Correspondence

Companionate marriage in India

Dear Editor,

I wish to comment on the article ‘Companionate marriage in India: the changing marriage system in a middle-class Brahman subcaste’ (Fuller & Narasimhan 2008). I have seen Dr Narasimhan a couple of times at the village of Tippirajapuram (TRP) and I think she has also visited our home at Chennai. While the authors have engaged in a considerable amount of effort to bring the article to its current form, in my view it contains a number of generalizations and inaccuracies, as well as unwarranted references to real-life people, both living and passed away.

Regarding ‘sweeping generalizations’, Fuller and Narasimhan seem to suggest that mothers-in-law in Vathima villages do not let their daughters-in-law to talk to their husbands, and that Vathima couples get together in secret for a minute or two when their children are conceived. I would say that these statements refer to exceptions, even assuming, but not admitting, the same to be true. They also state that these restrictions explain the small size of many Vathima families. In fact, the average size of a Vathima family is around the same as that evident in any other Brahmin community, if not larger. Furthermore, there are many affectionate mothers-in-law in Vathima, while many of the restrictions you have noted in relation to the mobility of daughters-in-law have been equally applicable for daughters in their parental homes.

Coming to the issue of referring to real people, I am not sure whether the authors requested informants’ agreement before bringing them into the article. Many of the people discussed would have never thought that they would be written about. It is not possible to use the excuse that the names have changed, as for any of the Vathimas in general and TRP inhabitants in particular, it is easy to identify each and every one of the people mentioned, especially for me as a TRP native.

On p. 746 the authors have referred to one woman in her late forties talking about a girl from one of the big families of TRP falling in love with a non-Vathima boy and then being forced to marry her cousin who was allegedly a promiscuous drunkard. It is possible that the girl might have had some reservations in accepting her cousin, but this is the case in every other marriage. The statement from the informant about the girl is incorrect. Another equally, if not more, hurtful and inaccurate statement refers to the cousin to whom the above girl is married as a promiscuous drunkard. The authors cannot escape by saying they have used the words ‘allegedly’. Can we use the same word ‘allegedly’ and describe others in whichever way we want? I also ask you to put yourselves in the shoes of ‘Bhavani’ and consider your reaction if someone says yours is a ‘mad family’. The members of my community do not want readers of your article to obtain incorrect impressions and false statements about it, and these should be corrected in the JRAI.

Mahalingam Narayanan

REFERENCE

Reply to Mahalingam Narayanan

Dear Editor,

We are sorry that some sections of our article ‘Companionate marriage in India’ have offended Mr Narayanan and that he regards it as misleading about the Vathima community.

In criticizing our ‘sweeping generalizations’, Mr Narayanan has in our view misunderstood what we wrote. On p. 739, we stated: ‘We should make it clear that some women had good relationships with their mothers-in-law and spoke of them warmly’. We did not say that all Vathima mothers-in-law are ‘cruel characters’.

The statements about couples getting together very briefly are an accurate report of what some elderly women told Haripriya. The article states that sexual restrictions ‘are said commonly to explain the small size of many Vathima families’, but we express our own doubt about this explanation. It is also important that this section is about the situation among the Vathimas fifty and more years ago, whereas the article makes it clear that the marriage system has changed dramatically, so that the lives of almost everyone, especially women, have greatly improved since then.

Mr Narayanan complains about our discussion of ‘real-life people’. Our research fully complies with the ethical guidelines of our employer, the London School of Economics, and the grant-giving body, the Economic and Social Research Council. These guidelines require us to explain the purpose of our research to everyone who is interviewed and to assure them that their names will be changed in publication. Everything said in interviews with either of us was said freely, and no Vathima has ever told us anything that they did not want to reveal. We do not accept that we have betrayed anyone’s trust, and everything that we have written complies with the ethical guidelines, as well as normal practice among academic authors, both in India and overseas.

We have been extremely careful not to make false or harmful statements about the Vathimas and we do not believe that we have done so. Thus we put the comment about Bhavani’s family as ‘mad’ in inverted commas, to indicate – according to standard English usage – that members of another family falsely claimed that they were mad. The story of Bhavani’s marriage in our article makes it plain that the community satisfactorily dealt with her problems and arranged a good marriage for her. In relation to the case mentioned on p. 746, we insist that the word ‘allegedly’ is correctly used and make it clear that the claim that a particular man was a promiscuous drunkard is indeed an allegation only. We also accurately report what was told to us, although we accept that other people might have made different statements about the case.

As is normal practice, we are required to respect the confidentiality of people who have spoken to us in accordance with the ethical guidelines referred to above.

None the less, we acknowledge that it is true that people can sometimes be identified despite the use of pseudonyms. We regret that this is the case in our article and recognize that we should not have allowed it to occur.

During the last few years, we have been extremely grateful for the hospitality, kindness, and helpfulness of numerous members of the Vathima community, in Tippirajapuram and elsewhere, and for their willingness to answer our questions and describe their way of life. In writing about the Vathimas, our only aim is to be fair and accurate. We believe that this article as a whole fulfills that aim. As we have said, we are sorry if we have caused offence, but hope that this response will be taken as an honest effort to clear up any misunderstandings.

CHRIS FULLER & HARIPRIYA NARASIMHAN
London School of Economics and Political Science

Editor’s comment

Mr Narayanan has indicated that he does not accept the authors’ response to his letter, but has asked for this correspondence to be published in the JRAI. The offence that can be caused to people who are the subjects of anthropological writing is not a new one, but it has probably become more common and more pressing now that more and more people across the world can read English and access the internet. The editors regard this correspondence as evidence, once again, of how important it is for anthropologists to think carefully about their responsibilities to those about whom they write.

Rees’s ‘To open up new spaces of thought’

Dear Editor,

I read with great interest Dr Tobias Rees’s article ‘To open up new spaces of thought: anthropology BSC (beyond society and culture)’ (Rees 2010). It was an insightful examination of Prof. David Westbrook’s volume Navigators of the Contemporary (Westbrook 2008).

I felt that a number of the important issues raised in Dr Rees’s review were mentioned only...
in passing and I think it is worthwhile to tease them out and look, with a critical eye, at what they might imply. Dr Rees appears to be astonished that a ‘law professor’ should write a book about ethnography: ‘One has to pause for a moment in order to recognize’, writes Dr Rees, that ‘David Westbrook is not an anthropologist at all’ (p. 158). ‘How come a successful, busy law professor makes time available to write up “a mission statement”’ (p. 28) for anthropology ...’ (p.159). The rhetorical phrasing of this question expresses a sense of incredulity that surprises me. Why does it need to be asked? It is often the case that new insights in a discipline are offered by scholars who are outsiders, or relative outsiders, to it. They may for instance be armed with useful knowledge and methods not possessed by insiders. Following Prof. Bruce Kapferer (2001:18), they can act like anthropologists who – because they are outsiders – are more able to reach the group’s ‘ontological presuppositions’. There are many examples of this tendency in anthropology. Victor Turner was a classicist, Edmund Leach was a medic. Indeed, one-time American Anthropological Association President William McGee was self-taught with ‘no ... training in anthropology’ (Rees, p.158). Beyond anthropology, Michael Ventris cracked Linear B, something which eluded trained classicists, by drawing upon methods he had learned as an architect.

In critiquing Prof. Westbrook’s analysis, Dr Rees argues that ‘anthropology is informed by the insight that general theories hinder rather than further ethnographic inquiry’ ... [W]hat is needed now is not general theories but rather the insight that theories are not the remedy. The way forward is inquiry’ (p. 161). I am not sure that this implied distinction between ‘general theory’ and ‘inquiry’ is particularly easy to sustain. Prof. Edward O. Wilson (1996: 41) argues that the hard sciences are akin to gold prospecting. Scientists find an area where there are still things we do not quite understand and they start digging. They are interested in discrete questions, just as Dr Rees suggests is true of anthropology (Rees, p. 162): ‘How does this work? Why does this do this?’ However, one cannot divorce their ‘inquiries’ from ‘theory’. One cannot ‘inquire’ without some kind of theory because an inquiry involves answering a question and the possible answers are inherently ‘theories’. To make the ‘native point of view’ (p. 162) comprehensible to the reader there will be some kind of theoretical foundation, no matter how implicit.

A ‘general theory’ is surely an extension of discrete theories: it is an attempt to answer a relatively broad – ‘general’ – question which unites various discrete inquiries. If this is a hindrance to ethnography, it is hindrance to any branch of science. It risks the same obvious formal fallacies, but this does not mean it is of no use – if drawn upon carefully – in making sense of an object of inquiry and furthering understanding. A general theory of canine behaviour is surely useful, even if there are philosophical pitfalls to be navigated, and the same is true, though even more careful navigation is required, of the general theories of human behaviour suggested by scholars such as Prof. Wilson (1975). It is not ‘general theory’ that is problematic in ethnography but ‘dogmatic general theory’, and this point is equally true with any other branch of science. As philosopher Prof. Daniel Dennett (1995: 39) notes, ‘essentialism’ may be a problem, but ‘cautious essentialism’ is necessary to pursue inquiry.

I am also unsure about Dr Rees’s suggested rejection of the ‘culture’ category. He argues that a ‘profound’ reason for ‘turning away from culture’ is ‘the emergence of new ways of thinking/knowing’. This is because if one wishes to inquire into ‘new ways of thinking/knowing’ that ‘escape established categories and thereby make reality accessible in new ways’ it is ‘counterintuitive’ to begin the inquiry with the ‘assumption that whatever is emergent is cultural and/or social’. It is ‘counterintuitive’ because it ‘codes whatever is potentially new as a variant of the old’ and thus ‘presumes ... cultural or social essentialism, timeless ontological categories’ (pp. 161-2). The very nature of language means that we code that which is new in terms of variants of the old. As Prof. Dedre Gentner (1982: 108) argues, we make sense of the ‘domain of inquiry’ by means of the ‘known domain’. The way in which we begin to comprehend anything new is as a development of that which we already know. To do otherwise, as Popper (1977: 160) noted, is a kind of revolution in which everything from the past – including its language and categories – just won’t do for these radical ‘modern’ times. Quite apart from the possible development of jargon and the danger this poses to coherent scholarship, such a revolution does not sit well with critical rationalism – cautiously critiquing each generation of knowledge. Moreover, continuing to employ the ‘culture’ category is only ‘essentialist’ if done dogmatically, uncritically ... in effect with the Platonic assumption that categories sit, immutable, in the

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World of Forms. Otherwise, in nominalist terms, it is a useful category with which to begin inquiry.

To a certain degree, Dr Rees’s review article was a rallying call, claiming that ‘what is needed’ is ‘the insight that theories are not the remedy’ (p. 160). However, there is an extent to which this ‘anti-theory’ stance – this view that any ethnography that engages in ‘general theory’ is ‘essentialist’ – is itself a danger to ‘inquiry’. Certainly, it has become accepted in anthropology to the extent that research articles which implicitly disagree with it may not be published for this very reason or, at least, may need to submit to demands that they be edited in order to be in line with the ‘inquiry’ orthodoxy accepted over-confidently by some reviewers. I am not entirely sure that this view should be accepted at all and, if it is, it should be with sufficient caution to avoid the pitfalls of essentialism to which Dr Rees alludes in his thought-provoking article.

EDWARD DUTTON Oulu University

REFERENCES


On the challenge – and the beauty – of (contemporary) anthropological inquiry: a response to Edward Dutton

I

In my essay ‘To open up new spaces of thought: anthropology BSC (beyond society and culture)’ I offered a sketch, in the form of a review of David Westbrook’s Navigators of the contemporary, of what I think the intellectual potential of a philosophically inclined anthropology of thinking today is or might be.

I am grateful to Edward Dutton for his comment on my sketch – and to the editor of JRAI for his invitation to write a response. This is a welcome opportunity to refine my argument and to explain why I think the challenge – and the beauty – of (contemporary) anthropological inquiry is precisely that it escapes the conception of science that Dutton, gesturing towards Karl Popper, advances.

My response to Dutton comes in four parts. In a first step, I briefly recall what has been at stake in my exchange with David Westbrook.1 In the second and third step I then turn to Dutton’s critique – to his suggestion that I naïvely claim that theory is in general useless and that I call for a total break away from the culture concept.2 The reply ends with a brief reflection on what I call ‘Dutton’s dilemma’.

II

My review of David Westbrook’s book is part of a broader conversation about the recent past and the near future of anthropology (for a recent effort to assess this space, see Rabinow, Marcus, Faubion & Rees 2008). The immediate reference point around which this conversation evolves is the insight, on the one hand, that the ethnographic project of classical modernity came to an end in the 1980s, and, on the other hand, that, as anthropology has developed in new ways, some of the discipline’s core questions and analytical tools have lost (for many) their former significance.

I regret that I cannot go into detail here to substantiate this claim (for a recent effort in that direction, see Rees 2008). Were I to do so, I would have to document how larger political and economic transformations in the world have challenged, since at least the 1970s, some of the core assumptions of the traditional ethnographic project (notably the reliance on a nineteenth-century philosophy of history and the assumption that the world can be spatially ordered in discrete societies and cultures). Furthermore, I would have to review how, since the 1980s (and largely in response to a reconfigured world), anthropologists have entered field sites that were formerly believed to be beyond their expertise (notably science, medicine, and media); how these at first rather ‘exotic’ research undertakings into novel domains were anything but short-lived and soon developed into state of the art research endeavours in virtually all major North American research departments; and how these
developments have led to a new kind of anthropology, one in its intellectual orientation certainly not unconnected to traditional ethnography — but one none the less set apart from its conceptual curiosities and preoccupations. Given that time and space are limited, however, I can merely claim that, owing to the various departures from the ethnographic project of classical modernity that anthropology has seen since the late 1980s, many of those anthropologists who have conducted research in the new domains have found themselves in an open, undefined space to which their established analytical terms did not speak and which rendered the stakes of their discipline unstable and even uncertain. For some, including myself, this uncertainty has been extraordinarily exciting — precisely insofar as it comes in the form of an empirical and conceptual challenge that cannot be faced with established scripts (and tools) of the past; it creates a no-longer, not-yet situation that invites genuine conceptual innovation.

My review of Westbrook’s book is an effort to articulate — and sustain — this open space (against what for me amounts to Westbrook’s tendency to close this openness). It is, as well, an effort, perhaps too programatically formulated, to insist that anthropology, insofar as it has traditionally been interested in the native point of view (and insofar as it is today interested in what I would loosely call ‘the emergent’), is uniquely suited to exist in this open space.

In short, my essay is part of a specific conversation about anthropology today (or some variant of it) and my suggestions can only be properly understood in the context of this conversation.

III

The first objection Dutton raises in his effort to ‘tease out ... with a critical eye’ some of the ‘implications’ of my article concerns my suggestion that what is needed ‘now’ is not a ‘general theory’ but rather the insight that theories are ‘not the remedy’. ‘The way forward’, I write, ‘is inquiry’.

The ‘now’ in the above quote refers to the current state of anthropology (an anthropology that exists in an open, uncertain space precisely insofar as it unfolds beyond the framework of the ethnographic project of classical modernity); the ‘remedy’ refers to Westbrook’s suggestion that current anthropology is in a predicament because it has no intellectual framework of its own; and the emphasis on ‘inquiry’ is meant, on the one hand, to counter Westbrook’s suggestion that because anthropology has at present no intellectual framework it is in need of ‘a new general theory’ (a call that is somewhat reminiscent of Talcott Parsons’ Carnegie Project on Theory, meant to provide a ‘general theory’ that would unite the social sciences), and, on the other hand, as an indication that the new conceptual framework cannot be invented in the abstract (i.e. theoretically), that it can only grow out of actual empirical engagement with the new research domains.

Dutton happily ignores this specific context. What he sees in these sentences is the articulation of a general distinction between theory as such and inquiry as such (he even goes so far to suggest that I define theory as useless and as a hindrance to science). Such a general distinction, which I nowhere articulate, he finds particularly difficult to sustain. As he writes (implicitly recalling Popper 1934), ‘[O]ne cannot divorce ... “inquiries” from “theories”. One cannot “inquire” without some kind of theory because an inquiry involves answering a question and the possible answers are inherently “theories”’.

Before I can properly explain why I have reservations in relation to Dutton’s comment, I need briefly to comment on his understanding of theory and the conception of science it ultimately suggests. For Dutton, or so he says in his comment, every form of inquiry is grounded in a theory. By theory he appears to understand — again the reference is to Karl Popper — a falsifiable hypothesis that is articulated with explanatory intent. Expressed in other words, for Dutton, a theory is a causal schema that is formulated in the form of a proposition that seeks to explain a given phenomenon (usually by referring to first principles that allow a decoding of the given phenomenon). A successful theory is one that corresponds to the things it seeks to explain (the latter exist independently of our theories but are not accessible without theories).

Built into this conception of theory-driven inquiry is a far-reaching conception of science: namely that science progresses by formulating theories, by falsifying and improving them through inquiry, until, eventually, one arrives at a theory that is (ideally) in complete congruence with what (following Rudolf Carnap 1928) one may call the logical structure of the world (which implies that the world must be finite and
more or less immutable in its logical structure — for otherwise one could not hope to progress in one’s knowledge of the world).

Certainly Dutton’s conception of theory is one — albeit often criticized and disputed — way of thinking about science (and humans). It has its origins in the Wiener Kreis and, more specifically, in Karl Popper’s efforts to overcome Otto Neurath’s distinction between theory and theory-independent fact. I do not wish to recapitulate the — for me — compelling critique that has been directed towards Karl Popper (notably by Thomas Kuhn, Paul Feyerabend, Theodor Adorno, and Jürgen Habermas); nor do I wish to rehearse the results of the many ethnographies of science — from Fleck and Canguilhem to Latour and Rheinberger — showing that everyday laboratory research has really little to do with the conception of science that was advanced by critical rationalism. Instead I would like to raise three reservations against Dutton’s (and Popper’s) theory-based understanding of science (two general, and one specific to anthropology).

My first reservation is that it would be a mistake to assume, as Dutton does, that the only possible alternative to theory-driven research is a naïve positivism helplessly lost in the false belief that unmediated access to reality is possible. One can certainly think through the conceptual presuppositions one brings to research without articulating them in form of a falsifiable theory (so one can inquire without theory — without being necessarily a naïve positivist).

My second reservation is that Dutton’s conception of science would result in a rather impoverished conception of research, for if one would follow Dutton’s proposals, research would be reduced to the rather meagre role of saying yes or no to a pre-formulated hypothesis (certainly most forms of research have a dynamic of their own that cannot be reduced to this yes/no logic).

My third reservation is that Dutton’s conception of a theory-driven science denies the possibility that fieldwork — whether conducted in a remote village or in a cutting-edge laboratory — is a business of surprise that is intrinsically guided by what I would like to call here the ‘theory-less but epistemologically not naïve’ effort to move beyond what one already knows.

Let me explain.

Certainly Dutton is right to say that one almost always enters a field with a certain preconceived conception, with an intuition, perhaps even with a worked-out theory one wishes to test (note that I suggest that conceptions and intuitions are not theory). But what happens once one enters the field? Is it not almost always the case that fieldwork — insofar as there is a mismatch between the field and one’s ideas (whether theory or intuition) — profoundly shatters the preconceived? That it literally derail the scenarios and assumptions one has laid out beforehand?

Perhaps such derailment is for Dutton mere noise — something that distracts the researcher from her actual business. For me, however, this derailment — this noise — is at the methodological core of fieldwork. It is at the core of it because it (at least potentially) invalidates the preconceived, robs the ethnographer of the conceptual framework with which she arrived in the field. The effect of derailment is a loss of orientation that creates the conditions for the fieldworker to learn to reorient herself in the field; to find, amidst a bewildering, seemingly chaotic perplexity, a way of bringing the field site (or elements of it) in its specificity into view; to learn ways of thinking it through in its own terms (all the while reflecting on the conceptual presuppositions one makes).

It follows that, for me, anthropology is a field science. And by field science I mean not only that the knowledge production happens in the field. I mean a knowledge-producing practice (a science) at the core of which is the field’s particular potential to lead astray; to profoundly derail the fieldwork scenarios and theories one has laid out before one entered the field; to lead into yet uncharted, not thought through terrains of marvel and surprise.

The challenge of fieldwork, this is to say, is to get lost, to be carried away, and to return with a genuine discovery — something unthought-of (traditionally this was the native point of view — that is why I stress it in my review essay).

Now Dutton may insist that the reorientation in the field involves theory, just as he may insist that the return from the field involves the effort of giving form and hence theory. But in contrast to Dutton, I think neither that the effort of thinking something through in its own terms has anything to do with a theory (an explanatory schema that advances first principles), nor that the effort of giving form to one’s discoveries can only proceed by way of theory. There are various ways of giving form besides theory — from narrative to concept work to interpretation (this point implies that humans can ‘understand’ in different ways and that theory is only one among several possible forms of ‘comprehension’).
I hope one can see now that my wish to defend anthropology against theory is neither naïvely positivistic nor anti-science (as Dutton suggested). It is merely an effort to render visible the possibility that is for me at the heart of anthropological inquiry: the possibility that elsewhere it could be different, that other ways of thinking and being in the world exist, that there is no intrinsic necessity to our forms of living, and that one can get to know them in their own right.

In my review essay of Westbrook’s book I have sought to stress that anthropology – precisely insofar as it is a field science, precisely insofar as it is interested in what has once been called the native point of view – is and has to be disinterested in general theory. I did so in order to stress that the openness Westbrook identified as problematic for anthropologists is really the intellectual centrepiece of (at least some variant of) anthropology. Hence anthropology is ideally suited to feel at home in this open, challenging situation – without a general theory.

IV

The second major critique that Dutton directs towards my ‘rallying call’ concerns what he identifies as my ‘rejection of the “culture” category’. In my essay I suggest that ‘one’ reason why one may want to question the self-evidence with which anthropologists employ the category of culture is that the things we may discover in the course of our fieldwork are possibly such that they escape the conception of the human and the world that is built into the culture category.

For Dutton such a suggestion is puzzling. After all, he writes, ‘The very nature of language means that we code that which is new in terms of ... the old’. Humans, or so Dutton suggests, are speaking beings. They exist in the world by way of their language – and hence anthropology, as science of the human, must be the science of culture (a linguistic concept).

For Dutton my refusal to see this evident truth of human nature is a sure sign of my ‘revolutionary’ inclinations (in fact, he equates me with the socialist revolutionaries Popper criticized in 1957). He reads my suggestion to think and practise anthropology beyond (not after) culture as saying that ‘everything from the past – including its language and categories – just won’t do for these radical modern times’.

Such revolutionary ambitions, Dutton cautions, are naïve – and in their naïveté they are dangerous (insofar as they go against human nature).

Again Dutton de-contextualizes a comment of mine and then treats it as a general statement. My suggestion may be radical from a philosophical perspective – insofar as it suggests that culture is only one form of thinking about humans – but revolutionary? Nowhere do I suggest that ‘everything’ from the past is useless for understanding our ‘radical modern times’ (a phrase that appears nowhere in my essay). I do not even suggest that one should abandon the culture concept.

The reason why I try ‘to open up a space’ beyond the self-evidence with which anthropologists employ the category of culture (and of society) is a reaction – if this is the right term – to Westbrook’s idea that what we need to do ‘now’ is to adjust our old culture concept (culture as timeless, bounded entity) to our fluid times (an adjustment, or so Westbrook suggests, that will re-instil conceptual security in anthropologists).

Looking at one kind of anthropology – the anthropology of thinking – I suggest that anthropological research is not intrinsically bound to culture. Culture, I argue, may even be an obstacle to anthropological research, especially at a time when research addresses new themes that potentially escape the definition of the human that is built-into the culture concept.

In order to clarify why I find that culture is (for me) limiting the intellectual potential of anthropology, I would like to introduce a distinction between two ways of practising anthropology.

The first is grounded in having found a convincing answer to the question constitutive of the discipline, ‘Qu’est-ce que l’homme?’. With this answer in hand, the anthropologist may then enter the field (however constituted) and make sense of the ordinary as well as the exotic.

Arguably, the most prominent answer to the question concerning the human that (American) anthropologists have worked with is ‘culture’. Humans are, or so cultural anthropologists self-evidently claim, cultural beings. Expressed in other words, they are meaning producers. They operate with symbols, be it in a complex ritual or in a simple experiment, and live in symbolic systems. Nothing humans can do or think of exists without meaning – even the very idea of ‘nature’ is ultimately a cultural category, is meaning. The terms of an anthropological analysis, from the cultural point of view, must
therefore be interpretative ones – derived either from philology or from semiotics, from hermeneutics or deconstructivism.

The second kind of anthropology is anything but hostile to the first one, but in its interpretation of anthropology as an intellectual endeavour it radically differs from it. Where the first kind of anthropology first defines humans – for example as cultural beings – in order to then travel the world and decode the diversity of life forms with the help of its answer, the second one is interested in finding out if elsewhere (I use this term here in a spatial as well as in a temporal sense) other forms of thinking the human exist, forms that in their conceptual specificity escape our established ways of thinking and knowing what was once called ‘Man’ (consequently, it avoids beginning with answers).

For example, confronted with the claim that humans are cultural beings, this second kind of anthropology will inevitably wonder when ‘culture’ became a convincing answer to the question of what the key distinction of humans is. Certainly, ‘culture’ is not a timeless category. It was not always around, has not always and at all times been central to thinking about things human. Culture, this is to say, has a history and is place-specific (e.g. the definition of humans as an essentially speaking beings that Dutton employs emerged roughly two hundred years ago in Europe, cf. Foucault 1966).

The same sense of wonder this second kind of anthropology – and this is where fieldwork becomes central – directs to the present. Are humans elsewhere conceptualized differently? Are today, for example in the sciences, new ways of answering (or posing) the question concerning the human emerging? New in the sense that they trouble – or insert movement in – our established categories of knowledge/thinking? In what areas? In yeast biology? In drosophila genetics? In behavioural dog genomics? In Avian Flu research? In HIV vaccine research? Where, and in which ways, do we see the *humanum* in motion, in metamorphosis? These questions are at the core of what I have called a philosophically inclined anthropology of thinking (even though it is not restricted to things human).

The reason why I find Dutton’s insistence on culture intellectually delimiting is that it denies the very possibility of the research endeavour I have outlined here. How else are we to read his insistence that we cannot turn away from culture because of ‘the very nature of language’? Dutton, apparently, already knows. Humans, insofar as they are at home in the world through language, are cultural beings (that is why anthropology cannot exist beyond culture – and why the call to practise anthropology beyond culture equals for him the call of a dangerous utopia-driven revolution).

But what if elsewhere people think humans differently? For example, what if they insist that language cannot be a distinction of humans because animals and plants and stones have language too? Or they insist that the humans and cows and cucumbers are really the same thing? Or they explain that the only difference between humans and other animals is that in humans the L1 retrotransposon is activated by adult neurogenesis in the dentate gyrus of the hippocampus? All culture?

For me, the problem is not – as I explicitly state in my review – that a culture-focused analysis would not be useful or interesting. What I find problematic about culture is, on the one hand, the reduction of others to our own conception of the human (a form of symbolic violence), and, on the other hand, the denial of the very possibility that elsewhere it could be different (or that new ways of thinking about things human could emerge). The consequence, for me, is boredom in the face of the eternal repetition of a script – culture.

The intellectual significance of a philosophically inclined anthropology of thinking is that it views ‘culture’ as merely one form of thinking about humans, one among many possible ones. It thus opens up a space beyond the assumption, stated by Dutton, that anthropology, insofar as it the science of the human, is a science of culture (maybe that is revolutionary – but not in the way Dutton suggests).

Its intellectual significance is furthermore, or so I like to think, that it can bring contemporary anthropology into view – and this is what I have tried to stress in my review of Westbrook’s book – as a wonderful opportunity. Precisely because it unfolds beyond our established scripts (for Westbrook a dilemma) it requires us to not only think about new research domains, but also to think about them in new ways, beyond the established scripts of the past. Culture can be one significant element of such thinking – but it should (for me) not be the only one. Hence the title of my essay: ‘To open up new spaces of thought: anthropology BSC (beyond culture and society)’.

V

I wish to end this review by bringing into view what I would like to call ‘Dutton’s dilemma’.

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**Journal of the Royal Anthropological Institute (N.S.)** 16, 892–900

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This dilemma is manifest in the final paragraph of his commentary:

Dr Rees’ review article was a rallying call, claiming that ‘what is needed’ is ‘the insight that theories are not the remedy’ ... [T]his anti-theory stance ... is itself a danger to ‘inquiry’. Certainly, it has become accepted in anthropology to the extent that research articles which implicitly disagree with it may not be published for this very reason or, at least, may need to submit to demands that they be edited in order to be in line with the ‘inquiry’ orthodoxy accepted over-confidently by some reviewers. I am not entirely sure that this view should be accepted at all ...

These lines document how Dutton’s holding on to his theory-based conception of science has seemingly alienated him from much of contemporary anthropology. Given that for him the only alternative to theory is naïve empirical positivism, he cannot help but wonder about contemporary anthropology; it is theory-naïve, even theory-hostile; it is not interested in formalization and generalization; it critiques standardizing efforts; in short, it is against true science and scientific progress.

I hope I have shown in this reply that thoughtful, sincere research (science) is possible without theory, and that the challenge – and the beauty – of (contemporary) anthropological inquiry is that it may occur beyond the either-or opposition held on to by Dutton.

TOBIAS REES McGill University

NOTES

1 I should note here that important voices in this exchange are – to mention but a few – those of Dominic Boyer, Jim Ferguson, Douglas Holmes, and George Marcus. And I would like to add that not a single line of what I write would be possible without the work – and the suggestions – of Lawrence Cohen, Fritz Kramer, and Paul Rabinow.

2 I bypass Dutton’s suggestion that I am negating the (for good ethnographers evident) possibility that non-anthropologists may make positive contributions to anthropology. His point is the somewhat commonplace assumption that observations by outsiders are valuable because they do not share ‘the group’s ontological presuppositions’. As there are no clear lines to delimit inside from outside (or one ‘ontology’ from another one), I would merely like to indicate here that he treats this question of mine out of context and that the very fact that I discuss Westbrook’s book in a review article, published in one of the most reputable anthropology journals, suggests that I do consider his contribution significant.
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