuing graduate study in Literature or Anthropology, my parents continually pointing out that the novelty and adventure of fieldwork in anthropology had much more to offer than research and teaching in English/American literature. I had not yet committed myself to anthropology, and so in 1972 departed for a one-year Masters' degree course in England at a rather unknown “red brick” university in Colchester, to the east of London. At the University of Essex, this masters' course was in the Sociology of Literature, coordinated by Stanley Mitchell, translator of one of Georg Lukács' classic works – *The Historical Novel* – into English in 1969.¹⁰ The course seminars were organized around sociological theories of literature, and included works by Lukács, Lucien Goldmann, and Pierre Macherey. Note that the realist tradition in literature within

9 Renewed interest in this book has now arisen in Portugal and Brazil (Silvério 2014), with particular reference to the notion of *poliamor* and the practice of swinging. My parents had already conducted interviews with couples in the USA who engaged in swinging in the 1960s (O'Neill N. & G., 1972).

10 Also available at the time in English were Lukács volumes *Writer and Critic* (1970) and *Studies in European Realism* (1972).

which I had already been immersed during my B.A. degree in New York reappeared here, but with a Marxist oriented angle deriving from the fields of aesthetics and a budding French structuralist literary criticism.

Purely accidentally, in conversations with two friends from Galicia at Essex (Alberto Meixide Vecino and Abel Caballero Álvarez) who were in their own masters' courses in Economics, I gained a keen interest in the mountain regions of Ancares and Caurel, as a potential fieldwork site for my thesis project on folktales.¹¹ Why folktales? Because they provided an ideal link between the domains of literature and anthropology. I proposed to do a collection of oral literature narratives in an isolated village, as a contribution to the field of the Sociology of Literature, but with a sociolinguistic and ethnographic angle, not merely a literary one.

What kind of references did I use, and from which academic traditions did they come? Let us first take a glance at the fieldwork site. What is certain now, looking back at this stage of my “immersion” in anthropology, is that a critical perspective, and an inclination towards minute detail, predominated long after my years of reading in English and American literature.

3. CAUREL, 1973

Three summer months in a small hamlet a few hours by foot from the town of Seoane de Caurel¹² in the province of Lugo provided enough materials to analyze in this masters' thesis. This first prolonged field stint of mine was carried out without the slightest training or preparation. I used what grammars of *galego* I could get

11 Via Abel Caballero, I later met Raúl Iturra of the Department of Anthropology at Cambridge, who had been researching another but very different rural community in Galicia (Iturra, 1980).

12 Some years later, I met Rainer Lutz Bauer (Bauer, 1983), who conducted research on this town.

hold of in Santiago de Compostela,¹³ before travelling by train to Monforte de Lemos, Folgoso del Caurel, and then Seoane. My principal contacts in Santiago were Xosé Manuel González-Reboredo at the Instituto Padre Sarmiento de Estudios Gallegos, whose library I frequented assiduously and, slightly later, Xaquín Rodríguez Campos, of the Department of Philosophy and Social Anthropology at Santiago's University. The village I finally chose – Visuña – was extremely distant from any local towns, without electricity, and lost at the bottom of a deep valley. In my unconscious, just as the Boasian American anthropologists believed, I suspected that the more elderly villagers would have probably “preserved the culture” of the region in a more complete fashion.

I was totally taken by surprise, after a month or so of recording folktales, to find children to be my major storytellers. A section of the thesis was dedicated solely to Children's Tales, as one of the most interesting patterns I found was that of the endings of numerous tales in Castilian phrases, spoken by wild and threatening animals such as wolves and foxes. All other (domesticated) animals (sheep, goats, dogs) would speak Galician, as did the narrators. This pattern I saw as a symbolic reproduction of a *diglossia* situation (Ferguson, 1972), wherein Castilian served as the dominant language, and Galician as the dominated one. However, I had not been trained in linguistics, so my bibliographical references were limited. Some useful texts in English were Douglas (1968), Giglioli (1972), Fox (1968), and Sartre (1963), while others on Galicia included Arias (1963), Calero (1971), Montero (1966, 1973), Piñeiro (1967), Riego (1971), and Risco (1962, 1971). We must recall, nevertheless, that in Essex my recourse to references on Galicia was minimal, and even when in Santiago de Compostela briefly prior to diving into the Caurel region, there was little time for bibliographical research.

13 Calero, 1968; Grande, 1968; Instituto de la Lengua Gallega 1971/1972.



Valley of the *Serra do Courel* where the village of Visuña is located, showing one of its *barrios*, 1973. (All photographs by the author)



One of Visuña's peasant families, 1975.



Stove in my host family's kitchen, 1973.

Of course, at the time, the overall situation of the Galician language was quite bleak, as was that of the very peasant economy¹⁴ I had unwittingly immersed myself in within the

14 Beiras 1967, 1972; Miguez, 1967.





Sharpening a scythe with my host, 1973.

Caurel mountain range. Note that I had not arrived in Galicia with the slightest notion of the wider and growing anthropological literature on “peasant studies” in Great Britain, the US, France, and other countries.¹⁵ So I began studying what would some decades later be termed a *subaltern* population of peasants, dominated politically, socially, educationally, linguistically, and geographically. The animal tales I collected, in which these hierarchical relations were marvellously reproduced in symbolic fashion, thus served as a means of highlighting

15 My familiarity with this growing multi-disciplinary literature on peasants was limited prior to my later anthropological studies in London, although some key works were already cited in my thesis bibliography (Potter, Foster & Diaz, 1967; Shanin, 1971; Wolf, 1966, 1969).

local perceptions of social domination by the speakers of the Castilian language. During the three months I spent in Caurel, very little time was available for collecting or reading any relevant bibliography, and Lisón-Tolosana’s survey of the region (*Antropología Cultural de Galicia*, 1971) did not prove of much use. After the conclusion of the fieldwork, there was virtually no more time in Galicia to delve into bibliography, and I had to return to Essex in order to write and submit the Masters’ thesis in September of 1973.¹⁶ As I then transferred to London in October of 1973, this time restriction continued, limiting the bibliography of this thesis to a mere two pages (O’Neill, 1974).

In conclusion, what references and theoretical inclinations I possessed at the start of this first brief fieldwork stint were a mere amalgam of combined readings from four major fields: realist literary works (both in New York and Essex), preliminary training in American cultural anthropology, Marxist theories of aesthetics and literary criticism (in Essex), and my own individual idiosyncratic readings in folklore and sociolinguistics.¹⁷ Note that, in contrast to the later situation during my Portuguese fieldwork, *no specific author stood out as having had a profound influence on my thinking*.

However, I had been completely “converted” to the study of Northeast Iberian rural society. The experience of these three months in Caurel provided me with a sense of total identification with immersion fieldwork of the Malinowskian type (“speaking, working, feeling, and thinking like the natives”). It gave me a sensibility towards peasant populations in remote mountain ranges, command of the local language, a sense of having received intensely generous hospitality on the part of

16 Although the formal date of the degree (With Distinction) was 1974.

17 As mentioned earlier, these readings were piecemeal and a result of my disciplinary transition from literary studies to anthropology, incorporating authors such as Linda Dégh, Richard Dorson, Alan Dundes, J. L. Fischer, Vladimir Propp, Y. M. Sokolov, and Leon Trotsky.

the villagers¹⁸, and a feeling that I had comprehended the plight of this subaltern group and succeeded in “translating their culture” in however minimal a form in my masters’ thesis.¹⁹ And I also developed a severely *critical view* of the overall social situation of Galicia and the Galician language as marginal, subjugated, and dominated within a peasant, “underdog” population.

In short, the personal experience of the fieldwork was much more intense than any use or application of one or another author or theoretical stance.

4. LONDON, 1974

When in late 1973 I arrived in London, I experienced another form of culture-shock. British functionalist anthropology – still alive and well among part of the faculty of the London School of Economics and Political Science – was unspeakably boring. After an interview with José Cutileiro and Julian Pitt-Rivers, I was accepted in the doctoral program and attended a battery of courses and seminars coordinated by the latter two Mediterraneanists, as well as Ioan Lewis, Jean La Fontaine, James Woodburn, Peter Loizos, David McKnight, and Maurice Bloch. But

18 My social origins as a partially Hispanic American were of significance here, I think. While my mother was from rural Pennsylvania (with ancestors from Alsace-Lorraine in France and Baden-Baden in Southern Germany), my father’s father was an O’Neill from Puerto Rico who migrated to New York, where he met my father’s mother, who was from Écija in Andalusia. Practically bilingual, I learned Spanish with these paternal grandparents in New York, so my accent in Galicia was probably classified as sounding American but with a Hispanic note. A cousin of my father’s was a prominent figure in the field of psychology in Puerto Rico (O’Neill, A. M., 1978). Even today, when I vote by correspondence from Portugal in US elections, I always tick two “ethnic” categories: white or European, as well as Hispanic or Latino.

19 Two major articles which I later published contain the essence of my argument in this Masters’ thesis (O’Neill, 1984a; O’Neill, 1992b).

reading Africanist volumes such as Meyer Fortes’ *The Dynamics of Clanship among the Tallensi* put me to sleep in the library. I was developing an interest in doing fieldwork in a Southern European or Latin American region, and in terms of geographical coverage, only one of the four major branches of anthropology in London at the time was sufficiently interesting – the Mediterranean school. But this was a regional preference; in terms of theory, the Mediterraneanist group had its own dose of aridity. The four main thrusts in London anthropology at the time were; a) British functionalism, linked closely to the African colonial past; b) Marxist anthropology, with offshoots in Parisian structural-Marxism, and the newly formed London journal *Critique of Anthropology* (with which I collaborated)²⁰; c) an Indianist group; and d) the Mediterraneanist tendency, with strong links to Oxford. There was absolutely no kind of “Europeanist” group existent, despite isolated cases of individual ethnographers conducting research on European regions. So the geographical proximity of the Mediterranean was of interest to me, although the most attractive theoretical stances came from the Marxist and structural-Marxist camps. Mediterraneanist students were frequently seen as only half-baked anthropologists, not willing to go out to the field in the Third World; a repeated comment that I heard went along these lines: “You’re going to do ethnographic fieldwork in Portugal? That’s not real anthropology, but simply rural sociology!”

At the traditional Friday-morning seminar at LSE in 1974, I recall attending a whole semester of presentations revolving around Talal Asad’s polemic volume *Anthropology and the Colonial Encounter* (1973), some of which descended to shouting and screaming between followers of the functionalist school and partisans with Marxist leanings. Indianists and Mediterraneanists felt somewhat marginal to this crossfire. I certainly caught

20 The colleagues involved in these early years of the journal included, among others, Josep Llobera, Joel Kahn, Maïla Stivens, Anne Bailey, Steven Nugent, Victoria Goddard, and Nukhet Sirhan.



the spirit of the Marxist critique of functionalism and structural-functionalism, echoed in the generally critical angle of many (if not most) of the articles in *Critique of Anthropology's* early years.²¹ But Mediterranean anthropology – due to its obsession with the (supposedly) universal values of *honour and shame* – as yet provided no specific author for me to identify with, apart from my supervisor José Cutileiro (whose monograph stimulated me more for its literary style than its theoretical orientation). I simply could not seem to situate the hamlet I studied in Caurel within this Mediterranean framework. Although in 1973 and 1975 I sensed the vibrancy of this challenging new area of anthropological studies of the Mediterranean (John Davis' volume *People of the Mediterranean* confirmed this slightly later, when it was published in 1977), there occurred no “click” of profound identification with any particular author or theoretical stance. The whole world of literary realism (and structuralism) in which I had been so immersed in New York and Essex seemed to have vanished into thin air.²²

Things changed radically in the spring of 1974. Two developments were significant, the first having to do with readings and the second with political transformations in Portugal. In the Museum of Mankind's library in London, I came across a book which had a decisive influence on my choice of fieldwork locations – António Jorge Dias' *Rio de Onor: Comunitarismo Agro-pastoril* (1953). This monograph, influenced by Ruth Benedict's concept of the “Dionysian personality”, dealt with a village social organization similar to that studied in Central and Northern Spain by Joaquín Costa and José María Arguedas²³, termed *colectivismo agrario*. This isolated vil-

21 I myself exhorted a highly critical view of Cutileiro's monograph in a jointly written article in this journal (O'Neill & McAdam Clark, 1980).

22 My Portuguese supervisor sensed this, and in one of our tutorials emitted the following sardonic comment, referring (in the abstract) to British anthropologists: “So, Brian, you left the great authors of world literature for these b.....s !”.

23 I had not yet read these authors, nor at this early stage the monographs on similar communities in Central Spain by Susan

lage in the Northeastern Portuguese province of Trás-os-Montes, on the border between the district of Bragança and the province of Zamora, captured my fascination. One of Dias' chapters referred to an entire region, between Rio de Onor and the provincial city of Chaves, characterized by forms of *comunitarismo*, or collective hamlet and village structures owned and repaired by corporate groups of families. The *conselho de vizinhos* was an echo of the Spanish *consejos de vecinos*. In fact, I recalled (although I had not studied these structures closely) similar elements in Caurel. I had seemed to come upon a region where my prior experience in Galicia might become an advantage, in both geographical and linguistic terms.

My fascination increased. Why was Dias' monograph not translated into English? How many Mediterranean anthropologists had any inkling that such collective communities existed in Northern Portugal? Could these villages and hamlets be in fact characterized by such idyllic egalitarian structures, expressing a kind of archaic rural “democracy”? These kinds of queries occurred to me as I read on and on. While Fortes' dull monographs on Africa put me to sleep like aspirins, Dias' writings on these isolated pockets of collectivism acted like espresso caffeine. I became jittery and impatient to begin another stint of more prolonged fieldwork, either in Caurel once more or alternatively in Northern Portugal somewhere near Rio de Onor.

The second development erupted in April of 1974, with Portugal's Carnation Revolution. From my student residence hall in Bloomsbury, this entire process might have seemed quite remote, had it not been for my reading (by subscription) of the Portuguese weekly newspaper *Expresso*. The 25th of April²⁴ brought a

Tax Freeman and Ruth Behar, or James Fernandez' works on Asturias, although I had read the latter's 1966 article on folklore and nationalism (Fernandez, 1966). See also, although much later, António Medeiros' comparative research on Galicia and the Minho province in Portugal (Medeiros, 2006a, 2006b, 2013).

24 As I conclude this text in Lisbon, on 25 April 2014, commemorations of the 40th anniversary of this significant event are under way.

definitive end to the colonial war involving five former Portuguese colonies in Africa, and to the authoritarian regime of the New State (*Estado Novo*) headed by Salazar and Caetano. This was an extremely interesting process to watch from afar in London, but gradually I came to formulate an alternative research project in Portugal which might be carried out within this highly refreshing and stimulating “revolutionary” context that seemed to be budding. Note that the Franco regime in Spain was still in power; although the similarities between the regions of Trás-os-Montes and Caurel were striking, this new overarching factor linked to the political climate was particularly influential in guiding my choice and preference of future fieldwork towards Portugal rather than Galicia.

So, come 1975, during our LSE “pre-fieldwork seminar”, I prepared two places for fieldwork simultaneously. As time went on, and as I awaited various requests for funding for a year’s fieldwork in both Galicia and Portugal, I precipitated the situation and returned to Caurel to begin a longer stint of ethnography. I returned to the same village and the same host family, but was in Santiago at the time Franco’s regime fell. In February of 1976, I received a letter confirming a grant from the International Section of the Calouste Gulbenkian Foundation for fieldwork in Bragança, near Rio de Onor. This letter caused me a dose of anguish, as I balked indecisively for more than a fortnight, unable to decide between remaining in Caurel (where things were going well again) or shifting my trajectory and crossing over into Portugal, to study the region characterized by *comunitarismo*. Later on, in the aftermath of Franco’s regime, the Caurel mountain range was renamed Courel.²⁵ The 1916 poem by Robert Frost – “The Road Not Taken” – was fresh in

25 At this time, in February of 1976, I had just presented two sessions in the Seminario de Etnografía y Folclore co-organized by Xosé Manuel González-Reboredo and António Fraguas Fraguas at the Instituto Padre Sarmiento de Estudos Gallegos (Comunidades Campesiñas da Serra do Caurel and Contos Populares de Lugo).

my mind, as I tended towards the more adventurous, more difficult task of learning yet another language (although one not very distant from *galego*) and moving to Portugal. The fact of obtaining funds was an important factor, but not a decisive one: the national political context in Portugal still constituted an inviting field, as did the theme of rural collectivism.

The years in London from 1973 to 1975 thus saw a definitive shift from literature into anthropology, via the accumulation of a new layer of British social anthropology on top of my earlier partial training in American cultural anthropology. This process was accompanied by the continuation of a critical spirit, particularly with regard both to the limitations of functionalist ethnography as well as the incompleteness of the “Mediterranean” ignorance of Northern Iberian communities.

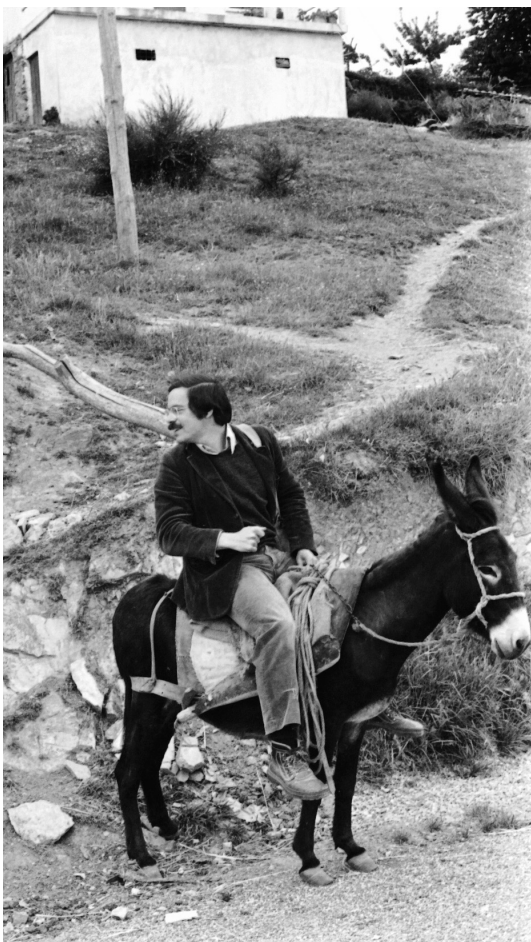
5. TRÁS-OS-MONTES, 1976

At first sight, the field location I chose in Trás-os-Montes – in the municipality of Vinhais, forming part of the district of Bragança – exhibited many characteristics reminiscent of Caurel. This area, now the National Park of Montesinho, is just south of Verín, and close to A Gudiña. Agriculture, livestock, altitude, landscape, and house structure looked remarkably similar. My interest in oral literature had not disappeared, but I found little to record, so my attention turned to the *tornajeira* tradition of reciprocal labour, or *entreajudá*, which I had already observed in Galicia. *Tornajeiras* constituted unpaid exchanges of work between families, and occurred predominantly during the summer harvests and the winter pig-slaughter (*matança do porco*).²⁶ But I rapidly came to realize that at these occasions, apparently egalitarian in nature, the extremes of the social hierarchy exchanged very unequal amounts of labour and time. The rye threshings (*malhas*

26 I later published an extensive analysis of these pig-slaughters (O’Neill, 1989a), which I had little space for in my 1982 Ph.D. thesis.



de centeio) of wealthy *proprietário* families might last almost two days, whereas a threshing of a poorer *jornaleira* would take only an hour or two to execute. Yet each household would normally only send one helper to the others. These imbalances and inequalities fascinated me, and I quickly realized that the level of *comunitarismo* evident in this hamlet of less than 200 inhabitants was qualitatively different from that I had read about in Rio de Onor.²⁷ Hierarchies and inequalities seemed to pop up everywhere, rather than egalitarian, supposedly democratic practices.



The author near the entrance to the Northern Portuguese hamlet, 1976.



Lareira in a middle-level *lavrador* household, 1976.



Unmarried elderly peasant, 1976.



Threshing work-party (*malha*) at which unequal *tornajeira* labour exchanges took place, 1977.

27 A session with the Portuguese ethnologist Benjamin Enes Pereira, in the Museu de Etnologia in March of 1976, with detailed maps of this region between Bragança and Chaves, helped me select a number of possible hamlets for study in the area.

The linguistic scene was a struggle. Even though I spoke both *galego* and *castelhano*, learning rural Portuguese to fluency took about a year. I began by transforming my *galego* into Portuguese, but

even so, as the villagers spoke so much about local fields and animal names, and cursed a lot, it was only in the second year of fieldwork that I began to feel confident. Later on, in Lisbon (see Conclusion below) I was systematically ridiculed for my Trás-os-Montes accent, which took some time to lose; friends told me to “stop speaking Portuguese as if you were in the 18th century”, or to avoid “speaking Portuguese like a country bumpkin”. Clearly, the geographical isolation of the hamlet crystallized a certain accent and specific grammatical habits, which I could not yet distinguish from standard national Portuguese.

Now, a second aspect of social life began to take the spotlight very quickly, as the agricultural activities abated in the winter of 1976. Long interview sessions at night provided hours of genealogical recordings. I had never dreamed in New York, or even less in London where I detested most of the arid kinship studies that I had to read, that marriage and family structures could become so captivating. Perhaps a taste of these structures was already visible in Caurel, but the time-span of three months there (even augmented by another few months in 1975) did not provide sufficient intimacy to delve into this dimension. Four elements caught my attention: a) a double conjugal system, in which formal church marriages were limited to a select few in the hamlet, alongside a wide array of “informal” unions, concubinage, civil marriage, and temporary liaisons²⁸; b) a high rate of male and female celibacy²⁹; c) the *natolocal* post-marital residence of various villagers for periods of up to 15 years³⁰; and d) a strikingly high

rate of illegitimacy.³¹ This last aspect haunted my genealogical diagrams, as I had to design a specific form of illustrating so many half-brothers, half-sisters, unknown fathers (*pais incógnitos*), and *zorros*. *Zorro*, or *zorra*, was the term reserved for a bastard never recognized juridically by her/his biological father.³²

What all this information brought about in my second year of fieldwork was the perception that, in fact, I wasn't actually studying “kinship” at all. At least not in the abstract. The entire hierarchical system of social groups – or social classes if you will – was intimately interwoven with this pattern of *two separate but interlocking marriage domains*, one characterizing the wealthier households and the other predominant among the day-labourers. So, actually, I had stumbled upon a sphere of bastards, concubines, servants, and shepherds which served to support and uphold the hamlet's social hierarchy itself. This was kinship plus inequality.

Did I find any help within the suitcase of monographs I had brought with me, and which sat beside my straw-filled pillow in my minuscule room? No. In fact, I tried very diligently not to read any of that literature, which included volumes like John Davis' *Land and Family in Pisticci* (1973) and John Campbell's *Honour, Family, and Patronage* (1964). The Mediterranean, with its nuclear families, early marriages, absence of bastards, and among the Ashanti by Fortes (see O'Neill, 2011) – consisted in the continued residence of the bride and groom in their respective parents' households, the groom only sleeping in his wife's parents' house at night. All meals, and all daily work, were dedicated by the bride to her natal household, and by the groom to his natal household.

31 Over 11 decades in this hamlet, 47 % of baptisms were of bastard infants.

32 Later on, in the early 1980s, João de Pina-Cabral and I exchanged views on these patterns of illegitimacy, which also characterized the rural region of the Alto Minho where he had conducted fieldwork (Pina-Cabral, 1986). Shortly afterwards, in an article of mine entitled *Dying and Inheriting in Rural Trás-os-Montes*, I emphasized the link in Vinhais between bastardy and the regions strict practice of post-mortem inheritance (Pina-Cabral, J. de et. al., 1983).

28 I was later to contextualize this double marriage system within the scope of Duby's research on medieval marriage, and Goody's analysis of European marriage patterns.

29 I had not yet read Bourdieu's classic 1962 article on “célibat” in Southern France, later reissued as *Le Bal des Célibataires* (2002a) and translated into English as *The Bachelors' Ball* only as late as 2008.

30 This form of residence – described for Ireland by Fox, Rio de Onor by Pais de Brito, Central Spain by Freeman,



neolocal residence, and obsession with honour and shame, seemed anathema to this strange and curious hamlet and its weird marital customs. Nothing seemed to fit anywhere. I became seriously confused and alienated. But I pushed on, taking down genealogies of virtually every household and confirming the illegitimacies with documentation in the Parish Registers and the Civil Registry Office in the local town, Vinhais. This historical research took more than half a year to complete, as I could not photocopy the volumes but rather had to copy the information by hand. This “underworld” of informal unions, bastards, fleeting romances, and hidden relationships was so fascinating that I tried at one point, on enormous graph paper, to sketch out all of the clandestine amorous links in the hamlet with several colours. This proved so difficult and labyrinthine, that I had to abandon the task. But the query remained and hung over my head throughout the rest of the fieldwork: how am I going to make any sense out of all of these Caribbean-style single mothers, absent (or merely nocturnal) fathers, and their bastard children?³³

I procured some kind of explanation for all of these patterns within the forms of social reproduction of households, via practices of inheritance and succession to the role of “household head”. This was not an easy task, but just as in the case of illegitimacy, villagers talked quite naturally about the matter. It became very clear that bastards were excluded from inheritance, at least if they remained unrecognized by their father. Equal partition was the rule, although indirect practices of favouritism for one or another child did occur. There was no tradition of primogeniture in the region, and no preference for men over women. So what I concentrated on was the timing of the first

marriage of a daughter or son in wealthy and middle-level households; here the first to marry usually obtained a privileged position in the family, leaving all other siblings in a dilemma, which led either to their leaving (emigrating or marrying out) or remaining celibate (which did not prevent them from having children within the “parallel world” of non-marriage). Later, I was to term this a definite and conscious *strategy*, but at this point in the fieldwork the word did not occur to me. In other words, practically half of the hamlet lived somewhat removed from formal marriage; a small minority was selected as privileged heirs to the household line, albeit never in a purely juridical fashion. Although I did not use the term in my thesis, retrospectively now, the hamlet looked (metaphorically) at the time collectively very schizophrenic or bipolar.

I was lucky to have been able to extend my funding beyond the first year, which meant corresponding with my supervisor concerning the value of staying another year or not. As José Cutileiro had shifted his career from anthropology to diplomacy at this time, Peter Loizos – now a Mediterraneanist specializing in Cyprus, and later visual anthropology – became my supervisor; my lengthy reports on the fieldwork led him to encourage staying longer, so my total fieldwork stint reached two and a half years. During this second year and a half, much of my historical research was done, and I went a few times for library research to Porto and Lisbon. Contacts with other anthropologists, whether Portuguese or foreign, were scanty. As I stayed so long, and (as was the case in Caurel) as hospitality was so contagious, villagers repeatedly urged me simply to stay, and marry the schoolteacher or the priest’s sister (also a schoolteacher), despite both the latter being in their mid-40s at the time (I was 27). The day I left I must have carried about 7 bags full of *fumeiro* in the departing bus, next to which two elderly village women wiped tears from their eyes. In fact, the return to London was so traumatic, so depressing, that I had to extirpate the reverse culture-shock from my system by writing about it. In the Portuguese translation of

33 Any fear of being a kind of retrospective voyeur was dissipated immediately, as I came to realize that all of these relationships – even in the priest’s and the Church’s eyes – were perfectly “natural” or doxic to the inhabitants, never constituting any form of shameful, repressed, or reprehensible behaviour.

my thesis (1984) I included a final essay entitled “Carta de Regresso a Londres”, in which I tried to document the profound sensation of loss, nostalgia, and distance following such intense and humanly rewarding years within such a welcoming community.

Continuities with the past? I felt like Stendhal’s mirror, slowly passing across a landscape, registering the blue sky as well as all the mud on the road below. I had turned into a realist observer, minutely dissecting the tiniest and least visible details of hamlet life. Had my critical spirit disappeared? To the contrary. Although I had no inkling that all my queries and perplexities concerning the dual marriage system actually did have a logical explanation, my impulse was simply to collect sufficient materials, and as rigorously as possible, to be able later to analyze the system in retrospect. What I ended up doing was to ask carefully directed questions at key moments to selected villagers.³⁴ I became hyper-critical of Mediterranean anthropology, which seemed to have ignored such regions. I was also critical of peasant studies, which seemed to have relegated to oblivion these simple “smallholding peasants” who – in contrast to the conflict-ridden communities of Southern Portugal, Southern Spain, and Southern Italy – never struck, never rebelled, and never revolted. Had they simply, in Gramsci’s terms, been subtly but successfully duped into hegemonic indifference? When earlier in 1975 I communicated to my first supervisor my intent on conducting fieldwork in a Northern Portuguese region, he responded dryly: “Is anything interesting happening up there? Wouldn’t *any village or hamlet*, however isolated, be intrinsically of interest to study during a period of social transformation?”

34 This kind of questioning sadly ignored for decades in anthropologies methodology manuals has now been more consciously codified by Devillard, Mudanó & Pazos (2012). See also O’Reilly’s comment: “One of the beauties of ethnographic research is that as you learn you ask more questions and as you ask more questions you learn different things that send you off in different directions. The key is to be flexible” (2005:181).

6. STRATEGIC CHOICES AND INSPIRING AUTHORS

Once the fieldwork period in Portugal came to an end, in September of 1978, clearly an entirely new and different phase was inaugurated. The British commonly term this the (usually) alienating stage of “writing up”.³⁵ The four consecutive years following this shift back to London, culminating in the defence of my thesis in September 1982, effectively closed the decade of the 1970s.

What followed – from my move to Portugal in 1982 as a research assistant in the Calouste Gulbenkian Foundation and in 1984 to the Institute in which I still teach today³⁶ – constituted a qualitatively different phase. Delving into this phase would take us onto another plane of analysis altogether, involving another decade and the anthropological world in Portugal as a whole. So let us concentrate on our central theme – practice theory – and precisely how I came upon and used this theory during this final four-year phase spanning the transition from the 1970s to the decade of 1980.

I have strategically chosen two terms to guide these quasi-conclusions: *strategic choices* on the one

35 Writing up suggests to me a caricature of an anguished ethnographer facing a desk and floor of chaotic, disorganized fieldnotes, photocopies, photographs, maps, sketches, and other assorted aids collected and hoarded during the fieldwork stint. Although I never queried colleagues or teachers about the term at the time, I am fascinated by it now: writing “up” must have meant turning this chaos into some kind of (illusory?) order, from the lower level of documents spread out on the floor. Bearing in mind my literary inclinations, I wonder what writing down might have meant.... I wrote this note prior to coming upon Karen O’Reilly’s marvelous book on fieldwork *Ethnographic Methods* (2005), in which both the phrases writing down and writing up are meticulously treated, in a novel and attractive fashion (O’Reilly, 2005:Chapter 8).

36 The University Institute of Lisbon (Instituto Universitário de Lisboa), formerly termed ISCTE (Instituto Superior de Ciências do Trabalho e da Empresa).



hand, and on the other, *inspiring authors*. Not that it is intrinsically useful simply to hierarchize or classify authors whose reading had varying effects upon our own work. But it might be helpful to apply the microscope to two books and two articles that had an electric effect, and which served to guide practically the entire structure of a thesis and a future book.

One might apply a concentric model, somewhat akin to Evans-Pritchard's famous circles denoting the ecological and mental conceptions of the Nuer world in 1940. Firstly, we might think of an entire array of conjuncturally useful references, from the most obscure local document photocopied to published articles and books, whether in English or Portuguese. Secondly, a narrower circle would include works which proved highly fruitful and provided some degree of stimulation. Finally, an "inner circle" refers to works which played a key role in inspiring our thinking and our ordering of field materials. Sometimes, these inspiring texts – or inspiring authors – can turn the tide of suffering which can characterize the process of *writing up* into the playfulness of a game. This is what happened to me. Let us see briefly precisely how.

Two points are of import here. At this time, and despite a year of French at Columbia during my college years, my command of the French language for reading purposes was minimal. When I began teaching at ISCTE in the mid-1980s, I found most of my undergraduate students had much more advanced reading skills in French, so I panicked. Luckily, in 1989 I obtained a Bourse de Haut Niveau from the CNRS for a two-month teaching and research visit to the Université de Paris X at Nanterre, where I collaborated in courses administered by Georges Augustins. Following that stint, I became reasonably fluent, and never again read anything in French as slowly as before. In this year, a major book by Augustins was published³⁷, which I was quite sad had not been published a decade earlier. Augustins'

37 *Comment se Perpétuer? Devenir des Lignées e Desins des Patrimoines dans les Paysanneries Européennes*. Nanterre: Société d'Ethnologie, 1989.

synthetic comparative analyses of patterns of succession and inheritance within peasant households in Europe produced a loud echo in my Portuguese materials from the 1970s. But the important point is that, at the beginning of the decade, while trying to "write up" in London, my receptivity to French anthropological writings was still virtually nil.

The second point concerns the persistence of difficulties in trying to insert my materials within a Mediterranean regional framework. Either the hamlet I studied was located well outside the Mediterranean world, or its situation was severely marginal. Reading and re-reading Pitt-Rivers (1963), Peristiany (1966), Campbell (1964), and Davis (1977) only highlighted the difficulty of shifting our views to a more Central European mountain context, with comparisons extensive to the Pyrenees, the Alps, and even the Himalayas. This dilemma prodded me to delve more deeply into what was beginning to be termed a kind of *historical anthropology*, with a more European-oriented vision. Authors like MacFarlane, Laslett, and Berkner offered a range of methodological and theoretical angles of much more fruitful application to the peasant households I had analyzed. Even Laslett and Wall's diagram system for the annotation of internal household structure (1972), with all of its drawbacks, was highly useful for plotting out every one of the 57 families I had mapped out genealogically. However, Laslett himself was stupefied at the extremely high rates of illegitimacy I had documented, and his theoretical framework (highlighting the "bastardy-prone sub-society") could not sufficiently explain the patterns I was struggling to understand.

I found, therefore, a convenient and partially conclusive alternative to the Mediterranean framework within both these historical and demographically oriented works, as well as within the Oxford monographs of Cutileiro³⁸ and Lisón-Tolosana on Portuguese and Spanish communities. I had not read, prior to fieldwork in Portugal, the first edition of Lisón-Tolosana's *Belmonte de los Caballeros* (1966), so its impact on my 38 I refer here to the aforementioned A Portuguese Rural Society (1971).

writing was significant, particularly for the author's microscopic analysis of local social groups, and its use of the "generational model" of Julián Marías. Although close to the Mediterraneanists, neither Cutileiro nor Lisón were obsessed with the honour/shame nexus, nor indeed viscerally at all with a Mediterraneanist worldview. Crucial to my own work were their incorporation of local (and national) historical materials, even though these were of a different nature from the Parish Registers so fruitful to me in Northern Portugal. At the time, I had not consciously registered yet that both authors constituted excellent examples of what later became referred to as "native anthropologists".³⁹ Both had adopted – or in their own words, impersonated – the style of a British ethnographer "looking back" or looking obliquely at their own natal communities in Spain and Portugal, as it were, through the looking-glass of the Oxford anthropological monograph. For some reason – was it actually literary? – Both of these books seemed to me to have penetrated much more deeply into the social life of the rural worlds they analyzed.

Of course, these two studies were quite different from an array of American "community studies" of Mediterranean and European rural villages available in the 1960s and 1970s. So the utility of these sometimes myopic descriptive portraits of Italian, Greek, or French towns was limited, and the limitations of defining what the "community" itself was, crept up on me as I read in a comparative fashion. In a subtle fashion, I felt unconsciously a sort of distance from these North American community studies. For some typical examples, see the monographs of Wylie (1957), Banfield (1958), Friedl (1962) or Lopreato (1967). The edited volume of Potter, Foster, and Diaz (1967) contains a

39 Three key articles that come to mind here are Narayan (1993), Mascarenhas-Keyes (1987), and Zulaika (1995), although Cutileiro himself had made an early contribution to this problematic (1973) and, even earlier, Srinivas had focused on the problem in his own case in India (1966).

comprehensive treatment of community studies in peasant contexts, with particular attention to American authors. Slightly later, of course, Benedict Anderson gave an entirely different meaning to the term *community* in his classic formulation concerning *Imagined Communities* (1983).

And now we arrive at the "inner circle" of entirely central texts and authors. Two specific books were absolutely decisive here, along with two articles. The first of these was Jack Goody's *Production and Reproduction: A Comparative Study of the Domestic Domain* (1976), which, curiously, no-one in London at the time seemed even to notice. What had caught my eye slightly earlier, in an article of Goody's (1970) about vertical and lateral inheritance in Europe and Asia, was his use of the concept of "strategies of heirship" in rural regions of the zone he had begun to term *Eurasia*. This wide comparative view of the Eurasian continent was quite novel, and I read no other works at the time which applied such a comprehensive (today we would say "global") angle on European village communities. I had indeed seen in the Portuguese hamlet precisely how these strategies of heirship worked in practice. When I – a simple graduate student beginning to "write up" my field materials – spontaneously expressed my enthusiasm for the book, an Africanist functionalist professor hastily commented that it was "a simply horrendous volume, and badly written at that". She was clearly incapable of shifting her outdated theoretical orientation towards what Goody termed his style of *comparative sociology*. The comment at first sight shocked me, but I quickly dismissed it as a hopeless affirmation of parochial, empiricist positivism. I did not, however, go on with a more exhaustive reading of Goody's further works on mortuary practices in Ghana, literacy, or critiques of the notion of the savage mind.

The key factor here was the *click* verifiable between Goody's formulations in *Production and Reproduction* and my own fieldwork materials. Now, we must note that much later on (actually, only in 2008!), I came upon a highly revealing article of his in which a totally heterodox



opinion of ritual was taken (Goody 1977), indicating his profoundly rebel stance vis-à-vis his colleagues at the time. Also, and following the publication of his *The East in the West* in 1996, all the way up to now in his latest volume *The Eurasian Miracle* (2010), the author's iconoclast and hyper-critical views on the West's theft of numerous Oriental cultural patterns and developments has captured my attention. Goody has now become – not only within anthropology but also the rapidly expanding multi-disciplinary field of *global history* – a sort of devil's advocate for a globalized birds-eye view of the cultural history of Eurasia.⁴⁰ What I seek to emphasize, though, is firstly that this rebel streak in his writings is a relatively recent phenomenon, and, secondly, that at the start of the 1980s *only one book* was sufficient to provide crucial clues and theoretical stimulation for the writing of my thesis.

A second click then occurred when – during my readings of some of the *Annales* social historians – I came upon the 1976 English translation of an article by the French sociologist Pierre Bourdieu entitled “Marriage Strategies as Strategies of Social Reproduction” (1972a). I had read two texts by the same author – one on the Kabyle house, and the other on Algerian peasants' concepts of time – neither of which had caught my attention in particular. But this piece, with its microscopic enumeration of economic, monetary, symbolic, and psychological factors invoked in Southern French marriage and inheritance practices, hit a note of comparative relevance with my Portuguese data. Specifically, the concept of *strategies* captivated me (echoing Goody's strategies of heirship), and I read the analysis of peasants' obsessions with the transmission of their households, landed property, money, and symbolic capital (name, prestige, honour) with bated breath. Note that I read French very haltingly at this time, so only some months later came upon the author's monumental article preceding this

40 See, for instance, the two important and polemical volumes *The Theft of History* (2006) and *Renaissances: The One or the Many?* (2011).

one, entitled “Célibat et Condition Paysanne” (1962), which I read very slowly. As was the case with Goody's 1976 volume, here I found another anthropologist (the “early” Bourdieu, more a philosopher and ethnographer than a sociologist) who did *not* fall into the trap of interpreting a rural European community as necessarily Mediterranean in nature. This was crucial for my readings. Finally, with Goody's book and these two articles on Southern France, I had come upon theoretical frameworks which removed me from the Mediterranean panacea.⁴¹

A third click occurred upon reading of the same author's *Outline of a Theory of Practice* (1977 [1972b]), which I came upon totally by accident. A small number of thesis-writers in our Department at the LSE decided to create a reading group, which met every fortnight, and spontaneously pooled our readings of selected volumes that had recently appeared on the scene. Two aspects immediately struck me about this book: its emphasis on the inequalities and imbalances in the exchanges of agricultural labour-time (*thiwizi* in Algeria), and its highlighting of the multiple strategies involved in marriage practices. Note that at this time *practice theory* as a coherent body of concepts was just becoming solidified; I had not yet read in French the author's *Le Sens Pratique* (1980), and the *Outline* had only been published in English translation a few years earlier, in 1977. Now, also of import in the volume were the concepts of *domination*, *social reproduction*, and *tempo* (the musical rhythm of actors' performances and manipulations of rituals). I was not particularly attracted to the idea of *habitus*. But what also seemed quite novel in the book was the sense of “action” and volition in the notion of marriage strategies. Action theory, or processual theories, were

41 Note that there was no coherent Europeanist anthropological school at this time, despite a steadily growing body of ethnographic literature particularly on Southern and Eastern European regions. But this is another story, with little space to develop here...For example, Jackson's important edited volume of 1987 (*Anthropology at Home*) had not yet been published.

not yet so clearly enunciated.⁴² All of these elements in this volume rang bells with the Portuguese materials on bastardy, marriage patterns, celibacy, and the social reproduction of rural households.⁴³

But more important than these ethnographic similarities, in my retrospective view, was the *hyper-critical spirit* that characterized Bourdieu's writing style. This is a crucial point. On virtually every page of the book, I had underlined phrases or jotted notes in the margins, frequently mentioning parallels with Portugal. Yet it seemed that I was writing in a sort of vacuum, with the functionalists, the Indianists, and the structural Marxists still battling it out in seminars at the LSE. The Mediterranean camp fast concluded that my materials were quite distinct from their honour/shame and patronage obsessions, while the Marxist tendencies showed a partial, albeit receptive, attitude towards some of Bourdieu's ideas. As we know, Bourdieu's works were published with a time-lag in the USA and Great Britain, as Müller has noted, in her excellent obituary (Müller, 2002), so there were not many of his other texts available in English for me to extend upon. We might distinguish here between an array of later works in France about the author⁴⁴ and a series of volumes on his work in English.⁴⁵ And following his death in 2002, a shower of reviews highlighted the importance of his *oeuvre*.

42 See Barnard's later volume, particularly Chapter 6 "Action-centred, Processual and Marxist Perspectives" (2000:80-98).

43 See my own later publication specifically focused on Bourdieu and some of his theoretical applications in Portuguese rural society (O'Neill, 1989b). The definitive work in Portugal on Bourdieu's work is the volume compiled by Madureira Pinto and Borges Pereira (2007).

44 This now long list might include overall surveys of Bourdieu's work Accardo & Corcuff (1986), Bidet & Leneveu (1996), Caillé et. al. (1992), Corcuff (2004), Encrevé e Lagrave (2003), Hong (1999), Pinto (1998) or Pinto, Sapiro & Champagne (2004) as well as such critical attacks as that of Verdès-Leroux (1998).

45 Some of these are: Grenfell (2004), Lane (2000), Robbins (2000), Shusterman (1999), and Swartz (1997).

But my key point here is less concerning practice theory *per se* as one among many anthropological theories today, and more about the impact that *this early phase of practice theory evident in the 1977 volume of the Outline* had upon my thesis writing. I suspect that my reading of this book transformed the writing of this thesis from agony into a game. A whole series of formerly chaotic, labyrinthine elements seemed immediately to fall into place. I suspect that my literary realist vein was somehow resurrected by this book, and my own critical spirit heightened by Bourdieu's own critical view of everything and everyone. Contrary to many critics of the author's work as a whole, I believe that this early work (1962, 1972, 1977) incorporated an action-oriented theoretical stance which did *not* stop at the "objective" level of descriptive analysis, but went onto another plane – the "subjective" individual strategies and steps taken by *social agents* in the social arena. Far from becoming a pessimistic realist portrait of Stendhal's mud, the author's microscopic dissection of choices, strategies, dispositions, and orientations evinced a growing theory centred upon the actors' (or agents') potential moves within a specific legal and social context. Escaping from Durkheim, it subtly interweaved Marx with Weber.⁴⁶

Can we really be so influenced by one sole book? Why not? Like Goody, Bourdieu was a rebel. Despite his sometimes awkward style – running on in long paragraphs, with occasional footnotes even longer than the main text – and multiple lateral references to sociology and philosophy, his relentless attacks on sloppy thinking, antiquated social science models, and the internal contradictions within his own colleagues' reasoning, all together make for intensely stimulating reading. Clearly, his background in philosophy probably preconditioned his mindset towards a sharp and incisive mode of hyper-active critiques of other authors' ideas and other theoretical stands. Basic premises, methodological steps, and conclusions are resolutely dissected and reviewed, with a heightened sense of possible

46 Note that, as in the aforementioned cases of Cutileiro and Lisón-Tolosana, as well as that of Zulaika (1995), Bourdieu also studied his own natal village.



alternatives. Similar to Freud's, Marx's, and Lévi-Strauss' searches for the ubiquitous mental, politico-economic, or social *unconscious*, Bourdieu also procures realities behind the superficial curtain of the *illusio*, within the corporeal bodily expressions of a mental *habitus*, or the apparently ordered and coherent "observable empirical behaviour". It is here that social agents exert their individual and collective strategies, which in practice theory formulations distances this model from other more limited interactionist or transactionalist views. And, in a rather pleasant balance, all of this germinating practice theory is expounded slowly and carefully throughout the *Outline*, at the same time incorporating and interlacing high theory with micro-level ethnographic examples and descriptions of Kabyle local society in Algeria. For someone like myself, with a strongly literary background and a structuralist entry into anthropology, the nature of this text was extraordinarily attractive. Here was an entirely *new way of looking* at any corpus of ethnographic material from anywhere. Due to its pitiless breakdown of an entire rainbow of theories, texts, and authors, the volume provided – following successive re-readings – an extremely useful tool for sifting through and analyzing what at first sight (and was this an *illusio*?) appeared to constitute inchoate, disordered, and unexplainable social patterns.

Thanks to this animated prop – section after section, chapter after chapter – the writing of my thesis turned into a sport. From this point on, it was as if I had all along been weaving a realist novel...

7. CONCLUSION

I shall stop now, as this short story has come to an end. Many factors have been left out of this idiosyncratic retrospective look at the 1970s. The following decade – particularly after the completion of my thesis and move to Portugal to take up a research post in the Gulbenkian Foundation in 1982 – brought a number of quite new and different

patterns.⁴⁷ My adaptation to Portuguese academic life brought with it linguistic, cultural, and mental assimilations, along with more extensive contacts with anthropologists in France, Spain, Holland, and the USA. Clearly, in the 1970s I was most influenced by American and British anthropology, with a dose of Galician and Spanish ethnology. Later, in the 1980s and 1990s, Portuguese and French ethnologies left their marks on me as well. Does this make me genuinely hybrid? A less vaguely defined field of "European" anthropology began to emerge, and the publication of my thesis in Portuguese opened the path to renewed contacts with the hamlet studied in Vinhais.⁴⁸ As the decade went on, it seemed less and less probable that I would return to the US. Fieldwork *per se* came to a stop, except for a few brief visits to Trás-os-Montes, as teaching absorbed much more time and effort than I had expected. I gradually became an expatriate.⁴⁹

47 Crucial at this point was the invitation by Joaquim Pais de Brito to publish a Portuguese translation of my 1982 London thesis as number 7 in the series *Portugal de Perto*, of the publishing house Publicações Dom Quixote in Lisbon.

48 I made a number of visits to the hamlet in the 1980s and 1990s, but the most nerve-racking one was a year or two just after the publication of my monograph in Portuguese (1984b). I had offered a copy of the book to half-a-dozen of the families with whom I had worked, and I was somewhat apprehensive about villagers' potential reactions to my materials on bastardy, non-marriage, and single mothers. To my surprise, no one reacted at all to these aspects of the book, which indicated that these patterns were perfectly doxic, but the priest apparently threw a tantrum concerning the list of landowners in Chapter 3, where his household was identified (albeit with pseudonyms) as one of the four wealthy proprietários. We had to have a long conversation over dinner at his house in order to reach relative agreement on the matter. This problem did not, of course, arise in relation to the English-language version (1987) of this 1984 volume. For more on this kind of dilemma, see Brettell (1993).

49 This was of course a slow process, beginning at the start of my stint in Europe in 1972, and continuing during that decade, in Great Britain, Spain (Galicia), and Portugal. As time went on, no burning desire to return to the States seemed to arise.

The 1990s brought yet more unplanned developments. A sabbatical year in 1994 provided the opportunity of discovering an entirely novel fieldwork region in Southeast Asia, concretely a Portuguese Creole community of Eurasians in an urban sector of the city of Malacca in Western Malaysia (O'Neill, 2000, 2002, 2004). This shift of interests of course brought with it a kaleidoscope of new topics and interests, which have continued to occupy me until the present. Trás-os-Montes did not fall into oblivion though, and I visited the hamlet in the winter of 2008/9 in order to take a look at pig-slaughter celebrations some 33 years after I had analyzed them in the 1970s.⁵⁰ I had never, previous to this Asian research, been interested in ethnicity or social identities, which have occupied a good part of the spotlight now. And teaching – particularly in a course entitled “Minorities of Southeast Asia” – has brought a closer reading that I had earlier of Edmund Leach’s classic writings on Highland Burma in the 1950s.⁵¹ Here was yet another rebel author, as hyper-critical of everyone and everything as Bourdieu, if not even more so. Our modern penchant for “multiculturalism” was certainly evident in this region of Burma as early as the 1940s, when Leach mapped the region during the II World War. Malaysia as well, as I fast came to learn, was an equally fascinating multicultural field.

But we are digressing. One last pattern must be mentioned here, in closing my argument. Inspiring authors are always good to think with, so towards the end of the decade of 2000, I came accidentally upon a

50 See the Prefácio and Posfácio ao Prefácio to the 2nd edition of my monograph *Proprietários, Lavradores e Jornaleiras* (2011) for a brief discussion of these pig-slaughters.

51 See Leach’s original monograph *Political Systems of Highland Burma* (1954) as well as the comprehensive update of anthropological information on the Kachin Hills Region, compiled in Mikael Gravers’ edited volume *Exploring Ethnic Diversity in Burma* (2007). For an application of some of Leach’s ideas in his Sri Lanka monograph (1961), see my 1986 article.

whole series of works by the Cambridge anthropologist Jack Goody which I had no idea even existed. As mentioned earlier, in reference to the 1976 volume *Production and Reproduction*, following the publication of the author’s *The East in the West* in 1996, a new phase of theory was inaugurated. Concentrating on the macro level of one continent – Eurasia encompassing both Europe and Asia – Goody rarely descends to the micro level of local ethnographies. My own work on a Portuguese-Eurasian population (and my “native” informants actually term themselves *Eurasians*) will serve to place real people onto this map, contributing a novel dimension to this macro plane of research on global history and the cultural fusion of East and West. So, pertinent to my argument throughout this article is the *critical spirit* pervading all of these works of Goody concerning Eurasia.⁵² The hegemonic role of Europe, and particularly of imperialist Great Britain, is relentlessly undermined. Clearly, this is not necessarily a Critical Anthropology directly reminiscent of Hymes’ revolutionary *Reinventing Anthropology* (1969), nor of George Marcus’ more recent resuscitation of the term in his *Critical Anthropology Now* (1999), but the spirit of persistent and pervasive hyper-critical comparisons, and analyses of multifarious authors and theoretical currents, reveals a kindred mind-set with that of Bourdieu.

Will I be able to transpose this critical spirit to a curious enclave of purported descendants of the Portuguese, lost in an obscure corner of Southeast Asia? Can this critical angle be transported from Portugal to this modern “survival” of a colonial empire? Have I stumbled upon a post-colonial enigma?

I am perhaps speculating too much. Let us return to the path taken in this essay. Can we combine empirical, “objectivist” ethnographic fieldwork with Grand Theory and a new Critical Anthropology? May we remain rebellious, iconoclastic, and ultra-critical? With the proper strategies and pondered choices, yes. Do we need inspiring authors, and inspirational texts? Of course. Let me return to my chaos of fieldnotes,

52 In particular, see Goody (2006, 2010, 2011).



photographs, tape-recordings, maps, and (above all) books collected in Malacca, with Kafka and Rabelais on my bookshelf above my desk. Let us be realistic: however distant Caurel or Trás-os-Montes – or even for that matter, Malaysia – may be geographically, in my mind, whether from the 1970s or the 1990s, they continue to inspire me. Is Practice Theory still relevant? Let us practice what we have preached. Clearly, we have truly put Practice Theory into practice.

But above all, as realist authors at heart, let us get back to the blank, white page...

Bibliography

As this article is particularly reflexive and retrospective, I initially struggled in trying to limit its references to the 1970s. I rapidly realized that this was an impossible task, and simply gave up. The reader, I hope, will pardon this idiosyncrasy, wherever s/he finds citations to later works in the 1980s, 1990s, and indeed following 2000 (which in some cases appear merely to identify later works of authors or colleagues I refer to or worked with in the 70s). These references are thus quite non-linear, and may “jump” forward a number of years or even decades. Such is post-postmodernism....

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Resum

Quines són les connexions entre el lloc del treball de camp i els autors, textos i teories que inspiren l'etnògraf abans i després de la seva estada? Presentem aquí, en retrospectiva, la sinuosa trajectòria recorreguda en dues localitats estudiades durant la dècada dels 70 – les muntanyes de Caurel a l'est de Galícia i la regió de Trás-os-Montes al nord-est de Portugal. Cap d'elles semblava encaixar dins l'antropologia britànica del Mediterrani dominant i en expansió al llarg de la dècada. Ambdós llogarets semblaven tot menys minúsculs i igualitaris paradisos rurals. Les estructures familiars duals, les dinàmiques domèstiques complexes, els intercanvis laborals asimètrics, les formes de matrimoni alternatives i l'elevat nombre de fills bastards que els caracteritzaven requerien una anàlisi radicalment diferent. La inspiració va venir de la mà de la sociologia comparativa de Goody sobre les estratègies hereditàries a Euràsia, i de la teoria de la pràctica de Bourdieu. La meua formació universitària prèvia en literatura realista i microanàlisi descriptiva va acabar per tenir – degut al treball de camp mateix – un ressò considerable. I l'esperit rebel i penetrantment hiper-crític de Goody i Bourdieu va servir d'inspiració.

Resumen

¿Cuáles son las conexiones entre el lugar del trabajo de campo y los autores, textos y teorías que inspiran al etnógrafo antes y después de su estancia? Se presentan aquí, en retrospectiva, la sinuosa trayectoria recorrida en dos lugares estudiados durante la década de 1970 – la sierra de Caurel en el este de Galicia, y la región de Trás-os-Montes en el nordeste de Portugal. Ninguno de ellos parecía encajar en la antropología británica del Mediterráneo dominante y en expansión a lo largo de la década. Ambas aldeas no parecían en absoluto minúsculos e igualitarios paraísos rurales. Las estructuras familiares duales, las dinámicas domésticas complejas, los intercambios laborales asimétricos, las formas de matrimonio alternativas y el ingente número de hijos bastardos que las caracterizaban requerían

un análisis radicalmente distinto. La inspiración vino de la mano de la sociología comparativa de Goody sobre las estrategias hereditarias en Euroasia, y de la teoría de la práctica de Bourdieu. Mi formación universitaria previa en literatura realista y microanálisis descriptivo tuvo – debido al trabajo de campo mismo – un influjo considerable. Y el espíritu rebelde y penetrantemente hiper-crítico de Goody y Bourdieu sirvió de inspiración.

Paraules clau

Teoria de la pràctica, antropologia crítica, treball de camp, Galícia, Portugal

Palabras clave

Teoría de la práctica, antropología crítica, trabajo de campo, Galicia, Portugal

