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# Theories in anthropology and 'anthropological theory'

Roy Ellen *University of Kent at Canterbury*

What makes a theory 'anthropological' beyond it being a theory that anthropologists use? Assuming a framework that understands anthropology in its broadest sense, this article invites us to remind ourselves what theories are actually supposed to do. Distinguishing theories in terms of the scale of presumption in their claims, it argues for a pyramid of nested levels of explanation. As we move from the base to the tip of the pyramid, so our explanations and interpretation of data must become increasingly simple to accommodate the forms of measurement that each level demands. Given this model, how can evolutionary theories based on individual behaviour geared to immediate survival and reproduction be reconciled with theories that best explain the uncertainties of 'emergent systems', or that consider how individual actions are in turn constrained by the systems of which they are part? It is concluded that anthropology has always acquired its vitality by being critically 'conjunctural', and must be ultimately and necessarily a strategic cross-disciplinary theoretical compromise.

The meanings we associate with the word 'theory' are wonderfully various. Creationists say of evolution that it is 'only a theory', meaning that it is not entirely believable. Undergraduates often complain that there is 'too much theory', meaning that they find it difficult and would much prefer ethnography. A colleague, attending a bricklaying evening class in the 1980s, found it amusing that the instructor should, after half an hour of trowelling cement, call his class to the chalkboard for what he called 'theory'. This involved pointing to diagrams distinguishing 'English bond' from 'Flemish bond'. It was theory – in the sense of 'the naming of parts' – rather than physical practice. Amongst my doctoral student contemporaries in late 1960s London were those who felt inadequate because they 'did not have a theory', and others who paraded their theories around like totems: you were a Marxist or a structuralist, or you might have your cake and eat it too and be a structural Marxist. I was always uncomfortable with such posturing, and with the idea that there was somehow a mix-and-match market-place of ideas in which you might acquire the right aesthetic and ideological combination. Theories, it would seem, serve many purposes, and certainly do more than help us make sense of truculent data: they define us as scientists, scholars, researchers, and individuals, and in terms of the perceived quality of our work. In my view, theory

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should not be something that constrains and terrorizes, but rather something that serves and liberates us.

It is not my intention to reproduce here another history of theories in anthropology, or to rank theories in any particular order, or to claim supremacy of one over any other. There is no shortage of textbooks, introductions to theory, histories, and collections of annotated readings; nor do I have in mind books on theory that argue and advance a particular theoretical project above all others. Rather, I want to discuss *theorizing*, to ask what we mean by 'anthropological theory' in relation to the theories anthropologists use. I shall try to achieve this objective within a discourse that understands anthropology broadly, in both its biological and social science senses. But let us first briefly examine perceptions of theory between 1965 and 2007, a period sufficiently long to permit the identification of some significant fault lines.

## Folk representations of theory

Lucy Mair (1969), in a review of Marvin Harris's *The rise of anthropological theory* (1969), peremptorily castigated his approach by questioning the integrity of the anthropological theory that had arisen. Harris's 'immense' book, you will recall, is indeed a *magnum opus* of all anthropological theories that have existed since the Enlightenment, from the perspective of cultural materialism, each theory being measured as a success or failure against the ideal omega point of what Jonathan Friedman (1974: 444) was later to describe as Harris's 'vulgar materialist' goal. In a curious way this simplistic and selective approach allows Harris to offer us a 'unilinear' account of the evolution of theory, and although there are differences of emphasis and order, the standard histories of anthropological theory between, let us say, 1850 and 1970 are generally presented in this way. On the whole, one lesson we have learned from the new histories of the subject initiated by Stocking (1968) is that retrospective attempts by 'presentist' anthropologists to periodize intellectual history as so many successive and mutually exclusive paradigms (evolutionism, followed by diffusionism, followed by functionalism, etc.) are fairly unconvincing when subjected to close inspection.

If we look at theorizing since the 1960s we get a rather different picture, which no doubt would have much depressed Harris. The post-1960s at first led to a theory boom in the social sciences and the humanities, a diversification of theorizing, and the feeling that theorizing was perhaps the highest form of intellectual activity known to humanity. The Theoretical Archaeology group was founded in 1977, a response to the alleged atheoreticality of the subject in the mid-1960s, and in an attempt to resemble and emulate socio-cultural anthropology, which its founders cited approvingly. But this turn, as Bruce Trigger has observed, tended to reproduce the fragmentation and sectarianism of socio-cultural theories, with theorists 'trapped in separate, non-communicating discourses' (2006 [1996]: 484; cf. Hodder 1999: 12), and in retrospect seems to have been more about polemical attention-seeking and the deliberate cult of heretical dogma than about the serious testing of ideas. In both anthropology and archaeology there was a veritable smorgasbord of theory, much of it undigested, leading Shelly Ortner (1984: 126), echoing Lowie (1920: 33), to characterize it as 'a thing of shreds and patches'. It was heady stuff, and despite everything, this anarchy of ideas with its conflicting ideological certitudes seemed to reflect a genuine frustration with the poverty of thought in their respective subjects.

The most obvious and epistemologically far-reaching development in this crisis of contradictory theorizing, discrediting and undermining the 'stable' paradigms, and


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leading to a 'loss of vision' was, some would say, the failure of the 'social science project' itself, of the positivist laws-and-causes 'social physics' kind. This was achieved partly through postmodern critique, particularly simplistic science versus anti-science claims in which science became a 'metanarrative' to be treated with incredulity and theory with scepticism (Reyna 2001: 10 and fn. 1). Many have been persuaded to avoid theory altogether, or at least to disguise it in an 'ethnography as theory' or 'praxis' approach. But it was also a virtually inevitable outcome of the internal contradictions of a search for a grand 'natural science of society'. It is much easier to see with hindsight how postmodernism and the crisis of representation resulted in a reformulation of anthropological practices, the repudiation of grand theory, a redefinition of the notion of theory, and the 'retreat into' ethnography, and even a retreat from the project of anthropology altogether; but as Henrietta Moore observes, this contest 'between those who claimed ethnography to be fiction and those who claimed it to be science' (1999: 6) caricatured the careful and grounded way in which many anthropologists were trying to theorize at the time.

There are a number of observations we can make about these developments, which we might group into two apparently contradictory trends: one (anti-theory, and anti-science) moving away from positivist and science-based ideas of theory altogether, and a second (pro-theory, but not necessarily pro-'science' in the narrow sense) that laments its neglect. In terms of the first, the retreat into ethnography, although at its worst self-referential, obscure, and irrelevant, has at least moved the theorizing to a different level of understanding and interpretation, and forced us to think seriously about qualitative research practices and writing. Grand theory, with one major exception – Darwinism in its various manifestations – is unfashionable, while since Ortner's (1984) influential overview, theories have themselves become more composite, partial, and eclectic, one might almost say 'vaguer'. There is every reason to celebrate diversification, but what some people characterize as 'theoretical change', especially over the last forty years, has in fact been more like shifting 'concept metaphors' with huge grey areas of overlap and ambiguity between different paradigms. For Moore, the purpose of a concept metaphor is to 'maintain ambiguity and a productive tension between universal claims and specific historical contexts' (2004: 71), for example 'global', 'gender', 'self', 'body', or (in science) 'mind', 'meme', 'nature'. These are more tentative 'pre-theoretical commitments', a kind of conceptual shorthand, stimulating thought; they 'act as a descriptive gloss or posit causal forces that remain unexamined [and] are essentially suffering ... from under-theorization' (2004: 80). One could well argue that what counts as theory in the humanities and social sciences has been of precisely this kind. Think, for example, of what we mean by 'the theory of art'.

The second trend, apparent from roughly the late 1980s, has run counter to this. The retreat into ethnography, empiricism, and particularism suggested that both anthropology and archaeology were 'theory-lite' and lacking in appropriate rigour. It was this mood that led to the founding of the 'Group for Debates in Anthropological Theory' in 1987 (see Ingold 1996), and a spate of books on theory around the same time. It also saw the appearance in 2001 of a new journal, *Anthropological Theory*. While the kind of theorizing went far beyond conventional natural science models, and has often been hostile to it, in some respects the growth of science-based approaches can be seen as part of this reaction.

Thus, the trends here described for the last forty years paradoxically encouraged both the subversion and proliferation of theory. The problems of explicitly endorsing particular universal theories, plus post-postmodernist scepticism, also encouraged the


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retreat into ethnography (rather, please note, than empiricism). Indeed, there is probably 'no longer a single anthropology', while the character of 'the theoretical' is itself in question. Theory has become rather 'a diverse set of critical strategies which incorporates within itself a critique of its own locations, positions and interests' (Moore 1999: 9, 18). Depending on your taste, this is either a brave new world of theoretical promiscuity, the rejection of theory altogether, or something of an impasse.

## What does theory do?

Given these contradictory trends, a cornucopia of partly digested new ideas, and the rise of anti-theory, I think we need to remind ourselves what theories are actually supposed to do.[1] Firstly, they provide us with a framework through which we can explain and interpret data, and they should do so parsimoniously. So, we might define theory as 'a supposition or body of suppositions designed to explain phenomena or data' (Ellen 1984: 9), but it is also generally agreed that 'theories' are high in abstraction and scope when compared with other kinds of generalization that are low in abstraction and scope, derived from observation and from which they are induced (Moore 1999: 19 fn. 1). Some theories will simply be concept metaphors, and we should have no problem with that. Most 'theories' in socio-cultural anthropology do not easily generate testable hypotheses or qualitative correlations, partly because past attempts to correlate have shown us how tricky this can be. So, if we correlate 'matriliny' with female status, we subsequently discover that matriliny is not a 'thing' and its borders are vague. It is this that warns us away from the dangers of simplistic quantification and measurement, and the more statistical manifestations of the wider comparativist enterprise, such as the *Ethnographic atlas* (e.g. Murdock 1967).

Secondly, whatever form they take, different theories enable us to see the same data in an alternative way, and are intrinsically equal representations until tested against the data. They can be perfectly intuitive, but often they are at their most helpful when they are counter-intuitive, giving us an unexpected perspective on some familiar data, as in versions of the well-known upside-down map, or as in Samuel Butler's aphorism that a chicken is an egg's way of making another egg. Whether we select one theory or another – quantum mechanics or wave theory, exchange theory or reciprocal altruism – will determine the representation and interpretation of those facts, but – in conformity with the doctrine of contradictory certitudes – both positions might be simultaneously correct. Consider, for example, the well-known visual trick involving ambiguous figure-ground effects (Hilgard *et al.* 1979: Fig. 4), in which, depending on the interpretation of the relationship between the same visual stimuli, a vase becomes two human faces staring at one another, or think of the disturbing graphic art of Maurits Escher. These are just more comically engaging versions of the Necker cube idea. Or, consider Paul Cohen's simultaneous proof and disproof of the Riemann hypothesis (Davis & Hersh 1983), or Heisenberg's uncertainty principle. In the virtual world of contemporary mathematics, things can paradoxically be simultaneously true and false, proven or unproven, depending on the method we use, something Edmund Leach (1968: 78-9) had begun to explore in his provocative Reith lectures in 1967, and which returned to haunt him in the years preceding his death (Tambiah 2002). So, theories are not mutually exclusive, nor absolute; only better, worse, or just different representations and explanations of our data.

Thirdly, theories imply methodology but are not quite the same thing. So one problem we face immediately at this level of abstraction is how we should separate the two, when unfortunately the terms 'theory' and 'methodology' are regularly used to


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mean different things to different people (Holzner 1964: 425-6). In one sense, methodology is the systematic study of the principles guiding (in this context) anthropological investigation and the ways in which theory finds its application. Thus, we speak of Marxist methodology, ethnomethodology, or even Darwinian methodology. It is both a branch of philosophy that analyses the principles and procedures of inquiry in a particular discipline, and also the system of methods used in a particular discipline (Moore 2004: 75). Unfortunately, through a process of conceptual slippage, methodology has also become synonymous for many with specific 'methods' or 'techniques'; and in the context of anthropology methodology is sometimes reduced to participant-observation. I tried to distinguish the different senses of methodology in my 1984 introduction to the first volume in the ASA Research Methods series, defining methods as a 'general mode of yielding data', such as interviewing or taking a life-history (Ellen 1984: 9), but even then I was fighting a losing battle, partly because 'methodology' had become a required heading in research council grant application forms. I was therefore immensely gratified recently to come across a robust defence of the distinction between method and methodology, and therefore by implication between methodology and theory, in Pete Vayda's characteristically vigorous epilogue to his own festschrift, appropriately entitled *Against the grain* (2007). Methodology and method are, therefore, conceptually distinct, but implicationally mutual and pragmatically overlapping.

So, theory is an explanatory framework, a body of systematic suppositions derived from observation, some more susceptible to hypothesis-testing than others, generally one of several alternatives that can be used to gauge its explanatory utility, and implies a methodology but not a method. Moreover, the view on which this approach is founded – and which is fundamental to the view argued here – is also that theory cannot be reduced to a set of a priori assertions: it is something which has to be carefully selected in relation to the problem at hand, and just because its short-term novelty might have worn off is no reason to discard it completely. Good theory builds on the past, rather than mechanistically or rhetorically subverting it.

## What is an *anthropological* theory?

But if we can agree on what constitutes 'theory', we still need to ask: what is it that makes any theory anthropological? In one sense – both Foucauldian and teleological – it must be one that has been suggested or utilized by those we call anthropologists, and which has become part of the discourse of the subject as its disciplines and practices have emerged historically. True, at the end of the day we interact as anthropologists because of the problems we wish to explain, not because anthropology has some pre-existing claim to exist. Anthropological theories are clearly not unique to anthropology, and none of the characteristics often listed – culture, human origins, societal comparison, the 'other' – are the exclusive domain of anthropology. However, by the same token, anthropology has exported as well as imported ideas, as the proliferation of 'ethnography' throughout the social sciences indicates. The process of adoption and incorporation has never been new. I profoundly disagree with the assertion that 'anthropological theory' cannot exist 'because anthropology is both everything and nothing' (Moore 1999: 4), and want to insist that anthropology does have specific objects of inquiry that, while maybe not exclusive, combine in ways and with intellectual consequences that are different from other subjects.

But before we explore the characteristics that make anthropological theory necessary and distinctive, we must say something about what it is not. Historically, the idea of


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'theory', in social anthropology at least, and at least as conceived in the British tradition, has been a conundrum, since it has either sought to make a virtue of being a kind of comparative sociology (which makes it derivative of general social theory), or it defines its theory with respect to particular *kinds of* society or levels of organization ('notoriously 'primitive' society, but also 'small-scale', non-Western, etc.: Kuper 1988). In the context of the wider sweep of intellectual history, this is a twentieth-century aberration, since anthropology clearly does *not* address a single geographical or ethnographic space, while we are all 'others' of some and many descriptions. Another possibility is that it defines itself in terms of its principal approach to obtaining empirical data: participant-observation or 'ethnography'. I have already indicated my frustration at the way in which methodology has been downgraded to method, but here we find the opposite: a method upgraded to a theoretical approach, and at worst its reduction to the mere nuanced writing of ethnography. This has had the effect of subverting the possibility of generalization and comparison, and, as I suggested earlier, has been widely identified as a questionable deviation into an atheoretical stance, and even into caricature (Giddens 1996). Fortunately, others have risen to the challenge this development has posed. For example, the relevant historical literature has been carefully reviewed by James Urry (2006), while Tim Ingold (2008) has robustly held the middle ground of a unified anthropology, separating it from its portrayal as merely the study of the distant 'other', and from those who seek to replace it with a version of cultural studies (Kapferer 2007). Moreover, the idea of *comparing* social and cultural forms is intrinsically problematic (Holy 1987) since what we compare are generally second-order abstractions, often arising from contested ethnocentric and simplistic divisions of human socio-cultural diversity. Although we all slip into using the terms 'culture' and 'society' as if they were concrete units of analysis, a slippage shared by those as different in their theoretical predispositions as Kapferer and the cultural phylogenists, I hope we generally do so only as a rhetorical device, or as a kind of shorthand.

By contrast, there is a contemporary trend in British anthropology that has moved on from 'the project of social anthropology' to (re-)embrace a more integrated, holistic, and interdisciplinary conception of what anthropology is about, of which – until recently – Ingold (e.g. 1992) has been perhaps the most vociferous prophet and exemplar (see also Carrithers 1992; Layton 1997). This is now a pronounced shift, no longer a minority view, as reflected in the change of British Academy Section S3 from 'Social Anthropology' to 'Anthropology'. For some, this trend towards a more encompassing anthropology stops short of an engagement with biology and is really a reconfiguring of ideas within the humanities and social sciences, perhaps to embrace psycho-analysis (as in the case of Henrietta Moore) or materiality (as in the case of Danny Miller); for others it is a change of name only, and the existing concerns of social anthropologists continue much as before. But it is difficult to see how something called anthropological theory, which presumably seeks to answer the large and central questions about human distinctiveness and diversity, can be properly anthropological without some engagement with (rather than submission to) the biological. For Ingold it is, indeed, a muscular engagement; for others it is more an acceptance to varying degrees of different versions of the Darwinian orthodoxy. A theory that claims to be anthropological must also work at a level that engages with and explains commonalities not only between humans, but also between humans and other phylogenetically related organisms, and which accounts for what is distinctive about humans in comparison to other species. I think it likely that as we move between levels of decreasing phylogenetic inclusiveness,


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so the appropriate theory changes, more or less in relation to the degree to which learning and cultural transmission permit greater extents of 'niche construction' through social and technical complexity, and thereby the emergence of socio-cultural systems sufficiently complex to support emergent properties independent of individual biological transmission.

The tension, whether between 'interpretative anthropology' and anthropological science, or between biological science and social science, constitutes one important (perhaps *the* most important) integral feature of any body of theory that claims to be anthropological in its widest and most fundamental sense. Another is that it addresses 'culture' in some form. As some anthropologists have already buried this concept, on the grounds that its identity is forever elusive and that it is just another way of talking about society, so general and vague as to explain nothing, this criterion might seem to be a major problem (Kuper 1999). But although I reject as unhelpful the trend to use 'culture' to refer to anything that humans do (e.g. 'business culture', 'culture of low expectations', etc.), I would defend its conceptual distinctiveness and necessity for any understanding of human behaviour and its consequences. While it is true that culture is infinitely protean, proliferating, and diversifying, it is the stuff that makes us human, and why and how it should be so dominant in humans is still the most important question of anthropology. From a biological and evolutionary perspective, cultural learning and symbolic culture are evidently not present in all animal behaviours, and need therefore to be defined conceptually, explained, and even measured, just like any other phenomenon. As Sahlins has observed, the concept of culture – or something remarkably like it – will always re-invent itself: 'There is no way "culture" can disappear as the principal object of anthropology – or for that matter as a fundamental concern of all the human sciences' (2000: 158).

One major theoretical focus that satisfies conditions of distinctively anthropological theory is the new cognitive anthropology, or cultural cognition. To understand how this potential can translate under particular ecological conditions requires us to look to theories that address the interface between biology and culture, brain structure and evolutionary explanations (e.g. Reyna 2002; Samuel 1990), and to explore an embodied notion of cognition and the difficult area where the Cartesian distinction between material and psychological begins no longer to hold (Varela, Thompson & Rosch 1993). But to explain how and why information enculturated in the brain should change further on in time and have an impact on the reproductive capacity of individual human populations requires theoretical resources of a different kind, those that combine the most plausible and robust elements of evolutionary ecology, cognitive psychology, and cultural selection.

So, an anthropological theory, as opposed to, say, a sociological or psychological theory, is one that takes culture seriously. Works of anthropology generally promote the view that the understanding of contemporary and complex socio-cultural forms can only be obtained by examining *all* historical variations in human behavioural systems, past and present. Anthropology, therefore, looks at socio-cultural breadth, and seeks to explain difference and diversity. By contrast, works of sociology generally examine the complex societies of the present in order to produce inferences about the rest. And although the character of culture is intrinsically intriguing, it is its transmission that is structurally pivotal to our understanding of the human condition, and which thereby becomes not only a central problem of anthropology, but also central to the task of interconnecting different kinds of theory. As a process it concerns the mechanisms by


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which knowledge and practices acquired in previous life-cycles are learned, re-learned, negotiated, re-negotiated, modified, and reinterpreted to allow individuals to function socially and ecologically in shifting contexts and successive generations. Our major concern as anthropologists is to explain how objects, practices, ideas, patterns of interaction, and relationships continue to be transmitted sufficiently accurately to allow for the reproductive continuity, not of each unit of 'culture' or 'society', but of each locally or virtually delineated population. The question is ultimately a Darwinian one, but it requires different kinds of intermediate-level theorizing to answer it.

## Kinds and constellations of theory

Having at least opened up the question as to what it is that makes a theory anthropological, we can now begin to group theories in terms of the scale of presumption in their claims. I think it helps to distinguish three levels here, in terms of spatio-temporal scope and degree of abstraction. Firstly, there are those that claim to offer universal all-encompassing explanations of human behaviour. We conventionally call these 'grand' (or framework) theories, as they are concerned with final causes and emerge with some kind of assumption of exclusivity. They include structuralism, Marxism, and so on.

Secondly, at a lower level are those claims to explain co-variation in the properties of different kinds of social, cultural, or biocultural system, what we might call specific or middle-range theories. These are more concerned with immediate or proximate causes. This level of theorizing has always been that with which British social anthropologists have been most comfortable. The term 'middle-range theory' was first introduced by Merton (1968), though we might avoid confusion by pointing out that for Binford (1981), in the context of prehistory, 'middle-range theory' is that which attempts to explain by identifying 'diagnostic signatures' of living systems that can be used to interpret the archaeological record. Not entirely the same thing. However, one of the dangers of theories at this level – whether in archaeology or socio-cultural anthropology – is that they tend to become typological: that is, they assume a taxonomy of different socio-cultural types where those types necessarily exhibit an unrealistic degree of discontinuity at their boundaries. Typologies of different kinds of unilineal descent are a good illustration of this kind of thinking. Leach (1961), critical of this tendency towards 'butterfly collecting', tried to move the debate on with his topological models, but anthropologists are still struggling to develop a way of thinking about theory that distinguishes general frameworks, in which all other theories are embedded or to which they can relate, from specific theories proposed to explain particular relatively localized or thematic phenomena.

Thirdly, at the lowest level are theories that claim to shed light on particular cultural situations. Here the very specificity, density, interrelational complexity, and localness of the data usually exclude the application of tests using statistical methods, but enhance the value of qualitative approaches and Geertzian 'thick description'. Whether or not such qualitative approaches satisfy the conditions of science is here besides the point, though it is difficult to deny that they can contribute hugely to understanding, in both pure and applied terms. But theories at this level are not confined to multi-stranded interpretative accounts, and we might also include more positivistic causal analysis, as in Vayda's event ecology, the attempt to specify systematically the chain of local factors responsible for a particular occurrence of something.

These three levels, of course, overlap. For example, theories at levels 1 and 2 might claim to shed light on behaviour at level 3, while data generated using theory usually


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associated with level 3 can provide useful clues in developing theory at levels 1 and 2. Indeed some, as in Vayda's notion of progressive contextualism, provide systematic guidance as to how we might move from level 3 to more general claims. But there is another way in which we might cut up the theoretical cake, and that is in terms of whether the theories are reductionist, whether they deliberatively address issues of systematicity and complexity, or whether they are concerned with long-term diachrony rather than short-term synchrony. Such cross-cutting categories are neither mutually exclusive nor exhaustive.

*Simple correlative theories*

Reductionist theories are usually expressed in the form of correlative hypotheses involving a few variables, but these may be expressed at radically different levels of generality; in other words they cut across our first three distinctions. They are reductionist in the sense that they literally seek to *reduce* the number of variables in order to find the smallest number that can explain a particular set of data. At one end of the scale are correlations of the kind: growth in the neocortex is linked to an increase in the complexity of social life (measured using one carefully selected variable). At the other end are attempts of the kind presented by Vayda to provide specific causes for specific events: for example, how we might systematically account for the causal relationship between those factors leading to a particular outbreak of fire at the oil palm plantation/rainforest interface in north Sumatra.

*Theories of emergent systems*

The second constellation or cluster comprises theories of complexity and causal probability, which seek to explain how systems of three or more variables generate 'emergent' properties, and which display properties (including processes and cycles of socio-cultural reproduction) that are more than just the sum of their individual parts. Systems theory and its anthropological imitations obtained a bad name in the 1960s and 1970s because they seemed so detached from individuals and resembled in some instances the kind of vague holism or social physics that Geertz and Vayda equally disapproved of. But attempts to develop theories to explain complex systems in the physical and biological world have provided both legitimacy and stimulus to applying more refined approaches to socio-cultural and socio-ecological systems, including developments in network theory, mathematical treatments of chaos (Mosko & Damon 2005), catastrophe theory (Renfrew 1978), and work on complex adaptive systems (Eriksen 2005: chap. 3). There are, too, strategies to move from theories of complexity to simple correlative theories: for example, path analyses that evaluate the likelihood of different causal relations in a system of four or more variables (e.g. Goody 1973).

*Theories of socio-cultural change and transmission*

Finally there are those theories that stem from the conviction that explanations of cultural transmission must hold a special place in the articulation of theory. Anthropology is a historical science, but the theories appropriate to understanding how culture is embedded in the mind, or how it is innovated or transmitted from one individual to another, are not necessarily the same as those that help us understand the dynamics of what happens once patterns are established and how they might change through space-time, external to the body. Long-term transmission of knowledge, as Boyd and Richerson (1985) have pointed out, is generally better adapted to collective needs than to individual calculations.


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Individual calculations, by contrast, may be best with respect to short-term changes. For example, while seed selection in a particular year might reflect individual farmer calculations, over time repeated patterns of use tend to favour optimal solutions – such as diversity of varieties across a range of features.

Studies of diffusion were for a long time deeply unfashionable in anthropology outside of archaeology, but there is now a new kind of culture-historical diffusionism, which draws on natural history models of contingency of the kind favoured by Stephen Jay Gould and Jared Diamond, and which have built into them models of cultural selection, co-evolution, cultural phylogenetics, biocultural diversity and 'the epidemiology of ideas' (Sperber 1996). Such models try to explain what happens when aggregated transmitted cultural knowledge and practice and the potential for enculturation meet ecological constraints (such as different biodiversity profiles, or constraining land-masses). At the same time, such a theory must take into account the close relational (as opposed to ecological or genetic) proximity of most people in the world at any one time – since it is this that is the minimum condition for transmission to take place at all (Watts 1999).

## Evolution as framework theory

We can salvage bits from all grand theories, and in this sense they all have an attenuated and fragmented after-life. For example, binary contrast – such a central feature of structuralism – re-emerges as a standard feature of Eleanor Rosch's cognitive economy (1977), while conversational analysis has survived ultra-exclusive ethnomethodology, and convenient aspects of psycho-analysis have been extracted from a more integrated and generally discredited Freudian theory. However, the only theory that has been sufficiently robust to resist all challenges as an overarching framework is some or other version of Darwinism relying on selection for reproductive fitness. This is not because a Darwinian explanation is always the best to explain the data, only that as we increase the scope of our explanatory claims, so all other explanations can be nested within an evolutionary framework. Or put differently, all theory must be ultimately understood within the framework of explanations for human origins. If we accept this, then we cannot avoid looking at theories of human culture and sociality except in the broad context of other primate behaviours, including (and especially) those relating to reproductive strategies, in their widest sense, even if we are members of the Association of Social Anthropologists. Anthropology has been ill served by those who offer a knee-jerk response to the value of Darwinian approaches, or who ignore them altogether. Without conceding all of the arguments of cultural phylogenetics, evolutionary psychology, memetics, or evolutionary ecology, it is difficult not to conclude that contemporary expressions of cultural form and content are modifications of innovations that have occurred at different times over the last 100,000 years and which have diffused and been selectively retained and elaborated. Indeed, what is additionally remarkable about humans is the way in which they have the capacity to lose and repeatedly re-invent socio-cultural forms, and invent in parallel similar cultural entities and social arrangements without there necessarily being evidence of continuity in these traits. Repeated re-invention of similar types of socio-cultural complexity has led some to suggest the existence of what we might describe as 'a pre-adapted core of potential'. With this potential, time, and the right conditions, all can be re-invented given our basic biological make-up within a few generations (Tiger & Fox 1971: 14). From a human evolutionary point of view, whatever this potential might be, it had evolved by the time


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modern humans had spread out of Africa, or alternatively by the time any subsequent pan-human genome had been homogenized sufficiently through assortive mating to permit a common cultural potential.

So, what should we make of the Darwinian tendency in the context of socio-cultural anthropology? In my working lifetime, this began with the human ethology of the 1960s (typified by the early work of Robin Fox), followed by the sociobiology of the 1970s (associated in particular with E.O. Wilson), and, more recently, evolutionary ecology. Although the early post-war human ethology consensus was innocent of the new theories of behavioural genetics, it was these latter that were to give such interpretations their generalizing rigour and were, together with later developments, to connect the various historical incarnations of Darwinian social science that emerged during the second half of the twentieth century. One of the problems with these theories was always that despite the implication of 'genetic determinism', there was a gap in our understanding of the linkage between genes and complex human (particularly cultural) behaviours, a classic 'black box' to which social anthropologists could always point accusatively. With the promulgation by Richard Dawkins of the meme idea and the application of Darwinian theory to cultural variation and selection without the need of proposing linkage between gene and meme, these ideas acquired a new twist, which we find reflected in the evolutionary psychology, memetics, and cultural phylogenetics of the last decade or so. It is in this context, too, that we can see the emergence of the new cognitive anthropology.

Understandably, social anthropologists tend to see the inroads of the biological paradigm pretty much as a one-way street, with Darwinian approaches seeking to become an all-embracing and exclusive worldview, what E.O. Wilson (1999) has called 'consilience'. But it is certainly not entirely one-way intellectual traffic. For during the very same period to which I have been referring, the seemingly irreversible trend to molecular and genetic reductionism has been countered by a new, anti-essentialist emphasis (regarding the complexity of human socio-ecological systems) on the deconstruction of concepts of nature, and on historical ecology, co-evolutionary models, context-agency models similar to Giddens's notion of structuration, the notion of biocultural diversity, and the mathematics of chaos and catastrophe and of 'complex adaptive systems' (see e.g. Ellen 2006*a*). At the same time, Maurice Edelman and others have been demonstrating at the neuronal level the enculturation of the mind (e.g. Ellen 2006*b*), while Tim Ingold (e.g. 2000) – in tune with trends in developmental systems theory – has reworked a theory of the reproductive dynamics of social relationships which, while mindful of the influence of biological maturation and bodily engagement, eschews simple models of genetic reductionism. This is the context in which evolutionary processes take place, and although contextual systems generate the variation and selection from which evolutionary trajectories are derived, and are in turn altered in part by them, their scientific understanding cannot be limited to an understanding of such processes.

## Integrating theory

The ways I have suggested we might divide up the theoretical cake inevitably force us to go beyond the limitations of theory as it used to be understood by social anthropologists of the old school (e.g. Gluckman 1964). There can be little doubt that we urgently need to find ways in which evolutionary theories based on individual behaviour geared to immediate survival and reproduction can be reconciled with theories that best explain system dynamics, and that address how individual actions are in turn


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constrained by the systems of which they are part, and which at the same time provide the context of other individual actions. In this regard I take my inspiration from a body of writing that includes, indicatively, the early Leach (1961), Goody's (1966) developmental cycle, Giddens (1986) on structuration, and the insights of historical ecology. We need theory that can simultaneously accommodate both strong empirical causation and the uncertainties of 'emergent systems'.

Two recent examples, both from 2007, directly address the issue of integration, but they do so in very different ways. The first is an exchange between Alex Mesoudi, Andrew Whiten, and Kevin Laland (2007) and Tim Ingold (2007*a*), in *Anthropology Today*, following a paper by Mesoudi and his co-authors (2006) in *Behavioural and Brain Sciences*. This turned out to be a model confrontation between, on the one hand, what we might describe as the forces of 'consilience' and evolutionary psychology on a mission to colonize the social sciences and, on the other, those vigorously defending the subject matter of social anthropology and its methodologies. While it had the virtue of forcing some singular theoretical assumptions and prejudices into the open, it was inconclusive and ultimately unhelpful. The same might be said for the account of the present state of our subject in a 2008 issue of *The Higher* (Fearn 2008). By contrast, the second example, Daniel Nettle's 2007 Royal Anthropological Institute Curl Lecture (Nettle 2009), is a considered attempt to bridge the gap that the other protagonists sought to widen. To some degree it is also a view that reflects my own.

Nettle, rather than attempting to swallow up all explanations of socio-cultural patterns into a Darwinian bulldozer, opts – in effect – for a pyramid of nested theories and levels of explanation (Fig. 1) that evokes some of the distinctions in kinds of theory that I have already mentioned. On the right hand side of the diagram, I have used the names 'Darwin', 'Radcliffe-Brown', and 'Geertz' as grand exemplars reflecting their totemic role in certain kinds of anthropological discourse. I do not deny that there might well be arguments about their general appropriateness, and other theorists may have served the purpose equally well. Here they are merely mnemonics.

Despite the different claims made for these levels of theorizing, all are based on certain universal assumptions about what we crudely call 'human nature': for example, in the case of Geertz, say, assumptions about the universality of discourse construction, or what might a century ago have been called 'the psychological unity of mankind'. I

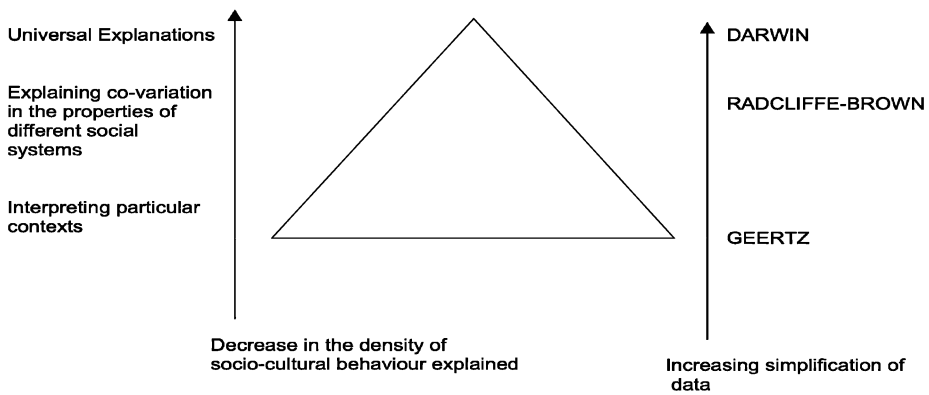



**Figure 1.** The nested properties of anthropological theory.


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agree with Nettle that explanations at these three levels are *not* alternatives (in the sense in which I was using the Necker cube illustration above), but do have to be consistent over the longer term or else the systems they are claimed to perpetuate would collapse.

A theory at the top of the pyramid is not a better theory, only a simpler one that claims to explain a wider range of data, but with much less to say about any particular case. As we move from the base to the tip of the pyramid, our explanations and interpretation of our data must become simpler to accommodate the forms of measurement that each level demands, and indeed they explain less and less of the diversity of human socio-cultural behaviour. There is, if you like, an explanatory deficit, and they explain fewer and fewer contingent variables, each of which need to be 'normalized' out for the theory to work. The mathematics of population genetics are indeed elegant and powerful, but they require levels of data simplification that tend to drive out other features that are no less worthy of our attention as anthropologists and, indeed, may themselves contribute to Darwinian fitness despite being intractable to measurement.

As someone trained in social anthropology, I am painfully aware of the shortcomings of superficial short-term fieldwork trawling for the kind of data that keeps evolutionary biologists happy, and I understand those critics who assert that despite the theoretical posturing and the quantification, such approaches often seem to have little to offer other than confirming what we already know. I am aware of the virtues of social anthropology as an approach using middle-range theory, and of methodologies that seek to contextualize and interpret rather than simply quantitatively measure, and that occupy what Herzfeld happily calls 'the militant middle ground' (2001: x). I am also aware how, by dividing up and modularizing data to achieve explanations at increasingly higher levels of application, we suppress the network and systemic features of the data we begin with, and sometimes fail to appreciate the artificiality of the boundaries between those units and the patterns established by connections across them that cannot be reduced in the same way. And although some anthropologists and evolutionary biologists are trying to address the same questions, most biologists are just not interested in the variety of perfectly legitimate intellectual (and in every sense scientific) questions that anthropologists seek to ask of their data. If you wanted to find out something about a particular population with a view to making a practical intervention, you would I suspect in the first place prefer the general authority of an experienced ethnographer, rather than the specific authority of a scientific account looking at a narrow hypothesis. If I want to understand the dynamics of community conflict, say, in the Moluccas, in order to resolve a problem, it would not be sensible to secure the services of an evolutionary biologist, because whatever the ultimate causes might be it is the proximate causes that need to be addressed.

And then there is 'culture'. Biologists and anthropologists (and anthropologists amongst themselves) may honestly disagree about how we define culture, though it would – I think – be useful to adopt a twin-track approach. By this I mean to treat culture simultaneously not only as non-genetic information transmitted between biological individuals and its material manifestations, but also as some complex Geertzian network of language-mediated symbols. Moreover, in the study of its movement through social relationships, 'culture' is no more or less real than atomic particles or the waves of quantum mechanics, and – along with the explanation of cultural diversity – lies at the core of what is distinctive about the anthropological enterprise and its theory. Everything in the end is subservient to the need to transmit non-genetic information over time, and to its development into increasingly complex emergent systems of social


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relations, networks of biological – indeed biocultural – interaction from which we can read varying Darwinian valencies.

Some of my colleagues in social anthropology still react with hostility to such approaches, rejecting the notion that it might have any relevance to their own intellectual project. These are fatal reactions, and it is important that we respond in an informed and positive way to enable us not merely to provide methodologically screened cultural data for evolutionary analyses but also actively to contribute new rules of engagement. Indeed, we might think that the really interesting questions in anthropology are those that do not readily yield to a plausible evolutionary explanation. So, while I am not suggesting that we are on the verge of a brave 'new synthesis' where consilience will rule all, and accept that it is neither possible nor desirable to attempt to integrate high-level theory to the point where it explains nothing at all (Hodder 2003), I do detect a wind of change that is compelling us to be less inward-looking and at the same time to challenge the forces of consilience. The Darwinian paradigm is a meta-theory of enormous power, such that anything human has a potentially measurable signature of selectivity and adaptivity, but at the same time we inhabit systems whose properties can never be reduced to them.

## A plethora of holisms

In conclusion, I wish to return to the notion of holism implicit in the very idea of anthropological theory (Parkin & Ulijaszek 2007), and to consider how different anthropological theories intersect. Of course, there is no single holism, but rather we are offered 'a surfeit of lampreys'. There is the holism achieved through privileging systems over causal reductionism, the holism of the ethnographic, Tylorian holism, biocultural holisms of the historical ecology kind, nomothetic universalism over idiographic relativism (the kind Marvin Harris [1969] enjoined us to favour), the holism in which a material object is seen as a focus for integrated approaches, the holism acknowledging that the components of human society are mutually constitutive, and the theoretical holism achieved through different kinds of Darwinian model. But in this case we might ask, with David Parkin, whether holism therefore becomes any view 'that embraces an undivided view of humanity' (2007: 3). Parkin seems to think not, while Ingold prefers a processual holism of 'open-ended comprehensiveness' to what he disparagingly calls 'totalization'. Certainly, the aim of any holistic anthropology 'should not be to bolt together components of being, such as mind, body and culture' or consist of vacuous 'exhortations for inter- or multi-disciplinarity' (Ingold 2007*b*: 209). Whatever holism we espouse, the argument seems to be that it should be methodologically defensible, say bridging different specialisms to solve a shared problem, or countering the kind of anthropological eclecticism and ever-circulating particularism caricatured by some evolutionary psychologists. In fact, Parkin appears to be extolling the virtues of a kind of 'polythetic holism' in which there are different horses for different courses.

Now, the Royal Anthropological Institute has attempted to foster an inclusive anthropology for well over a century, and there has indeed been intermittent intellectual engagement between the fields under its auspices. Although presidential addresses have seldom explicitly considered matters of theory, it is occasionally instructive to do so. Among those who have, I single out W.H.R. Rivers and Daryll Forde. In 1922 Rivers gave an address on 'The unity of anthropology'. Sadly this may be remembered more for its endorsement of the Egyptocentric version of diffusionism than for anything else, but it is worth noting that it contained some recognizable advocacy for what we would


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now call 'ethnographic holism' before Malinowski had claimed it for functionalism (Rivers 1922: 12), a statement on the necessary connections between archaeology and ethnology (meaning here the ethnography of contemporary and historic populations), between psychology and anthropology, between linguistics (here philology), ethnology, and archaeology, and between physical archaeology and ethnology, mainly from a historical point of view. Perhaps more instructively, in 1948, Daryll Forde gave an address on 'The integration of anthropological studies'. In this Forde bemoaned the demise of the unified anthropology that Rivers had called for twenty-six years previously, which was then becoming ever more disunited and sectarian. At that time, Forde – although he makes some very cogent arguments against fragmentation – was fighting a losing battle. By the beginning of the twenty-first century we can observe that the intellectual and institutional landscape has shifted entirely, and in some respects in directions that Rivers and Forde would have approved.

While there have always been those who have striven for an integrated anthropology, who have tried to build bridges, and who have accepted that our project must be simultaneously biological, social, and cultural, as well as concerned with both historical and contemporary human populations, developments over the last two decades have given these aspirations some substantive basis. One major outcome is a realization that the subdivisions of the new anthropology are as much determined by the kind of theory we use as by the empirical characteristics of the phenomena we study. Theoretical differences are not a necessary impediment to collaboration and not necessarily an issue at all. There are a wide range of issues where social anthropologists work with other human scientists and must have some understanding of their thinking if collaboration is to work. There is no reason why social anthropologists and palaeoanthropologists and linguists should not collaborate to understand the condition of early human sociality – indeed there is clearly plenty of evidence for this (e.g. Dunbar, Knight & Power 1999; James, Allen, Callan & Dunbar 2008). But, by the same token, evolutionary biologists, social anthropologists, and cultural theorists can all contribute to understanding contemporary teenage motherhood without feeling that one approach must be reduced to the other, and should be able to share their data constructively. It is to be hoped that all might have something to say collectively to the world of policy-makers as well.

Having biological and socio-cultural anthropologists inhabiting the same research space, perhaps also with linguists and archaeologists, serves to keep us constantly on our intellectual toes, forcing us to translate between different scientific languages, preventing us from being sloppy and complacent, and continually self-referential. My reason for being an anthropologist is precisely because it is not just another 'social science'. Indeed, it has always acquired its vitality (and, I would argue, its distinctive identity) by being critically 'conjunctural'. Despite the periodic predictions of its imminent demise, what has sustained my commitment to anthropology, in its broadest sense, is the observation that if it did not exist then someone would have to invent it. In other words, it provides a framework within which to pose generalizing and comparative questions about human diversity and origins, whether biological, social, cultural, or (indeed) biocultural or biosocial. There is no danger of anthropology disappearing because there will always be some kind of theoretical interface to address. Its pretensions to rise above the gaps of misunderstanding between subjects without falling into narrow disciplinary chauvinisms and conceits are – it seems to me – wholly necessary if we are properly to address the ultimate anthropological questions, and we can ultimately do this only through a strategic theoretical compromise.


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## NOTES

This is a revised version of the Royal Anthropological Institute Presidential address given at the ASA conference on 'Anthropological and Archaeological Imaginations', at the University of Bristol on 9 April 2009. It has benefited from the supportive criticism of anonymous reviewers and an editor who do not necessarily agree with everything I say. I am grateful to Daniel Nettle for permitting me to use unpublished material from his Curl Lecture, 'Nature versus culture, or how is human behaviour to be explained?', delivered at the British Museum on 26 September 2007. Part of the text that appears here was also presented at a meeting in Paris in November 2007 and subsequently appeared in *Ethnologie Française* (Ellen 2008). I am grateful to Martine Segalen, Andrés Barrera-González, and Gisèle Borie for permission to use this material.

[1] I have avoided a review of the literature in philosophy of science and epistemology on what constitutes a 'scientific' theory. This would have taken the argument in a different direction, and the issues are well covered elsewhere. I suppose my common-sense starting-point is that most people who consider themselves to be anthropologists have notions of 'science' and 'theory' that are sufficiently shared to develop the arguments reviewed here. On the whole, most practitioners of science or of anthropology do not feel it necessary continually to refer back to the great foundational debates in the philosophy of science and epistemology. Some do, and do it well, but in other cases where anthropologists get involved in such debates, they either do it badly, or their philosophical reflections stand in the way of effective analysis.

## REFERENCES

Binford, L.R. 1981. *Bones: ancient men and modern myths*. New York: Academic Press.

Boyd, R. & P.J. Richerson 1985. *Culture and the evolutionary process*. Chicago: University Press.

Carrithers, M. 1992. *Why humans have cultures: explaining anthropology and social diversity*. Oxford: University Press.

Davis, P.J. & R. Hersh 1983. *The mathematical experience*. Harmondsworth: Penguin.

Dunbar, R., C. Knight & C. Power (eds) 1999. *The evolution of culture*. Edinburgh: University Press.

Ellen, R.F. 1984. Introduction. In *Ethnographic research: a guide to general conduct* (ed.) R.F. Ellen, 1-12 (Association of Social Anthropologists Research Methods Series **1**). London: Academic Press.

——— 2006*a*. Introduction. In *Ethnobiology and the science of humankind*, Special Issue of *Journal of the Royal Anthropological Institute* (N.S.), S1-S22.

——— 2006*b*. Introduction: categories, classification and cognitive anthropology. In *The categorical impulse: essays on the anthropology of classifying behaviour*, 1-30. Oxford: Berghahn.

——— 2008. Anthropology and its subdivisions in relation to contemporary human science. *Ethnologie Française* **116**, 591-5.

Eriksen, T.H. 2005. *Engaging anthropology: the case for a public presence*. London: Berg.

Fearn, H. 2008. The great divide. *Times Higher Education*, 20 November (available on-line: *http://www.timeshighereducation.co.uk/story.asp?storycode=404341*, accessed 19 February 2010).

Forde, D. 1948. The integration of anthropological studies. *Journal of the Royal Anthropological Institute* **78**, 1-10.

Friedman, J. 1974. Marxism, structuralism and vulgar materialism. *Man* (N.S.) **9**, 444-69.

Giddens, A. 1986. *The constitution of society: outline of the theory of structuration*. Cambridge: Polity.

——— 1996. The future of anthropology. In *In defence of sociology: essays, interpretations and rejoinders* (ed.) A. Giddens, 121-6. Cambridge: Polity.

Gluckman, M. (ed.) 1964. *Closed systems and open minds: the limits of naïvety in social anthropology*. Chicago: Aldine.

Goody, J. (ed.) 1966. *The developmental cycle in domestic groups* (Cambridge Papers in Social Anthropology **1**). Cambridge: University Press.

——— 1973. Correlation and causal inference: a case study. In *The explanation of culture change: models in prehistory* (ed.) C. Renfrew, 711-16. London: Duckworth.

Harris, M. 1969. *The rise of anthropological theory: a history of theories of culture*. London: Routledge & Kegan Paul.

Herzfeld, M. 2001. *Anthropology: theoretical practice in culture and society*. Oxford: Blackwell.

Hilgard, E.R., R.L. Atkinson & R.C. Atkinson 1979. *Introduction to psychology*. New York: Harcourt Brace Jovanovich.

Hodder, I. 1999. *The archaeological process: an introduction*. Oxford: Blackwell.

——— 2003. Archaeology as a discontinuous domain. In *Essential tensions in archaeological method and theory* (eds) T.L. VanPool & C.S. VanPool, 5-9. Salt Lake City: University of Utah Press.

Holy, L. 1987. *Comparative anthropology*. Oxford: Blackwell.


© Royal Anthropological Institute 2010

Holzner, B. 1964. Methodology. In *A dictionary of the social sciences* (eds) J. Gould & W.L. Kolb, 425-6. London: Tavistock.
Ingold, T. 1992. Editorial. *Man* (N.S.) **27**, 693-6.
——— (ed.) 1996. *Key debates in anthropology*. London: Routledge.
——— 2000. *The perception of the environment: essays in livelihood, dwelling and skill*. London: Routledge.
——— 2007*a*. The trouble with 'evolutionary biology'. *Anthropology Today* **23**, 13-17.
——— 2007*b*. Movement, knowledge and description. In *Holistic anthropology: emergence and convergence* (eds) D. Parkin & S. Ulijaszek, 194-211. New York: Berghahn.
——— 2008. Anthropology is not ethnography. *Proceedings of the British Academy* **154**, 69-92.
James, W., N. Allen, H. Callan & R. Dunbar (eds) 2008. *Early human kinship: from sex to social reproduction*. Oxford: Wiley-Blackwell.
Kapferer, B. 2007. Anthropology and the dialectic of Enlightenment: a discourse on the definition and ideals of a threatened discipline. *The Australian Journal of Anthropology* **18**, 72-94.
Kuper, A. 1988. *The invention of primitive society: transformations of an illusion*. London: Routledge.
——— 1999. *Culture: the anthropologists' account*. Cambridge, Mass.: Harvard University Press.
Layton, R. 1997. *An introduction to theory in anthropology*. Cambridge: University Press.
Leach, E.R. 1961. *Rethinking anthropology*. London: Athlone.
——— 1968. *A runaway world? The Reith Lectures 1967*. London: British Broadcasting Corporation.
Lowie, R. 1920. *Primitive society*. New York: Bonni & Liveright.
Mair, L. 1969. Review of Marvin Harris, 'The rise of anthropological theory'. *Man* (N.S.) **4**, 144-5.
Merton, R. 1968. *Social theory and social structure*. New York: Free Press.
Mesoudi, A., A. Whiten & K. Laland 2006. Towards a unified science of cultural evolution. *Behavioural and Brain Sciences* **29**, 329-93.
——— 2007. Science, evolution and cultural anthropology. *Anthropology Today* **23**, 18.
Moore, H.L. 1999. Anthropological theory at the turn of the century. In *Anthropological theory today* (ed.) H.L. Moore, 1-23. Cambridge: Polity.
——— 2004. Global anxieties: concept-metaphors and pre-theoretical commitments in anthropology. *Anthropological Theory* **4**, 71-88.
Mosko, M.S. & F. Damon 2005. *On the order of chaos: social anthropology and the science of chaos*. Oxford: Berghahn.
Murdock, G.P. 1967. *Ethnographic atlas*. Pittsburgh: University Press.
Nettle, D. 2009. Beyond nature versus culture: cultural variation as an evolved characteristic. *Journal of the Royal Anthropological Institute* (N.S.) **15**, 223-40.
Ortner, S. 1984. Theory in anthropology since the sixties. *Comparative Studies in Society and History* **26**, 126-66.
Parkin, D. 2007. Introduction: emergence and convergence. In *Holistic anthropology: emergence and convergence* (eds) D. Parkin & S. Ulijaszek, 1-20. Oxford: Berghahn.
——— & S. Ulijaszek (eds) 2007. *Holistic anthropology: emergence and convergence*. Oxford: Berghahn.
Renfrew, C. 1978. The anatomy of innovation. In *Social organisation and settlement* (eds) D. Green, C. Haselgrove & M. Spriggs, 89-117. BAR International Series (Supplementary) **47** (i).
Reyna, S.P. 2001. Theory counts: (discounting) discourse to the contrary by adopting a confrontational stance. *Anthropological Theory* **1**, 9-29.
——— 2002. *Connections: brain, mind and culture in a social anthropology*. London: Routledge.
Rivers, W.H.R. 1922. The unity of anthropology. *The Journal of the Royal Anthropological Institute* **52**, 12-25.
Rosch, E. 1977. Human categorization. In *Studies in cross-cultural psychology*, vol. 1 (ed.) N. Warren, 1-49. London: Academic Press.
Sahlins, M. 2000. Sentimental pessimism and ethnographic experience; or why culture is not a disappearing 'object'. In *Biographies of scientific objects* (ed.) L. Daston, 158-202. Chicago: University Press.
Samuel, G. 1990. *Mind, body and culture: anthropology and the biological interface*. Cambridge: University Press.
Sperber, D. 1996. *Explaining culture: a naturalistic approach*. Oxford: Blackwell.
Stocking, G. 1968. *Race, culture and evolution: essays in the history of anthropology*. New York: Free Press.
Tambiah, S.J. 2002. *Edmund Leach: an anthropological life*. Cambridge: University Press.
Tiger, L. & R. Fox 1971. *The imperial animal*. London: Secker & Warburg.
Trigger, B.C. 2006 [1996]. *A history of archaeological thought*. Cambridge: University Press.
Urry, J. 2006. The ethnographicisation of Anglo-American anthropology: causes and consequences. *SITES* **3**, 3-38.


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Varela, F.J., E. Thompson & E. Rosch 1993. *The embodied mind: cognitive science and human experience*. London: MIT Press.

Vayda, A.P. 2007. Causal explanation as a research goal: a pragmatic view. In *Against the grain: the Vayda tradition in human ecology and ecological anthropology* (eds) B. Walters, B.J. McCay, P. West & S. Lees, 317-67. Lanham, Md.: AltaMira.

Watts, D. 1999. *Small worlds: the dynamics of networks between order and randomness*. Princeton: University Press.

Wilson, E.O. 1999. *Consilience: the unity of knowledge*. London: Abacus.

## Théories en anthropologie et « théorie anthropologique »

*Résumé*

Qu'est-ce qui rend une théorie « anthropologique », en dehors du fait que les anthropologues l'utilisent ? En posant une acception aussi large que possible de l'anthropologie, l'article invite à se rappeler à quoi servent en réalité les théories. En distinguant les théories par le niveau de conjecture de leurs affirmations, l'auteur propose une pyramide de niveaux d'explication imbriqués. En progressant de la base au sommet de la pyramide, les attentes et l'interprétation des données doivent devenir de plus en plus simples, afin de prendre en compte les formes de mesure exigées à chaque niveau. Sur la base de ce modèle, comment les théories évolutionnistes, basées sur des comportements individuels visant la survie et la reproduction immédiates, peuvent-elles être conciliées avec celles qui expliquent, au mieux, les incertitudes des « systèmes émergents » ou qui examinent la façon dont les actions individuelles sont contraintes par les systèmes dans lesquels elles s'inscrivent ? L'auteur conclut que l'anthropologie a toujours acquis sa vitalité par une approche « conjoncturelle » critique et qu'elle doit être en fin de compte, par nécessité, un compromis théorique stratégique transdisciplinaire.

Roy Ellen is Professor of Anthropology and Human Ecology at the University of Kent and Director of its Centre for Biocultural Diversity. His recent books include *On the edge of the Banda zone* (University of Hawai'i Press, 2003) and *The categorical impulse* (Berghahn, 2006). He was elected a Fellow of the British Academy in 2003.

*School of Anthropology and Conservation, University of Kent, Marlowe Building, Canterbury CT2 7NR, UK.*
*r.f.ellen@kent.ac.uk*


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