The anthropologist as nomad
Serendipity in Anthropological Research: The Nomadic Turn

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Errancy in ethnography and theory: on the meaning and role of ‘discovery’ in anthropological research

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prologue

When I was invited to contribute to a book on the ‘nomadism of the anthropologist’, I immediately shared the intentions of its editors. Quite apart from having conducted my own early research among the nomads, I also have a nomadic story linked to my professional background. Firstly, to my university training. Having come from philosophical studies, I didn’t attend any courses in Italy on anthropology. I was therefore relatively late in turning to the discipline, and was something of an anomaly compared to standards prevalent in the 1970s in many European countries (and today including Italy). Secondly, to my theoretical leanings. By training, I am not firmly committed to any one approach over another. I waver, sometimes perhaps unconsciously, between different paradigms: not in homage to a form of eclecticism, but rather as a consequence of a realization that not everything can be treated in the same way. If I have convictions (or idiosyncrasies) about theory, they have never been of any ‘school’. And lastly, my nomadism is linked to my ethnographic experiences. I have transited, if I may so put it, with a certain conviction through three principal ‘terrains’: the Arabian Peninsula, the Iranian coast of the Persian Gulf, and south-west Pakistan. In Arabia I was with the Bedouin nomads of the Great Nefud desert; in Iran with the fishermen of the Hormuzgan region; in Pakistan with the sedentary farmers of southern Baluchistan. Between the beginning of the first and the end of the third, just under twenty years passed, during which my perspective (and above all my ‘anthropological awareness’) was enriched and changed direction. It was
perhaps also sharpened by the diversity of contexts and objects on which I came to reflect.

I believe that certain aspects of my ‘nomadism’ show some of the limitations, as well as some of the strong points, of anthropology. The lack of absolute paradigmatic references, the transitivity from one disciplinary context to another, and the frequentation of different ethnographic fields can, in effect, impose limits for those who maintain that scientific work is the pursuit of a research programme geared to a hyper-specialist paradigm. But for others, and especially for anthropologists, empirical and theoretic mobility can prove to be a force and to constitute the truly vital elements of the discipline. This is because, by erring (personally or not) from one cultural context to another, and having incessantly to confront otherness (which is, in a sense, its bread), anthropology seems to be in a condition best to reflect, in its underlying inspiration, practice and epistemology, the reality of a ‘world in movement’ like that of today. This mobility in approaching its objects makes it possible to ‘see how’, and to ‘put into perspective’, as no other humanistic discipline can. The anthropological style of reasoning has by now ‘erred’ into bordering fields of knowledge, and is unlikely to be rejected by these in the future. Whatever may happen, the style of reasoning adopted by anthropology will remain a specific and necessary one for describing the ‘human condition’.

Stating the problem

I confess that these, and other more strictly epistemological matters, were not exactly the main focus of my attention when, in July 1978, I made my debut as a field researcher. Nor is it immediately possible to ‘stand aloof’ from one’s ethnographic research (or even to take such a possibility for granted). This possibility is, so to speak, ‘remodulated’ subsequently, on the basis of further experiences of field (and other) research, trial writings, and always the play of memory which of course opens up boundless and often unsuspected spaces for self-reflection, but also entails restrictions and imposes censorship.
If distance in time from the ‘terrain’ enriches ‘field memory’, that same distance also produces an estrangement from the initial results of our research, exposing them to a sensation of thematic and methodological obsolescence. However that same distance has the advantage of enabling us to rethink certain aspects and moments of the work, which at times appear suddenly, and unexpectedly, crucial.

On rethinking a number of things about my work in the field, both the earliest and the most recent, I realized that, however much in our writings we may strive to recount the reflexiveness, positionings and relationships of force in which we have been involved; the misunderstandings, the stratagems and the snares into which we have fallen or more or less luckily managed to avoid, we hardly ever recount the way we came into possession of certain apparently insignificant information which nevertheless helped us to make progress in our fieldwork. Or, if we do, we tend to present the matter in an incidental manner, without problematizing and discussing the exact role of such events in the process of our understanding. This non-problematization does not help to contrast the idea of anthropology as being a rather loose discipline largely founded on chance. And it ties up with another, fairly widespread idea in many academic circles, whereby our discipline would be the product of a ‘non scientific’ practice, and therefore worthy of scant attention inasmuch as it is deemed incapable, unlike others, of evincing generalisations, quantitative data and predictive models. I believe there is not much to be done in the face of prejudices of this kind, which are based on a reductive idea of knowledge. I think we ought instead to explore -- I might say in a Kantian way -- the avenues along which anthropology proceeds, and to highlight the points in common between sciences and knowledge which, though remaining separate by virtue of the different nature of their object and ‘style of discourse’, are often founded on similar reasonings.

One of the elements that join all types of research and learning together is what may generically be defined as ‘discovery’. As regards anthropology, the explanation of how we came into possession of certain information that enabled us to move forward in our research is methodologically crucial. It is connected with what I call ‘ethnographic discovery’, a topic that has in my opinion long been neglected in anthropological literature.
Before going into the details of the question through an example drawn from my own research, a few empirical as well as theoretical explanations are required.

On the empirical level, I must point out that by discovery I do not certainly mean having for the first time witnessed a rite, gathered a terminological system of kinship previously unknown, or been present at an encounter with a population of which no news had ever previously been received. Still less do I mean, by discovery, the innumerable things that come to be known in ethnographic routine, in the daily ‘fatigues’, as Lévi-Strauss observed, which reduce the ethnographer’s profession to ‘une imitation du service militaire’ (Lévi-Strauss 1984: 9). It is true that the anthropologist in the field keeps on ‘discovering’ things. But few of these will allow him (if ever) to reverse a perspective, to problematize in an absolutely new way a theme, or to hit on a factual confirmation of a characteristic aspect of a certain group or society.

By ‘discovery’ in ethnography I mean the identification of something that will allow the person identifying it to alter their perspective on a given theme or problem and, naturally, enable them go forward in the knowledge of their object.

The first social scientist to concern himself with what I call ‘discovery’ here was probably Robert T. Merton. In 1948, he published an article in which he asserted that ‘under certain conditions, a research finding gives rise to social theory’ (Merton 1948: 506). The three key points of Merton’s reasoning were: 1) ‘The fairly common experience of observing an unanticipated, anomalous and strategic datum which becomes the occasion for developing a new theory or for extending an existing theory’; 2) ‘The observation must be anomalous, surprising, either because it seems inconsistent with prevailing theory or with other established facts’; 3) ‘The unexpected fact must be ‘strategic’, i.e. it must permit […] referring to what the discovery brings to the datum than to that datum itself’ (Merton 1948: 506–7).

Abduction, evidence and serendipity

Although Merton makes no mention of it, his issue is linked to the notion of abduction which, according to Charles S. Peirce (1958: 7.218), indicates the logical
process (considered by him to be ‘the first step of scientific reasoning’) by which, by inference, we obtain information about certain things that have a high probability of being true. An example of reasoning by abduction might be the following: I bring home a basket of red apples. After a while I happen to see a red apple on the kitchen floor. I have excellent reasons to maintain that the apple comes from the basket, even though this might not be ‘true’.

Abduction is the logical process underlying what the historian Carlo Ginzburg, in an extensive and celebrated recognition of the processes of enquiry typical of the human sciences, from the history of art to psychology, from medicine to criminology (anthropology however was missing from this roll-call), defined as an ‘evidential’ process (indiziario, in the original). This was discussed by Ginzburg especially in relation to the problem of attributing works of art to a specific artist, to physiognomics and to the search for the causes of diseases in medical diagnostics. What is signified by reasoning according to clues is summed up by Ginzburg himself in the procedure adopted by a hunter:

Man has been a hunter for thousands of years. In the course of countless chases he learned to reconstruct the shapes and movements of his invisible prey from tracks on the ground, broken branches, excrement, tufts of hair, entangled feathers, stagnating odours. He learned to sniff out, record, interpret, and classify such infinitesimal traces as trails of spittle. He learned how to execute complex mental operations with lightning speed, in the depth of a forest or in a prairie with its hidden dangers (Ginzburg 1990: 102).

According to Ginzburg the impact of this paradigm lies precisely in its being ‘not very rigorous’, at least inasmuch as it cannot be quantified – a price which the human sciences seem obliged to pay if they are to achieve results of any importance and, most of all, of any perspicuity. The evidential approach to our knowledge of the world is found in everyday human behaviour, in the sphere of practical and technical expertise. It is closely connected with all types of knowledge or skills founded on coup d’œil, instinct and intuition. Although it also belongs also to the physical and natural sciences, this approach is however characteristic in particular of the human sciences. At least in the measure in which the latter almost always lack the possibility of elaborating an experimental method.
Merton, on the other hand, described the process qualified as evidential by Ginzburg, and abductive by Peirce, with the adjective serendipitous. Merton in fact takes up an English neologism dating from the mid-eighteenth century (serendipity) by means of which in time a series of events, behaviours and situations had come (almost always improperly) to be qualified (Merton and Barber 2004).

Serendipity is a word coined in 1754 by Horace Walpole, after he had come to hear of a ‘silly fairy tale’ (as he himself described it) published for the first time in Italian, in Venice, a couple of centuries earlier and in which is told the story of The Three Princes of Serendip.4

Walpole wrote to a friend,

As their Highnesses travelled, they were always making discoveries by accident and sagacity, of things which they were not in quest of: for instance, one of them discovered that a mule blind of the right eye had travelled the same road lately, because the grass was eaten only on the left side, where it was worse than on the right – now do you understand Serendipity? (Merton and Barber 2004: 108).

The term serendipity was used for two centuries with various shades of meaning, including that of ‘accidental discovery’. But this is not the signification Walpole wanted to give to the word he had coined. He in fact says that the three princes continually discovered, by accident and sagacity, things they had not been seeking. By accident and sagacity, hence by chance and on the strength of an intuitive reasoning ascribable to what Ginzburg calls precisely the evidential paradigm founded on abduction. One must insist on the conjunction ‘and’ because at times it has been maintained, as I was saying, that serendipity might stand exclusively for a chance discovery, such as that of a treasure underneath the floor of an old house, or that certain particular circumstances can enable us to act randomly in one way rather than in another. The examples of this latter way of understanding the notion of serendipity (and thus of ignoring its evidential-abductive component) can be summed up in the definition of the term given in the Concise Oxford Dictionary: ‘... the faculty of making happy and unexpected discoveries by accident’.

The difficulty of shaking off this generic (and not perspicuous) idea of serendipity and of tracing it back to an intuitive process based on inference remains also among
those who recognise this human mindset as having an important role in research. In an article based on his fieldwork in China, Frank Pieke, for example, recognises that serendipity is central in
discarding the notion that reality is law-governed and knowledge is finite ...
[and that] serendipity in this enterprise is less random and more proactive than suggested by the standard gloss of the term' [the definition of the Oxford Dictionary] ... [as it] describes the creative tension between structuration and event, and that balance between control and creativity which defines science as a vocation within a discursive community (Pieke 2000: 129–30).

Nevertheless in his detailed and interesting description of the research done by him in Beijing and in Raoyang, Pieke seems precisely to neglect the essential characteristic of serendipity: that of denoting random unintentional discoveries based on (abductive) intuition, and to confine himself instead to underling the ‘fortuitous’ character of certain encounters or situations that turned out to be important to the pursuit of his research.

On the other hand, according to Walpole’s idea, which at the empirical level anticipated the formal definition of the abductive process given by Peirce, are excluded from the qualification of serendipitous all ‘discoveries’ by chance, whereas in that definition may be included discoveries made ‘by sagacity’. These, albeit starting from a datum stumbled on by chance, make it possible to attain to more general conclusions about someone or something. To this specification must be added another, namely that Walpole’s definition also includes the idea that what is discovered is not being sought at all: in the ‘silly fairy tale’ the sons of King Serendip continually discovered ‘things they had not been in quest of’.

I now propose to illustrate, through a concrete case deduced from my personal experience in the field, this manner of proceeding which, if not exclusive to anthropological reasoning, does however play an essential role in ethnographic practice.

Prepare yourselves therefore to read ‘a silly fairy tale’. But not without first being informed of my personal ‘nomadic’ context into which it fits.
a silly Fairy tale (part 1)

After reading philosophy I had turned my attention to Americanistics, also publishing a work on violence in Amazon societies based solely on ethnographic literature. However my closeness at that time to some French Africanists induced me to consider Africa, rather than South America, as a possible field of research.

At the beginning I had in mind an extremely vague project on the nomads of the Horn of Africa. But, thanks to a contact procured for me by my then thesis tutor Marc Augé at the École des Hautes Études en Sciences Sociales, I was enrolled (and here serendipity did not come into it at all!!) by a development research company and was aggregated to a fairly numerous and highly heterogeneous team of experts, with the task of conducting a two-year survey on aspects of ‘social change’ in Saudi Arabia. After an exhausting wait of various weeks, I left France (and an Italy troubled by obscure terrorist plots), and arrived in Riyadh on 15 July 1978.

In that period ‘my anthropology’ was inspired by a ‘critical’, para-Althusserian Marxism, complete with the ‘dynamist’ approach of Georges Balandier and with the ‘Manchester School’ perspective, both of which took on the dimensions of conflict and change as landmarks central to research. I had in fact for a number of years been mixing with some French anthropologists who were particularly sensitive to the use of categories and analytical tools originating from non-orthodox Marxism.

When I reached Arabia I felt reasonably well seasoned theoretically, but I was ill-trained in the techniques of field research. My background was philosophical and the lectures in anthropology attended at the EHESS in Paris during the two previous years were not, in this respect, of much help.

If I consider my beginnings I must admit that I treated my research from a prevalently theoretic, and probably too abstract, angle. I did however activate the tools at my disposal: my studies of domestic groups in East African agropastoral societies; travel literature on the Middle East, and particularly Arabia; the segmentary lineage theory, a spot of history, geography and linguistics. Modern works, and notably monographs, on the Bedouin of the Middle East and especially of Arabia, were rare. After the masterpieces by Musil on the Rwala (1928) and the book by
Dickson on the Bedouin of Kuwait (1949), in practice nothing more at all had appeared until the classic study by Emanuel Marx on the Negev Bedouin (1967), followed by the concise, but very good, one by Donad P. Cole on the Ahl Murra of Rub‘ al-Khali (1975); by a very interesting sociological study by Paul Bonnenfant on the urbanization of the nomads in Saudi Arabia (1977); and by a number of works on the Bedouin of Syria and Arabia by Fidelity and William Lancaster (whose extremely interesting book on the Rwala came out in 1981, when unfortunately my field research had already ended).

My almost total initial ignorance of the language forced me to rely on local or Maghreb interpreters, and only after several months was I able to follow some of the relevant aspects of conversations hinging on hitherto circumscribed topics. My initial linguistic handicap prompted me to focus my attention on the economic and social side of life in the Bedouin communities. In any case these subjects came under the fields of enquiry contemplated by the project for which I was working (transformations of productive processes, social change, urbanization). Also, they could call for the application of the theoretic tools at my disposal to objects of interest to me: relations of force and forms of control of the nomadic communities by the state; unequal distribution of resources; formation of new elites, etc.

Arabia was not an ‘easy’ field for me. True, you could find everything there (from cassette readers to under-the-counter alcoholic drinks, from Australian fillet to Cuban cigars and Bulgarian cheese). But the foreigners in that country lived out on a somewhat ‘distorted’ social limb. The authorities did nothing (on the contrary!) to attenuate this state of chronic separation from local society, and police control (though ‘discreet’, as I was able to try out on a couple of ‘critical’ occasions) was far-reaching. In some centres, where wahhabi orthodoxy is more deeply rooted, than elsewhere in Saudi Arabia, the ‘mistrust’ of non-Muslims and foreigners in general could be cut with a knife.

I arrived in Arabia during the reign of Khaled ibn Sa‘ud (1975–82), just when the first serious contradictions were beginning to show between the ‘Islamic’ face of the country, pervicaciously proclaimed by its political and religious hierarchies on the one hand, and the reality of an urban society increasingly overwhelmed by
modernisation and consumerism on the other. The 1970s were a period of sudden accelerations, some of which upset important balances within the Muslim world itself. If the seminal event of this period was the Iranian Revolution of 1979, in Arabia the shock-moment was the occupation, precisely at the end of that same year, of the haram in Mecca by a few hundred rebels led by Juhyman ibn Muhammad al-‘Uthaybi and by Muhammad ibn ‘Abdullah al-Qhatani, this lastnamed having been proclaimed mahdi (‘he who leads’) by his supporters. They had been hoping for a popular uprising leading to the removal from power of the Sa’ud, deemed politically corrupt and betrayers of the faith. Many of these rebels were students of the Islamic University of Medina and disciples of Egyptian ‘ulamā’ affiliated with the Ikhwan (Muslim Brotherhood) who had been expelled by Nasser and welcomed to Arabia by Faysal, Khaled’s predecessor, in the 1960s. The episode, quelled with bloodshed after much hesitation by the Saudi authorities (in the haram at Mecca it has been forbidden to carry arms and to spill blood since pre-Islamic times), was only a widely acknowledged signal of the deep unrest felt throughout Saudi society in those years and a prelude to today’s conflicts.5

In short, the atmosphere in Arabia was tense and, unlike what I later had occasion to experience in Baluchistan (where things were also extremely explosive, but entirely different), I was myself viewed with suspicion by all. On the whole the Bedouin looked kindly upon me. But owing to their not always easy relationship with government bodies, I was continually at risk of being viewed by them as a possible channel of information for the authorities. Meanwhile the authorities appeared not to take much notice of me, though actually they were very well informed about what I was doing. They were worried, as I later heard, that my work might touch on politically ‘sensitive’ topics (which was fairly obvious when talking of the distribution of resources). Even the officials from the company by whom I was employed had their suspicions, sometimes of the same kind of the Saudi officials.

At the time of my own research (I don’t know what exactly happened afterwards) there were a number of ‘tribes’ (qabāyel) of high ‘reputation’ (sharaf) who seemed not yet to have ‘settled their scores’ with the Sa’ud. To one of these tribes belonged the Bedouin among whom I spent most of my research time. They were a sub-fraction
of the Shammar, the main supporters of the Ahl Rashid of Hail, and had been sworn enemies of the Sa’ud in the struggle for the control of the Arabian Peninsula between the mid-nineteenth century and the 1920s. Having favoured the study of this group caused me quite a few difficulties in organizing my enquiries into the relations between the Shammar and the Saudi authorities, and all the more so because some ‘sections’ (ashair, fukhud) of this important tribe had for decades settled in Iraqi territory while keeping in touch with those still in Saudi Arabia. About these relations I was simply asked by the Bedouin themselves not to ask questions.

What had driven me to focus my work on this group is hard to say exactly. Certainly the historical background contributed. My approach tended to privilege socio-political change and transformation, and to play down more proficient approaches to the symbolic aspects of cultures, including the structuralist one then still in vogue in Paris. Chance, too, however played its part: the courteous (and unexpected) collaboration of a functionary from the local emirate. And perhaps also a certain aesthetic influence that fitted my predilection for the historical dimension. When I visited Hail for the first time, many of the constructions, though abandoned and in ruins, dated from the days of the Rashid. Some of these crumbling buildings (which the bulldozers were clearing away) still revealed their interiors decorated with red and yellow geometrical motifs, as in the watercolours of Anne Blunt. Seen from the top of the kasr, Hail was surrounded on all sides by dense tresses of palms, whose intense green contrasted subtly with the pinkish sands of the Great Nefud desert. On rereading, once back in Europe, the description of these places left by Doughty in that masterpiece, *Travels in Arabia Deserta* (1888), I ‘retroactively’ reinforced these impressions, traces of which will certainly be found in my early work (Fabietti 1984).6

Much of my research was devoted to outlining the processes of structural change induced in Bedouin communities by the introduction, in the reproductive cycle, of new kinds of resources: wage-paid work, government subsidies, and above all land, granted by the state first for rent, and later as property. Perhaps the most significant result of the research carried out by me in Arabia consists in the outlining of some aspects of a newly emerging social scenario.7
Since these were Bedouin it seemed obligatory to deal with questions regarding the segmentary-lineage theory and hence with their genealogies. At the time of my research on this subject in Arabia, things had, in some ways, come to a dead end.

Obviously it was known (and long had been) that genealogies did not 'tell the truth'. It was also known that they were liable to processes of compression, fusion, fission, selection, forgetfulness and invention. It was, in short, known that they were the fruit of a manipulation both unconscious and intentional. There was consequently no point whatever in trying to trace the history of the Bedouin tribes on the basis of genealogies. The great book by Max Von Oppenheim, *Die Beduinen* (1939–68) was for me only a terrifying list of names.

Genealogies were known to change continually. More stable at levels farthest from our time, they grew increasingly fluid as they drew closer to the present. Bedouin genealogies in fact explain not so much the past as the present. They justify the state of political relations among groups. Briefly, the more politically united they are, the closer their respective ancestors become; the fewer relations they have, the farther away from each other are their respective ancestors (and not vice versa!).

Precisely during my stay in Arabia I happened to read an article by Robert Montagne dated 1932. Montagne, who in the 1920s was director of the Institut Français de Damas, had been able to make contact with a number of Bedouin of the frontier which today separates Syria from Saudi Arabia, and among these were some Shammar. To my great surprise, Montagne had included in that article the genealogical structure precisely of the sub-fraction on which I had focused my own work. Between the ‘data’ in my possession and those supplied by Montagne there were however some notable discrepancies. In Montagne’s work the group in question (ar-Rmal) presented itself as comprising the following nine sub-fractions:

Hazim, Gared, Khanshar, Mneikher, Kelab, Seloug, Racham, Omur and Dweilé. On the basis of the data that I had personally gathered, this group was formed by as many sub-fractions. But their names were almost all different: Msellam, Khasciarán, Khanshar, Khatlán, Hadlán, Muhammed, Racham, Amra and Ali. Only two names
recurred in both cases: Khanshar and Racham. In just over fifty years (the time lapse between Montagne’s survey and my own), no less than seven of the nine sub-fractions making up the ar-Rmal group had changed!

Who was telling the truth? Of course, it is possible that Montagne may have received second-hand information, or that I myself had been misinformed .... Having checked the exactness of my information, and having no evidence to maintain that Montagne’s information was bogus, I resigned myself.

I felt there was nothing else to do but try to explain, with convincing formulas and words comprehensible to an (Italian) anthropological public at that time unfamiliar with the subject, what had been the ‘genealogical dynamic’ of the Bedouin tribes of Arabia. To render better the idea of what this ‘genealogical dynamic’ was, I felt (apropos of ‘errancy’ between areas of knowledge ...) that a term taken from the vocabulary of the sciences of the Earth might suit my case: actualism. In geology, actualism (or uniformism) is the theory whereby the agents currently active in changing the Earth’s landscape are the same as those that altered it in the past. Translated into genealogical terms, actualism to me meant something very simple: knowing that genealogies justify only the present and not the past, the processes (and causes) on the basis of which the Bedouin groups tighten or slacken their alliances must in the present be the same as those which determined the arrangement of genealogies in the past and from which future transformations stem. Per se, the idea was certainly not new. But it seemed to me that the notion of actualism, which I kept turning over in my mind during the months of my stay in Arabia, might serve to represent better than any other the image of Bedouin genealogies as fluid, changing entities, whose transformation was however guided by always identical factors. Rather than change as a result of someone’s decision or due to the occurrence of sudden events, genealogies had to change, or so I thought, as a consequence of slow, uniform processes, imperceptible not only to the eye of the ethnographer (which might be excused ...) but also to that of the Bedouin themselves. What ‘intrigued’ me most, in the case in point, was that although I had two ‘snapshots’ of the genealogy of a group, it was not possible to capture the passage from one snapshot to another.

But it was not these themes, I repeat, on which I had focused my research.
However, one day something unexpected, and anomalous, happened.

On my way with a group of Bedouin from the sub-fraction with which I had my main contacts, to an oasis on the edge of the southern Nefud, we came across the home of a Bedouin belonging to the same sub-fraction, who was introduced to me as a profound connoisseur of Shammar genealogies. When the conversation got going I couldn’t help asking him to list the names of the segments (fikhna) that were part of ar-Rmal. This man confirmed what the other Bedouin had told me until then, namely that ar-Rmal was made up of the nine groups named earlier: Msellam, Khascharān, Khanshar, Khatlān, Hadlān, Muhammed, Racham, Amra and Ali. When the conversation turned to other topics he suddenly added:

‘Msellam, Khatlān, Hadlān, Racham and Amra, together with Seluj and Farha, are Umkhalaf (allies).’

This last statement seemed to leave some of my fellow-travellers flabbergasted and, though puzzled, they were unable to contest it in any way.

When I tried to understand better what the genealogist pundit meant by introducing a distinction between ar-Rmal and Umkhalaf, the Bedouin himself answered that the sub-groups called Umkhalaf were so denominated by the fact that ‘they were very closely connected with one another’; that they ‘were very close’.

I was not sharp enough to understand whether in that critical situation I had been the witness to a feat of mnemonic skill by the expert genealogist, or whether a minor ‘political drama’ had just been enacted. Who was the genealogist speaking to? To the anthropologist or, as it occurred to me later, to the members of his own lineage group? Was that Bedouin telling the others that ‘manoeuvres’ were in progress, or was he exploiting the situation for purposes known to himself only? One last possibility: had I been escorted on purpose to him so that I might ‘spread’ the news of a change, capable of reaching the ears of some important personage? At that time a number of sub-fractions were receiving lands from the government, whilst others were on the waiting list. These questions will clearly never be answered. Nevertheless something had happened with that genealogical ‘manifestation’.

If we compare my snapshot of the ar-Rmal genealogy with that of the expert genealogist, it is clear that missing from the Umkhalaf group were the sub-groups
Khanshar, Khasciarrân, Muhammed and Ali (which are however part of ar-Rmal), whereas within the Umkhalaf appear the names of two new sub-groups: Farha and Seluj. Whilst Farha does not feature in the genealogy collected by Montagne, Seluj ‘returns’ (Seloug, in Montagne’s transcription ‘à la française’).

How was it then that Seluj (Seloug) had first been present (Montagne) and subsequently vanished (Fabietti) and now reappeared among the Umkhalaf groups?

Now there is something that really might be called a ‘serendipitous situation’! A new, unexpected datum, something I had not been in quest of, was, once it had been correlated with other data, not only confirmation of a dynamic which no one had ever been able to observe in its ‘factual’ reality; but it also helped to reconfigure the picture of supposed political relations present in that moment on the ground.

The situation can in fact be interpreted as follows.

The sub-groups called Umkhalaf had intensified their relations by interacting with one another to a greater extent than they themselves had been doing at that moment with the Khanshar, Khasciarrân, Muhammed and Ali groups.\textsuperscript{10} Very probably the Khanshar group, from which the shuyukh of ar-Rmal have by now long originated, had set up, with the advent of land distribution, strategies aimed at reinforcing their own already politically pre-eminent position.\textsuperscript{11} For that purpose it had favoured the alliances with a few groups (Khasciarrân, Muhammed and Ali) to the detriment of others whose only answer was to intensify their relations to the point of defining themselves, by contrast, as Umkhalaf.

The fact that in the Umkhalaf group the names of two sub-groups appear which were not included in the present formal ar-Rmal genealogy, can be traced to a process of the same identical nature, and is an essential part of the evidential procedure sparked by the serendipitous encounter with the Bedouin genealogist. Farha and Seluj might in fact have been two groups with which the other Umkhalaf had just recently started to interact in a continuous manner. The fact that one of these two groups, the Seluj, had in the past been part of the formal genealogy of the ar-Rmal (Montagne), whilst at the end of the 1970s it was excluded from it, might mean that for a certain period it had left the field of interaction with the other segments, but was currently ‘getting closer’. The idea that the principle of actualism was a metaphor also valid in anthropology could have been considered by no means odd.
Had the phenomena that I have described evolved in line with the dynamics of that time, the genealogical relations between the sub-groups included in the formal ar-Rmal genealogy and those that were part of Umkhalaf could have led to what is known as genealogical segmentation. However the outcome of such a process never appears concluded once and for all. Indeed the phenomena of coalescence and fission were such as to involve, at later points in time, other possible groups, which in the period of my research did not feature within either the ar-Rmal or the Umkhalaf. Some of the sub-groups, which in the late 1970s appeared in ar-Rmal and/or Umkhalaf, could, with time, have ended up interacting with groups different to those of that time, and thus have disappeared from the genealogy of both, in the same way in which other groups might have appeared both within ar-Rmal and within Umkhalaf (as for example in the case of Farha).

This reasoning cannot therefore but give rise to a partial predictive model, in the sense that it affects only the outline of a dynamic form. The reason for this ‘partiality’ of the model lies precisely in the fact that whilst it may be maintained that the dynamic of genealogical changes is due to identical processes, it is not possible to know what relations the groups will develop in future and with whom.

I hope that the silly fairy tale told by me may have served to illustrate with some degree of truthfulness the procedure that may happen to be adopted in field research, and the type of generalisations (among the various) which it is possible to produce in anthropology. Other approaches in ethnography certainly exist. But I believe that in our (human) ‘sciences’ the evidential, or if we care to call it serendipitous, or abductive method is one that has too often been sacrificed, in the written and oral tradition of ‘how to do research’, first to models inspired by an ‘objectivist’ tendency, and later to models that have perhaps granted too much to the dialogical and reflexive dimension of fieldwork. Can the awareness of such a method be an anchor, if I may so put it, for our anthropological nomadism?

notes

1. As George E. Marcus wrote ‘what happened in 1980s is that many other disciplines, especially humanistic ones, like history and literary studies, in an effort to renew themselves with a social relevance became passionately interested in many of anthropology’s established framing concepts.
and postures. In literature, history and art, interest in the nature of cultural difference and the expressions and exercise of power through cultural innovation became the fashion in a far less parochial manner’ (Marcus 2005: 678).

2. This opinion has undoubtedly been reinforced since the ‘interpretive turn’ established itself, which nevertheless established a timely break from the earlier ‘objectivist’ approaches, after these had proved to be widely untenable.

3. ‘Discoveries’ of this type certainly occurred in the past (at least from the point of view of western observers), but from the period of Jean de Lery and his Tupinambas, to that of James Clifford and his postmodern museums, the space for discoveries of this kind dwindled to virtually none at all.

4. The name of ancient Ceylon, now Sri Lanka.

5. For a general picture of the recent political developments and of the internal opposition to the Sa’ud, see Al-Rasheed 2002.

6. Once again what T.H. Lawrence wrote in his introduction to the 1921 edition of Doughty’s book was confirmed: ‘The more you learn of Arabia the more you find in Arabia Deserta’. The subject of the ‘construction’ of one’s ethnography on the basis of ex-post suggestions is certainly an epistemologically relevant one.

7. I realize today that the work I devoted myself to, although I did also arouse some appreciation, was not backed by an outstanding amount of ‘empirical data’. On the subject of ‘data’, it is true, it is not possible to decide ‘how many’ these must be, and often one hears it said that it is mainly the plausibility of the picture we produce that constitutes the guarantee of the validity of our ethnography. Nevertheless it is certainly true that my own work remained fairly abstract, possibly due to my philosophical background which fostered in me an excessively speculative attitude (at least so it seems to me today).

What I was in a position to present is in any case an analysis of the processes of social differentiation which the attribution of land produced within the Bedouin community. By combining an analysis of the social structure with that of a pastoral economy increasingly subjected to market dynamics, I attempted to show how the processes of differentiation (and of probable social stratification tending towards a structural stability) were the product of factors such as the attribution of land, the market, political relations which fractions of tribes maintained both among themselves and with the central authorities; but also how factors of individual choice, strategies of risk differentiation and of social prestige contributed to articulate the whole process. These analyses were not included in a detailed manner in my book on the Shammar (Fabletti 1984), but in a series of articles which I published later.

A further sphere of reflection offered to me by my research on the Bedouin was the renota quasstia of the structurally ‘sub-productive’ nature of the domestic Bedouin group, and therefore of the policies of intervention in the nomadic sector. These were however reflections that I conducted a few years after having concluded my research. The starting point came to me from the celebrated work of Marshall Sahlins on the sub-productive nature of the domestic group, in which I came across once again, several years after first reading it, quite a while after the end of my research in Arabia. I criticized the ‘developist’ approaches to the Bedouin reality not certainly through any attachment to an image of the cultures studied by anthropologists as something that should be preserved in its ‘authenticity’. This idea, in itself naïve in the social sciences, would be still more bizarre if referred to the nomads of Arabia. Considering their capacity to adapt to ever new situations to which, for thousands of years, they have had to face (from the birth of the urban phenomenon to writing, from the ‘hydraulic’ states to the rise of Islamic religion, from the advent of western technology to the oil boom of the 1960s and to the subsequent regional crises), it could be said, by reversing the title of a celebrated book by Bruno Latour, that the nomads of the Middle Eastern regions ‘have always been modern’.

My criticism contemplated first of all a redefinition of the domestic Bedouin group released from excessively ‘pantological’ images elaborated in the past. In this way I was trying to reproblematize the subject in the context of the question both of development and on development, and to show how the central issue was that of the hegemony which the ‘stronger’ languages (those of the agents of development) exercise upon those of the people affected by the development plans, and above all in the language (and in the minds) of the clients of the projects themselves (in
this case the Saudi authorities). On this point I owed much to the well-known essay by Talal Asad (whom I met for the first time in Arabia) on translation in the tradition of British social anthropology, a work that came out several years after the end of my research (Asad 1986); but also to the works on the domestic community by Claude Meillassoux with whom I had mixed assiduously in Paris in the previous years (Meillassoux 1975).

8. Although the nomads have known writing since time immemorial (obviously I am not talking about their literacy rate!), their genealogies have always been strictly oral. The oral character of genealogical memory in fact allows that flexibility in the ‘use’ of genealogies which is extremely functional to the practical life and to political relations among the groups. It has to be said that as a consequence of the steady imposition of central administration upon the lives of the nomadic populations, genealogies can become stable in that they are transcribed to political purposes, that is to say as a means of affirmation of, and demand for status or particular rights from the central authorities. This is a theme dealt with in more recent years by Shryock (1997) with reference to the Bedouin in Jordan, a subject which at the time of my own research I knew nothing about except for a couple of feeble clues which however I did not take into consideration.

9. I had found the concept of actualism, or uniformism, in Charles Lyell’s Principles of Geology (first published in 1831) when I had devoted myself, as a philosophy student and as a dabbler in anthropology, to the birth of this discipline and to its relations with the sciences of the Earth and with archaeology in the Victorian age.

10. By ‘interactions’ is meant the complex relations which two or more groups can maintain at various levels: economic, logistic, matrimonial, and ‘political’ in the strict and in the broad sense of the term.

11. A process, that of the distribution of land to the Bedouin by the Saudi Crown (to which the land belongs by decree), begun in 1968 and which however only subsequently concerned the Shammar of the Had province.

references


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