In contrast to *babalawos’* characteristic confidence about their ability to pronounce truth, anthropologists in recent years have tended to view such questions with a mixture of anxiety and boredom. The story of how we got to this point of truth-weariness in anthropology is often told and sometimes lamented (e.g. Gellner 1985, Spiro 1987, Wilson 2004, cf. Myhre 2006). My purpose in retelling it here is not so much to comment on the rights and wrongs of current attitudes to truth, but rather to articulate the concept of truth that they presuppose. My central claim in this respect is that while in the relatively short history of the discipline attitudes to truth have shifted dramatically, the notion of truth with which they have been concerned has remained the same, namely the familiar idea that truth is a matter of representing the world as it actually is. Pivotal to longstanding debates about anthropology’s status as a social science, this commonsense understanding of truth has been cast within the discipline in terms of the distinction between society and culture on the one hand and nature on the other. Since, from an anthropological point of view, people’s representations pertain to culture and society, while the world as it actually is pertains to nature, truth is a function of this foundational anthropological distinction.

In what follows, my first task is to show that the changing attitudes towards truth in anthropology – from confidence through scepticism to apathy – can be understood with reference to shifting conceptions of the relationship between
culture/society and nature, including the notion of ‘human nature’, which, along with ‘culture’ and ‘society’, has played such an important role in attempts to demarcate the intellectual remit of the discipline. This, however, raises the question of what happens to the concept of truth when the distinction between culture/society and nature is rescinded altogether, as it has been in recent strands of anthropological thinking. Can the concept of truth withstand such a negation of its major premise or does it fall with it? In the next section I shall argue that this question poses a major challenge to the discipline of anthropology at its current juncture – the challenge that lies at the heart of this book’s agenda. In particular, I shall show that while the move away from taking the culture/society versus nature distinction as the analytical starting-point is anthropologically imperative, its implications for the role of truth in anthropological reasoning remain unexplored.

I begin by examining three broad stages in the conceptualisation of the relationship between culture/society and nature. Borrowing from Maurice Bloch’s idiosyncratic periodization in a recent overview of the discipline’s history (2005), I use the terms ‘evolutionism’ and ‘diffusionism’ to label the first two stages, and add ‘constructivism’ to label the third one. Of course much has been written about the tenets of each of these currents and about the specific manner in which each defined the anthropological project. My review of this familiar ground is not intended to clarify my own theoretical position in a general way. My purpose is to show what each of these theoretical positions entails for the conceptualization of truth in anthropology. Thus, I present the story of truth in anthropology in three instalments, each corresponding to a particular period in the history of the discipline. This three-stage trajectory, I suggest, can be seen as a progressive weakening of the link between
the concepts of culture and society and that of nature, and particularly human nature. 

While evolutionist anthropology posited a substantive relationship between socio-cultural phenomena and human nature, diffusionist approaches turned this relationship into a purely formal one, and thus opened the way for the constructivist turn, which severed the relationship altogether. Inasmuch as truth in anthropology has been deemed to depend on just this relationship, the stock of truth has fallen as the relationship has weakened.

**Evolutionism, ‘diffusionism’, constructivism: the fall of truth**

At its inception as a professional discipline in the 19th century, anthropology was supremely confident about its ability to make truth-claims about human beings. Working within the parameters of the broader field of natural history that was so invigorated by Darwin’s theory of evolution, anthropologists embarked on elucidating the basic features of human nature by comparing its manifestations in different parts of the world. Alongside the study of human beings’ physical and ecological features, research on their varied social and cultural characteristics was considered a major part of this enterprise. That socio-cultural variation could be taken as an index of an underlying human nature was guaranteed by the evolutionist premise which deemed that different forms of social organisation and cultural expression were natural inasmuch as they corresponded to different stages in human beings’ development as a species. Indeed, establishing an isomorphic relationship between stages of natural evolution and socio-cultural ‘advancement’ was a central concern, involving, for example, attempts to correlate differences in biometric measurements with distinct social or cultural characteristics.

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1 Bloch treats social constructivism as a variant of diffusionism (2005: 7), which in a sense, as we shall
This naturalism enabled 19th century evolutionary anthropology to think of itself as a scientific enterprise capable of delivering truths about human phenomena: its methods and procedures (the use of empirical evidence, experimental methods, explicit rules of inference and so on) placed it on par with other sciences that had been established as professional academic fields since the Enlightenment. Like them, anthropology could represent the natural world correctly, as it is, which was to say that anthropology could arrive at the truth.

In order to understand the particular character of evolutionary anthropology’s claim to truth, however, it is important to note its peculiarly ‘reflexive’ quality. The term ‘reflexivity’ here should not be confused with ‘reflexivism’, which we associate with much more recent developments in the discipline (although, as we shall see, the two are indeed connected). Rather, evolutionary anthropology was reflexive in that, unlike other natural sciences, its own activities themselves had to be understood, in a broad and basic sense, as forming part of the field of phenomena it set out to investigate. Anthropology, after all, was a human phenomenon in its own right: it was an example of a highly distinctive socio-cultural manifestation that was characteristic of the ‘advanced’ societies in which anthropologists lived and worked. So if anthropology was the study of human phenomena in general, not least the study of socio-cultural variation, then it would, in some sense, have to include itself in its own remit of study.

As we shall see, this irreducibly self-referential quality of anthropology had far-reaching consequences for the development of the discipline, ultimately feeding into the kind of post-positivist scepticism about truth that has become so familiar in recent decades. But here we may note that, without necessarily undermining them, the
requirement of reflexivity at minimum placed an added constraint on anthropologists’ claims to truth. Like other scientists, anthropologists could claim truth for their propositions (hypotheses, theories, refutations, etc.) only if they represented the facts of nature, and particularly the facts of human nature, as they are. But unlike other natural scientists, anthropologists had also to be sure that whatever claims they made about (human) nature were coherent with the one fact of which they had to be certain, namely the human ability to represent nature as it is – in other words, the human capacity for truth which they themselves, and modern science in general, exemplified. Anthropological accounts of human nature that failed to account for their own possibility, in this sense, were at least incomplete and at worst self-contradictory. Hence, the justification of anthropological truth-claims could not only be a matter of methodological procedure. It also had to be one of substantive anthropological argument. For truth to be the form of anthropological inquiry, if you like, it had also to be part of its content. This meant that the conceptual machinery of truth – the very distinction between the natural world and human beings’ representations of it, notions of evidence, of reason, and so on – doubled up on itself reflexively, becoming as much an object of anthropological scrutiny as it was a matter of methodological deliberation.

Evolutionary anthropology amply met the requirements of reflexive justification. It did this by putting questions of truth and falsehood, and particularly different peoples’ propensity towards them, at the heart of the story it told about human beings’ natural evolution through different stages of socio-cultural development. According to this story, all human beings, from the most primitive to the most advanced, had the capacity to represent the world around them as it is, i.e.
they all had the capacity for truth. However, alongside other obvious markers (such as degrees of technological sophistication, social complexity, and moral refinement), one of the things that made human beings naturally different from each other was that they had developed this capacity to different degrees. At the earliest stages of evolution humans were hostage to intellectual confusions of various kinds. Manifest in such pervasive ‘superstitions’ as animism and magic, these confusions included a difficulty with abstract thought, a tendency to misattribute causation (e.g. ascribing agency to natural forces), as well as a proclivity towards category mistakes of various sorts (e.g. treating things as people, confusing signs with their referents, and so on). As humans moved to more advanced stages of evolution these confusions were lifted step by step. So, the more advanced a society was, the more it was able to represent the world transparently, as it actually is. The apogee of this evolutionary trajectory from false superstition towards reasoned truth was modern-day science, understood as the systematic attempt by humans to offer true accounts of the world around them. Hence the reflexive justification: anthropologists could tell the truth about human nature because they themselves exemplified a stage of human evolution that was characterised, precisely, by the ability to tell the truth. Human beings, in other words, could tell the story of their own evolution by virtue of it, so anthropological truth-claims were, if you like, self-justified.

Except that this version of anthropological reflexivity was tautological. The idea that truth comes naturally to anthropologists by virtue of their advanced state of evolution as civilized men was circular, since the difference between civilization and the evolutionary stages that were supposed to lead up to it was already defined in terms of, among other factors, the propensity to truth. To be primitive was (partly) to

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2 Proponents of the so-called ‘psychic unity of mankind’ thesis, such as Edward Burnett Tylor, would
be prone to error, and to be civilized was (partly) to be inclined to truth. So attempts to periodize social and cultural differences into evolutionary stages served not to reveal the underlying natural differences that cause them, but rather to ‘naturalise’, as we would say today, social and cultural differences which anthropologists, for social and cultural reasons of their own, deemed significant. The natural superiority of modern civilization, by virtue of which evolutionary anthropologists could lay claim to it reflexively, was, to recall Bronislaw Malinowski’s famous criticism, a ‘conjecture’ that said lots about the Victorian anthropologists who made it and little about the facts of human nature (Malinowski 1926: 132).

The failure of evolutionary anthropologists to clinch their argument about natural stages of socio-cultural evolution by empirical means left the way open to alternative ways of accounting for the variety of peoples’ social and cultural arrangements, which professional anthropology was now beginning to record with the help of increasingly systematic methods. Evolutionists’ main critics in the 19th century, the so-called ‘diffusionists’, argued that, rather than being mapped on to a putative natural sequence of evolution, social and cultural variation should be seen as the result of patterns of diffusion, in which particular social and cultural traits travelled differentially across the world depending on contingent circumstances (demographic flows, ecological and adaptive factors, particular historical circumstances and so forth). Here I follow Bloch in using the term diffusionism in a broader sense (2005: 6-7), to refer to the key idea that underlay the 19th century arguments about global patterns of diffusion, namely that the varied ways in which people live in different parts of the world are the result, not of natural necessity, but of historically contingent processes of socio-cultural transmission. Taking this as a core
premise, the forefathers of social and cultural anthropology as we know it today –
emblematically Boas in America, Malinowski and Radcliffe-Brown in Britain, and
Durkheim, Mauss and their circle in France – effectively shifted the intellectual object
of anthropology. By decoupling socio-cultural phenomena from any correlation with
natural evolutionary stages, they instituted society and culture as *sui generis* fields to
be understood in terms of their own.

From the point of view of anthropological truth and its justification, this may
seem like a major shift, since it appears to compromise the naturalist premise upon
which the confidence of anthropology as a science of human nature rested. If the
social and cultural variation in which anthropologists were so interested could not be
explained as an expression of human beings’ underlying nature, then in what sense
could its study even be scientific? At the very least, understanding such phenomena in
their own terms would imply anthropological methods whose ability to deliver truth
would rest on more than the naturalist claim to be providing accurate representations
of human nature. Furthermore, such a move away from naturalism also undermined
the evolutionists’ reflexive justification for anthropological truth-claims, ruling out of
court any notion that anthropologists’ privileged access to truth was natural: seen as
itself an example of socio-cultural variation, the propensity to truth or error became a
matter of social and cultural contingency rather than natural necessity. It followed
that, as far as this propensity was concerned, anthropologists and their ‘natives’ were
on the same boat, in principle capable of truth or error in equal measure.

From today’s standpoint, it may seem as though this flattening move, which
appears to obliterates the distance between anthropologist and native at the level of
first principle, would open the floodgates for the kind of relativism we now associate

1920[1871]).
with social constructivism and post-modern self-doubt. And certainly, in its 20th century versions, diffusionism involved foundational debates about anthropology’s scientific credentials, with some arguing that the distinctively human subject-matter of the discipline (which entailed accounting for human values, subjective feelings, opinions and so on) gave anthropological claims to truth a character that was vastly different from those of the natural sciences (e.g. Geertz 1973b).³

None of this, however, led to the kind of self-doubt that ‘science warriors’ of more recent years have come to associate with socio-cultural contingency. For, crucially, these methodological debates about scientific credentials were not about whether socio-cultural anthropology could continue to be deemed a science, but about how. The diffusionist move did not shake anthropologists’ confidence in their ability to offer true representations of the world, nor did it dent their assumption that in this respect they were better than the people they studied. Taking their claim to truth as given, what was at issue methodologically for anthropologists as they moved into the 20th century was how best to articulate the particular character of truth-claims about socio-cultural phenomena and how to provide epistemological justifications for them that would be as robust as those of natural science. Even American cultural anthropologists, with all their insistence on ‘cultural relativism’, saw no reason to give up the idea that the study of human culture was a scientific enterprise through and through (e.g. Boas 1940, Kroeber 1952). Understood as the idea that since particular cultural formations have to be interpreted in their own terms they do not lend themselves to universal generalisations, cultural relativism was proposed by Boas and

³ This tendency was perhaps strongest in America, where, with the foundational influence of Franz Boas, the nascent sub-field of ‘cultural anthropology’ was rooted partly in the Teutonic humanism of the geisteswissenschaften and the broader Herderian critique of the Enlightenment from which it stemmed. But Europeans too, drawing particularly from Durkheim a strong Rousseauian line on the distinctive characteristics of the ‘social’, were sensitive to basic breakdowns in the analogy between anthropological modes of inquiry and natural scientific ones.
his students as a point of scientific methodology – a proposal about the principles that would have to inform a properly exact study of human cultures.

Key to understanding why the move from evolutionism to diffusionism did not inhibit anthropologists’ confidence as scientists is the role that the notion of human nature continued to play in their reasoning, albeit in a sparser guise. For while diffusionism hollowed out the concept of human nature from its explanatory power when it came to accounting for the diversity of human phenomena, it did not deprive the concept from its key heuristic role, namely that of singling out human phenomena as a distinct field of study. In a sense it strengthened it. Social and cultural diversity was no longer to be mapped on to an underlying axis of natural diversity that rendered differences between people analogous to differences between species. Rather, it was now taken as the index of our underlying unity as a species, which qualitatively marks us off from the rest of nature. What we all share by nature is the capacity to be socially and culturally different from each other – our unique nature, so to speak, is to be cultural.

Retaining naturalism in this purely formal sense, and enfolding within it the substantive concern with socio-cultural diversity as a *sui generis* phenomenon, kept in place the analogy between anthropology and the natural scientific approaches from which it was otherwise distinguished. Both the object of anthropology and its methods of study differentiated it from other sciences. Nevertheless, treating social and cultural orders as a part of nature meant that they could still be studied in the same sense, if not necessarily in the same way, as other natural phenomena. They could be described, as any other aspect of the world could, and analysed, so as to form the basis for propositions that were as robust as those of the other sciences, subject as
they were to similar standards of evaluation (corroboration or refutation by empirical
evidence, logical cogency and coherence, relevance to existing knowledge, etc.). In
this way the notion of human nature continued to act as the ultimate guarantor for
anthropological aspirations to truth.

This formal naturalism also allowed diffusionism to meet the requirement for
a reflexive justification of anthropological truth (i.e. the idea that whatever claims
anthropologists might make about their own ability to arrive at the truth should be
compatible with the stories they tell about others’). Again, it did so by retaining the
foil of the evolutionists’ argument while discarding its substance. To be sure, the idea
that peoples’ propensity to truth or error is socially and culturally contingent nullified
any notion that anthropologists’ claim to truth is naturally superior. However, it left
intact the basic premise that motivated the evolutionists’ tautologies about natural
superiority, namely the seemingly self-evident fact that, *qua* scientific,
anthropologists’ claims to truth were indeed superior to those of the people they
studied. That this should still seem self-evident was a direct corollary of the role that
naturalism continued to play in the definition of anthropology as a science. According
to this image, people of all societies make sense of the natural world around them,
including themselves as a part of that world, by means of their own cultural
repertoires and according to their own social arrangements. However, only some
societies have devoted themselves to developing systematic methods and techniques
that may ensure that their representations of the world are actually correct. Science in
modern Western societies is exactly that: a sophisticated socio-cultural attempt to
represent nature (or, in the case of anthropology, society and culture as a part of
nature) as it actually is, or, to use the word, truly. Thus the very terms in which
anthropological claims to truth were defined, namely as the ability to represent
(human) nature as it is, pertained directly (and reflexively) to the substance of the diffusionist argument about the uniformity of human nature and the diversity of societies and cultures.

In this way the propensity to truth or error continued reflexively to mark out the difference between anthropologists and natives respectively. What had changed was that the evolutionist tautology of postulating these propensities as natural was now replaced by the more tractably empirical task of explaining them with reference to social and cultural factors. While this task hardly exhausted anthropologists’ preoccupations during this period, it was at the core of their endeavours throughout the discipline’s ‘short 20th century’ – its golden age as a confident producer of knowledge about the social and cultural world, which lasted roughly until the 1970s. In fact, from the point of view of the requirement for reflexive justification, the story of the discipline during this period can be told as a series of competing attempts to account for the (always self-evident) fact that the people anthropologists studied should so often and in such varied ways get the world around them wrong (myths, rituals, magic, and all the other odd ‘beliefs’ that went with them). Were these erroneous beliefs and practices perhaps useful to them in some way (functionalism)? Did they somehow help preserve the social structure of the group (structural-functionalism)? Did they express core values of its culture (cultural interpretivism) – its gestalt personality perhaps (culture and personality school)? Were they metaphors for basic social concerns (symbolism) or examples of people’s false consciousness of them (Marxism)? Or were they in some deep sense expressions of how the human mind works in general (structuralism)?

For a long time, the self evidence of their claims to truth as scientists prevented anthropologists from turning such questions onto themselves. Thus
anthropologists failed to treat their own ostensive ability to represent the world truly as a phenomenon requiring explanation with reference to the particular features of modern Western society and culture. Nevertheless, as I shall argue more closely a little later, the reflexive bent of anthropologists’ abiding concern with explaining native errors was always present by implication. Here I note only that this concern was invariably framed in terms of a series of ‘master’ contrasts and narratives that mapped directly onto the reflexive distinction between native error and anthropological truth: myth versus history, ritual versus technology, magic and religion versus science, or, the most encompassing, tradition versus modernity.

Staple to all forms of anthropological argumentation, these kinds of questions and distinctions received their most explicitly ‘reflexive’ treatment in the so-called ‘rationality debate’, which raged among anthropologists in the 1960s and ‘70s, with some philosophers weighing in as well (e.g. Horton 1967, Wilson 1974, Hollis & Lukes 1982, cf. Lukes 2000). At issue in this debate was not only why ‘indigenous people’ (as they were now often called) might get things wrong in the way that they do, but also how these errors, and the ‘modes of thought’ that sustain them (Horton & Finnegnan 1973), could be compared to the standards of reason and truth to which anthropologists themselves, as scientists, subscribed. Were indigenous peoples’ ‘apparently irrational beliefs’, as they were branded (e.g. Sperber 1985), simply the result of a misapplication of the universal standards of reason that science exemplifies or did they display a logic of their own? If they did have their own logic, then how did this compare to the logic that informs scientific reason? And, crucially in terms of the reflexive justification of anthropology as a science, how might the latter be used to account for the former, thereby proving its superior claim to truth in the pudding?
Still, by the time these systematic comparisons between the inner workings of ‘indigenous’ and ‘Western’ (scientific) forms of reasoning were being debated, the confident assumption that the latter were superior to the former no longer went free of challenge. Within the rationality debate itself, some argued that if indigenous beliefs instantiate a logic that is coherent unto itself, with principles of reasoning that were different to those of Western science, then to judge those beliefs by the standards of Western science was inappropriate (e.g. Lévi-Strauss 1966, Winch 1967). For if anthropological analysis of indigenous systems of belief effectively reveals that there was not just one standard of reason but many (many ‘rationalities’), then to continue to uphold one of them as the standard for evaluating the rest is contradictory. Anthropology should give up the hubris of seeking to uphold the superiority of scientific claims to truth by showing how they could account for others’ errors, and rather recognise that if others’ beliefs appear erroneous to us that is because they are being held up against standards of truth that are alien (and therefore inappropriate) to them. Instead of being a cross-cultural Rottweiler for science, then, anthropology should adopt the standpoint of the people it studies in order to counter science’s claims to universal validity, by showing that multiple and mutually incommensurate claims to truth are possible.

The ascendancy of such arguments for epistemological relativism, which began to be made across the board in anthropology roughly from the late 1960s onwards, mark the period I propose to call ‘social constructivist’ (‘cultural constructivist’ would also do). Under an assortment of influences, including phenomenology, hermeneutics and Weberian sociology, structuralism, post-structuralism and semiotics, Wittgenstein’s philosophy of language, as well as

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4 The latter suggestion had been tabled controversially much earlier in the 20th century by the French
critiques of natural scientific method itself (e.g. Kuhn 1962, Feyerabend 1975, Rorty 1979), the very idea of scientific truth began to be considered either naive or dogmatic. Specifically anthropological variants of such arguments gained pre-eminence within the discipline partly for contingent reasons that had to do particularly with the end of colonialism. Self-conscious about the role of anthropological knowledge in colonial projects, and working in an atmosphere of anti-authoritarian radicalism in intellectual circles (civil rights, student activism, opposition to Cold Warfare and so on), anthropologists on both sides of the Atlantic began to see political emancipation in the idea that Western scientific enterprises such as their own had no privileged claim to the authority of truth. Where previous generations had balked at treating anthropology on a par with the socio-cultural phenomena it had purported to study scientifically (other than in the purely formal sense implied by diffusionism), in the postcolonial era anthropologists saw this as a move towards the kind of cross-cultural egalitarianism that could be embraced. To view anthropologists’ claims to authoritative truth as social constructions was to place them on par with indigenous truth claims. If no truth is any less socially constructed than any other, then all societies’ claims to truth are, at least in principle, equally valid.

However, for purposes of the present argument it is important to emphasize that the conditions for such scepticism were already present in the diffusionist idea that what human beings have in common by nature is the capacity to be socio-culturally different from one another. To be sure, this suggestion improved on the evolutionist tautology — that of first defining socio-cultural differences as natural ones and then explaining them as such. Still, the suggestion is itself a paradox. Much like familiar anxiety-inducing paradoxes such as Socrates’ knowing that he knows

philosopher and ethnologist Lucien Lévy-Bruhl, and had been elaborated further by the British social
nothing, the culture-is-natural credo of diffusionism entails its own negation.

Consider, after all, the twofold implication diffusionists drew from it: on the one hand all claims to truth are socially and culturally contingent and, on the other, some of them are better than others because they reflect nature as it actually is. While validly drawn, the two implications are mutually inconsistent. If evaluating truth-claims involves testing them against nature, then there must after all be some truth-claims that are not socio-culturally contingent, namely a set of statements that show how nature really is. Without such a set of statements, how could we ever decide which truth-claims actually get nature right? Conversely, holding on to the idea that all truth-claims are socio-culturally contingent, implies giving up the notion that they can nevertheless be evaluated with reference to nature as an objective benchmark. If all truth-claims are socio-culturally contingent then any statement about what nature is like (objectively, really, actually) must also be so, and therefore must itself require further evaluation, and so on to infinity, without ever getting to the putative rock-bottom benchmark of nature as it is. Hence the paradox: when either of the two entailments of diffusionism is true the other one is false.

The move from diffusionism to social constructivism, then, can be understood with reference to this paradoxical dilemma. Diffusionist anthropology had chosen to uphold its claim to scientific truth by holding dogmatically onto the idea of a nature that stands outside the realms of society and culture. Social constructivists took the alternative option. Taking the diffusionist credo to its ultimate conclusion (if culture is natural, so to speak, then nature is cultural), they subjected nature under the realms of society and culture and thus relinquished anthropology’s claim to scientific truths that could only be guaranteed by nature’s objective reality. Which of course obliterated anthropologist E.E. Evans-Pritchard, as we shall see in the next chapter.
any attempt at ‘reflexive’ justification: the idea that each claim to truth is in its own way partial (in both senses of the word) went hand in hand with reducing to the absurd the notion of nature as a neutral benchmark for truth, and regarding all claims to truth as socially and culturally ‘situated’, as it was now often said.

In fact, one of the most significant effects of social constructivists’ break with naturalism was to turn the requirement for reflexive justification squarely on its head. For evolutionists and diffusionists alike the point of whatever reflexive story they told about anthropology’s claims to truth could only be to bear out their obvious superiority. With this superiority now denied, the role of reflexive argument became the opposite, namely to place anthropologists and the people they studied on an equal footing by showing how their truth-claims were equally situated and partial. At the level of substantive ethnographic inquiry, this opened the floodgates for the study of all aspects of so-called ‘Western’ societies, including that of science itself. To this day, however, this anthropological ‘symmetry’, as Bruno Latour calls it (1993), has not been extended to proper ethnographic studies of anthropologists themselves – their daily life as professional academics, their conduct during fieldwork, its correlation to their claims to expertise, and so on.⁵ Rather, much as with evolutionism and diffusionism, social constructivists’ reflexive attention to their own practices as anthropologists has been limited to the level of epistemological and methodological discussion.

In particular, it has taken the form of an injunction to ‘reflexivism’. I use the ending ‘-ism’ here to highlight the ideological tenor of this rendition of anthropological reflexivity. A form of epistemological double vision, reflexivism seeks to incorporate into anthropological practice the idea that, as social
constructions, the truth-claims anthropologists’ make about the societies they study are necessarily also a function of their own claim to authority as anthropologists. Hence, rather than taking (or offering) those truth-claims at face value, which would be to collude with the structures of authority that underpin them, anthropologists must make sure to situate such claims with reference to the conditions of their production – the power differentials of ‘West’ versus ‘rest’, issues of class, race, gender and so on, and even, at the psychoanalytic level, the personal propensities of the researcher him or herself. While binding anthropologists to what the philosopher of science Isabelle Stengers calls the stance of ‘irony’ (Stengers 2000), this effort to ‘deconstruct’ anthropological knowledge brought with it a flurry of creative experimentation with the form of anthropological writing, as anthropologists strove to find ways to subvert their own authority so as to shift power to the people about whom they wrote (‘giving voice’, ‘speaking truth to power’, and so on).

By the time I came of age in anthropology, at the turn of the 21st century in Britain, it had become fashionable to dismiss some of the grosser excesses of social constructivism – the potentially debilitating infinite regress of always having to deconstruct any attempt at positive knowledge; the danger of nihilism that lurked in giving up the level-headed intuition that some things are just truer than others, pure and simple; the sheer self-indulgence of making it a methodological imperative to talk about oneself before talking about others (“enough about you, now can we talk about me?” said the native to the reflexive anthropologist). All too often, it was felt, the reflexivist recipe for double vision (one eye outward, the other inward) produced a blur, in which sight of the original remit of anthropology – the exciting prospect of making sense of socio-cultural phenomena the world over – was lost.

5 As Steven Sangren has pointed out, the shame of not having got round to conducting such a study has
Perhaps unfair in its refusal to acknowledge the more anthropologically productive effects of bringing the self in to the fray of research in this way (e.g. Rosaldo 1989, Wikan 1990), the dissatisfaction with constructivist anxieties about truth led in some quarters to a call to arms in defence of science and ‘common sense’. Indeed, if the most public controversies within the discipline in the 1980s and ‘90s are anything to go by, the most prevalent rejoinder to social constructivism has been to insist on the time-honoured idea that anthropology can aspire to scientific truth for as long as it holds fast to the basic facts of ‘human nature’, which, as it is often asserted, must obviously act as a constraint upon the socio-cultural variability that so dazzles the constructivists (e.g. Freeman 1983 contra Mead 1961[1928]; Obeyesekere 1992 contra Sahlins 1985).

The atavistic character of this kind of level-headedness has in recent years become particularly evident in the field of cognitive anthropology, whose proponents are now probably the most vociferous critics of social constructivism (e.g. Sperber 1985, 1996, D’Andrade 1995, Bloch 2005). Effectively mirroring the constructivist aversion to the idea of human nature, cognitive anthropologists argue that the real chimera is culture, at least insofar as it is taken as a domain of phenomena that is somehow separate from or beyond nature. Yes, human beings have a peculiarly sophisticated capacity to represent the world around them, including the capacity to represent other representations (and in fact other species do have analogous, albeit in important respects more rudimentary, capacities). But this capacity is itself natural, not just in the formal sense admitted by earlier generations of anthropologists as we have seen, but rather in a fully substantive and scientifically tractable way: cultural representations are a function of human mental capacities, and these, as rapid

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so far been deemed lesser than the embarrassment doing so would no doubt cause (Sangren 2007).
advances in cognitive science now purport to show, are just the result of natural processes (neuronal and so on) that take place in the human brain. So to treat cultural representations as a domain unto itself and then worry whether or not nature as it is in itself can be accessed through them is unduly to mystify matters, since cultural representations are themselves as natural as trees. On a naturalist premise, truth pertains not to an ontological mismatch between culture and nature, but rather to scientifically tractable epistemic matches between some parts of nature (brain-instantiated representations) and others (the world, the people in it, their brain-instantiated representations, etc.). Not only can anthropology aspire to arrive at such truths in its own representations of socio-cultural phenomena, but it can also set up questions regarding human beings’ capacity for truth in this sense as a prime object of anthropological investigation (e.g. Boyer 1990).

Nevertheless, such returns to naturalistic defences of scientific truth in anthropology have been a minority pursuit. Indeed if truth was something of a battle-cry in the more polarised public controversies that culminated in the so-called ‘science wars’ of the 1990s (see Herrnstein-Smith 2005) – at that time, either you believed in truth or you didn’t –, the present attitude to such questions within the discipline seems to be weariness. This has partly to do with the fact that, as far as mainstream, bread-and-butter anthropological research is concerned, over the past couple of decades the sceptical message of constructivism has been largely absorbed. While by no means all anthropologists these days would pledge explicit allegiance to constructivism as a coherent theoretical doctrine, and many profess their awareness of its pitfalls, its basic tenets of relativism and reflexivism have arguably become professional reflexes within the discipline. In that sense, barring the cognitivists, we are all working constructivists.
Hence, for example, the strong tendency in recent years to refrain from comparative theoretical generalisations and to favour instead accounts of particular ethnographic instances for their own sake – ‘assemblages of anecdotes about this and that’, as Bloch laments with a degree of hyperbole (2005: 9). Hence also the now dominant idea that if theory is to be sought anywhere, that should be not at the level of supra-cultural propositions conceived in the analyst’s unavoidably essentialist imagination, but rather at the level of concrete and historically contingent infra-cultural diffusions – the writ-large relativity of the globalized political economy of modernity, trans-nationalism, empire and so on – of which the literature on Afro-American religion is, as we saw, one expression. And hence, finally, the marked tendency in recent years to supplant questions regarding the truth-value of particular anthropological arguments with concerns about their ethical credentials and political consequences – the move from objective models to moral ones, as Roy D’Andrade has put it (1995).

If the tendency to take constructivist tenets for granted in recent years has often been more a matter of instinct than argument, this is also because the familiar arguments from first principles in which the science and culture wars of the 1980s and ‘90s so often exhausted themselves were seen to lead to sterile position-taking. No amount of constructivist irony could ever persuade a naturalist (or, more broadly, a ‘realist’) to give up on his point-blank-obvious intuition that there’s a world out there that both constrains and acts as a benchmark for the truth-claims we may make about it. And no amount of realist level-headedness could ever persuade a constructivist to give up her always-cleverer insight that since the world is given to us only in the ways we represent it, it cannot be conceived independently of the truth-claims we make about it. To the extent that anthropology’s disciplinary niche as an explorer of social
and cultural variety involves an inclination towards the latter position, anthropologists’ broad tendency at present is to practise constructivism without, as it were, preaching it too much – or not, at least, if this involves engaging in, by now, apparently futile clashes at the level of first principles (Culture is natural! No, nature is cultural!).

Dissatisfaction with these ostrich tactics – the more or less conscious choice to ignore a debate that remains unresolved within the discipline – is one of the motivations for this book. Its aim, however, is decidedly not to resume the debate in the terms in which it has so far been conducted and, as it now seems, exhausted. On the contrary, the book adopts as a starting point that putting the question of truth back on the anthropological agenda requires a basic shift of the terms in which we discuss it. In particular, we need to depart from the central assumption that has remained constant throughout the trajectory of anthropologists’ debates about truth, namely that the notion of truth itself is to be understood, broadly, in terms of the relationship between representations and the world they represent, or, in the more specifically anthropological terms set forth above, the relationship between culture and nature. Put in a nutshell, the story I have told about anthropologists’ changing attitudes to truth over the past century and a half is about the gradual exhaustion of the epistemological implications of just this assumption – starting from anthropologists’ initial confidence that the truth of their cultural representations of human nature was guaranteed by nature itself, and moving step by step to the ironic realisation that nature cannot provide such guarantees since it must itself be a cultural construct. So the collective yawn with which the question of truth now tends to be greeted in anthropology will be fully justified for as long as the concept of truth is left suspended between the poles of nature and culture and the antinomous attractions that they exert.
Of course neither the injunction to break with so-called representationist\textsuperscript{6} assumptions about what truth is, nor the proscription of the so-called binary opposition of nature versus culture, will sound particularly original in the present intellectual climate. On the former count, it should be remembered that a whole swathe of 20\textsuperscript{th} century philosophy has been about presenting alternatives to representationist accounts of truth – American pragmatism, late Wittgenstein, and just about all of what Anglo-Saxons call ‘Continental’ philosophy may serve as examples. Indeed, one response to the epistemological impasses of anthropology in recent decades has been to turn to such philosophies for inspiration, sometimes mining them (though rarely explicitly) for alternative visions of anthropological truth (e.g. Jackson 1989, Moore 2005, Mhyre 2006, Pina-Cabral 2009, cf. Morris 1997). As for the distinction between nature and culture, denying its universal validity, and lumping it contemptuously together with a whole canon of offending ‘binaries’ (mind/body, West/rest, male/female and so on), has been de rigueur in much anthropological teaching and writing for twenty years or more.

Nevertheless, part of my argument in this book is that, run alongside each other, these two platitudes add up to something interesting. In particular, I seek to show that pursuing the anthropological platitude about the cultural variability of distinctions between nature and culture to its ultimate consequences, as has been done in recent writings that are anything but platitudinous, raises the prospect of formulating alternatives to the representationist account of truth in a manner that is distinctly anthropological. So, having framed the question of truth in terms of the quintessentially anthropological distinction between nature and culture, the strategy now is to pursue an ‘internal’ anthropological critique of that distinction in order to

\textsuperscript{6}I borrow the term ‘representationism’ from Richard Rorty (1991), though its currency is in any case
reframe the question of truth without deferring to received (and of course only in that sense platitudinous) philosophical wisddoms – a point on which I shall comment further in due course.

Towards a recursive analysis of truth

The nigh on catholic renouncement of the distinction between nature and culture in contemporary anthropology as an odious binary indicates the sheer weight of influences that have made this attitude plausible. It is almost as though all major theoretical developments in late 20th century social theory – from phenomenology, semiotics and systems theory to post-structuralism, practice theory, actor network theory, and even some variants of cognitive theory⁷ – had conspired to converge on a single point: Descartes was wrong, if not evil, and all vestiges of his, and any, dualism must be purged. As with all vilification, however, the problem is that the en masse adoption of this message has all too often been at the expense of its content – gaudily cutting down ‘Cartesianisms’ like weeds while leaving their roots untouched in the soil. Perhaps the most blatant sign of this in anthropology is that the repudiation of the distinction between nature and culture is typically done in the name, precisely, of cultural relativism. So, the very concepts of nature and culture as we understand them are themselves cultural constructions – a mark of our Cartesian mindset – and therefore should not be projected ethnocentrically onto others who may not classify the world in these particular terms. Once stated like this, the oddity of this view, which verges on self-contradiction, becomes obvious. It is only by relying on the distinction between nature and culture – precisely the form of ‘classifying the world’ we are supposed not to project onto others – that we are able (indeed bound) to

wide.
repudiate the ‘ethnocentrism’ of the distinction itself. We should not universalise the
distinction between nature and culture, we say, because other cultures do not make it,
and thus re-inscribe its universality in the very act of denying it.

The strand of nature/culture debate I want to pursue here can be characterised
as a concerted attempt to overcome this lazy paradox. The stakes in this line of
anthropological thinking should not be underestimated. Insofar as the relativizing
move of rendering the distinction between nature and culture culturally contingent is a
special case of the kind of constructivism that lies at the heart of the current apathy
towards questions of truth, resolving the paradoxes inherent in this move allows us a
way to think ourselves out of the *impasses* of constructivism itself. At stake, then, is a
position that goes beyond constructivism, not by striking a balance between it and the
naturalist/realist intuitions it has sought to tutor, but rather by radicalising it to such an
extent as to come out, as it were, on its other side. The question is this: How might the
‘non-universality’ of the distinction between nature and culture be conceptualised
*without* recourse to that very distinction? And, following on from that, what might the
implications of such a way of thinking be for the question of truth in anthropology,
which has so far been wedded to this distinction?

Although charting the varied sources of debate that have led up to this type of
question is too large a task to undertake here, it is worth noting that the critique of the
anthropological matrix of nature versus culture has arisen within the discipline as an
extension of anthropologists’ longstanding investment in what has come to be known
as ‘cultural critique’ – a mode of anthropological argument that stems from the
inclination to ‘relativize’ things by using ethnography to show how they could be
conceived differently. In an incisive discussion, Keith Hart shows that the task of

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7 See for example Hutchins 1995, and Pedersen 2007 for critical review.
relativizing assumptions which other disciplines, as well as the wider public, may take for granted has been a major part of anthropologists’ intellectual mission from the early years of the discipline. Cultural critique in this inclusive sense has cut across the discipline’s otherwise determining theoretical divides and national traditions (Hart 2009). Nevertheless, with its abiding concern with cultural relativity, American cultural anthropology has arguably provided the most fertile ground for the cultivation of this kind of stance, as shown, for example, by the record breaking sales of such books as Ruth Benedict’s *Patterns of Culture* (1934). The rise of reflexivism in the 1980s in the US, promoting a ‘repatriation’ of anthropology in which not only sundry aspects of modern Euro-American culture but also, crucially, anthropology itself become the object of anthropological reflection, can be seen as the ultimate logical consequence of a cultural critique that is played out from within the coordinates of cultural relativism (e.g. Marcus & Fischer 1986). Anthropology itself, including its *ur*-binary of nature versus culture, is thus relativized as one among myriad cultural constructs.

Remarkably, however, what often goes unmentioned when the story of late 20th century cultural anthropology gets told (e.g. Kuper 2000) is that the same tradition of cultural critique that bore us reflexivism in the 1980s, also delivered a turn of thinking that was both less inward-looking and, partly for that, more radical in its critical implications: instead of relativizing anthropology as a cultural construct, this tack of research effectively relativized culture (and its opposition to nature) as an anthropological one.

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8 For example, the positivist outlook of Durkheimian sociology did not stop Marcel Mauss from using Maori prestations as a vantage point from which to criticise modern markets any more than the relativist premise of Boasian culturalism dictated to Margaret Mead that she should use her fieldwork among adolescent girls in Samoa to show up the peculiarities of American parenting (Mauss 1990[1950], Mead 1961[1928]).
The turning point arguably was David Schneider’s *tout court* attempt to debunk the anthropological concept of kinship. To be sure, Schneider’s argument could be viewed as a straightforward example of relativist cultural critique US-style, which is how Schneider himself presented it. Following his original research in Micronesia on Yap kinship categories, Schneider ‘repatriates’ his project by examining the cultural construction of kinship among middle-class residents of Chicago (1968). There he finds that the ‘core symbols’ of American kinship, as he called it, are organised around a culturally elaborated distinction between what are deemed to be ‘facts of nature’ (particularly sexual intercourse and the blood-ties to progeny it is supposed to engender) and the cultural conventions that are deemed to bring them under control (particularly the codes of conduct that are enshrined in marital law, and the kin-relationships to which they give rise). To the extent that this distinction also informs the cross-cultural study of kinship by anthropologists (from Lewis Henry Morgan’s distinction between descriptive and classificatory kin terms to Claude Lévi-Strauss’s contention that kinship systems offer varying solutions to the universal problem of humans’ passage from nature to culture), the very concept of kinship is an ethnocentric projection of a Western cultural model (see Schneider 1984, cf. Leach 2003: 21-32).

What brings Schneider’s critique to the brink of something more than just an argument from cultural relativism, however, is his willingness to adumbrate the implications of the cultural contingency of the nature/culture distinction for the infrastructure of anthropological analysis itself, thus prefiguring the kinds of recursive arguments that came later, and for which he arguably opened the way. While his critique was a pioneering example of an anthropological strategy that has since become very familiar, namely that of weighing up the ethnocentric baggage of all
manner of categories that had previously been taken for granted as supra-cultural analytical tools (‘religion’, ‘ritual’, ‘politics’, ‘labour’, ‘property’ and so on). Schneider’s critique amounted to more than that by virtue of the fact that its target was a category as foundational to the discipline as kinship. As borne out by his own subsequent reflections on the matter (1995), Schneider was well aware that to undermine the natural basis for the study of kinship was to undermine willy nilly the most basic premise of the anthropological project of cross-cultural comparison itself. What made kinship different from religion, ritual, politics and so on, giving it its privileged position as the bedrock for cross-cultural comparisons, was that it was assumed to be the most basic point of contact between, precisely, the universal facts of human nature and their variable cultural elaborations by different social groups. So, for Schneider, to explode the study of kinship was to deal a blow on the foundational matrix through which anthropologists conceptualized similarity and difference itself. Of course, as Adam Kuper remarks in an acerbic review of Schneider’s contribution to American cultural anthropology, it is unsurprising that Schneider did not take his maverick move the whole hog so as to undermine the anthropological notion of culture alongside that of nature (Kuper 2000: 122-158). To do so would have nullified the premise of his critique, namely that kinship, and the distinction between nature and culture upon which it is based, is an American cultural construct. It would radicalise his relativism to such an extent that it would end up undoing itself.

This is exactly what Roy Wagner did in *The Invention of Culture* – a book dedicated to Schneider with an acknowledgement of the ‘germinal’ character of its debt to him (Wagner 1981: ix). Like Schneider, Wagner is concerned with the work that the distinction between culture and nature does for anthropology and, again like Schneider, he contrasts this distinction to ways of thinking and acting elsewhere so as
to show that it exemplifies distinctively Euro-American presuppositions. But what allows Wagner to escape the paradox of charting such contrasts in terms of cultural difference is that, unlike Schneider, he made the concept of culture itself the target of his critique. I dwell on the rudiments of his argument at some length, drawing not only on *The Invention of Culture* but also on its ethnographic prequel, *Habu: the Innovation of Meaning in Daribi Religion* (1972), where Wagner developed the core elements of his model with reference to his ethnography of ritual and cosmology among the Daribi people of the Highlands of Papua New Guinea.

In line with the discussion of the previous section – and in resonance with the word’s etymological root in the Latin *colere*, ‘to cultivate’ (Wagner 1981: 21) –, anthropologists imagine ‘culture’, in its broadest sense, as a set of conventions by which people order and make sense of themselves and the world around them (i.e., again in the broadest sense, nature). Since conventions established at different times and places by different groups of people vary, and the very idea of a convention implies a particular social setting in which it is abided, we speak of ‘cultures’ in the plural. The languages people speak, their social arrangements and political institutions, their means of subsistence, technological wherewithal, economic activities, ritual practices and religious beliefs, ways of seeing the world, perhaps even their ways of feeling in it – all these, we consider, are conventions that people establish and live by. So the job of the anthropologist is to describe the conventions of the people he studies, having learnt something of their inner workings as he himself has lived by them during fieldwork. And since these conventions are ultimately ‘artificial’ (1981: 49), in the sense that they are established in order to organise and make sense of a world that is prior to human action and is by this token ‘given’ or ‘innate’ (ibid), anthropologists are also charged with accounting for the conventions
they describe. They may interpret how and why the conventions in question make sense to the people who live by them, explain how and why they emerged as they did, and even draw conclusions about humanity in more general terms – in short, they may engage in the project of anthropological analysis and theorisation in its varied and competing guises.

Much as with Schneider on kinship, Wagner’s critical move is to relativize this set of assumptions by, on the one hand, showing their peculiarly Euro-American character and, on the other, using ethnography from elsewhere to explore alternatives. On the former count, he observes that the assumption that human activity is directed towards gaining a handle on the vagaries of an otherwise disorderly and unpredictable world, or ‘nature’, by establishing shared cultural conventions lies at the heart of the way we think of sundry aspects of life in Western societies. Schneider’s own depiction of the regulation of natural urges by the moral and legal sanction of marriage in American kinship is one example, but Wagner’s all-embracing account ranges over a vast cultural terrain, from ideas about artistic refinement and scientific classification to the artifices of advertising and the cultivation of personality. Crucially, to the extent that such varied domains of our culture (as we think of them, again, conventionally) are deemed to be different ways of rendering the world meaningful, they are also all underpinned by a particular way of thinking about meaning itself. Typified by the distinction between symbols and the things for which they stand, this ‘semiotic’, as Wagner calls it, is a corollary of the opposition between culture and nature. Meaning, according to this view, arises from human beings’ ability to represent the world by bringing to bear upon it sets of arbitrarily defined (and hence conventional) symbols. Thus cultural conventions order the world by deploying symbolic structures which organise it into distinct categories by means of their
otherwise arbitrary relationships to ‘signs’ – a take on meaning that is familiar to anthropologists from structuralist theory, and which endures in different ways, for example, in the otherwise divergent constructivist and cognitive approaches of contemporary anthropology (see also Holbraad 2007: 196). Indeed, it is just this view of meaning that is expressed in the basic idea that the job of the anthropologist must be to represent the culture he studies. Social structures, systems of exchange, modes of production, collective representations, indigenous beliefs, cultural logics, core symbols, local knowledge, cognitive schemata, logics of practice, world systems, transnational flows, political economies, multiple modernities, invented traditions – all these are ways of expressing the results of anthropologists’ efforts to depict and understand the conventions of the people they study by deploying and elaborating upon their own.

So this is the first sense in which culture is an ‘invention’, as the title of Wagner’s book would have it: supposing that the only way for people to have culture is to order the world by means of conventions, the anthropologist transfigures his experience of other people’s lives by using his own conventions to enunciate a set for them too. ⁹ But of course what the anthropologist finds when living with the people he studies are not structures, beliefs, symbols and so on, but just people living their lives and, when he is lucky, talking to him about them. And one possibility that emerges when one attends to how people actually live is that their activities might not, after all,
be directed towards establishing, abiding by, or elaborating conventions. Accordingly, the counterpart to Wagner’s argument about the sway that the notion of culture as convention holds in the West involves using his ethnography of the Daribi in Melanesia to show how culture could be conceived differently.

In *Habu*, named after a key Daribi curing ritual in which men impersonate ghosts, Wagner argues that the aspects of life the Daribi consider most salient (ritual, myth, exchange, magic, naming, and more) are directed, not towards controlling the world by subjecting it to collective conventions, but rather towards the opposite, namely using conventions as a base-line from which to engage in acts that are meant to transform them, by way of improvisation, into something novel and unique. So, from the Daribi point of view, all the things that the anthropologist imagines as ‘culture’ – ‘grammar, kin relationships, social order, norms, rules, etc.’ (1981: 87) – are not conventions for which people are responsible, but rather the taken-for-granted constituents of the universe that form the backdrop of human activity. They are ‘innate’, in Wagner’s terms, inasmuch as they belong to the order of what just is rather than that of what humans have to do. Conversely, the things that the anthropologist imagines as ‘nature’, including not only the unpredictable facts and forces of the world around us but also our own incidental uniqueness as individual persons, for the Daribi constitute the legitimate sphere of human artifice (see also Strathern 1980). Human beings, according to this image, do not stand apart from the world, bringing it under control with their conventions, but rather partake in the world’s inherent capacity to transform itself, by transgressing the conventional categories that the Daribi take for granted.

Appeal to the stability of convention (putative or otherwise) may in some cases have more to do with the analyst’s needs than with those of the people he studies.
So, for example, when in the habu ritual Daribi men impersonate ghosts that are held responsible for certain illnesses, they are not acting out a cultural convention – conforming to a cultural script, underpinned by indigenous categories (‘ghost’), beliefs (‘illnesses are caused by ghosts’) and so on. Rather, like a jazz musician may ‘bend’ a conventional scale to improvise a solo that sounds alive and unique (cf. Wagner 1981: 81), they subvert ‘innate’ distinctions, and in this case particularly the distinction between living humans and dead ghosts (Wagner 1972: 130-143), to bring about an effect that is powerful precisely because it recasts or, in Wagner’s term, ‘differentiates’ the categories they take for granted. Taking as the granted state of the world that dead ghosts are dead ghosts and living people are living people (the ‘collectivising’ categories of convention), in the habu men take on the characteristics of ghosts, temporarily enacting the startling possibility that dead ghosts can indeed come to life and interact with humans. In doing so, they artificially bring about a novel effect, namely ghosts that are also men, by temporarily transgressing ordinary distinctions between life and death, men and spirits, and so on. So, much as with jazz or ‘true’ acting in David Mamet’s sense (1997), the success of the habu relates to people’s capacity to render the predictable unpredictable, rather than the other way round (see also Holbraad 2010). However many times the habu may have been done in the past, its power depends on the degree to which the participants can make it ‘come off’ in the act as a fresh subversion of convention. In this sense – and contrary to anthropological arguments about ritual as a transfiguration of ‘structure’, ‘culture’ or ‘ideology’ (e.g. Geertz 1973a, Sahlins 1985, Bloch 1992, Rappaport 1999) – the

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10 Wagner’s technical term for these creative acts of transgression is ‘obviation’ and, following The Invention of Culture, he devoted his Lethal Speech (1978) to developing this concept with reference to Daribi mythology. As Wagner remarks (1981: xvi), taken together, Habu, The Invention of Culture and Lethal Speech form a trilogy that presents a sustained treatment of the ‘dialectic’ of convention and invention.
habu is an anti-convention *par excellence*, or, in Wagner’s word, an *invention* (see also Wagner 1984, Strathern 1990).

So the second sense in which culture is invented, for Wagner, presents itself in direct contrast to the first. Anthropologists invent a culture for the people they study in assuming that what makes them different (viz. an example of ‘cultural variation’) must be the particular way in which they organise their lives conventionally. By contrast, the Daribi are different in that their energies are focused, rather, on ‘differentiating’ their conventions so as to bring about singular moments of invention. Hence if our slot for ‘culture’ is the slot of what people ‘do’, and our slot for ‘nature’ is for that to which they do it, then in the case of the Daribi the slot for ‘culture’ is taken by the activity of invention, and that of ‘nature’ is taken by ‘innate’ conventions. In that sense Daribi culture *is* invention.

The question, then, arises: if what counts as culture in the case of the Daribi is in this way opposed to what counts as culture in anthropology, then what could an anthropological account of the Daribi look like? If, in other words, what makes the Daribi ‘different’ is their orientation towards invention, then how can an anthropology that brands all differences as divergences of conventions (‘cultural variation’) make sense of Daribi life without inventing for it a ‘culture’ that distorts it? Which goes to the crux of our original question: how can (in this case) the Daribi’s radical divergence from our distinction between nature and culture be articulated without falling into the Schneiderian trap of thinking of it as ‘cultural’?

Wagner’s solution to the conundrum relates to the third and final sense in which culture can be conceived as an invention for him. This is, effectively, an exhortation to move from the first sense in which culture has always been an invention for anthropologists (namely the idea that making sense of other people’s
lives must come down to formulating – and in that sense inventing – the conventions by which they live) to the second one (namely the Daribi-derived idea that human activities may be oriented towards subverting conventions in order to precipitate singularly novel effects). If the assumption that people like the Daribi must have a culture that consists of conventions gets in the way of making sense of the fact that culture in their case consists of processes of invention, then the onus is on the anthropologist to move away from his initial assumptions and conceive of new ones. Hence the ethnography of Daribi ‘invention’ must precipitate a process of invention on the part of the anthropologist. Departing from the conventional anthropological notion of culture as, precisely, convention, the anthropologist is called upon to transform the notion in a way that incorporates the possibility of invention as described for the Daribi – in other words, to invent the notion of ‘culture’ as invention.

For purposes of my broader argument about the relationship between Wagner’s discussion of invention and my own concern with truth, it is important to pause here to address a possible sticking-point regarding Wagner’s strategy. If the critical suggestion is that to make anthropological sense of Daribi invention one has to recast the very act of making sense anthropologically as an act of invention too, then one may wonder about this rather miraculous-looking coincidence. What is the significance, for Wagner’s argument, of the convergence between the content of what seems basically a parochial ethnographic argument (e.g. the invention of the Daribi’s habu ceremonies) and the presumably broader form of its anthropological analysis (i.e. the invention of culture as invention)? If the idea is that anthropological assumptions are to be revised in light of the ethnography that stumps them, then are we to conclude from this convergence of content and form that, for Wagner, this
revision is to be understood as a form of derivation? And would this imply that, had the Daribi diverged from anthropological assumptions about culture as convention in some different way (i.e. if what had stumped our assumptions was not their orientation towards invention but rather something else), we would then need (somehow) to think of the task of anthropological analysis along those lines, whatever they might be?

Wagner is not very clear on this point, and the pleasure he takes, as we shall see in a moment, in playing with the analogy between what people in Melanesia do and what anthropologists who study them could do probably contributes to the ambiguity. Nevertheless, I would suggest that at stake in his argument is not a straight ‘derivation’ of anthropological form from ethnographic content. What necessitates the move of inventing anthropological analysis as an act of invention is not the content of Daribi ethnography as such, but rather its alterity – i.e. the fact that its content (whatever this may be) contradicts the assumptions anthropologists who treat culture as convention bring to it. After all, while Wagner’s Daribi ethnography is sufficient to precipitate the argument for anthropology as invention, it is demonstrably not necessary. Any ethnography that contradicts the initial assumption that culture is a matter of convention – any ethnography, that is, that instantiates the challenge of ‘alterity’ in this sense – would place the onus of invention on the analyst. This is borne out by the fact that, as we shall see presently, a variety of anthropologists have advanced analyses that are as inventive as Wagner’s, without necessarily dealing with ethnographic depictions of the processes of invention as such (e.g., among others, Marilyn Strathern’s invention of ‘dividual’ persons precipitated by the ethnography of Melanesian exchange (1988, 1992) and Eduardo Viveiros de Castro’s invention or ‘multinaturalist’ ontologies precipitated by Amerindian cosmology (1998a, 1998b)).
Neither necessary nor miraculous, the convergence between ethnographic content and analytical form in Wagner’s argument, I take it, is a matter of strategy. By using the Daribi’s concern with invention as an example of the kind of analytical challenge presented by all ethnographic alterity as such, Wagner effectively redoubles the resources from which to draw in order to characterise that very challenge – which is to say, to invent it as the challenge of analytical invention. This, I would suggest, makes for the distinctively anthropological tenor of his invention – an important point to which I shall return later, when I come to characterise the strategy of my own argument on truth. Wagner could, after all, take the requirement for anthropological invention in the face of alterity as a purely methodological question, to be discussed, as is often the case with these things, in abstract quasi-philosophical terms. What he does instead is cast this discussion in the form of an anthropological comparison: the analogy between the Daribi’s concern with invention and the anthropologist’s need to adopt something analogous as an analytical tack allows Wagner to place the two alongside each other.

Set forth as the ‘epistemology’ of the ethnographic argument presented in Habu (Wagner 1981: xv), The Invention of Culture does just that. Putting in place, as it were, the conditions of possibility for invention on the part of the anthropologist, much of the argument of the book is devoted to showing that such processes of invention are as present in modern Western life as they are in Melanesia. The difference, for Wagner, is that whereas Melanesians take responsibility for their acts of invention while taking conventions for granted, for Westerners it is the other way round. Jazz and acting, which I have mentioned, are only particularly ‘marked’ examples of this – displaying not only the presence of invention in our lives, but also its relegation to an irreducibly ineffable realm that lies beyond human control (we
speak, significantly, of ‘natural talent’, ‘inspiration’, or even ‘genius’, by which we mean ‘something… je ne sais quoi’). In fact, argues Wagner, the possibility of invention is implicated in everything we do and say. Even the simplest declarative sentence, insofar as it is not just a trivial statement of what we already know conventionally (e.g. literal statements about our accepted categories, such as ‘men are mortal’, or definitions such as ‘bachelors are unmarried men’), involves an element of invention. For example, when I say, as I do now, that invention is part and parcel of all non-trivial communication, or if I suggest that Roy Wagner is a genius, what I say is only interesting – it ‘says something’ – insofar as it presents a *subversion* of what is already conventionally accepted. I am in effect asking you to conceive of communication and of Wagner differently from what (I assume) you already do. In this sense I am putting forth an invention in a way that is analogous to what Daribi men do when they impersonate ghosts (and compare this with such exemplarily boring claims as ‘communication involves mutual understanding’ or ‘Mozart was a genius’ – boring, precisely, because they ‘say nothing new’: they merely state conventions that are already established and accepted). The same, argues Wagner, holds for all meaningful action that is not merely trivial.

Just as with conventional accounts of rituals such as the *habu*, the problem, Wagner suggests, is that the irreducibly inventive dimension of meaning remains opaque as long as it is viewed through the prism of convention. To equate the meaningful with the inventive is itself an example of invention insofar as the assumption ‘against’ which this point gains its originality is that meaning pertains to convention – the ‘semiotic’ of symbols that stand for things in the world that Wagner identifies, as we saw, as the key corollary of Euro-American distinctions between culture and nature. So if it is to be understood as more than such an act of trivial
classification, the notion of invention requires an alternative account of meaning. Such an account lies at the heart of Wagner’s own invention of the notion of invention, and takes the form of a dialectical contrast with the semiotic of convention – a ‘figure/ground reversal’, as he often puts it (e.g. Wagner 1987).

Convention relies on the assumption that the realm of symbols and the realm of the things for which they stand are opposed – culture to nature, representation to world. Conventions arbitrarily ‘fix’ the meaning of symbols which can then be used to express things by being ‘applied’ to the world (e.g. I assume we know what ‘genius’ means and who ‘Wagner’ is, so when I say ‘Wagner is a genius’ I apply the former to the latter – an operation that is formally indistinguishable from my doing the same for Mozart). Hence, in the semiotics of convention, the fixing of meaning and its application to the world to express something ‘about it’ are logically separate, the former being the precondition for the latter. Invention, Wagner argues, turns this image inside out, point for point. When the Daribi impersonate ghosts, Benny Goodman goes off on a solo on his clarinet, and Wagner invents the semiotics of invention (and I call him a genius for it), meaning is not a precondition for expression but rather an outcome of it. According to this view, every act that the semiotic of convention would brand as an ‘application’ of symbolic meaning to the world is in fact an extension of meaning. So, to stick to the discomfiting example, to say ‘Wagner is a genius’ does not ‘apply’ the notion of genius to the person we call Roy Wagner but rather extends what we mean by both: Wagner’s particular form of brilliance putatively extends the notion of genius in a novel way and, equally, the notion of genius extends the conception of the kind of person (or anthropologist) we may think Wagner is. Instead of a ‘gap’ of mutual independence between symbol and thing, representation and world, we have a relation of mutual dependence, whereby
meanings modify each other in the act of being brought together. Just as with the
*habu*, then, the semiotic of invention implies that everything (‘representations’ and
‘world’ alike) is meaningful, and the task of expression is to mediate the relationships
between meanings so as to engender novel ones – not to ‘convey’ meanings
representationally, but to *create* them by transforming the ones that are already
given.\(^\text{11}\)

We may conclude, then, that if making sense of the Daribi’s divergence from
our assumptions about nature and culture precipitates an act of invention on the part
of the anthropologist, it must also precipitate *a departure from just those assumptions.*
The anthropologist’s task of making sense of people that are ‘different’ can no longer
be a matter of ‘representing’ the differences in question – to do so, as we have seen, is
effectively to obliterate them. Rather like the *habu*, the anthropologist’s task on this
view is to transform the categories he takes for granted in the very act of bringing
them to bear on the differences of which he seeks to make sense. Making sense, in
other words, must involve the semiotics of invention.

This way of thinking of the task of anthropology ‘solves’ the Schneiderian
paradox. Instead of trumping ethnographic alternatives to the distinction between
nature and culture by branding them as cultural, Wagner’s position allows them to
trump that distinction itself, by transforming it into an altogether different way of
thinking about difference. Indeed, to think about difference, on this view, *is to think
differently*: to transform one’s most basic assumptions in light of the differences that

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\(^{11}\) It is for this reason that calling Wagner a genius, be it true or not (and more on truth later), is at least
more interesting than calling Mozart one. While even the latter claim must involve a kernel of
expressive invention (it is not, after all, a *mere* tautology), it is clear that more than two centuries of
citing Mozart as an archetype of genius have made the claim itself something of a cliché, which is to
say, not much more than a statement of already established convention. Calling Wagner a genius, by
contrast, is ‘controversial’ (and hence interesting) to the extent that it raises novel possibilities for what
being a genius might involve, as well as putting Wagner himself in a new light (indeed, he may be the
only person who should not be offended on being reminded that calling him a genius is an
invention…).
trump them. Adapting a term from Marilyn Strathern, whose own seminal critique of anthropological assumptions about nature and culture was developed in mutual influence with Wagner (Strathern 1980), I call this task of, as it were, ethnographically driven anthropological self-trumping, ‘recursive analysis’ (cf. Strathern 1988: 320).

Bringing into focus the reciprocal relationship between anthropological analysis and ethnographic description (the form and the content of anthropological work, so to speak), the notion of recursivity is also meant to signal this approach’s basic departure from the dilemma between naturalism and constructivism. As we saw in the discussion of the ‘reflexivity’ of anthropological truth-claims in the previous section, the move from naturalism to constructivism in anthropology left intact the basic assumption that both anthropologists and the people they study are in the business of representation: people in different societies represent the world in different ways and the anthropologist’s job is to represent (describe, interpret, explain and so on) these representations, which he calls ‘cultures’ (construed, in Wagner’s terms, as sets of conventions). As we also saw, however, the turn to constructivism shifted the balance between these two orders of representation. Where naturalism had assumed that anthropological representations are superior to indigenous ones since they are systematically geared towards reflecting the world as it actually is, constructivism countered that ‘the world as it actually is’ is necessarily also a cultural

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12 The term ‘recursivity’ has wide and varying applications in mathematics, linguistics, computer programming and elsewhere. As Helen Verran (2001: 89-91) points out, it became widely used as a metaphor in popular culture as well as social theoretical discourse in the 1980s (e.g. Strathern drew her usage from Nancy Munn (1986: 156-7)). In general terms, recursivity is defined as a property of definitions, proofs or algorithms that contain forms of self-reference (an online computer hackers’ dictionary illustrates this with a joke: when you search for ‘recursion’ the first hit you get reads ‘see recursion’). My usage here inflects this idea: I use the term to refer to operations whose formal properties are modified by the contents on which they operate. For example, imagine an algorithm whose sequence of instructions changes in light of each result it produces; or, to recall Wagner on invention, a concept that changes every time it is used to express something. Such a usage, drawing on Strathern to say something slightly different, exemplifies itself.
representation (a ‘cultural construct’), so anthropological and indigenous representations are in this respect no less or more valid than each other. The move to recursivity, then, can be seen as the next step: an attempt to tip the balance in favour of the ‘indigenous’ yet further – so much so that it be made a vantage point from which the very assumptions on which the dispute between naturalism and constructivism relies – about such notions as nature, culture, representation, construction and so on – can be called into question and refigured or, in Wagner’s terms, ‘invented’.

This book’s concern with truth emerges directly out of this way of thinking about recursivity. Where, one wants to ask, does the move to recursive analysis leave the notion of truth in anthropology? For it would seem that such a move raises a major dilemma. On the one hand, seeing recursivity as a further step on the trajectory from naturalism to constructivism would appear to imply seeing it also as a further move away from the notion of anthropological truth. In fact, thus construed, the move to recursivity seems to amount to nothing less than an annihilation of the very idea of truth as anthropologists have conceived it. For anthropological debates have relied on a concept of truth that is wedded to the distinction between nature and culture, as well as to the notion of representation, and these are precisely the assumptions which recursive analysis puts into question. How could one possibly hold on to the notion of truth while discarding the whole conceptual infrastructure in which it is embedded?

On the other hand, it would also seem that, more or less implicitly, recursive analysis does want to uphold at least some sort of appeal to truth. After all, what makes recursive analysis so different from constructivism is that, while the latter seeks to abstain from the kind of truth-judgements that characterise naturalism, the former proposes assertively to invert them. Where naturalist approaches privilege the
‘scientific’ truth-claims of the anthropologist at the expense of the people he studies, recursive ones affirm the indigenous perspective as against that of the anthropologist (see also Argyrou 2002). To take the (recursive!) example, when Wagner uses his account of the habu as an ethnographic pivot with which to recast the anthropological concept of culture, is he not in some pertinent sense giving the ‘truth’ to the Daribi? Furthermore, if this affirmation of Daribi ethnography is supposed to serve as an allocentric vantage point from which not only to criticise, but also to reinvent anthropological assumptions, does this not also imply an appeal to some sort of truth on the part of the anthropologist after all – on the part, say in this instance, of Wagner as both critic and innovator? The ethnographically-led suggestion that the notion of culture as convention be replaced with a notion of culture as invention does, in fact, sound a lot like a claim to truth; it seems, for instance, to censure one view and propose another much like any ordinary, ‘representational’ truth-claim would. But if part of the recursive effect of these very claims is to obliterate the notion of truth altogether, then what kind of claim is being made here and how could it even be articulated as such?

This book seeks to disentangle this dilemma. What concept of truth might a recursive anthropology entail? How might such a concept operate in the transformed (or better transformational) conceptual landscapes that recursive analyses are meant to engender? How, indeed, might it help to sustain such transformations – including, among other things, the transformation of anthropology itself, as a recursive tack would demand? And how open might such a concept be to being recursively transformed itself?

In seeking to answer these questions, this book can be seen partly as an attempt to complete the image of recursive analysis as it emerges not only in
Wagner’s own argument on invention, but also in a growing body of work that has developed in confluence with it in the past thirty years or so – be that through direct and mutual influence (e.g. Strathern 1988), or more indirectly (e.g. Viveiros de Castro 1998), or by a different route altogether (e.g. Latour 1993). While a review of this literature is beyond my present remit, it is worth noting that authors who, like Wagner, have used ethnography recursively to transform the infrastructure of anthropological analysis have not only had little to say about truth, but have often done their best to distance themselves from it. It is remarkable, for example, that Wagner should call the conceptual apparatus presented in *The Invention of Culture* an ‘epistemology’, and yet fail to incorporate in it an explicit discussion of the notion of truth – an abiding epistemological concern if ever there was one, not least for anthropologists. Seen in light of this silence, one may surmise that Wagner’s emphasis on the notion of ‘invention’ (viewed here as an antonym of truth as well as of convention) is meant to connote a move away from concerns with truth altogether.

Such a tendency is characteristic of the broader literature that can be said to develop, in one way or other, a recursive approach. This is illustrated in the work of the three aforementioned authors, Marilyn Strathern, Eduardo Viveiros de Castro and Bruno Latour, who, along with Wagner, have probably been the most influential in this field. In *The Gender of the Gift* (1988), for example, Strathern describes her attempt to use Melanesian ethnography recursively to refigure basic anthropological assumptions about society and the individual, as well as basic feminist assumptions about gender and power, as an exercise in analytical ‘fiction’:

> If my aims are the synthetic aims of an adequate description [of Melanesian ethnography], my analysis must deploy deliberate fictions to that end […] The
question is how to displace [our metaphors] most effectively. (Strathern 1988: 10, 12)

Elsewhere she calls this activity ‘an exercise in cultural imagination’ (Strathern 1992: xvii).

More explicitly, Latour’s sustained attempt to use ethnographic and historiographic accounts of scientific practice ‘in action’ to transform the epistemological framework that is assumed to inform it (e.g. 1987, 1988), results in a shift away from the vocabulary of truth, towards an analytical language that reflects the irreducibly ‘fabricated’ character of scientific truth-claims: from fact to ‘factish’ (1999: 266-292), from matters of fact to ‘matters of concern’ (2004) and, most explicitly, from truth to ‘circulating reference’ (1999: 69-74). So, for Latour, looking past what he famously calls a ‘modern constitution’ bent on ‘purifying’ nature from culture (1993) involves revealing scientists’ truth-claims as fragile projects of ‘construction’, which are to be understood not as cultural constructions of nature, but rather as hybrid and contingently negotiated amalgamations of elements from both – ‘naturecultures’ (Haraway 2007).

It would seem, then, that writers who are keen on recursivity find the notion of truth too tainted by its association with the distinction between nature and culture to be of use. Viveiros de Castro is perhaps the most explicit on this point. In ‘The relative native’ (2002), a landmark article in which he sets out the ‘meta-theoretical premises’ (2002: 113) of his analysis of Amerindian animism in earlier work (e.g. Viveiros de Castro 1992, 1998), the author presents the concern with truth as a key

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13 The article is published in Portuguese with the title ‘O nativo relativo’ (the passages presented here were translated in draft by me and kindly revised and refined by the author). It is worth noting that Viveiros de Castro’s recursive publication strategy, so to speak, of drawing meta-theoretical implications from previous ethnographic work is analogous to Wagner’s move from the ethnography of
point of contrast between anthropologists’ traditional project of interpreting and/or explaining indigenous ‘beliefs’, and an anthropology that takes indigenous thinking seriously enough to ‘use it, draw out its consequences, and explore the effects it may have on our own’ (2002: 129). With reference to his research on Amerindian animism, he writes:

Would taking the Amerindians seriously mean ‘believing’ in what they say, taking their thought as an expression of certain truths about the world? Absolutely not; here is yet another of those questions that are famously ‘badly put’. Believing or not believing in [indigenous] thought implies first imagining it as a system of beliefs. But problems that are properly anthropological should never be put either in the psychologistic terms of belief, or in the logicist terms of truth-value. […] We know the mess anthropology made when it decided to define natives’ relationship to their own discourse in terms of belief: culture instantly becomes a kind of dogmatic theology. And it is just as bad to shift from ‘propositional attitudes’ to their objects, treating native discourse as a repository of opinions or a set of propositions: culture turns into an epistemic teratology – error, illusion, madness, ideology… (Viveiros de Castro 2002: 130, references omitted)

Habu to the ‘epistemology’ of The Invention of Culture. In fact, exemplarily recursive in its agenda, Viveiros de Castro’s earlier body of work advances an argument that also runs parallel to Wagner’s. The particular character of human-animal relationships in Amazonian animism, branded synthetically as ‘perspectivist’ (see also Lima 1996), precipitates an analytical realignment of the distinction between culture and nature, whereby the concept of culture (rather than nature) becomes the site of universal similarity, and that of nature (rather than culture) becomes the site of relative difference. This ‘multi-naturalism’, as Viveiros de Castro calls it (1998a, 1998b), recalls Wagner’s suggestion that, for Melanesians and other non-Westerners, culture is what is ‘innate’, while nature is ‘artificial’. ‘The relative native’, then, can be read as an attempt to draw out further the recursive implications of such conceptual realignments for the practice of anthropology: What does an anthropology that adopts the
Getting anthropology out of its mess must, for Viveiros de Castro, involve moving away from questions of truth altogether. Later on in the essay he develops the point with reference to the notion, typical of Amerindian animism, that peccaries might be human:

I am an anthropologist, not a swinologist. Peccaries […] are of no special interest to me, humans are. But peccaries are of enormous interest to those humans who say that the peccaries are human. […] The native’s belief or the anthropologist’s disbelief has nothing to do with this. To ask (oneself) whether the anthropologist ought to believe the native is a category mistake equivalent to wondering whether the number two is tall or green. […] When an anthropologist hears from his indigenous interlocutor (or reads in a colleague’s ethnography) such things as ‘peccaries are human’, the affirmation interests him, no doubt, because he ‘knows’ that peccaries are not human. But this knowledge (which is essentially arbitrary, not to say smugly tautological) ought to stop there: it is only interesting in having awoken the interest of the anthropologist. No more should be asked of it. Above all, it should not be incorporated implicitly in the economy of anthropological commentary, as if it were necessary (or essential) to explain why the Indians believe that peccaries are human whereas in fact they are not. What is the point of asking oneself whether the Indians are right in this respect – do we not already ‘know’ this? What is indeed worth knowing is that to which we do not know the answer, namely what the Indians are saying when they say that peccaries are human. […] Hence, when told by her/his indigenous interlocutors […] that peccaries

‘multi-naturalist’ premise of Amerindian cosmology look like? For the author’s own commentary on
are human, the anthropologist should ask her or himself, not whether or not ‘s/he believes’ that they are, but rather what such an idea could show her/him about indigenous notions of humanity and ‘peccarity’. (Viveiros de Castro 2002: 134-6)

This constitutes a trenchant analysis of the first horn of the truth-dilemma set out above: understood as a property of beliefs about the world, truth is indeed incompatible with a turn to recursivity for precisely the reasons Viveiros de Castro articulates (see also 2002: 116). True as it may be, however (and note the incongruous locution), this still leaves open the questions raised by the second horn of the dilemma: is it really possible to advance recursive arguments without recourse to some (other, altered, suitably recast) concept of truth? Replacing the tainted truth-vocabulary of representationism with that of ‘invention’, ‘fiction’, ‘imagination’, ‘translation’ and so on is no help. What are the truth-stakes in such analytical activities, one wants to ask, and how do they relate to more traditional ways of imagining the role of truth in anthropology? At any rate, it would seem odd for proponents of the recursive approach, after having put the reinvention of the distinction between nature and culture at the very centre of their endeavour, to then stop short of also reinventing truth – a prime corollary of that distinction, as we have seen. If a recursive approach allows anthropologists to go beyond merely criticising prevailing concepts of nature and culture, by transforming them – positively – into something new, then why not try the same for truth?

While a central aim of the book is to demonstrate that such a venture holds interest in its own right, its motivation in doing so is also strategic. Showing that

Wagner, as well as Strathern, see Viveiros de Castro 2009.
recursive analysis has a stake in truth (showing, that is, *what* truth it has a stake in) is important in order fully to articulate its potential contribution to anthropological thinking at the present juncture. As seen in the previous section, the question of truth has been at the heart of the most foundational theoretical debates in anthropology since its inception, and has oriented basic divergences of opinion regarding the discipline’s role in the broader intellectual landscape (e.g. Are we supposed to participate in the project of science or stand outside it? Should we revel in human diversity for its critical potential or subsume it under the sign of the universal?). It is hard to see how the distinctive character and contribution of a recursive approach could be gauged properly without explicitly connecting it back to those debates about truth – other, that is, than by sheer rejection.

More particularly, raising the question of truth afresh helps to combat the persistent tendency to mistake recursive analysis for a kind of extreme (or at least extremely self-referential) version of constructivism. Treating the recursive critique of nature/culture distinctions as a form of cultural relativism is only one example. At the risk of glossing over the diverse criticisms that the body of literature I have called recursive has attracted, it is worth noting that charges one associates with attacks on constructivism, relativism and their theoretical cognates are a prominent feature of this critical repertoire. Suspicions of exoticism, for example, are typical (e.g. Carrier 1992). At their most extreme – usually in seminars and lectures – the rebukes go something like this: Surely Amazonians can tell a peccary from a human when they hunt! And don’t Melanesian people, composed ‘dividually’ of their relations to others according to Strathern, nevertheless go ‘ouch!’ individually when pinched? And so on, in come-off-it *touché*. 
Recalling Viveiros de Castro’s analysis, it is clear that such ‘smugly tautological’ appeals to common sense are misplaced. The accusation of preposterousness sticks only if you take notions of peccaries that are human, humans that are dividual and so on as representations (be they descriptive or analytical) of indigenous ‘beliefs’, ‘propositions’, ‘opinions’, or indeed ‘constructions’. However, the tendency to distance recursive arguments from considerations of truth is arguably a prime source of this misunderstanding. After all, when cast in purely negative terms (implicitly or explicitly), aversion to the concept of truth makes recursive arguments seem rather too similar to relativist/constructivist ones – such an aversion being as characteristic of the latter, though, as we have seen, in a different sense and for different reasons. Hence, from the point of view of critics invested in ‘common sense’ and the facts of the world, the difference between recursivity and constructivism can easily appear slight: both seem to boil down to the lowly common denominator of denying that anthropology is in the business of ascertaining truth. To the contrary: I contend that what makes recursivity most different from constructivism is precisely the fact that it is able positively to uphold a claim to truth – one that is as rigorous and exact as any claim to truth ever was. And it can do so, not despite itself, but by its own virtue: namely by recursively transgressing representationist assumptions so as to arrive at a different concept of truth altogether. It is to this task that I turn in the next chapter, in which I characterise the assumptions about truth that are embedded in the anthropological literature on divination, and specify the ethnographic conditions for their recursive revision.