

JOÃO DE PINA-CABRAL

## Larger truths and deeper understandings

### I

I start from a passing comment by Jean Lydall in the latest number of *Anthropology Today* (24, 4). It is a statement with which one is bound to agree at first sight. She claims that ‘anthropology should first and foremost be respectful of the values and opinions of the people it studies before making its own value judgments, and should search for deeper understanding rather than superficial “larger truths”’ (2008: 29). Although one is bound to agree on the whole with the author’s intentions, I want to argue that, hidden within her sentence, there is a type of prejudice that can be seen as detrimental to present-day anthropology: a prejudice both against generalisation and against the explicit search for common aspects of human experience.

That anthropologists, as ethnographers, should be ‘respectful of the values and opinions of the peoples’ studied seems to be beyond denial. In fact, however, this is only superficially so, as it is perfectly possible for ethnographers to fail to respect the opinions of many if not most of the people studied. For example, many of the people I encountered in Macau during the 1990s (Chinese, Eurasian and Portuguese alike) expressed openly disparaging, ethnocentric and often outright racist opinions concerning the members of ethnic groups other than their own. Should I have respected these opinions?

Precisely what does ‘respect’ involve in this case? Must we consider ‘respect’ here in relation to ‘value judgments’ as the author goes on to suggest? In short, should the ethnographer take a docile, accepting attitude (one of ‘respect’) towards all postures of the people studied? I doubt that Philippe Bourgois, for one, would agree with that. To the contrary, one might argue that ‘respect’ here is meant in a purely empirical sense, as truthful, detailed recording (as Jean Lydall does so masterfully in the remainder of her letter).

No one can doubt that the ethnographer is duty bound to report truthfully concerning the people’s ‘values and opinions’. But, then again, the issue is hardly as simple as that. Does ‘ethnographic respect’ imply that we must take as valid whatever we are told? Such a literal culturalist interpretation would soon make nonsense of any ethnographic exercise: one is so often told things that can hardly be taken at face value! For example, is the people’s opinion best represented by the values expressed by an aristocratic elder or by a low-class female adolescent?

I suspect that the problem here lies in a kind of slippage that is implicit in the more stereotyped accounts of ethnography; from ethnography as reporting what people say and do in a certain situated context to ethnography as reporting about a certain collectivity. The ethnographic report, in fact, has to be a mediation between the two and that cannot be carried out without comparison. I need go no further to suggest that any purely descriptive account of what are the values and opinions of a 'people' is unsatisfactory for the purposes of anthropology and ethnography. No culture can be described in its own terms – if for no other reason, for the simple fact that cultures are dialogical, conflictive processes.

I believe that the reason why Lydall's original statement seemed so unquestionably true is that it relied implicitly on a number of shared background associations. The ethical injunction that explains why an ethnographer must drop his or her 'own value judgements' depends on the assumption that the people studied are not only somehow different from the ethnographer's 'own people', but also somehow in need of being represented. In fact, 'respect' in this context depends on seeing Western/Other, Modern/Traditional, Powerful/Subaltern and Individual/Collective as coterminous polarities.

As it happens, however, these correspondences have all palled in our day and age. In any case, ultimately, it is paternalistic to believe that one's subjects of study, whoever they come to be, can be passively consensual in their collectively held beliefs. The reason why I say 'paternalistic' is that I believe that the association of these polarities bears within it an inescapable framework of temporalisation that sees Western/Modern/Individual as an inevitable future. By now, many of us have disparaged the beliefs of Modernity, but most of us are still not keen to shed their implications, even when they become a kind of dystopic fatality.

Now, to continue to analyse the sentence, the author opposes the 'respect for the people's opinions and values' to the anthropologist/ethnographer's 'own value judgements'. The implication is that opinions that fail to 'respect the people's values and opinions' are necessarily 'value judgements'. Therefore, if the anthropologist expresses opinions that diverge from those of the people studied, they are necessarily his or her own value judgements; that is, they are ethnocentric. Earlier on, we got stuck on the possible implications of the word 'respect', now we stop in front of 'value'.

Are all ethical principles necessarily culturally-bound values? Certainly yes, if we read value to mean exclusively 'socially constructed morality' – a set of norms and principles that are collectively held and historically situated. But are there no ethical dispositions that rise above specific historically located moralities, being inherent in our common human condition, in our ethical sense of human co-responsibility? This is not the place to develop this issue at length, but I for one am more likely to go by the opinion of philosophers such as Emmanuel Levinas who support the latter suggestion, rather than by the relativist vulgate that marks our contemporary anthropological common sense.

Now, this leads us straight to the last part of the author's sentence, as indeed it should: the opposition between searching for 'deeper understanding' instead of searching for 'larger truths'. As the reader may imagine by now, I believe this is an utterly false dilemma. The search for larger truths (larger than empirically grounded ethnographic observation, one presumes) is surely the only possible reason for carrying out ethnography. More even: it is a necessary precondition for ethnography as a method.

I take 'larger truths' here to mean generalisations concerning contexts of comparison that are wider than the local experiential context of research. In this sense, the method of ethnography requires such 'large truths' both *ex ante* – for there would be no good reason to practise ethnography other than a comparative drive – and *ex post* – for there is no way of writing ethnography that does not depend (even if only implicitly) on a considerable set of generalisations concerning our common human condition (and to an extent, of course, also our condition as mammals). Anthropologists do not have the choice of dispensing with generalisations; they only have the choice of hiding them, pretending that the call for 'deeper understanding' made them irrelevant.

In short, I argue that time has come again for us to engage openly and frontally with the task of generalisation. Recently, Maurice Bloch has argued in favour of searching for 'larger truths', saying that 'we might attempt to generalize about a phenomenon such as the recurrence of the association between truth and sight without ignoring important anti-universalist points' (2008: 22). Once again, however, we see here the sociocentric heritage in anthropology at work, deriding implicitly the search for wider forms of comparison.

For too long a time, anthropology has silenced the matter of universals of the human condition. When the topic surfaces, it is usually met with dystopian derision. The reason for this is that, in spite of some useful insights by Needham concerning 'primary factors' (1981: 20–4), most anthropologists have understood 'universals' in a positivist manner. That is, it has been thought that, if something is a universal, then it must be observable permanently in all historically known instances of human sociation. This, however, is a serious mistake, for it would be to presume that phenomena of human thought and action are subject to the same type of causality that characterises inert substances. Language and rationality inscribe new types of causality into human life that mean that no such deterministic universals will ever be found for the human condition. This need not make us disparage our comparative task, however. As we have no option but to proceed with it, we are duty bound to do it explicitly. Our error is in the positivistic formulation of 'universal' that we have been working with. The concept, therefore, must be redefined to cover what Needham used to call 'proclivities' (1978) – that is, dispositions of thought and action that are probable, frequent, spontaneous, and for which humans seem to have an inclination.

Finally, I feel I must state explicitly that, much like Robert Gardner, I believe that Jean Lydall's letter to *Anthropology Today* is a particularly valuable example of what good, long-term ethnography can do (2008: 28–9). The fact that I disagree with the implications of her statement does not even necessarily mean that I disagree with all that she meant by her statement.

## References

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JEAN LYDALL

## Response to João de Pina-Cabral

### II

To begin with, I want to clarify that my statement, which inspired this debate, was not a ‘passing comment’ as Pina-Cabral said, but was made specifically in reaction to Robert Gardner’s remark, made in reference frame of his film *Rivers of Sand*, that he does not ‘think anthropology is doing its job by being value-free’ and ‘it should accept responsibility to look for larger truths’. At first this remark appears disinterested, but in fact Gardner made it because his film had been criticised by anthropologists, including Jay Ruby (1991), Ivo Strecker (1978, 1988) and myself (1978), for its use of poorly grounded value-loaded generalisations parading as ‘larger truths’. That’s why I sarcastically countered him, saying, ‘anthropology should first and foremost be respectful of the values and opinions of the people it studies before making its own value judgments, and should search for deeper understanding rather than superficial “larger truths”’.

By taking my comment out of context and imputing a ‘prejudice both against generalisation and against the explicit search for common aspects of human experience’, Pina-Cabral misrepresents my approach to anthropology. In fact, it is the power of the particular to evoke intimations of the general that has inspired my ethnographic work.

Having clarified these points, the way is clear for a fair debate on the epistemological issues Pina-Cabral has raised.

1. ‘Should the ethnographer take a docile, accepting attitude . . . towards all postures of the people studied?’ The whole process of ethnographic fieldwork is one involving continual multi-lateral testing and accommodation of different points of view, or as Anne Salmond described it, a ‘process of reflexive interpretation . . . to bring about a “merging of horizons”, so that the viewpoints of self and other progressively overlap and understanding is achieved’ (1982: 74). Take, for example, the work journal entry I made on 27.7.1973: ‘Over our evening meal Baldambe and I have a big argument . . . Baldambe wants to convince me that the people of Angode, Mirsha, etc. are cowards and that those of Kadja are brave. I argue that the country of Angode, Mirsha, etc. is border country which has to withstand the attacks of the enemy, whereas Kadja is safe. It’s easy to be brave if one is not attacked; it’s easy to be afraid if one is bearing the brunt of attacks. We both shake our heads and say the other doesn’t understand, but we are not angry with each other, it is good to discuss’ (Lydall and Strecker 1979a: 458). Rather than dismissing or disparaging another’s views, an anthropologist needs to look for what lies behind and beyond them. I would now contend, for example, that Baldambe’s labelling of others as cowards at that particular time and place, and in that particular context of inter-tribal warfare, was a rhetorical strategy. He wanted to persuade himself of what he would say at

the next day's public meeting where he would taunt the men of the border regions, hoping thereby to persuade them to risk their lives in defence of Hamar territory.

2. 'No culture can be described in its own terms – if for no other reason, for the simple fact that cultures are dialogical, conflictive processes.' I think there is a kind of slippage here between different meanings of description and explanation. It is perfectly possible to have a description of a culture made in terms of the same culture, as for example *Baldambe Explains* (Lydall and Strecker 1979b), which is an account by a Hamar elder of Hamar customs as he knew them from experience and hearsay. It is not 'pure description', if such were possible, which I doubt, but rather an explanation in Hamar terms, hence the title. It is not, however, a theoretical explanation as it would have been if Baldambe had analysed the customs in terms of a theory, for example one postulating 'that cultures are a dialogical, conflictive processes'.
3. Instead of dropping one's own value judgements and thinking in coterminous polarities, I plead for an ethical obligation for anthropologists to become well acquainted with the subjects of their study, whether these are members of their own or another family, community, class, country or any other kind of group. As Philippe Bourgois points out (Haanstad 2001), participant observation, which is the central method of anthropology, also forces one to be political. The process of participant observation usually leads to a refinement of an anthropologist's attitude and understanding as he/she gains insight into the circumstances and points of view of others. On whosever side and on whatever issues anthropologists finally give their support cannot be prescribed, but at the very least they should become as knowledgeable as possible about the people and issues involved before launching into applied anthropology or political agitation.
4. 'Are there no ethical dispositions that rise above specific historically located moralities?' Perhaps there are on some transcendental level, but certainly not the ground. One of the first lessons I was taught in Hamar was that direct exchange, buying and selling, was bad, while demanding gifts was good. It took me a while to understand how important this morality was in the construction and maintenance of kindred and community networks that provide social and economic security in Hamar. In the Europe I am familiar with, direct exchange is considered good, begging is bad, and gifts should not be solicited. This morality underscores the market economy in which we live and on which we depend. The incursion of the market economy into Hamar is leading to a conflict in ethical values, while growing unemployment in Europe is leading to a revival of kinship ethics that clash with market ethics.
5. 'Deeper understanding' vs 'larger truths'. In my original statement I spoke out against 'superficial "larger truths"', not 'larger truths' as such. Now I find myself wrangling with the notion that Pina-Cabral takes 'to mean generalisations concerning contexts of comparison that are wider than the local experiential context of research'. If Pina-Cabral were to compare the Macau with other groups, would the generalisations he came up with be 'larger truths'? How could he be sure that they were true enough to warrant the label? Would it not be more honest to leave aside this presumptuous term and rather develop theories to explain the differences and similarities that we discover? Such theories, of course, rely on assumptions concerning our 'common human condition' and the effect on these of variable circumstances. By acknowledging that 'larger truths' are only theoretical

hypotheses, we would not only add some modesty to our claims but also open them up to challenge and the possibility of modification and improvement. Although the comparative goal is an admirable one, it is still imperative that the first stage for any anthropologist is to accumulate knowledge and search for deeper understanding at a local level. As this also involves the development and application of theories, there's no difference between understanding things at a local level and making comparisons on a wider scale.

6. Needham was a very wise man, and I wonder whether he ever bothered with as supercilious a term as 'larger truths'. I would suggest that as well as dropping this term, we should also stop struggling with the term 'universals', since it is not really universal, and simply use Needham's term 'proclivities' instead.

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## Reply to Jean Lydall

### III

Words deceive. When I penned down my earlier comments, I did not take into consideration the context of Robert Gardner's original statement. In fact, it is most likely that, when he spoke of 'larger truths', he had something else in mind. This being said, however, as Jean Lydall agrees, there is a debate to be had about ethnography and analysis: what is the dynamic between theory and ethnography? Does the duty to be ethnographically precise imply a fatality to be theoretically inexplicit?

As Jean Lydall exemplifies, the possibility of ethnography depends on a 'merging of horizons'. But there are two sides to this merging: (a) between the ethnographer and

the people studied, but also (b) between the actions and words of the same person as observed by the ethnographer in different contexts – something that might not be as evident in the sort of pre-modern, rural context that Lydall's examples presume as in the sort of urban contexts where most human beings today abide. Is it 'perfectly possible to have a description of a culture made in terms of the same culture'? I think not, because that already naturalises the notion of 'culture'. But if 'culture' is also an emic category (as is so often the case these days), then the problem is only compounded.

Thus we return to my original preoccupation: we must question the implicit presuppositions behind our methodological procedures. That is, the supposition that we describe 'groups', composed of 'individuals', that share one 'culture' – the monadic elements of the modernist imagination. That this still functions today as a kind of methodological default for anthropology is exemplified by a sentence where Jean Lydall says: 'If Pina-Cabral were to compare the Macau with other groups, would the generalisations he came up with be "larger truths"?'

Now, there is no such thing as a group called 'the Macau'. Macau is a city of well over 500,000 people in southern China, where gambling tourism is the primary economic activity. Its inhabitants are deeply divergent ethnically and even the expectation that they be divided into neatly classifiable 'ethnic groups' is problematic, requiring considerable theoretical elaboration (cf. Pina-Cabral 2002). In fact, the people I studied more closely – the Eurasians who call themselves *macaenses* – are a loosely-bound group whose members are prone to contextual manipulation of ethnic identity, linguistic performance and cultural positioning. All of this would be beside the point if it were not for the fact that it shows how the unitarist and primitivist conception of fieldwork is still with us even though it is no longer helpful.

Jean Lydall goes on to ask: 'Would it not be more honest to leave aside this presumptuous term ["larger truths"] and rather develop theories to explain the differences and similarities that we discover?' Yes, let us drop the vacuous implications of the expression, for what preoccupies me in the end also preoccupies her: how do we go about making generalisations and theoretical elaborations?

The thing is that Evans-Pritchard's arrogant elusiveness concerning theoretical engagement no longer serves us well; we have become victims of our own reserve. Faced with the repetitiveness of relativism, some of us have recently succumbed to the sirens of biology and ecology; much like, earlier on, to the sirens of economics, linguistics or psychology. If we are to respond constructively to these attempts at essentialisation by displacement (what used to be called reductionism), we have to find some way of formulating explicitly our anthropological theoretical heritage in terms of analytical categories that emerge out of processes of generalisation that go beyond ethnographic locality: 'universals' ... 'proclivities' ... 'larger truths' ... you name it. We have to return to the task of theoretical construction from where our modernist predecessors left it, albeit with new theoretical dispositions and new methodological expectations – the two things go together. That is where Needham's perplexities about proclivities might again turn out to be useful.

This is why I remain unconvinced that 'there is no difference between understanding things at a local level and making comparison on a wider scale'. For one, we have to question the very notion of 'locality' and the presumed unitariness of our objects of ethnography. Jean Lydall agrees that perhaps on some transcendental level there are ethical dispositions that rise above specific historically located moralities, but she believes they are largely beside the point for they are not 'the ground', as she puts it. But

I cannot see it that way. In fact, once our relationship to our objects of study becomes more and more complex as a result of the slipping away of the veils of exoticism that characterised the modernist moment, the issue of our co-responsibility (mutuality in fieldwork) becomes more troublesome, not less.

Both ethnography and anthropology as practices are, so to speak, ontogenetically dependent on the kind of transcendental generality in question. Our unwillingness to examine it is only made possible by our having naturalised the categories of analysis. Recently, we have witnessed another tendency in our discipline to slip from methodological relativism (the very stuff of anthropology) to epistemological relativism (the slide from ‘epistemology’ to ‘ontology’ that, ultimately, turns out to be the death bell of anthropology). At the root of this trend lies the same conservative unwillingness to rethink the modernist methodological defaults.

In short, anthropologists have no reason to be timid about theory, about generalising and even about looking for ‘universals of the human condition’. To fear that such formulations might be anything more than steps on the way – ‘theoretical hypotheses’ – would amount to espousing a positivist notion of science that is fully unwarranted today.

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# Response to João de Pina-Cabral

## IV

In reply to my first response, Pina-Cabral remarked, ‘words deceive’, to which I now rejoin, ‘words persuade or dissuade, both others and ourselves, for better or for worse’.

In a forthcoming publication F. G. Bailey writes, ‘All rhetoric is palaestral. The metaphor of the wrestling-school is a vehicle for the rhetorical struggle to pin down another person and make him/her accept a definition of the situation.’ This conjures up nicely what we are doing in this debate. Indeed, I would argue that all scientific discourse is infused with, and dependent on rhetoric.

Take for example how Pina-Cabral warns us not to ‘slip’ into methodological relativism, and how he deplores the recent ‘sliding’ into epistemological relativism, which ‘turns out to be the death bell of anthropology’. These are wonderful metaphors: To whom the bell tolls! However, they leave me feeling rather baffled. Instead, I like

to recall what H. W. Simons wrote in his introduction to *The Rhetorical Turn*, which started the ‘rhetoric of inquiry movement’ almost twenty years ago:

It is now clearer than ever that . . . in place of method, there is talk of methods; variable, creative, non-algorithmic. In place of covering laws, there is talk of contingent, historically situated truths, reflective of values and interests, and found more or less useful by cultures and communities which are themselves symbolically constituted. (1990: 2)

In this debate I have taken on the role of the ‘spiteful ethnographer’, a term I borrow from Clifford Geertz to describe an anthropologist who points out particular instances, which punch holes in other anthropologists’ generalisations. I suspect, however, that Pina-Cabral invited me to join the debate because he thought I, as someone who advocates thorough field research as a basis for analysis and understanding, would fit his stereotype of an anthropologist who holds a ‘unitarist and primitivist conception of fieldwork’, is deceived by the ‘veils of exoticism’, and ‘espouse(s) a positivist notion of science’. But this is not the case. Long-term research in southern Ethiopia has made me acutely aware of the historical, processual and variable nature of groups and localities, and of ritual, social, economic and linguistic conventions (or culture in general); of the agency involved in seemingly esoteric as well as obviously mundane practices; and has motivated me to theorise on such matters.

Fortunately, I found I was not alone in this venture, and for some years now I have been working with scholars of the International Rhetoric Culture Project ([www.rhetoricculture.org](http://www.rhetoricculture.org)): Stephen Tyler, James Fernandez, Donald Brenneis, James Fox, Helen Basso, the Tedlocks, Karl-Heinz Kohl, Robert Hariman, Christian Meyer, Anna-Maria Brandstetter and many others, including – of course – my sailor and research companion Ivo Strecker.

The project starts from the observation that ‘scholars have criticized the notion of “culture” as inviting a particular form of reification and implying a Procrustean vision of human existence’ (Strecker and Tyler, in press). This is similar to Pina-Cabral, who objects to an epistemology that ‘naturalizes the notion of “culture”’; ridicules ‘the supposition that we describe “groups”, composed of “individuals”, that share one “culture”’; and argues against the ‘presumed unitariness of our objects of ethnography’.

While Pina-Cabral urges us ‘to return to the task of theoretical construction from where our modernist predecessors left it, albeit with new theoretical dispositions and new methodological expectations’, the Rhetoric Culture Project proposes to ‘re-think the concept of culture and locate it in the domain where it ultimately belongs, that is rhetoric’. Thus we are led to consider ‘the ways in which rhetoric structures culture and culture structures rhetoric’, and to explore ‘the possibilities afforded by rhetoric to explain culture’. We do so by ‘paying more attention to the hidden in social discourse, the unsaid behind the said, the latent beneath the manifest, and the unreasonable as well as the reasonable sides of human existence’ (op. cit.).

If he had not already made his analysis, this agenda would have been well suited for Pina-Cabral to investigate the ‘contextual manipulation of ethnic identity, linguistic performance, and cultural positioning’ of the ‘loosely-bound group’ he had ‘studied more closely’ in Macau.

At the Rhetoric Culture Conference on Rhetoric and Linguistics (2002) I gave a talk about ‘The rhetorical energy of ideophones’, and at the conference on Rhetoric in Social

Relations (2005) I contributed a paper on 'Kinship: Mother and child of rhetoric'. As one can glean from these titles, I was certainly not 'timid about theory', and neither were the other conference participants. The Rhetoric Culture Project does not constitute a single all-embracing theory, but offers a timely and pertinent way of theorising, which both challenges and complements other theoretical approaches.

Finally I want to point out that it is a *non sequitur* to imply that I 'espouse a positivist notion of science' simply because I prefer to use time-honoured terms like 'hypotheses' rather than 'larger truths' in my quest to understand particularities in terms of our 'common human condition' or vice versa.

At the end of our wrangle, it seems that Pina-Cabral and I have no substantial disagreement, and so it is now up to each of us to go ahead with our theoretical programmes.

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