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DEBATE

Beyond an evangelising public anthropology: science, theory and commitment

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If my experience of anthropology in and on Africa is anything to go by, there has been too much of engaged or public anthropology and too little of anthropology as an intellectual pursuit animated by rigorous contemplation and practice on and around a set of shared curiosities. Distinguishing between academic anthropology and engaged or public anthropology requires a priori reflection on the scientific status of anthropology. This paper argues that anthropology's scientific potential has yet to be fully realised. Without a rigorous commitment to science, theory building, and an acknowledgement of associated epistemologies, as well as little patience for knowledge production as a collaborative endeavour, much anthropology today is little different from an evangelical and ideological commitment to saving souls, saving situations, winning converts and 'giving back'. The paper challenges anthropologists to commit to the essential task of producing critical knowledge of critical value, and to re-embrace and fulfil anthropology's core mission and ambition as an evidence-based field science. The need for anthropology as a rigorous and collaborative field science, liberated from 'western' and 'male' dominance, calls for a negotiated, inclusive and accountable ethics, evidence-based thick description, an understanding of interconnections and interdependencies, and critical and comparative theory building as a permanent engagement and as a dynamic and constructive debate.

Keywords: public anthropology; science; theory; commitment

If my experience of anthropology in and on Africa (Nyamnjoh 2012, 2013) is anything to go by, there has been too much of engaged or public anthropology and too little of anthropology as an intellectual pursuit animated by rigorous contemplation and practice on and around a set of shared curiosities. Even with cultural relativism in mind, and recognition that *habitus* and social position matter, conversion seems privileged over conversation, including among anthropologists differently and differentially positioned by factors such as place, class, race, gender and generation. Without a rigorous commitment to science, and with little patience for knowledge production as a collaborative endeavour, anthropology today, it seems to me, is preponderantly evangelical in its approach – 'fighting back', saving situations, saving souls, winning converts and 'giving back' to poor villagers and migrant labourers. Soul saving, however desirable, pleasurable and gratifying to anthropologists wedded to redeeming 'powerless people' (Kulick 2006), should be left to religious and political pundits and to NGOs, while

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anthropologists should commit themselves to the essential task of producing critical knowledge for our social world.

If engaged or public anthropology is the game, a more locally and ethically relevant variant in this context would have to seek conviviality between knowledge and public interest as a negotiated understanding with those studied – ‘natives’, ‘migrant labourers’, or whoever constitutes the ‘subject’ or ‘object’ of study – and not merely as unilaterally determined by anthropologists, or by those who fund anthropological research. Engaged or public anthropology conducted in an arbitrary manner is not dissimilar to a purportedly ‘objective’ laboratory-type anthropology underpinned by the assumption that non-Western-type societies are best studied by Western or Western-trained outsiders, defined a priori and often arbitrarily, well-meaning by definition and armed with *etic* perspectives – of the well-schooled, dispassionate observer and narrator, with senses trained to perceive beyond what lies on the surface, adequately provisioned against ‘going native’ – by virtue of being outsiders to non-Western cultures and societies. This distinction between the often assumed, even if unstated, scientific virtues of the *etic* outsider anthropologist and the failings of the supposedly overly emotional and entangled *emic* insider or native anthropologist brings to mind a similar distinction made by Kazuo Ishiguro (1989) in his novel *The Remains of the Day* (42–43), between the ‘dignity’ and ‘emotional restraint’ enjoyed by the English butler – who sees himself as most deserving of his master’s acknowledgement and approval for living in his image – and the limitations of butlers from elsewhere, who are incorrigibly emotional, unprofessional, and therefore lesser creatures, deserving of little more than to be talked down upon. The English butler – or non-native anthropologist in our case – epitomised by Stevens, the narrator and butler in the novel, extols the virtues of his authority thus:

‘dignity’ has to do crucially with a butler’s ability not to abandon the professional being he inhabits. Lesser butlers will abandon their professional being for the private one at the least provocation. For such persons, being a butler is like playing some pantomime role; a small push, a slight stumble, and the façade will drop off to reveal the actor underneath. The great butlers are great by virtue of their ability to inhabit their professional role and inhabit it to the utmost; they will not be shaken out by external events, however surprising, alarming or vexing. They wear their professionalism as a decent gentleman will wear his suit: he will not let ruffians or circumstance tear it off him in the public gaze; he will discard it when, and only when, he wills to do so, and this will invariably be when he is entirely alone. It is, as I say, a matter of ‘dignity’.

Continentalers are unable to be butlers because they are as a breed incapable of the emotional restraint which only the English race is capable of. Continentalers ... are as a rule unable to control themselves in moments of strong emotion, and are thus unable to maintain a professional demeanour other than in the least challenging of situations. ... they are like a man who will, at the slightest provocation, tear off his suit and his shirt and run about screaming. In other words, ‘dignity’ is beyond such persons. We English have an important advantage over foreigners in this respect and it is for this reason that when you think of a great butler, he is bound, almost by definition, to be an Englishman (Ishiguro 1989, 42–43).

Distinguishing between academic anthropology and engaged or public anthropology requires a priori reflection on the scientific status of anthropology. Science – ‘as the knowledge of things through their causes’, or as an ‘organised body of truth regarding some special object of thought’ (Fonlon 2009, 35) – is *ideally* neutral in its aspirations and principles. Hard or soft, natural or social, science should seek knowledge for the sake of knowledge through a methodic process of critical, systematic questioning, meticulous

data gathering, analysis and interpretation, and alert receptiveness to the humility of doubt, the force of evidence and the possibility of error. If method is ‘a systematic manner of carrying on the search for truth’, the scientific method is ‘the different ways in which scientific knowledge can be obtained and presented’, as well as ‘the general rules which should guide us in the arrangement of our arguments, our reasonings’ (36). If science is a systematic way of finding out and structuring our discourses on our findings and practice, the purpose of all scientific investigation, Bernard Fonlon reminds us, ‘is principally, to dig down, as far as possible, beneath the surface of phenomena to discover their causes and the laws according to which these causes operate, in order to bring these things into being’ (34). Harnessing scientific knowledge is not necessarily the business of science, nor that of the scientist, whose values may or may not coincide with the values of others equally interested in the knowledge made possible by science and scientific enquiry. Policing the rigorous, methodical, reliable and authoritative process of knowledge production is the passion of a committed scientist – committed to excellence in knowledge production – but policing the use of such knowledge is not the prerogative of the scientist, nor the monopoly of any social actor, however highly placed, dominant or powerful.

I argue that anthropology’s scientific potential has yet to be fully realised. Most anthropology is already ‘engaged’ and ‘public’ (i.e. committed or socially responsive), even if not always as collaborative in design as expected in calls for a ‘militant anthropology’ such as Nancy Scheper-Hughes’s (1995), and also in the sense that anthropology has yet to unshackle itself significantly and beyond rhetorical claims from the evolutionary and teleological assumptions that underpin the scholarly practices it inspires and supports as a discipline. Malinowski’s efforts to transform anthropology into an evidence-based field science are widely acknowledged, but anthropologists have not done enough to consolidate his scientific ambitions.¹ Far from working towards attaining this ambition, in 2010 the American Anthropological Association (AAA) decided to excise the term ‘science’ from its vision and conceptualisation of anthropology. In the debates that followed, this was interpreted by some anthropologists, and by the media, as an abnegation of any scientific pretensions on the part of members of the AAA, even though its executive board denied this was the intention.²

Anthropology remains blurry. It is all too easy to mistake it for everything that it claims not to be. In this situation of scrambled signs and uncertain meanings, questions arise even among anthropologists, such as: What is the purpose of public or engaged anthropology? Is it to encourage co-production, reduce bias and confer accountability? What, and who, qualifies anthropology as engaged or public? Is engaged or public anthropology determined a priori, by intention, practice or outcome? What constitutes legitimate intention, practice or outcome, and does it matter when, how and what is measured, by whom and for what purpose? How has the meaning of engaged or public anthropology evolved over time? What difference, if any, is there between engaged or public anthropology by a Western anthropologist at the centre of the unequal encounters engineered by colonialism and capitalism, and engaged or public anthropology by a non-Western anthropologist at the periphery of such unequal encounters? What role, if any, do the individuals and cultural communities studied have in determining the extent to which anthropology is engaged or public? When do we know they need our help? Or are we determined to help them despite themselves? To what extent does knowledge and mastery of local languages matter in the making of engaged or public anthropology? Who, beyond

anthropologists and perhaps those studied, is able to identify engaged or public anthropology? What power dynamics might there be between engaged or public anthropology at different levels and different points in the histories of interconnecting local and global hierarchies at play? Put differently, are there objective indicators for identifying and differentiating between public or engaged and other forms of anthropology, and amongst competing forms of engaged and public anthropology? Also, how adaptable is engaged or public anthropology to the dynamic nature of society as we know it? Does it need to be reconfigured and reconceptualised with time, experience and knowledge? (see *Current Anthropology* 36 (3): 409–440 for Nancy Scheper-Hughes's (1995) call for a 'militant anthropology' and responses to it; see also Robins (1996), Dominguez (2000), Kulick (2006); *Anthropology Southern Africa*, 35 (3–4): 100–123 for Shannon Morreira's (2012) call for an 'ethics of engagement' through an 'ethics of mutuality' and responses to it; and *Anthropology Today*, 29 (6): 1–22 for Catherine Besteman and Angelique Haugerud's (2013) 'desire for relevance' and five related contributions by Besteman (2013), Haugerud (2013), Gusterson (2013), Eriksen (2013) and Ferrandiz (2013)).

Contrived objectivities and subjectivities

As the 2010 decision by the AAA demonstrates, any anthropologist that claims to be scientific is regarded with suspicion within the discipline,³ notwithstanding the fact that, as Tom Boellstorff⁴ argues, 'Science takes place not just in laboratories but in field sites; it involves not just experiments but forms of nonreplicable observation, and not just in anthropology but in sciences as diverse as zoology and astronomy'. Science is not a one-size-fits-all enterprise.⁵ Indeed, science need not be more than a set of rules, steps and procedures, to be adhered to in a rigorous, methodical and reliable manner by members of a disciplinary community of practice in order for their work to be taken seriously and their knowledge claims considered authoritative (Fonlon 2009, 43–58). Somehow, this understanding of science seems lost to many an anthropologist. Napoleon Chagnon knows only too well the discomfort towards science in anthropological circles, having had his Yanomami data and interpretations called into question as unethical and manipulated, and as tainted by his divided loyalties between *academic* anthropology and *engaged* or *public* anthropology at the service of the Atomic Energy Commission of the USA, which hired him as part of a research team on chemical and biological warfare. Regarding criticism in 2000 by journalist Patrick Tierney (2002) and by fellow anthropologists, Chagnon, credited by some for bringing biology and anthropology together, defended his approach thus:

There are some anthropologists who seriously consider anthropology to ... be more like a religion than a science. It is a religion in the sense that the truths that anthropologists subscribe to ... are established more by faith than by evidence. And one of the truths is that biology has nothing to do with cultural behaviour. And I challenge that truth. And so, like the church, or like Islam, Popes and Ayatollahs step forward defending the faith and identifying who the heretics are, and burning them.⁶

To Chagnon:

when you start measuring things and making scientific predictions on the basis of hypotheses and testing hypotheses, anthropologists get really angry about doing that, because it means somebody can be shown to be wrong.⁷

Chagnon found solidarity among socio-biologists.⁸ At the American Human Behaviour Evolutionary Society Annual Meeting in Austin, Texas, following the attacks, Chagnon, the guest of honour, was celebrated thus by the person who introduced him:

The more you accomplish the more jealousy you inspire. The more jealousy you inspire the more vigorously you are attacked by your jealous competitors This tends to be stronger in disciplines like anthropology where there is absolutely no agreement about anything. [...] So, given the lack of any real standards for settling issues, the attacks become more vigorous. And I think Napoleon Chagnon has the distinction of being the only anthropologist that has ever been accused of genocide.⁹

Kenneth Good,¹⁰ one of Chagnon's critics, acknowledges that 'data is crucial', but argues that in 'cultural anthropology' it is disruptive of 'traditional, natural behaviour' to subject humans to experimentation, a challenge compounded by the fact that most anthropologists tend to work alone. That reality is any less caricatured or disrupted by a subjective approach remains to be tested. As Good remarks in relation to Chagnon – who in turn accused Good of marrying an 11-year-old Yanomami, using love and cultural relativism as justification – anthropologists are 'ethnocentric and even racist'. How does an ethnocentric and racist lone ranger anthropology take adequate account of the realities of the field? Does the manner in which we claim and proclaim reflexivity and ethics compensate adequately for the lack of objectivity and for the biases of ethnocentrism and racism? Is it enough to have first-hand experience of the people, places and spaces studied, or does perspective matter? What forms of relationality are appropriate for a discipline that claims the status of an evidence-based field enquiry? Robert Borofsky¹¹ shares these worries when he calls for: '... data that ... others can check, others can go back and look. Not just say "I have checked it, I've done it", and look in the mirror and say, "Am I honest? Yes, I am honest, believe me"'.

While most of us have, in principle at least, renounced calling our practice scientific in the narrow laboratory science sense of the word – and rightly so for anyone uncomfortable with the 'blind faith' in 'technological and biological solutions' that such contrived ideas of science have tended to inspire in states and governments desperately seeking solutions to social predicaments that also often require 'more intense thought, contemplation and deliberation'¹² – we are yet to agree on the full implications of such an admission, and to develop a negotiated consensus around a set of core values as a *social science* or a *non-science*. Does this, for example, imply we are willing and ready to be less prescriptive about how research questions are conceptualised and operationalised, and more forgiving to all those who challenge our disciplinary fixations with ethnography and commitment to the ethnographic present? Bearing in mind that blind faith in solutions inspired by the science of nature is no more dangerous than blind faith in the humanities and its prescriptions,¹³ should we throw the baby of science out with the bathwater when we criticise the laboratory and biological sciences? The extent to which we can continue to label what we do as human or social science is debatable, even as we claim objectivity to be an impossibility and readily remove any reference to 'science' from documents on the core principles of our discipline and practice. So what, and for whom, is the 'science' we coyly claim and disclaim placebo or sugar pill?

Different sensitivities and sensibilities account for varying rationalities subscribing to different epistemic orders. It is not enough to claim or deny a particular rationality, and to elaborate and draw on manifestos and professional codes of conduct to pass for or deny our status as a science, especially if we are to provide for the counter gazes and epistemologies that have risen to challenge, or at least complement, the dominant prescriptive gaze of the Western male anthropologist (Connell 2007; Comaroff and Comaroff 2012; Boyer and Howe, forthcoming). Political parties, religious bodies and non-academic professions have manifestos and codes of conduct just as much as anthropologists do, and often are better placed to change the world, or to maintain the status quo, as their missions demand, informed or not by our scholarly research.

If we can give up on scientificity and its repertoires of instantiation, or claim it in a muted and confusing manner, what legitimacy do we have to prioritise our subjectivities or purported objectivities over those of others, and to seek to police or to sit in judgement on the subjectivities of purportedly inferior others – be these fellow disciplines and scholars or those we study? How do we manage unequal relations of power among ourselves as anthropologists of different racial, cultural, class, ethnic, gender and generational backgrounds and social positions, and among those (characterised by equally taxing complexities) we study in ethical and other matters? Is acknowledgement of the various intersections within our identities as anthropologists enough? How do we resolve, in practice, the challenge of implementing an ethical code of conduct based on an abstract notion of a common humanity and the universality of human rights and dignity – particularly when universality is implicitly assumed on the basis of endeavouring to study other societies and supposing the study to be legitimate? In the light of these unresolved issues, how do we proceed with business as usual without the risk of being perceived as wanting to have our cake and eat it too? What would business unusual look like?

This nebulous and fuzzy context in which we operate could very easily play into the hands of opportunism and laissez-faireism. An apparent preoccupation with an emancipatory anthropology, and with an ethic of care, could serve to take attention away from the reality of persistent evolutionary thinking and practices. We have yet to answer the question of what anthropology as a science would look like if we were to distance ourselves, beyond rhetoric, from evolutionary claims of civilisation that have legitimated our studying and exercising power over those we perceive as culturally different.

How do we undo (and find a replacement for) anthropology informed by Western ideas of *missions civilisatrices* of the colonial encounter? How do we effectively challenge or provide for the hierarchies of humanity explicit or implicit in assumptions of civilisation, or the lack thereof, exported, externalised and reproduced in the formation of the discipline? If *habitus* is second nature, how do we, in the absence of any fruitful commitment to objectivity or impartiality and scientificity, ensure that we are conscious enough and ready to improvise conceptually and methodologically when faced with the unfamiliar, rather than resorting to capriciously shifting the goalposts of knowledge production? What does it mean for this very consciousness to be implicated in the reproduction of asymmetries of power within and outside our discipline (Dominguez 2000, 363)?

The controversy around representations of Yanomami by anthropologists such as James Neel, Napoleon Chagnon, Jacques Lizot, Kenneth Good and Raymond Hames,¹⁴ reveals that subjective anthropology is not necessarily less unethical and less of a

caricature of those studied than anthropology with objective or scientific pretensions. 'Real life' can be distorted as much by the manipulations of quasi-experimental field methods modelled around laboratory sciences as it can be by fieldwork infused with obvious or subtle evolutionist assumptions and salvationist fantasies. The biases of instrumentalisation are not that dissimilar from the biases of conversion. Both the supposedly objective and scientific Chagnon and the subjective and compassionate Good and Lizot have been accused of unethical practices and excesses. While Good married an 11-year-old Yanomami girl, sanctioning the act by cultural relativism, Lizot used his power and wealth with impunity to lure young Yanomami boys to indulge in sexual liaisons with him in exchange for gifts. This raises the question of the relativity of ethics and how to arrive at a consensual repertoire of ethical considerations co-determined not only by anthropologists and their professional associations, but also in collaboration with those studied.

We cannot swing from cultural relativism to ethical universalism as it suits us. Good's marriage illustrates the importance of a broad and inclusive negotiated idea of research ethics. As Terence Turner observes, by Yanomami standards the marriage was not unethical, as it was at an age when Yanomami girls are ready for marriage, but, not being Yanomami, by the standards of his American society, Good was marrying a girl who was not of legally responsible age. But as Good argues in his defence, where does one draw the ethical line in matters of love in the field? And what happens to an anthropologist who has lived among a people for such a long time that 'that line, if there was one, seems to fade away', as he or she feels and acts like the 'Other'? Gone native, Good adds: 'You cannot discard the fact that when you get to know a people well, the similarities far outweigh the differences, but at first contact you see just the opposite'.

An ethical line between two contentious moral codes cannot be drawn arbitrarily and exclusively by anthropologists. It requires a significant amount of power to decide between two moral codes that are incredibly at odds. To what extent are our ethical choices made out of convenience? To what extent was Good's belonging in the field reinforced by the marriage, and to what extent did his new wife become a part of his ethnographic toolkit? Good denies that he married a Yanomami woman to jumpstart his anthropology career, saying his accusers 'have absolutely no idea how much it inhibited my anthropological work', even as he admits that his book with David Chanoff (Good and Chanoff 1997) on the marriage has been well received, translated into many languages and attracted National Geographic and high profile television appearances. Taking subjectivity for granted, how can one begin to account for interest, self and collective, public and otherwise? Is this enough for us to question whether a single framework would ever be possible or even suitable?

Theory building: a worthy public engagement

There is need for a clearer distinction between the core mission of anthropology, and anthropology at the service of various causes. As humans, and as anthropologists, we notice what we want to notice, or what we have been conditioned to notice, or what we consider to be important for the issues at hand. Our quests are guided not only by what is, but also by the type of questions we ask, and by the worlds we embody. What seems clear and obvious to someone particularly positioned with one set of questions in mind is obscure to another from a different vantage point with a different set of interrogations.

Social truth is negotiated, layered and nuanced; it also has an inevitable flexibility and volatility. In social research, silly questions beget silly answers. We need theories to sharpen and direct research questions and to facilitate the production of meaning in an ever-changing world of relationships of force, and the force of relationships (Connell 2007; Comaroff and Comaroff 2012; Boyer and Howe, [forthcoming](#)). A promising theory suggests that something is worth observing because it is grounded on past observations, and is able to point research in fruitful directions precisely because the fruitful directions of past research contributed to affirming its status of theory. We need theories as interpretive frameworks for making sense of field data which, however rich, cannot quite speak for themselves. Our commitment to ethnography is not unconnected to an instinctive suspicion of sophisticated research techniques (e.g. quantitative surveys, questionnaires, experimentation, data analysis software such as Nvivo, SPSS, etc.), especially when these are not matched by the sophistication of research questions and findings (Comaroff and Comaroff 1992, 10; Cerwonka and Malkki 2007, 170). Given this commitment to ethnography and to participant observation as a privileged tool of data collection, and given the implicit and explicit centrality of our senses (both biologically and socially shaped), is it not appropriate, in the interest of knowledge production and theory building, to contemplate and systematically provide assurance – in sensory perception terms as well as in terms of the social shaping of our nature (taming, domestication or education of our senses to suit our purpose as ethnographers) – that we have the minimum qualifications to pass for, and aspire to excel as, ethnographers? What corrective mechanisms do we set in place for those of us who may, our enthusiasm for ethnography notwithstanding, fall short biologically and socially, of our collective great expectations in the making of an ethnographer?

Ethical codes of conduct and manifestos do well to make anthropologists feel the potential cost of their errors to the reputation of the discipline. Because of how the discipline conceptualises the field and fieldwork, anthropologists are more aware of the direct costs to their discipline and personal reputation of their actions, especially those perceived to be wrong or unethical – which in turn makes it easier to fall into the role of magnanimous philanthropist. Could an effective return to Malinowski's idea of anthropology as a field science make anthropologists more accountable to those they study and to their discipline? Could it also allow for an alternative perspective that the anthropologist might not necessarily possess without a scientific gaze? What aspects of the discipline would make it scientific, were we to take account of the history of reflection on anthropology's scientific status from Malinowski through structuralism to the anti-science stance of the post-structuralist period? Would being a science make practitioners of anthropology more readily honest and willing to admit error, both to fellow anthropologists and to those they study? Not necessarily, as scientists, like all humans, are not saints and sometimes fall short of the ideals of their practice. A favourite theory, finding or knowledge claim may be clung to grimly in the face of mounting contrary evidence. But with science being a collective pursuit, there is always the prospect that sooner or later someone elsewhere will, through a restudy, expose the inadequacies of the stubborn theory in question. As the Yanomami controversy demonstrates, anthropology is all the richer with more anthropologists researching the same peoples, places and spaces, and comparing findings. This potential to be richer through critically scrutinising convictions and conventions taken for granted is to be expected, especially since previously colonised 'natives' and formerly (and currently)

marginalised social categories now feature among practicing anthropologists, and epistemological debates have unearthed the prospect that anthropological insights are possible from beyond the confines of the discipline (Gupta and Ferguson 1997; Nyamnjoh 2012, 2013).

When Clifford Geertz (1973) invited us to seek to understand social action in a complex and layered manner, arguing that ‘small facts speak to large issues’, such as ‘winks to epistemology’ and ‘sheep raids to revolution’, ‘because they are made to’ (23), this was a promising call for theory building in anthropology. By recognising that ethnographers ‘are reduced to insinuating theories’ because they ‘lack the power to state them’ (24), Geertz (1973) was arguing for legitimate and informed theorising adapted to anthropology as a discipline with its nose ‘closer to the ground’ – one that was more effective with ‘short flights of ratiocination’ than with long bouts of abstraction. Geertz was keen on anthropology reaching for the skies, but with its feet firmly on the ground. Equally, comparative perspectives were welcome, through mobilising previously discovered facts, using previously developed concepts and trying out previously formulated hypotheses, not necessarily ‘from already proven theorems to newly proven ones’, but ‘from an awkward fumbling for the most elementary understanding to a supported claim that one has achieved that and surpassed it’ (25).

Such improvisation of theory (Cerwonka and Malkki 2007) is an acknowledgement of anthropology’s status as a social field of inquiry, and also of theory building as a permanent and consistent engagement. In this regard, Geertz (1973, 26) argues, ‘the essential task of theory building ... is not to codify abstract regularities but to make thick description possible, not to generalize across cases but to generalise within them’. By ensuring that ethnographies contain the yardsticks for their own verification and appraisal, it is possible, Geertz maintains, to ‘draw large conclusions from small, but very densely textual facts; to support broad assertions’ (28). It could be argued, however, that generalisation across cases – what Dominic Boyer and Cymene Howe term seeking the ‘*transparticular*’ or the ‘process of striving to wrest away from a case study the cluster of insights that are worth mobilizing’ (forthcoming) – is possible and to be encouraged, especially in instances of ethnographies with demonstrated efforts at systematic and patient theory building. This aspiration is all the more significant, given that anthropology’s ‘most telling assertions are its most tremulously based’, and the more one claims to have found out, the more one and others’ suspicion grows that one may not quite be getting it right (29). Not quite getting it right is not the same as being impossible to get it right. Painstaking humility and modesty are essential, given the ‘intrinsically incomplete’ nature of cultural analysis, an exercise that gets ever less complete the deeper it goes (Boyer and Howe, forthcoming), and given Good’s point that the more we get to know well those we study the less different they become.

The ‘essentially contestable’ (Boyer and Howe, forthcoming) nature of ethnographic assertions emphasises the need for a meticulous, systematic approach to theory building and theory testing in order to mitigate the temptations of theory poaching and slash and burn approaches to theorising. Without humility and systematic commitment to theory building, the risks of jeopardising anthropology with an ill-informed cacophony of theoretical positions are all too real. Contrived objectivity impairs the imperative to pay ever closer attention to ‘concrete social events and occasions’ and to ‘the public world of common life’ in seeking ‘connections between theoretical formulations and descriptive interpretations’ (30).

Boyer and Howe (forthcoming) point to and encourage current efforts underway to transform and take theory seriously, and to engage in ‘lateral theory’ building in anthropology through greater self-awareness, flexibility and experimentation in the development of ‘a new lateralist ethics of theory’. Complex social realities call less for dichotomisation than for understanding interconnections and interdependencies. Culture and nature are more complementary than the Yanomami controversy suggests. Equally complementary are outsider and insider perspectives, especially if, and when, field research goes beyond token participant observation, to include full-time and long-term immersion in the world and lives of those we study. Such long-term interaction seldom leaves the anthropologist and their hosts unaffected. If conducted in earnest, the interactions between anthropologists and hosts grow into an intimate, intuitive, insightful and empathic comprehension that inspires and guides their thoughts and actions with nuance, complexity of perception and understanding. Fieldwork becomes, in this context, an exercise in intercultural conversation, dialogue and exchange of views and understanding in an intimate and open intercorporeal and intersubjective manner between researchers and their hosts (Merleau-Ponty 1964; Jackson 2009). Intercultural encounters demand close interaction in confidence, and in exploring mutual interests and understanding, bound by the collective and individual histories, predispositions and culture-specific perceptions of those involved. A sensitive, compassionate and transforming experience develops between anthropologists and their hosts that should be acknowledged as an integral part of the process of distilling from such encounters for scholarly consumption. It is critical to recognise and provide for such ethically bonding intercorporeal and intersubjective interactions in knowledge making and theory building (Devisch 2011). Open-ended conversations among anthropologists of various backgrounds, social positions and intellectual persuasions, and between anthropology and other disciplines, as well as between anthropologists and non-anthropologists outside of the academy, would benefit theory building enormously. However, granting that all these interactions would have to happen in some place or space, this begs the following questions. How does one create or animate such interactions to ensure they happen? How does one open up these possibilities when the academy limits these places and spaces to seminars and conferences; and not the beer hall, the hairdresser’s and other places where theory building takes place as persons build explanations for their observations and experiences of life? Theory has the capacity to detect and account for sameness through open-ended conversations that are simultaneously accommodative of difference, creative innovation and renovation globally is what anthropology needs.

Confused and confusing commitments

Few anthropologists would doubt the importance of science, yet few believe that science alone suffices to change the world – hence the declared or assumed marriage of science and ideology in the perspectives and practices of many, implicit in the Yanomami controversy. We need clear, distinctive indicators of what is fundamental to anthropology as a science. We need evidence of a widely accepted and rigorously theorised body of knowledge in order not to quibble when we are asked by students and outsiders to our discipline about what constitutes the substance of anthropology, and how different, substantively, anthropology is from its doubles, lookalikes and mimics.

An investment in clarifying the scientific and theoretical underpinnings of anthropology would assist significantly in determining different forms of anthropology, and in throwing light on how similar or different anthropologists are from researchers in other disciplines – and indeed concerned non-academics – equally preoccupied with social responsiveness. Engaged scholarship in the form of science at the service of ideology has a long tradition in post-colonial societies. Among colonised ‘native’ intellectuals, it took the form of variants of articulations of freedom and liberation struggles, some more radical than others, depending on the ideological persuasion of those involved, in a world increasingly abstracted and polarised.

In Africa, at independence, especially between the 1960s and 1980s, the imperatives of modernisation – what James Ferguson (1999) refers to as ‘expectations of modernity’ – meant that few states – and intellectuals – could afford the luxury of science for the sake of science. As Bate Besong Cameroonian advocate for engaged literature famously puts it, one does not sit down to write a love poem when one’s house is on fire.¹⁵ As the thinking went, the urgency of liberation, modernisation and development was such that only a combative or an engaged science could do justice to it. Within the evolutionary logic that conflates progress with betterment, and considers the new to be invariably superior to the old in a linear hierarchy of possibilities, anthropology has been harnessed by exogenously induced ideas of social change – the pursuit of modernisation and development through mimicry. If science this was, it was more a science of necessity and/or convenience, than one perpetuated and carried out for its own sake.

Anthropologists – including Malinowski, who believed rigorous evidence-based anthropology could be applied effectively by administrators and others desirous of anthropology’s applicability (Malinowski 1945, 1–13) – were equally engaged as handmaidens or footmen of colonialism and empire (Mafeje 1998; Comaroff and Comaroff 1991, 105–125; Magubane 2004). It could be argued that the public mission of anthropology – “its evil twin” (Ferguson 1997) – has been part of its core calling since the time of conquest and empire, and that the two faces, academic and applied, are in a complex and changing historical relationship. However, commitment that might appear emancipatory in the immediate and short term could, in the long term, and often at a closer look, actually serve to compound domination and oppression. If colonial anthropology was engaged, it was not an engagement to liberate, but to justify and excuse the violence of colonialism, even when apparently critical of aspects of colonialism or working to foster ‘development’ in the colonies and among ‘the native’ populations that caught the sympathy of the anthropologists.¹⁶ Teleological colonial anthropology has survived as applied and consultancy anthropology. In academic institutions, it camouflages itself as the anthropology of modernisation, development or globalisation (Gardner and Lewis 1996; Cooper and Packard 1997; Edelman and Haugerud 2005). In light of anthropology’s inextricable implication in highly unequal relations of power, Don Kulick (2006, 934) argues, ‘any attempt to theorise anthropology’s alignment with powerlessness must take into account its origins in and its continued alignment with power’.

In Africa, for example, while some ‘native’ or ‘not so native’ anthropologists have critically questioned dominant evolutionary theories, others have tended to yield rather easily to the expectations and prescriptions of modernity. Like evangelists, some anthropologists are engaged with bringing about salvation for their communities of study. Social change (not always clearly defined or seriously interrogated) is considered a

burning issue to be promoted or circumvented selectively, and research themes and locations are chosen with the objective of contributing to alleviating or bringing about the end of poverty, exploitation, inequality, marginalisation and the loss of authenticity. There is the implicit assumption that the academy is the most effective site from which to pontificate on social change. This paradoxically reinforces the ivory tower instead of critically interrogating it, as social change becomes yet another aspiration to which academics must commit expertise and creative imagination with evangelical zeal. A call for voice or liberation among the people of a remote local community by an anthropologist may be hailed – often uncritically – but just how different is such a call from another by a fellow academic – a ‘native’ or ‘feminist’ anthropologist perhaps – for anthropology to be liberated from ‘Western’ or ‘male’ dominance? Addressing powerlessness within the discipline of anthropology itself might require a critical look at what Don Kulick (2006, 934–935) calls, ‘the libidinal structure’ within which the discipline has taken shape and operates, and that involves answering such questions as: ‘What is the nature of the pleasure that anthropologists derive from the powerless?’; and to what extent might such unconscious disciplinary structures ‘work to engineer particular silences and sustain particular relations of power rather than challenge them’?

Notwithstanding the rise of alternative or complementary development thinking and practice, some development practitioners such as the World Bank and the International Monetary Fund – many with anthropologists among their ranks – rarely study social change in a holistic and historicised manner. Emphasis is often on approaches to development as a unilinear process of routinised, standardised, calculable and predictable practices (Ferguson 1990). Teleology and analogy are privileged to the detriment of the systematic study of on-going processes of creative negotiation of the multiple influences evident in the (re)invention of traditions and modernities in post-colonial societies.

Conclusion

Anthropology would contribute more appropriately not through an evangelical and ideological commitment to saving souls and giving back, motivated or not by guilt, elitism or paternalism. In any case, such ‘anthropological desire to be aligned or identified with the powerless’ is often prone to ‘unfulfilment and failure’ (Kulick 2006, 933). A rediscovery and fulfilment of anthropology’s core mission and ambition as an evidence-based field science would yield more fruit, and could, at the end of the day, save souls – not of the poor and powerless, but perhaps of the elitist, paternalistic and guilt-ridden among the ranks of anthropologists. Kulick (2006, 943) argues that ‘the anthropological investment in the powerless is not only about the Other, but also about the self and the self’s relation not only to the Other, but to its own academic and social structures’, and ‘there risk is great that the Other will function as a prop or a substitute for the self, facilitating the self’s coming to terms with its own relation to power and deriving pleasure from doing so’. As Lyn Wadley commented on reading an earlier version of this paper:

Anthropologists should always use scientific paradigms, regardless of their chosen field of study – to me this is a non-negotiable issue and I am totally behind Napoleon Chagnon in this regard. If researchers favour the realms of belief over evidence then there are no boundaries and almost any behaviour can be justified. The problem is that ‘anti-science’ is a comfortable attitude because embracing scientific paradigms would oblige the

anthropological world to attach some rather serious labels to Kenneth Good and his fellow travellers... Institutions and funding bodies tend to force researchers into the saving of souls by having an outreach section on all application forms. I support your suggestion that perhaps we should look to saving the needy in our own ranks.¹⁷

Anthropology does not have to be, or mimic, a laboratory science to qualify as a science. It should, however, move away from a propensity for vagaries towards a more thoughtful, concrete navigation and critique of social processes, behaviours and people. A certain amount of courage is required to forge new knowledge systems and to recognise that many of the current ones are insufficient. To be critical, and rightly so, of a certain tendency towards empiricism characteristic of many a laboratory science should not blind us to the fact that 'it is the essence of science that we know not merely the fact but the reason for the fact' (Fonlon 2009, 36). The alternative to laboratory science is not non-science or *laissez-faireism*. Even as an 'art' anthropology would still need to elaborate on the techniques, the attention to detail and meticulousness, and the creative, experimental *modus operandi* to legitimate its authoritative claims. As a field science, however, anthropology is different from a laboratory science, just as it is different from a non-science. It is as problematic to assume that there is one best way of being scientific as it is to assume that there is one best way of being social and human. Sciences, just like civilisations, do not have to converge to be recognised as such. Thus, the real question is not whether or not anthropology is a science, but rather, what kind of science should anthropology be? Anthropology's scientific status should derive from its methodical and systematic observation of socially constructed worlds in which anthropologists are as co-implicated as those they study, doubling as observer and observed. Critical of laboratory science as it should be, anthropology does not, and should not, pretend to be the prerogative of objective outsiders schooled in disinterested *etic* perspectives. Because real life is many-sided, it necessarily needs to be observed from many vantage points, just as it needs various theories and approaches applied together. This is a sentiment well captured by the Nigerian novelist, Chinua Achebe (1964, 55), with the Igbo proverb: 'The world is like a Mask dancing. If you want to see it well you do not stand in one place'.

Anthropology stands to benefit from seeking to minimise impartiality and bias through reflexivity, inclusivity and collaboration in an authoritative process of meaning and knowledge making (Devish 2011, 218; Nyamnjoh 2012, 67–68). The scientific form anthropology takes also inflects the question of its appropriate forms of public-ness. As a field science whose expertise is predicated, in a fundamental way, on the generosity of others, anthropology must establish appropriate forms of relationality, and explore ways of building balanced reciprocity into its designs and operations without succumbing to salvationist fantasies (Kulick 2006). If ethical considerations are part of the equation (Scheper-Hughes 1995; Lambek 2010), how do anthropologists, aware of the fact that anthropology relies heavily on the generosity of those it studies and also of the relativity of ethics, proceed to elaborate research codes of ethics entirely without consultation or negotiation with the 'natives', 'migrants' or the 'Other', and expect it to be celebrated as a caring and compassionate discipline?

It is emancipatory, especially for communities caught in the web of evolutionary thinking, for anthropology to seriously commit itself to theory building and, in the process, be ready to invest in 'a substantial repositioning of the generative project of theory making' (Boyer and Howe, forthcoming) by turning to the global south for

inspirational insights on the nuances and complexities of an interconnected and dynamic world (Connell 2007; Comaroff and Comaroff 2012). By investing in the idea that, in a world of nimble footed reality, and presence in simultaneous multiplicities, there are no final answers just as there is little room for the theoretical stagnation of eternal repetitions and re-runs (Boyer and Howe, forthcoming), anthropology is able to offer hope and the possibility of transformation in much more certain and beneficial ways than an unsubstantiated, emancipatory commitment. This is the time for sincere contemplation and regrowth within a discipline that prides itself on its ability for adaptation. Knowing is a lifelong commitment to reflexivity, dialogue and accommodation, involving relentless curiosity and an alertness to challenge conventional ways of seeing, thinking and being. Re-embracing anthropology's core ambition in this regard is public engagement worthy of celebration.

Notes on contributor

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Notes

1. Armed with a vision and method of science as a rigorous programme of field research, Malinowski advocated long and prolonged periods of field work, careful and systematic data collection through verifiable procedures of checks and rechecks with a consistency that strongly discouraged the slash and burn approaches of opinionated knowledge producers in a hurry (Malinowski 1945, 73–83).
2. For more on the AAA and its controversial decision on its science status, see <http://www.aaanet.org/about/whatisanthropology.cfm> <http://blogs.plos.org/neuroanthropology/2010/12/10/anthropology-science-and-the-aaa-long-range-plan-what-really-happened/>. Accessed October 25, 2013. <http://www.nytimes.com/2010/12/14/opinion/114anthro.html>; http://www.nytimes.com/2010/12/10/science/10anthropology.html?_r=0.
3. Probably because of a long-standing association with the overly stressed importance of objectivity, or perhaps because of a misunderstanding of what exactly 'scientific inquiry' entails, or both.
4. Tom Boellstorff, professor of anthropology at the University of California, Irvine. Accessed October 25, 2013. <http://www.nytimes.com/2010/12/14/opinion/114anthro.html>.
5. Robert J. Shiller, one of the winners of 2013 Nobel Memorial Prize in Economic Sciences, in an article titled 'Is Economics a Science?' argues that by focusing 'on policy, rather than discovery of fundamentals', economics as a science is different from physics, for example, in its efforts to 'combine its mathematical insights with the kinds of adjustments that are needed to make its models fit the economy's irreducibly human element'. Instead of abandoning its claim to being a science, economics, Shiller argues should 'broaden its repertory of methods

- and sources of evidence' to strengthen its scientific credentials in order better to distinguish itself from charlatanism. Accessed November 8, 2013. <http://www.project-syndicate.org/commentary/robert-j-shilleron-whether-he-is-a-scientist>.
6. *Secrets of the tribe*, a documentary film directed by José Padilha, released in 2010.
 7. *Secrets of the tribe*. 2010.
 8. Accessed October 25, 2013. <http://www.insidehighered.com/news/2013/02/18/interview-con-troversial-anthropologist-napoleon-chagnon-author-noble-savages>.
 9. *Secrets of the tribe*. 2010.
 10. *Secrets of the tribe*. 2010.
 11. *Secrets of the tribe*. 2010.
 12. See Premesh Lalu's stinging critique of the manner in which the South African government overly privileges the natural and laboratory sciences to the detriment of the humanities in its allocation of research funds through a competitive system of appointment of research chairs administered by the National Research Foundation. Accessed February 14, 2014. <http://mg.co.za/article/2012-02-24-still-searching-for-the-human>.
 13. See Premesh Lalu in an earlier piece – 'Restless natives, native questions' – on why 'The humanities in SA will not survive unless we recognise they have sometimes been harnessed to political agendas'. Accessed February 15, 2014. <http://mg.co.za/article/2011-08-26-restless-natives-native-questions>.
 14. All direct quotes referenced on the Yanomami controversy are from *Secrets of the tribe*, and as transcribed by me.
 15. This was contrary to the position taken by Albert Camus in opposition to Jean Paul Sartre's 'socially responsible writing, or *littérature engagée*', when he refused to brand someone who had written a poem about the beauty of spring as a 'servant of capitalism', saying he preferred 'socially responsible people to socially responsible literature' Accessed November 7, 2013. <http://www.spiegel.de/international/zeitgeist/camus-and-sartre-friendship-troubled-by-ideologic-al-feud-a-931969.html>.
 16. See, for example, Audrey I. Richards' 1939, *Land, Labour and Diet in Northern Rhodesia*.
 17. Lyn Wadley, South African National Research Foundation (NRF), A-rated Honorary Professor of Archaeology, University of Witwatersrand, Johannesburg.

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