

Anthropology in a New Era: A Conjunctural Assessment

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Abstract: The conditions for the practice of anthropology as a social science in Europe have again changed considerably since the beginning of the new century. In this paper I was asked to make an assessment of what are the contemporary conjunctural constraints that mould our practice as anthropologists. I start by considering the political environment that frames our institutional practices on opposite sides of Europe. Then I go on to propose that we need to be more explicit about the slow and silent erosion of the background assumptions that used to underly our anthropological thinking throughout the twentieth century. I propose that, both in methodological and theoretical terms, we are facing today a new anthropological synthesis—using this last word to refer to the broader analytical parameters that frame our discipline.

Keywords: Anthropology, social and cultural; European anthropology; Durkheimianism; Franz Baermann Steiner; anthropological synthesis; embodied

Over the past twenty years the conditions for the practice of anthropology as a social science in Europe have changed considerably—not only politically, but also methodologically and theoretically. It is perhaps time again to cast our gaze forward and seek to understand what our present conjuncture expects of us in our practice as anthropologists. I came to Prague from Portugal and Britain, on the other side of Europe, where I have worked for the past four decades. Thus, my more established grooves of thinking are strongly influenced by the Durkheimian tradition which, in any case, has followed me since the days when I was trained as an Africanist anthropologist in Southern Africa in the mid-1970s. I feel, therefore, that it is indeed a privilege to come to the Czech Republic to dialogue with you,

at a moment when Europe, and the world more broadly, are again undergoing such major and rapid change.

Anthropology in Europe

I am unavoidably reminded of two other moments that deeply marked my career—both professionally and intellectually. The first was the Fall of the Wall of Berlin and the parallel founding of the European Association of Social Anthropologists. I will never forget the sheer elation of being able to welcome in Coimbra in 1990, for the first time, anthropologists coming from Eastern Europe. There was a strong Czech presence at that meeting who went on to organize the following EASA meeting in Prague, in 1992, of which none of us who participated will ever forget. Václav Hubinger, Ernest Gellner, and others played a significant role at that meeting. Memorably, the Rector of Charles University started the proceedings with a speech in Latin, signifying thus the sense of shared historical heritage that we were celebrating. These were heady moments on the one hand, but they were also uncertain moments, on the other. Czechoslovakia was splitting just as our meetings were taking place. You will excuse if I quote the famous aphorism of Voltaire, but we were pregnant with the future.

In 1986, Portugal had entered into the European Economic Community, soon to become the European Union, finally making feasible the democratic and anti-colonialist project that had driven the 1974 Carnation Revolution. For us, Portuguese academics, after half a century of intellectual and political oppression, the early 1990s were moments of great responsibility, when we felt that it was finally the time to build modern universities, create up-to-date research centres, and launch adequate postgraduate programmes. We knew we had no time to lose. For us then, the company of the Eastern margins on the other side of Europe felt like an indispensable balance in a Europe that, as an ecumenical field of communication, might otherwise have been too heavily weighted towards the North.

The second moment that I need to remind you of was when, indeed, our worst fears were validated. As a consequence of the 2008 financial crisis in New York, the central European countries who controlled the Euro decided to let the financial dogs loose upon us—utterly failing to understand that Europe had become an integrated whole. The Portuguese felt this as a deep betrayal, as indeed, and contrary to other countries, our national financial accounts and our economic performance had been perfectly adequate by international standards. Only we were small, and we were made to pay for that. Our universities were left in a sorry state, our students no longer had grants, our libraries could no longer afford to pay the fees of access to the scientific journal databases, and an obscurantist prime

minister made a speech telling our graduates simply to emigrate if they wanted to get a job. By 2012, it looked like all that we had built over the previous two decades might have been cast to the winds.

In that year, I was invited to be the head of the School of Anthropology and Conservation at the University of Kent in Canterbury (UK), a challenge I could not turn down. The years that followed that move were not in any way less perplexing. As a result of my change of address, I had a first-row seat to observe the unfolding of the Brexit debacle. By the time I returned to Portugal in 2020, it was clear to all of us that British universities had suffered a major setback of which they might not be healed for many decades to come. To this day, British universities, which were normally the top recipients of European research funding, remain outside the European funding system, with all the damage that that implies to their international standing. We hope that coming governments see through the sheer suicidal stupidity of that, but the harm done may not be retrievable in the medium term.

Coming to the present moment again, then, the Russian War against Ukraine is beating at our walls and the situation seems to be increasingly giving rise to harsh political divisions in Eastern Europe. From the perspective of one who visited Ukraine in 1996 and saw the sorry state that one of Europe's richest countries had been left in by the Soviets, I feel deeply sympathetic to the fate of its people.

I must stress, however, that the matter presents itself to me yet in another light; I feel that we have to look at this from our *own* perspective. You and I are not just anybody, we are academics. This means that we have a vested interest; we have a professional point of view. It is like saying that sausage makers have an interest in the fate of pig farming. As much as we might regret many aspects of European politics, we are interested in our particular kind of business and that means that we, academics that we are, must fight for freedom of speech, a politically independent court system, a healthy education system, safe and humane academic employment, easy international circulation of scholars. In short, a political system that favours academic excellence. I still remember vividly what Portuguese universities were like before the mid-1980s when none of that existed. Whatever misgivings many of us may have about NATO politics, as social scientists we can only thank our gods that we still live under the kind of institutions that the European Union fosters, and no other neighbouring country even mildly provides.

I want to return, then, to what this means for anthropology in our present conjuncture. Perhaps I should start by explaining that I use 'conjuncture' here to mean "an emerging social condition, which identifies a constellation of politico-economic-cultural forces that correspond to a particular condensation of contradictions" (Pina-Cabral and Theossopolous, 2022). In particular, I seek to

dialogue with Johannes Fabian's distinction between coevalness and contemporaneity (1983). He argued that mid-century Euro-American anthropologists, who supposedly studied Other peoples (with a big O), were unwittingly generating asynchronous temporalities that produced duplicitous standards of analysis. Suppressing the coevalness of their subjects of study, social scientists were banishing exotic cultures and underprivileged classes to an 'other' time, generating what he called an allochronic condition. Fabian's critique of primitivism brought together a concern with personal experience with an engaged awareness of the history of globalization. For him, being 'coeval' is not only a condition, but an ethical injunction; more than living at the 'same time', it means addressing similar affordances in one's world. It presupposes substantial commensurability, an ability to share nuanced comparisons, a sense of cohabitation and co-responsibility.

To sum up, as Europeans, we are coevals. You and I share both a common time and a common worldly engagement. In Europe, the Russia-Ukraine War is probably the last battle of the wars that founded Europe—from the Franco-Prussian War to the Cold War. But it is also surely, at the same time, the warning siren of the coming of a new geopolitical era. (Since I delivered my lecture in Prague on the 6th of September, the tragic events in the Middle East only confirmed this.) The processes of change we are witnessing move in a deeply uncertain direction. And we should remember that this necessarily affects not only what we think, but also *how we do that thinking*.

The Durkheimian background

During the second half of the past century, Durkheimian social anthropology was the main driving force behind anthropology in Europe. This carried with itself a particular tradition of accounting for our disciplinary history that based it on exotic research upon imperial lands. Note that I am not here reproducing the totally unfair claim that anthropologists of the past were all 'imperialistic'. That was primarily a trick that American culturalists invented in the 1980s to establish their newly found hegemony. Having first come across our discipline in South Africa in the middle of the *apartheid* era, I can personally assure you that the anthropology we learnt at the University of Witwatersrand or the University of Cape Town at the time was anything but imperialistic. My teachers and their own teachers were and had been since the 1930s deeply engaged anti-segregationists, anti-racists, and anti-colonialists (see Hammond-Tooke, 1997). One of my lecturers, David Webster, was even shot by the regime, one morning, as he left his front door (Webster, 2009). Their teaching was precisely what made me adopt anthropology as a vocation in the first place.

The story that we were hearing from the anthropological historians that came of age in the 1970s—brilliant thinkers like Adam Kuper (e.g., 1983) or George Stocking Jr. (e.g., 1995)—was one that associated the kind of ethnography we wanted to do and the kind of theory we wanted to address to exotic, imperial, faraway lands. In fact, people whose research took place closer to the imperial homelands were treated with disdain and not allowed to enter anthropology departments—as was the case in Oxford with my own doctoral supervisor, John K. Campbell (see MacClancy, 2020).

In 1977, when I arrived in Britain, I was asked what I wanted to study and I responded that, having been raised in Africa, I was keen to get to know better my own European country of birth. ‘That means you have to go to Oxford to study with the Mediterraneanists there’, I was told by Michael Gilsenan, no less. And this I did. The discipline to which I was introduced there framed its own history in strongly Durkheimian terms, as a product of Radcliffe-Brown’s theoretical inspiration and of Evans-Pritchard’s Maussian deviations from it.

Imagine my surprise when, twenty years later, at the end of the 1990s, I finally discovered that the real inspiration behind British Mediterraneanism in the post-War period had not been that one at all. When he wrote his famous ethnography of a Spanish mountain town Julian Pitt-Rivers was not dialoguing with the social anthropologists we were taught to read, but with Georg Simmel, Alfred Schütz, and Erving Goffman. Actually, he had tried to explain that in his book, but he had been discouraged from doing so by Evans-Pritchard. Then, two decades later, he tried timidly to redress the error, but no one cared to listen (1971 [1954]). Only in 1999, did it emerge by the hands of Richard Fardon and Jeremy Adler that his supervisor had not been Evans-Pritchard or Meyer Fortes, as we had been told, but Franz Baermann Steiner, a Czech Jew who had been trained in Prague, Vienna, and Palestine and whose work had simply been erased from history because he had died too young and left it largely unpublished (Steiner, 1999a and 1999b and Adler and Fardon, 2022).

This was more important than it might seem because, contrary to what we were being told, the methodological inspiration in Pitt-Rivers and his early Mediterraneanist colleagues (like John Campbell or Paul Stirling) had not been an attempt to prolong to Europe the kind of theory that Malinowski had fostered. No, the direct inspiration was, via Steiner, the European ethnological tradition of studying the marginal populations of Europe—as Steiner himself had done in the Carpathian Mountains, not too far from Prague (Pina-Cabral, 2020). And then it turned out, that Mediterraneanists were not the only ones that needed to re-write their intellectual history. Mary Douglas’ concept of pollution, Louis Dumont’s concept of hierarchy, Srinivas’ conception of caste, Paul Bohannan’s theory of

substantivist economics, Laura Bohannan's visionary discussion of the plurality of what we usually call marriage—all of these were directly suggested to them by Franz Steiner. The Durkheimian history of British social anthropology that was being taught at Oxford, Cambridge, and Manchester throughout the lives of Evans-Pritchard, Meyer Fortes, and Gluckman and their immediate disciples was not wrong, but it was sadly incomplete. It wiped out the Germanic/Jewish/Central European inspiration of whole aspects of our field. We are now having to trace the evidence of those continuities and influences.

Anthropology and participation

This matters because we are presently witnessing a major change in our own condition as producers of anthropological knowledge and of ethnographic research. In this regard, I first want to argue against the widespread notion that the social sciences are necessarily unscientific in some way. This claim is based on an inferiority complex that resulted from mid-century positivistic background assumptions. By 'background assumptions' I mean all that we take for granted without very explicitly addressing it. The mid-century anthropologists that had a tremendously important role in divulging the notion of ethnographic research assumed a founding distinction between 'culture' (which was supposed to be a rule-bound system) and 'nature' (which was supposed to be causally determined). The philosopher Peter Winch, a close friend of Evans-Pritchard, wrote in 1958 a deeply influential book—*The Idea of a Social Science and its Relation to Philosophy*—which proposes an anti-scientistic view of social research explicitly inspired by the thinking of his anthropological friend.

Yet, just a few years later, when Donald Davidson demonstrated that norms were causes after all (Davidson, 1963: 691), the whole epistemological edifice upon which the mid-century notions of ethnographic methodology were based came tumbling down. While many philosophers were noting this by the 1990s, few anthropologists ever did. The primary reason for this is that, by then, the major influence in the field were the culturalist students of Parsons at Chicago—namely Clifford Geertz and David Schneider (cf. Sahlins, 2011: 6–7). In fact, of late, it is the very philosophers of science themselves, that have come to let us know that, and I quote one of the most inspiring contemporary philosophers of biology, John Dupré: “there is nothing in the rule-governed nature of social facts that presents any particular object to their scientific investigation.” (Dupré, 2016: 15)

As it happens, the power of background assumptions runs far deeper than even Dupré managed to realize. According to him, what makes knowledge scientific is its dependence on empirical evidence: which he clarifies by saying that “scientific

knowledge must ultimately be to some extent answerable to some aspect of our experience.” (2012: 5) On the whole, I agree with him, but then I take a corollary of his view that, as it turns out, he does not agree with. For me, in the social sciences, the ultimate source of empirical examination and evidence gathering is social encounter itself. The very root of all social scientific research is what I call ‘the ethnographic gesture’—that is, the decision by a person to move towards a form of human life that he or she determines as ‘different’ and addresses it in its conjunctural specificity (Pina-Cabral 2023). It is not the only method, but it is the foundational methodology for addressing all human differentiation empirically.

To my surprise, however, Dupré disagrees. This is how he puts it:

“I mean no disrespect to the tradition of cultural anthropology that does pursue a certain kind of participation with the societies it aims to investigate, and there may be a particular kind of knowledge that requires this kind of methodology. But surely it is not the only kind of knowledge possible of an unfamiliar culture? (...) And even if participatory anthropology gains a certain depth of understanding that is not available to other methods of study, it surely pays a price for this in breadth, or generality?” (2016: 16–17)

I find this argument is based on two false assumptions: (a) Dupré is ignorant of the history of ethnography—and the people to blame for that are anthropologists themselves, who settled comfortably to a kind of myth, a comfortable mid-century legend of how ethnography arose in the beaches of Kiriwina and (b) this legend carries with itself a set of implications that, as it turns out, are contrary to the very theories that Dupré sustains by reference to biology, which is his area of expertise.

I have outlined this point in greater depth in a recent article in *Critique of Anthropology* (Pina-Cabral, 2023). So, I will make my point here summarily. We have to assume that what Dupré means by ‘participant anthropology’ is the Geertzian defence of ‘participant observation’. In what turned out to be one of the most widely read texts in the anthropological canon (*The Interpretation of Cultures*, 1973), Geertz defends a view of ethnographic research that is based on a semiotic notion of culture as disembodied information. This individualist and substantivist view of meaning and sense-making is of a piece with the mid-20th century era in which it arose, and it has echoes in other sciences as well. Again, by relation to the science he dominates, evolutionary biology, Dupré makes a very similar point critiquing the focus on meaning as information. For example, he argues that “The assumption that the genome merely stores information is becoming untenable, and it now appears rather as an object in constant dynamic interaction with other constituents of the cell.” (Dupré, 2012: 265)

Yet, when he comes to ethnography, which is not his field of expertise, he just allows the silent background assumptions to take over. The cause of this, I want to argue, is our own lack of information concerning our anthropological history. How many of us are aware that Malinowski *did not* invent the method of ‘participant observation’, as it is said in the Wikipedia entry for the term, and that he never even used the expression in any single one of his texts? How many of us know that the first use of the words ‘participant observation’ in the late 1920s by American sociologists did not apply to an ethnographer going out into the field, but to a trained informant—that is, a ‘participant’ that was taught to ‘observe’ (see De Walt and De Walt, 2011)? The expression only started being used by anthropologists in the United States, and later around the world, by the mid-1950s.

It is our fault that most of us believed the mid-20th century culturalist legend and did not check the actual history behind it. One day, having lunch with George Stocking, I too voiced that legend, and it was him that corrected me, saying that Malinowski had not invented the method and that he never even called it that. I went to do some research and I soon discovered that Stocking was absolutely right: Rivers had been the one to theorise the method one year before Malinowski went to experiment with it in the field (Rivers, 1913). Furthermore, Malinowski was not the only one at the time using Rivers’s suggestions. The people surrounding Robert Park studying urban Chicago during the same period were also experimenting with it.¹

At that time, they called it the ‘intensive research method’ and they had in mind the need to actually expose the ethnographer to the everyday life and the modes of living of the people they studied. Evans-Pritchard has a passage explaining that it is not enough to know that Azandes hunt with arrows. It is actually necessary to try to fire an arrow, even if initially with little success (1973). This type of research demanded a kind of engagement in terms of time and effort that anthropologists elsewhere were not willing to undertake. Intensive ethnography necessarily affects the ethnographer’s own ontogenesis as a person. That is troublesome and some may not be willing or capable of undergoing it.

This is why Marcel Griaule felt obliged to justify himself in his lectures on the ethnographic method, proposing an easier alternative to it, which he called the ‘extensive research method’ (published posthumously in 1957). He knew that the kind of expedition-type ethnography he practiced was a totally

¹ And why do we fail to consider today as one of our best methodological predecessors Nels Anderson, whose masterpiece *The Hobo* (1923) is almost contemporary with Malinowsky’s *Argonauts* (1922)?

different methodological proposal from Rivers's. Griaule's was the mould for Lévi-Strauss' expeditions in South America. My point here is that, at a moment when other social scientists are turning what they call 'the ethnographic method' into a sort of quick fix based on a few interviews, anthropologists should return to the pure waters of Rivers's inspiration and seek to be more 'intensive' in their research.

Why then must we drop the convenient but misguided notion of 'participant observation'? Because of the very reason biologists give for rejecting information-based, semioticist explanations in biology. See the example given by Nick Lane: "Biology is not only about information. Just as human delinquency cannot be blamed on individuals only, but partly reflects the society in which we live, so the effects of oncogenes said to cause cancer are not set in stone but take their meaning from the environment." (2022: 458). When I go to the field, I do not go there only to gather words, texts, rules, and laws—discourses, as culturalists call it. Ethnography is *not* 'a discourse on discourse', as Viveiros de Castro famously put it (2002: 113). Discourse is only the means of what I go there to do, because what makes the research ethnographic is that it is intensive—that is, it engages the ethnographer not only with discourse but also with what Heidegger used to call *Das Man* (the public aspect of life)(1962: 165) and we, following Marcel Mauss and Bourdieu, have taken to calling *habitus*. What is the habitus? It is the very set of worldly affordances that, in a certain social context, frame the meaning-making activities of the participants but which they do not carry at the tip of their tongues, as it were.

If I ask someone to describe to me her society's habitus, she will simply laugh at me. Discourse is an aspect of ethnographic research but, without being able to frame it within practice and its implications, the ethnographer will never be able to make sense of it. And this is why Pitt-Rivers used to insist that, when I want to understand some recurrent practice, I cannot limit myself to what the people say about it. I have to go beyond that and search in history, in the material objects used, in the texts that framed these concepts but that those who use them never even read. As he put it:

"The meanings which a single word has in different contexts, or had in the forgotten past, are guides to the premises which underlie its daily conscious usage, but daily usage is indifferent to contradictions arising between its various senses, and leaves them to be sorted out at the level of action. (This is the case of honour also). Thus it is not necessary to analyse a word in order to know how to use it correctly." (Pitt-Rivers, 2017 [1992]: 72)

That was the kind of ethnography that Pitt-Rivers carried out when, influenced by the lectures of Steiner on Simmel's notion of personhood (1999a: 208–298, see 1999b: 225n1), he went to Andalusia to research forms of personal valuation (honour and shame), or the cosmological implications of the notion of grace (see Pina-Cabral, 2022b).

The ethnography of the present and the foreseeable future, as it happens, is less and less characterised by distant exotic fields and increasingly engaged with peoples and topics that are everywhere around the world to be observed. Human differentiation starts at home and, in many ways, we are back to the condition in which people like Steiner were in the 1930s, when studying Jews, Roma, and Ruthenians in the Carpathian Mountains. In fact, as I observe daily these days, we can show empirically that Dupré is wrong in his prejudices. Ethnographies of our contemporary world—fully coeval in their reach—have been coming out of late that address brilliantly and creatively the sort of concerns that our world produces for us (e.g., Sanabria, 2016 or Grohmann, 2022). It is, after all, a world whose social constitution is changing profoundly right in front of our very eyes.

But then, to get to my second and final topic, so have changed the background assumptions that inform our theoretical discussions as anthropologists. Dupré fails to understand that it is in the small details that the most universal observations can be made—ethnographers do not suffer and have *never suffered* from a lack of 'breadth or generality', as he put it. Only, we seem to have been a bit lazy and we have often failed to observe that the cosmological background on which present-day scientific discoveries depend has changed very radically since the 1990s.

A new anthropological synthesis

I find that many important theoretical changes of context have been taking place, which confirm much of the work that the post-structuralist anthropologists of the 1980s were pointing to. I propose that, both in methodological and theoretical terms, we are facing today *a new anthropological synthesis*—using this last word to refer to the broader analytical parameters that frame our discipline. There are at least four main areas of theoretical change that directly affect the anthropology we do today. These may seem to be rooted in such a broad range of fields of expertise that they are too much for any one of us to grasp. That would indeed be the case, were it not for the fact that a large number of philosophers have emerged over the past twenty years (such as Karen Barad, John Dupré, Nick Lane, or Shaun Gallagher) who have done that work for us and have come to decipher the major lines of innovation that are emerging, namely in the area often called the 'sciences of life'.

The first change to notice is the new *processual philosophy of biology* that has come to the fore in the first decades of the new century. Anthropology's hegemonic view is based on a substantivist, materialist cosmology that biology has largely left behind (e.g., Nicholson and Dupré, 2018). We should no longer focus on things (or 'material culture', as it is often called) but on processes—that is, on changing frames of energy. For example, this implies a profound change of aspect in the discussion of the old problems concerning the nature of 'substances' and their circulation in the constitution of persons (e.g., Marriott, 1976). Anthropological substantialist suppositions (what Sahlins called his 'materialism', 2017: 117) have to be thoroughly overhauled.

The second is the impact that *quantum physics* has had in broader cosmological aspects of our scientific background assumptions. As Karen Barad (2007) among others has demonstrated, we are observing daily how a de-substantialised and processual nature of quantum physics is affecting our notions of presence and of being. Consequently, an ontology of human existence must address complexity and entanglement and how organisms and personal entities are *emergent entities* created by what philosophers call 'downward causation' (e.g., Dupré and Nicholson, 2018: 27). This casts a shadow on the individualist and atomistic conception of persons and organisms that we inherited from mid-century Parsonianism. The notions of persons and organisms as partible (not individual) and as developmentally emergent impose themselves (see Pina-Cabral, 2017).

The third is the way in which a *contemporary view of evolution* has emerged that no longer corroborates the hyperindividualist ideology that characterised the so-called Neo-Darwinian Synthesis at mid-20th century, characteristically exemplified by Richard Dawkins' famous book *The Selfish Gene* (1976). Recent philosophers of evolution have taught us to see that complexity is also a feature of evolution, that the tree of life model is fallacious, that epigenetics is far more important than it seemed, that environmental adaptation is the norm and not the exception.² Once we get rid of the older models of evolution, the very primitivism that founded anthropological theory at the end of the nineteenth century and that continues to be our background assumption to this day can just be summarily dumped. Our new focus must not be on organisms as entities but on entities as emergent phenomena (see Lane, 2022). A view of the person as a process in constant development, destitute of mental or physical essence, imposes itself. We have to work out how sociality operates not only between different so-called 'cultures', but also in different scales of human life: person, company, and community operate differently, even as they interact constitutively.

² Concerning the extended evolutionary synthesis, see Laland et al., 2015.

Finally, *philosophers of cognition* inspired by phenomenology have made deep inroads ever since the turn of the century into the kind of post-structuralist critique that emerged with figures like Foucault, Derrida or Merleau-Ponty. They propose a view of cognition that goes way beyond the bounds of the representationalism that characterised the 1990s American culturalists of the so-called ‘semiotic turn’. Cognition does not take place only in the brain, for it is embodied, embedded, enactive, and extended by way of extra-cranial processes and structures. For example, Shaun Gallagher has been explaining very convincingly for a while that ‘embodied cognition can be expressed by the general hypothesis that cognitive processes are fundamentally rooted in the morphological traits and affective systems of the human body.’ (Viale et al., 2023) This means that, finally, we can give a response that is at least partly satisfactory to the quandary that Rodney Needham left with us when, in 1972, he concluded that he did not know what it meant ‘to believe.’ Or when, in 1987, he noted that, having studied binary opposition, he could only see it as based on an affordance provided by the very sidedness of our bodies. For years his *Belief, Language, and Experience* (Needham, 1972) and his *Counterpoints* (Needham, 1987) were like timebombs about to explode over our anthropological theoretical certainties.

Furthermore, largely as a development upon Merleau-Ponty’s notion of ‘the intercorporeity of primary intersubjectivity’ (see Gallagher and Miyahara, 2012), mental processes have started to be seen as thoroughly social phenomena: all meaning is ultimately ‘participatory sense-making’ (see De Jaegher and di Paolo, 2007). This radical new outlook on intentionality sees it as essentially plural (and not only the intentionality of persons but also the intentionality of all life forms). As Gallagher and Miyahara put it, “To the extent that we are all born into a community, our environment is full of intentional practices from the very beginning of our lives.” (2012: 139).

This means that the world is not passively out there for the taking; it is built out of a dynamic of social engagement (Pina-Cabral, 2017). “I see the other’s actions as an affordance for my own possible action (which may be very different from hers); I see the other’s action as interactionable or as calling forth a response on my part.” (Gallagher and Miyahara, 2012: 137) What this means is that my own emergence as an organism, and my own postulation of a world with which I interact as a person, never come unmoored from my social inherence, since all sense-making is ultimately participatory (see Pina-Cabral 2018).

Conclusion: one of the sciences of life

In light of all that, I conclude that not only is anthropology a science, but it is part of the broad field of the ‘sciences of life’, as it is necessarily rooted in the study of the processes that shape the essential embodiment of all human experience. The implications that such a major change in analytical expectations will have in anthropological practice are not really possible to predict. There are some central aspects, however, that can immediately be discernible. Firstly, the sociocentric oppositions between *individual* v. *group* and *participant* v. *observer* will reveal themselves to be deeply insufficient to describe the complexity of processes of social engagement, with major implications both in analytical terms—where metaphysical pluralism will run its course through most areas of anthropological theory; and in methodological terms—where the groupist, identitarian approach to the ethnographic encounter, which has led to a multiplication of ‘codes’ of supposedly ‘ethical’ practice, will reveal its ultimate perversity.

Secondly, the new, non-representationist framework for understanding cognition will open up at least two new areas of analysis: the difference between *human* and *animal* will not disappear, of course, but it will assume a greater sophistication, namely in terms of more complex notions of evolution and of a new approach to ethical concerns; and the polarity between *conscious* and *unconscious* will also be re-examined, leading to new approaches to the relation between habitus and history. Indeed, the emergentist framework that now offers itself allows for the development of a kind of neo-structuralist analysis which has much to offer anthropological theory once the groupist and primitivist assumptions that we inherited from the modernists are progressively eradicated. Thirdly, in terms of an approach to the process of constitution of political and economic forms of hegemonic domination. The notions of emergence and entanglement have much to offer in this regard, namely in terms of the analysis of environmental concerns.

I am aware that what I can leave here are merely pointers for what are necessarily going to be long and largely unpredictable processes, but I think that it is worthwhile to show how we are indeed before an exciting moment of theoretical and methodological renewal, where anthropology will again be able to grasp its long-term universalist themes, presenting them in a newly illuminating fashion. I believe that, if we have to learn anything from the amazing impact that ethnography had on mid-twentieth century intellectual life, it is the precise contrary of what Dupré ignorantly believes. Our particularist ethnographic methodology and our universalist anthropological theory have always been combined (see Pina-Cabral, 2008). As a matter of fact, they were combined precisely because they feed each other.

From the beginning, studies such as Firth's work on Tikopia (1936) or Evans-Pritchard's work on the Nuer (1940) were both long-lasting theoretical references in analytical discussions in philosophy, politics, and the social sciences and works of deep local import for the populations studied. It is the very emphasis on detailed and intensive encounter that provides the clue to their universalist import and their particularist relevance. If not *intensive*—that is, if they had not affected ontogenetically the person of the researcher—these ethnographies would have lacked that which makes them so foundational.

Thus, I sustain that, more than merely relevant for anthropology as a discipline, ethnography is the ultimate and primary means of empirical evidence-gathering for all of the social sciences, without which more distanced and mediated approaches will quickly glide down the ideological scale. In fact, I believe that ethnography carried out in 'intensive' fashion, precisely because of its intensity, will be increasingly indispensable when faced with the challenges that AI is presently posing to all social research. Intensive ethnography will ever remain the very root of the sociological encounter, which the scale-hopping that AI necessarily implies will always be dependent upon if it is to remain tied to actually humanly relevant concerns.

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