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The objects of evidence

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For anyone interested in exploring the concept of evidence, *Questions of evidence: proof, practice, and persuasion across the disciplines* (Chandler, Davidson & Harootunian 1994a) is a good place to start. Thirteen major scholars from a range of disciplines discuss how it has been understood within their respective fields, including history, art history, history of science, philosophy, biology, law, and literature. The underlying strength of the volume is its emphasis on the historicity and disciplinary specificity of evidentiary protocols. In every chapter readers can follow vibrant debates on epistemology, played out most immediately in a series of commentaries and responses that accompany the main pieces, but which connect to wider conversations within the academy. Yet what ought to strike any anthropologist who reads the volume is that none of the thirteen main essays is by an anthropologist.[1] Whether or not this was a product of timing and chance, as with many edited collections, it is relatively difficult to find social and cultural anthropologists writing about the concept of evidence in an explicit or sustained manner.[2]

To say that anthropologists are not interested in evidence is a step too far. In fact anthropologists regularly refer to evidence in their writings. Pick up any journal, skim the articles, and you can find the word being used. Or take a look at what many Ph.D. students are obliged to: the Wenner-Gren Foundation's application forms for predoctoral field research. Question 3 asks: 'What evidence will you need to collect to answer your research question? How will you go about collecting this evidence?' (Answer in one page.) So there is plenty of evidence that we are users of evidence. What does not really exist, however, is an explicit set of discussions on the concept of evidence – or how it is used in practice – within the discipline. In a way, we can read the history of anthropological thought as a series of debates over questions of evidence, from Franz Boas and Bronislaw Malinowski's early critiques of the social evolutionists, to more recent debates, such as that between Marshall Sahlins and Ganananth Obeyesekere, or, in a wider arena, David Stoll's critique of Rigoberta Menchu's *testimonio* and the responses it sparked within the anthropological community. In each of these cases

disagreements over interpretation, argumentation, and the provenance of anthropology hinge, at least in part, on the constitution of evidence. Anthropologists deploy the concept of evidence and often contest the ways in which others 'misconstruct' or 'misuse' it, in their view. But rarely do they define what they mean by it in any depth.

In this chapter I suggest that socio-cultural anthropologists need to develop more sustained discussions on the concept of evidence. I do this in part by bringing a recent spate of articles (Csordas 2004; Hastrup 2004; Mosse 2006; see also Carrithers 1990) that tackle evidence head-on into conversation with the chapters included here. One of my primary aims is to underscore the fact that evidence has to be seen as both an epistemological and a methodological concern. It is often the latter we turn to first in our thoughts, and that which colleagues in other disciplines remark upon as what makes anthropology unusual or, even, problematic. How can such an intersubjective exercise as fieldwork produce evidence? Of course questions of method are important. But for any human science 'method does not make the object' (Fabian 1994: 87-88). For that we need epistemology. What I want to present here, then, is something of the 'epistemological unconscious' (Steinmetz 2005) that shapes the objects of evidence in and for anthropology.

In the broadest terms there are at least three reasons why social and cultural anthropologists should devote more attention to the concept of evidence. Perhaps above all, reflection on the concept of evidence should prompt us to consider the standards of judgement to which we hold our own and others' work. Why do we consider an ethnography good or bad? What makes an argument compelling or unconvincing? We all have standards. But how are they set? As George Marcus has argued, there is a sense in which anthropologists refuse to address these questions head on: students are often trained and socialized according to the rather vague benchmark of 'you only know a good ethnography when you read one' (1986: 266). Marcus made this observation more than two decades ago now, but the kind of Malinowskian ethnographer's magic it captures is still very much in play. More recently Kirsten Hastrup (2004) has raised a similar point by relating standards of judgement to questions of evidence. How, she asks, do anthropologists 'become convinced of "being right"' (2004: 458)? And how do we convince others that we are so? How can we turn fieldwork experience – a highly personal, temporally bound, and intersubjective method for collecting data – into objects of evidence? What justifies our convictions or conclusions, and how can they be marshalled in and through ethnography? By asking these questions, we gain important insights into the processes of judgement that shape our professional debates. And evidence surely ought to precede any sound judgement.

Devoting more attention to the concept of evidence might also help us to understand some of the intellectual divides within the discipline. There are plenty of these. They are often not as real as they are made to sound, but they do exist, gaining occasional airing in blunt and polemical tones (see, e.g., Bloch 2005; Leach 1961; Needham 1974; Sahlins 2002; Sperber 1985; Wolf 1980). Some of us feel an affinity for the humanities and arts, others for the natural sciences. Some of us deride the concept of culture. Some of us are wary of words such as 'power' and 'resistance'. Some of us claim X's understanding of culture (or power or resistance) is flawed, and that Y uses it more effectively. There are some anthropologists who think studying 'at home' is poor practice. Others moan about postmodernism, and still others that little of value has been published after 1980. Again: in practice these derisions, concerns, critiques, and moaning are not as divisive or debilitating as they appear. And for every anthropologist

who cares enough to align him- or herself 'with' something and 'against' something else, there will be several more who feel no such compulsion, who see value in different takes – occupying what Bruce Knauft (2006) has recently referred to as 'the middle'. It is, moreover, not necessarily a bad thing to have disagreements, even heated ones. But whether productive or not, it is notable that arguments over evidence are rarely set out within disciplinary debates. Socio-cultural anthropology does not have a well-developed language of evidence with which to thrash out its differences. Taken together, the chapters in this book explore how and whether such a language might be useful for understanding the differences in outlook and practice that characterize the field.

Whether, indeed. This second argument raises the important question of whether there are good historical reasons for an underdeveloped language of evidence within socio-cultural anthropology. As I discuss later, Martin Holbraad's chapter in this book raises this question in relation to what he sees as a misguided reading of evidence as a quest for 'indubitability'. More generally the absence might be attributed to anthropology's tradition of participant observation, into which any kind of positive knowledge does not easily fit (cf. Hastrup 2004). It may also have to do with the material and sensual nature of our data; we may be increasingly familiar with archives, for instance, but the roots of anthropology are grounded in social experience, not documents. However, even with these points in mind there is much to be gained through more explicit considerations of evidence, as my third argument further suggests.

Finally, then, reflection on the concept of evidence might give us a language with which to engage colleagues in other human sciences, the humanities, and the natural sciences, as well as actors and interest groups in the wider world. In experimental psychology, for example, as the chapter here by Charles Stafford shows, the nature of evidence is a central and fairly explicit concern. Experimental psychologists have a language of evidence in a way we do not, and the scale of their research has an important effect on the scope of their field. If we want to engage with the work that grows out of that discipline (or any other), it makes sense for us to consider where our own approaches might fit in. Likewise, take just one example from what I have called the wider world. As an increasing number of anthropologists are documenting (e.g. Drexler 2006; Gregory 2006; Skidmore 2003; cf. Chari, this volume), human rights and social justice activists are thinking long and hard about the nature and social effects of the evidence presented in their work. The sophistication with which these presentations are being made ought to prompt some thinking of our own, not least for how we might define the connections between scholarship and politics (cf. Wilson 2004). A truly public anthropology ought to have a language of evidence at its disposal, a way of presenting its findings in a manner that speaks across the academic divide.

The primary aim in this volume is to reflect on how evidence works in and for the discipline of anthropology in its generation of knowledge. It is in this sense that the volume is concerned with evidence as a problem of epistemology as much as, if not more than, a problem of method. A conscious effort was made to gather colleagues working within Anglo-American traditions on a wide range of topics, in a number of specializations, and from different theoretical points of view. Thus Maurice Bloch, who has recently (2005) been calling for a return to grand theory in anthropology, presents on what he understands as the crucial link between evidence and sight as the bedrock of truth. Christopher Pinney also addresses the link between vision and evidence, but with a concern to show how truths are manufactured and contested. Anthony Good,

reflecting on his work as an expert witness and researcher in legal anthropology, assesses the nature and impact of cultural evidence in courts of law. Sharad Chari, a geographer with extensive fieldwork experience in both India and South Africa, focuses on the ways in which activists in Durban document their lives to present evidence of discrimination, reflecting in the process on the forms and formation of political evidence. Stefan Ecks compares the prescription of antidepressants by general practitioners and psychiatrists in Kolkata (Calcutta), and relates these to the rise of evidence-based medicine and the burgeoning field of medical anthropology. Martin Holbraad, a Caribbeanist with training in analytic philosophy, ruminates on the similarities and differences between questions of evidence for practitioners of Santería in Cuba and those of the anthropological community. Webb Keane, extending his innovative work on semiotics, turns attention to questions of the materiality of evidence within the anthropology of religion. Charles Stafford explores the differences between objects of study in experimental psychology and anthropology, and how each field's understanding of evidence can shed light on the other's. And Nicola Knight and Rita Astuti offer a challenging reading of the ways in which anthropologists marshal evidence to make collective ascriptions, arguing that cognitive anthropology and the cognitive sciences more generally provide useful insights into the potential pitfalls of such ascriptions. Taken together, the chapters here are testament to the fact that questions of evidence are animating ones deserving of more considered attention. Indeed, they pick up on what seems to be a growing recognition within the discipline that our conceptions of evidence have not received their due (Csordas 2004; Hastrup 2004; Lau, High & Chua in press).

The remainder of this introduction is divided into two main parts, throughout each of which I offer support for the three claims made above. In the first part I review some key definitions of evidence and relate them to the agendas and concerns of anthropology. In the second part I sketch four themes that recur in this volume's chapters which I hope will prompt further discussion and debate: *scale* (how anthropologists circumscribe their objects of study); *quantity and quality* (how 'much' and what 'kind' of evidence is needed to substantiate a claim); *certainty* (the extent to which evidence can settle any claim); and *intention* (how evidence is produced and whether it has an agency of its own – how and whether it 'presents' itself to us). These are not the only themes that can be raised in relation to the chapters, of course; each is but one point of entry that might help us understand the diversity of anthropological modes of reasoning.

Defining evidence

In the *Oxford English Dictionary* the first definition of evidence is 'the quality or condition of being evident; clearness, evidentness'. As Bloch and Pinney highlight (this volume), this quality is often linked to the sense of sight. To be 'in evidence', the *OED* states, is to be 'actually present, prominent, conspicuous'. What needs to be stressed, however, is that in any professional or academic inquiry the primary definition of evidence is insufficient. Indeed, for an anthropologist (among others) the quality or condition of being evident exists more as a desire than an actual state of affairs. We want our work to make the objects of anthropology evident – even those amongst us who would (rightly, in my view) reject the idea that those objects could ever be *fully* evident. But the body of work that achieves this goal is small to non-existent. Is there any approach to the understanding of kinship, or ritual, or violence, or exchange which can claim, on the basis of the evidence it presents, to be settled? This is not the way evidence works in anthropology or the human sciences. It is only when we get to a

secondary *OED* definition of evidence that we find something akin to what we need. We need evidence first as a tool, not as a quality or condition. In anthropology evidence is always also an argument. 'Evidence has to be evidence of or for something, and that something is an hypothesis in the broadest sense' (Csordas 2004: 475). Thus, in what the *OED* links to the word's legal uses, evidence is defined as 'information, whether in the form of personal testimony, the language of documents, or the production of material objects, that is given in a legal investigation to establish the fact or point in question'. From the *OED* definition based on the word's legal uses, we are given a crucial point. Evidence exists in relation to questions (cf. Daston 1994).

Questions are what lead from evidence-as-tool to evidence as the quality or condition of being evident. R.G. Collingwood's short essay 'Historical evidence' (1946) helped make this correlation especially clear, and is itself the best exception to the neglect of the topic in the modern disciplines (Chandler, Davidson & Harootunian 1994*b*). Collingwood argues that history, like any profession, has to establish and work by a set of evidentiary protocols. Evidence, in other words, has a disciplinary specificity. Collingwood says that history 'is a science whose business is to study events not accessible to our observation, and to study these events inferentially, arguing to them from something else which is accessible to our observation, and which the historian calls "evidence" for the event in which he is interested' (1946: 251-2). History cannot be produced if this understanding is abandoned or abused.

It is necessary to read Collingwood's essay on two levels. At its core, it offers the crucial insights that evidence has disciplinary specificity and that, because of this, 'nothing is evidence except in relation to some definite question' (Collingwood 1946: 281). In the process of making these points, however, Collingwood engages in what has come to be seen as heavy-handed boundary-drawing (Chandler *et al.* 1994*b*: 2). By setting out what history is, Collingwood sets out what it is not. Like Malinowski's introduction to *Argonauts of the Western Pacific* (1984 [1922]), 'Historical evidence' creates the template for the methodology of a profession; like Malinowski's introduction, it is a template that has been both enacted and recalibrated. In the end, however, as Chandler *et al.* (1994*b*: 2) suggest, Collingwood's understanding of disciplinary specificity is too specific. The lesson is that in attending to the disciplinary specificity of evidentiary protocols, we need to be aware that evidence is defined not only by questions but also by competing pressures and regimes.

Disagreements within a profession often hinge on what counts as evidence, and for what claims. Take the following brief example from a review by Richard Wilk (1998) of Jonathan Friedman's *Cultural identity and global process* (1994). This example allows us to expand on the points I made earlier about standards of judgement and anthropology's divides. Wilk 'found the book a stimulating read, full of original ideas that point the way toward the next generation of global anthropology' (1998: 288). Yet he is also of the opinion that the global (or what Friedman calls 'global systemic processes' [1994: 1]) is 'difficult territory for anthropology' (an opinion Friedman would surely support) and that some of the leading scholars working on it seem to have a hard time connecting 'highly abstract processes and the tiny fragments of evidence we control' (1998: 287).

> In global anthropology richness and creativity of ideas and diversity of theoretical connections are not complemented by much empirical evidence. In Friedman's book (as in others of the genre) the author's veracity rests on clever argument, dazzling and sophisticated references and connections, and striking examples which appeal to our own experience, all of which are provided in abundance. But

the theoretical richness raises the question of what kinds of comparative, historical, or ethnographic evidence a truly global anthropology will need to call upon if alternative propositions are to be evaluated (1998: 287-8).

It is not the place here to assess Friedman's work in light of Wilk's comments, and still less the merits of global anthropology or global systemic processes (even as I have made the case that anthropologists need to be more in touch with their procedures of judgement). Rather the point is to highlight how the objects of anthropology are settled and unsettled through appeals to specific notions of evidence. What counts as empirical evidence or ethnographic evidence? Are these constant, or can a case be made for their variability depending on the context or subject under discussion? These are the types of questions that reveal disciplinary fault-lines and barricades. In Wilk's case they do not appear as deep or heavily fortified as for Collingwood in his day, but they are there none the less. Wilk is basing his cautions over global anthropology on his understanding of anthropological evidence. At the time, at least – 1998 – he had yet to be convinced that global anthropology could produce the necessary evidence at all – something that would bridge the divide between grand ideas and actual observations. The problem for Wilk seems to be that global systemic processes are not necessarily something that can be observed anthropologically: namely that global systemic processes cannot be fashioned into anthropological evidence.

As I mentioned in passing, Knight and Astuti (this volume) warn us that ascription is a dangerous act. But my assumption is that we can ascribe to Friedman some respect for the importance of evidence. The essays in *Cultural identity and global process* do not appear to be written with a wanton disregard for any sense of evidence. Friedman refers in most chapters to the ethnographic record, developing extended discussions in some cases, such as that in chapter 9 on Congolese *sapeurs*. On face value we have to accept that one person's 'dazzling and sophisticated references' (so described) might be another's appeal to the record – to the evidence accrued. It is true that scholars sometimes throw caution to the wind, or go out on a speculative limb without the safety of evidence behind them: think of some of Edmund Leach's more provocative essays. Friedman's book is not written in this vein. There is no point at which he claims to be making arguments or connections without the backing of evidence, and in the absence of such a claim, I believe we have to accept that some evidentiary protocol is in place. The problem, of course, is that because such protocols are often left implicit, it becomes difficult to have a discussion about the nature of the evidence being deployed.

To extend this point let me turn to a piece in which some anthropologists might expect the concept of evidence to come in for some scrutiny: James Clifford's introduction to *Writing culture*, 'Partial truths' (1986). This is one of those essays that elicit strong reactions. It is a good example of scholarship that has been used to draw the kind of 'for' and 'against' lines in disciplinary debates to which I alluded above. But it remains an important piece to read because of the attention it draws to anthropology's rhetorics of authority. In picking apart these rhetorics, a number of concepts come in for criticism – most notably 'truth', which is Clifford's main concern, but also 'fact' and, even, to an extent, the notions of 'certainty' and 'reality'. Reading the chapter in light of the issues raised in this volume, however, what seems notable is the extent to which 'evidence' escaped interrogation during the crisis of representation. Where there is concern with facts and truth, evidence cannot be far behind. As several of the authors here suggest (Bloch; Ecks; Good; Holbraad; Pinney), 'evidence' is often linked to

questions about truth, and, given such, the relationships that obtain – whether in places, like Cuba and Madagascar, or projects, like experimental psychology or semiotics – need to be investigated.

The word 'evidence' appears only once in Clifford's essay, when he cites a book by Richard Price as 'evidence of the fact that acute political and epistemological self-consciousness need not lead to ethnographic self-absorption' (1986: 7). Here it functions as an unproblematic concept. Yet later on in the essay, questions of evidence are implicit in the argumentation, as when Clifford notes that 'to recognize the poetic dimensions of ethnography does not require that one give up facts and accurate accounting' (1986: 25-6). If one is not giving up on facts and accurate accounting – and one should not, *as Clifford clearly suggests* – neither is one giving up, and neither can one do without, some understanding of evidence. It is what we do with facts – not only what questions we ask of them, but how we justify them to be 'facts' in the first place – that makes ethnography important. Anthropology ought to produce an excess of details, to borrow an image from Veena Das (1998: 179). But that excess has to settle around an argument, or, at least, a set of questions, in order to be legible as anthropology. It can only do this properly when the facts or details are marshalled as evidence of or for something.

Having considered Clifford's work briefly, let me present an unjustifiable caricature of what the crisis of representation was about in order again to drive home the points about why evidence is worthy of more detailed investigation. The crisis of representation is often understood as an attack on anthropology's status as a 'hard' science – as a discipline that can speak truths. More bluntly, the crisis of representation was used to draw the battle-lines between anthropologists who thought they had more in common with literature professors from those who thought they had more in common with biology professors (those who thought they had most in common with other social scientists existed somewhere akin to Knauft's [2006] 'middle' – neglected).

In this light, however, evidence comes partly unhooked from terms such as truth and fact. A literature professor may not emphasize a language of truth and facts, but she might still well employ a language of evidence. My point here is to suggest that the concept of evidence can mediate the unproductive and even misleading bifurcation of anthropology into 'scientific' and 'poetic' camps. Clifford himself is careful not to reproduce this bifurcation in his essay, despite the fact that it is often read as an attempt to shore up the differences. Reading his essay with an interest in evidence, I argue, can help disambiguate its reception. Clifford's work, and much of the 'postmodern' work with which it is associated, has never abandoned a notion of reality and never operated without a standard of judgement set by some evidentiary protocol. How one evaluates that protocol is a different matter. But there are, I would venture, few anthropologists who will say that all interpretations are equally valuable – equally *valid*, perhaps, but that is not the same thing. It is worth noting that Clifford titled his essay 'Partial truths' and not 'No truths'. To borrow a line from the law professor Mark Kelman, inasmuch as we make a commitment to evidence – to the very possibility that it can exist, somehow, somewhere, for somebody – '[w]e are all ... in that sense, closet positivists' (1994: 188).[3]

The concern with truths has become an important entry point into questions of evidence within the emerging body of anthropological literature. As Hastrup (2004) argues, for instance, anthropology cannot trade in the kind of positive knowledge with which 'the truth' is so often associated. Anthropological evidence 'cannot be empirical

knowledge in conventional positivist terms' (2004: 461) because of the irreducibly social nature of our objects. Jonathan Spencer also captures this point: 'Because ethnographic experience is so specific as to be unrepeatable – a fact which in itself removes ethnographic evidence from most understandings of scientific data – generalisation is peculiarly problematic' (1989: 152). So just as we study 'social facts', we might be said to produce 'social evidence'. This does not mean that evidence has to be agreed upon as such by the community from which it is drawn; to make an argument about the function or meaning of a ritual or kinship relation does not require group consensus by a congregation or family. What it does mean is that we have to accept that our arguments and interpretations about social life cannot be validated by an externally objective source. As Hastrup puts it, 'our relation to the object is already instilled as part of the object when we begin to understand it' and so '"evidence" cannot be disengaged from the objective of the investigation' (2004: 468).

In his recent Malinowski lecture, David Mosse (2006) explains how he found this out the hard way. Mosse's (2005) study of an irrigation project in western India run by the United Kingdom's Department for International Development (DFID) was fought tooth and nail by many of the colleagues with whom he had worked. They did not recognize themselves in his academic writing, still less their work, and disagreed with many of his conclusions. In part they simply did not understand on the basis of what evidence he came to his findings. They felt poorly judged and personally criticized (see Mosse 2006: 942) and that 'the ethnography dismissed empirical evidence' (2006: 943) – such as improved seed technology – in favour of a highly subjective personal take caught up in a curious concern for meaning.

While we do not all have 'the natives' knocking at the doors of colleagues in our departments to refute the findings in our book manuscripts (see Mosse 2006: 947), Mosse's experience with the DFID is part of an increasingly common dynamic within the discipline. To paraphrase Caroline Brettell (1993), they are reading what we are writing. We are used to facing objections from our colleagues, and many of us do share what we write with those written about, often with an eye to incorporating their comments and, even, engaging in debates with them. But as Mosse's experience makes clear, there is something especially troubling, emotionally and epistemologically, when the objections that come from our fieldwork informants, interlocutors, colleagues, and friends are as strenuous as the ones he faced. Because we are caught with them in the social evidence we produce, it can be particularly unsettling to be challenged in this way. Not many anthropologists would be glad to know that the people they studied – people with whom they forge close ties and often friendships – do not recognize themselves in ethnographic registers. We might be willing to accept misrecognition at the level of genre and style ('I don't understand that academic jargon'), but misrecognition of being ('That is not me!') cuts closer to the bone.

Mosse stood his ground, more or less. He did not simply amend his work because some former DFID colleagues felt he got it (and them) wrong. He tells us how he took their arguments seriously, and that the objections threw him into some moments of self-doubt. But in the end he was not convinced by them, not least because of what he came to see as his erstwhile colleagues' Janus-faced approach to the issue of evidence. On one hand, it seems, they demanded 'hard evidence' from Mosse to back his conclusions. How, for example (see Mosse 2006: 943), could Mosse reasonably conclude, as they understood it, that 'success' in the development project was not measured by the

encouraging 'scientific data' but rather the extent to which they could justify a particular model of development practice as authoritative? On the other hand, they wanted to amend his conclusions on the basis of what they saw as the larger truth of their work; they were people with good intentions and that was what should be conveyed. 'Indeed', Mosse notes, 'my colleagues' positivism concealed an essentially relational epistemology which rejected the notion of "evidence" as external to the situation' (2006: 944-5). There was no such thing as just the facts. In an important sense, facts and evidence were thought to be matters of group consensus and moral visions.

The development workers' objections, and his counter-objections, left Mosse with a nagging problem: '[H]ow was I to defend the "rightness" of my ethnography against those who could say "you are wrong, I was there", or "what evidence do you have to back this statement" or even, "come on, we know you!"' (2006: 949). How could Mosse both recognize that anthropological evidence is relational – at best a partial truth – and, at the same time, insist on standing his ground? The irony did not escape him (see Mosse 2006: 954 n.26). Just as the development workers were closet relativists – hiding behind a faith in the really real – Mosse had been acting as something of what I referred to above as a closet positivist. Mosse's experience shows that, however much we accept the situated nature of our knowledge, when push comes to shove – as it almost did in Mosse's case – it dawns on us that we *do* think we can be right, and that we can be so on the basis of our professional credentials in accordance with the evidence we understand ourselves to possess. This state of things is in fact a healthy one. Keeping the subjective and objective in a dance with one another is the best way to prevent professional scleroses. It allows us to keep positivism and postmodernism in the safe confines of rhetoric, since neither is really real.

One way in which anthropologists become convinced of getting it right – and perhaps the most powerful – is through the recognition of patterns in the social life they observe. To conclude this section, then, let me offer a few remarks on what this means.

Claude Lévi-Strauss once admitted experiencing discomfort over how, when taken in isolation, the things humans do 'are, or seem, arbitrary, meaningless, absurd' (1978: 11). Whether or not one finds comfort in the meaningfulness offered by structuralism, the recognition of order through the discernment of patterns has often served as the discipline's epistemological security blanket. In the absence of something that is equally accessible to others in a professional community – our colleagues cannot independently confirm our observations – the emergence of patterns in ethnographic writing (both intra- and intertextually) speak for themselves, as it were, and can be circulated as reasonably stable pieces of evidence. If you are like me, you get very excited when (a) in fieldwork you observe similar actions, reactions, or events in different settings, or when people who have no direct connection to one another reinforce your understanding of them through what they say and do, and/or (b) these fieldwork experiences resonate, in both expected and unexpected ways, with what you read about in other ethnographies, all the more so if you work with, say, the Yoruba and the other ethnographies are about Gê-speaking peoples or Japanese businessmen. Of course for many anthropologists making the leap from (a) to (b) – from culturally specific patterns to cross-cultural or even universal ones – is considered misguided or unjustifiable. But at some level the pattern argument has to hold. We could never package and transport our intersubjective experiences without it. While we may not be able to point to evidence as a clearly bound object, then, we do something similar

by tracing its outlines in the flow of social life (Carrithers 1990; Csordas 2004: cf. also Mosse 2006: 949).

Of course, the anthropological interest in patterns is not new: most famously expressed by Ruth Benedict (1934), it was also set out by E.E. Evans-Pritchard (1962). But within this classic work, questions of evidence and of its emergence through patterns remain largely implicit. Franz Boas did remark in his Introduction to Benedict's *Patterns of culture* that 'the old method of constructing a history of human culture based on bits of evidence, torn out of their natural contacts, and collected from all times and all parts of the world, has lost much of its hold' (1934: xix), but Benedict does not advance her teacher's historical particularism through a language of evidence within the text itself. It is relatively recent that the pattern argument has been set out with the concept of evidence in mind, most fully in an essay by Michael Carrithers (1990; see also 1992, chap. 8).

Carrithers frames his concern with evidence around the question of whether anthropology is an art or a science. Written as it was in the late 1980s, the concern is given life by leading figures of that day: Clifford Geertz and James Clifford, who are cast as proponents of 'art', and Dan Sperber, who is similarly recruited for 'science'. The details of Carrithers's take on their respective works (Clifford 1988*a*; Geertz 1988; Sperber 1985) are not of central importance here. Suffice it to say that he sees value in both the interpretivism of the artistic approach and the positivism of a scientific approach, while cautioning, sensibly, against any 'absolutist view' (see Carrithers 1990: 265) (a view he attributes to each of the three men, although I think unfairly). Despite offering serious criticisms of the hermeneutic approach, in the end he presents a vision of anthropology that has more in common with Geertz and Clifford than it does with Sperber (cf. Brady 1990: 273). He does not accept that anthropology can produce anything close to a positive knowledge. (Following Ian Hacking [1983], he argues that no discipline could produce this, as all are 'human activity and as such ... not so alienated from the world as to produce an absolute truth, absolute facts, or absolute confidence' [Carrithers 1990: 264].) At the same time, he does not want to surrender the possibility that anthropology is a 'serious activity' (Carrithers 1990: 263). Like Collingwood he argues that disciplines have their own evidential protocols and canons. Carrithers believes that anthropology can make a durable contribution to our understanding of the social on the basis of the evidence it presents, an understanding that might be taken up by others and used for profit. His final point is important: 'We may ask [of ethnography] not certainty but reliability' (1990: 272). Ethnography would in this sense be like the best kind of scholarship, since certainty is not the concern of either the human or natural sciences as properly practised. The problem is how reliability can be gauged. This is where patterns come in.

Carrithers argues that for something to be a pattern is has to be recognizable as such at the intersubjective level. It has to be consensible, which he defines as 'the ability of people to perceive things in common, to agree upon and to share perceptions' (Carrithers 1990: 266). It does not mean that everyone who has observed something actually does see the pattern in question; the requirement here is simply that the pattern is publicly intelligible. To illustrate this argument, Carrithers presents short extracts from the ethnographic record, including an episode in the work of Raymond Firth (see Carrithers 1990: 266-72). In this episode, Firth's friend, Pa Rangifuri, who is a chief's son, is described by Firth as *teke*, a Tikopia word which can mean 'unwilling (to do something)', 'angry', or 'objecting (even violently)':

When we got to his house we found him highly agitated. He and I greeted each other with the usual pressing of noses, as publicly recognized friends, but for him that was an unusually perfunctory gesture, and he paid me little attention. He was uttering brief incoherent statements: 'I'm going off to see' ... 'They said their axe should cut first' ... 'But was it for a dirge, no! It was for a dance!' Men were trying to soothe him down by respectful gestures, and to enquire the reason for his agitation. Tears were streaming down his cheeks, his voice was high and broken, his body quivering from time to time (Firth in Carrithers 1990: 266).

Even without knowing much about the Tikopia, Carrithers notes, it is possible for readers of this passage to understand that Pa Rangifuri was upset. It is worth noting, too, that we do not need the whole passage to understand this. Some anthropologists (e.g. Sperber 1985) might question Firth's use of a phrase like 'highly agitated' or his claim that Pa Rungifiri's nose rub was perfunctory. But we recognize the upset through the most 'stripped-down' elements of Firth's account, the fact that tears were streaming down Pa Rangifuri's cheeks and that his body was quivering from time to time. In the most general and basic sense, then, what this ethnographic extract provides us, according to Carrithers, is an intelligible (because discernible) pattern of human behaviour, what he calls a 'human pattern'. 'Pa Rangifuri's tears, the incoherence of his words, and his general demeanour are distinct, vivid, and discriminable from other patterns such as, say, "riotous jollity"' (Carrithers 1990: 266). As such, Firth's ethnography can be evidential.

But what is Firth's ethnography evidence for? It is not enough for a reader of anthropology to say, 'Well, tears, shaking: sounds like he's upset, so ... he must be upset'. If anthropology teaches us one thing it is that we cannot make such assumptions. Indeed, anthropology is often at its best precisely when it can challenge our common-sense understandings of emotions, or relatedness, or the dynamics of political power. Firth's work becomes evidential not out of what we might call this 'first-level' recognition of pattern, which is fairly superficial. (*News flash: 'Anthropologist provides evidence that Tikopians cry'*.) Rather, Carrithers claims, it becomes evidential through its confirmation as a human pattern within 'the dense and interwoven specificity' (Carrithers 1990: 269) of Firth's ethnographic *oeuvre*. When human patterns emerge out of ethnographic ones, confirmed as such by a community of critical readers, and in a sense independent of the intentions of an author, they gain shape as ethnographic evidence. Exactly what the incident Firth described can be used as evidence for is still open to question. But that is precisely what makes it evidential in Carrithers's opinion. 'The episode as told has robustness and independence from its use by Firth. It could be used by someone else to illustrate fraternal rivalry, generational conflict, an anxiety to pacify chiefs, or the very peculiar position of axes among the Tikopia at the time' (Carrithers 1990: 271). It is this robustness and independence that confirms its reliability. It becomes what Webb Keane (2005) has referred to in an essay on anthropology's epistemologies as a 'portable objectification'. As Keane suggests, we cannot do without these objectifications, a fact which tells us something important about the peculiar nature of our brand of 'positivism'. As I have argued, this is a positivism that should not and cannot insist on absolutes. It is, rather, a positivism that allows us to sense (and positivism, after all, is about the ability to *sense*) that we can 'get it right'. As Carrithers (1990: 272) might put it, it is about shifting our concerns from the ontological and epistemological to the practical.

There are several problems with the pattern argument. Let me mention three. The first is perhaps not so much a problem as a depressing conclusion (or at least what some

might see as depressing): that, in our practicality, we are nothing more than the academy's *bricoleurs*. Second, as I suggested above, and as Roger Keesing has pointed out, we cannot assume 'the cross-cultural transparency and translatability of patterns' (1990: 274). It is in this second sense that any anthropology with universalist aspirations faces significant and perhaps insurmountable problems. Patterns are not always as evident or concrete as we might hope. And neither can we be sure that they are ever fully revealed. (There can be patterns within patterns.) Finally, consider, as it were, the metaphysical mechanics of the pattern argument. As anthropologists we substitute the unrepeatable nature of our fieldwork experiences (our versions of experiments) with the appeal to patterns. This is a strange surrendering of our subjectivity; it carries the danger of making us over into something like spirit mediums. But in this we find a key stream of our 'epistemological unconscious'.

Four key themes raised in this volume

It would be possible and no doubt profitable to frame the chapters in this collection strictly in relation to the discussions thus far. Some version of the pattern argument, for example, is inherent to the structure of Bloch's provocative chapter. Turning to a wide variety of sources – from the theological and philosophical literature to the Zafimaniry's own observations – the evidentiary force of Bloch's argument rests on the appeal he makes to recurrent patterns of the link between the senses and truth. Likewise, the very nature of his 'pattern recognition' raises questions similar to those Wilk asks of Friedman about the provenance of our discipline. Can ethnography and philosophy produce commensurable sources of evidence? Is this a mix of empirical observation with dazzling and sophisticated references? These are some of the connected questions which can be raised in relation to the following. Rather than restricting the focus to them, however, I want to use the first main section as a point of departure for considering others themes and issues: scale; quantity and quality; certainty; and intention. Each is helpful for understanding methodological and epistemological aspects of the objects of evidence.

Scale

Of all the issues questions of evidence raise, scale is perhaps the most important. What are anthropology's objects of evidence? What 'size' are they? These are not only some of the most important questions of evidence we face, but also the most vexing (Strathern 1996).[4] Scale is a bugbear in our efforts to come up with evidentiary protocols. As the brief discussion of Friedman and Wilk makes clear, the scale of our work, conditioned by the shape and size of our evidence-catching nets, helps fuel disciplinary debates about not only what we can properly investigate, but also how. Likewise, when our work is read by outsiders (and some colleagues), the oddity of anthropology is often traced to the penchant we have for using a vignette or anecdote about what we observed one Tuesday morning in an open-air market outside Timbuktu eighteen years ago to explain the workings of political power in Mali, or African economies, or globalization. The vignette and the anecdote have been powerful vehicles of both communication and confirmation within the discipline. They are some of the most useful tools in our chest of rhetoric, an indication of our faith in metonymy to explain the human condition.

   All of the chapters here raise issues of scale, from Bloch's foray into grand theory, which sweeps across the spatial and philosophical maps of history, to Good's comments on cultural evidence, which reveal the difficulties anthropologists have in mapping

their ethnographic knowledge into the much roomier certainties prompted by courts of law. But I want to focus on the chapters by Stafford, Chari, and Knight and Astuti to develop the issue.

Stafford points out in his chapter on numeracy that the default assumption within anthropology is that we examine human relations in 'very fine, even "microscopic", detail' (p. 122). It is this microscopic focus, which emerges out of intersubjective relations, that can make anthropological evidence dubious in the eyes of some other academics. Yet in a sense Stafford argues that the scale of our work is not really the problem; compared with some experimental psychologists, who examine such things as infant staring times, the objects of anthropology seem huge indeed. There is, moreover, what many outsiders might consider a strange twist in our approach: ethnographies are particularist but in that particularity are supposed to take everything into account. For an anthropologist – particularly one who works in an 'exotic' locale (be that Kansas or the Kalahari) – everything is potential evidence (cf. Csordas 2004).

Thanks to 'collaboration' between an experimental psychologist and an anthropologist among the Pirahã, in the Amazon, Stafford is able to examine questions of scale in relation to the two disciplines in some depth. This collaboration, between the experimental psychologist Peter Gordon and the linguistic anthropologist Daniel Everett, was far from a match made in heaven: it turns out that Everett, the long-standing expert on the Pirahã, had neither much interest nor time for Gordon when he set up his experiments to test Pirahã numeracy in the village. As Stafford relates, in the results that Gordon published almost no mention is made of the cultural and linguistic specificities of his case study: he abstracts Pirahã understandings of numeracy into a universalist frame, treating the data as so much experimental evidence that can be fed into a larger hypothesis-testing machine. What is interesting is that when Gordon did try to include some 'cultural' analysis in a paper, it was dismissed by a peer reviewer as an attempt to smuggle mere anecdotes into a scientific journal. For evolutionary psychologists working on numeracy, it seems it simply cannot matter that an anthropologist such as Everett spent twenty years working with the Pirahã, or that culture more generally has a place in the halls of science. Everett, in turn, argued, in classic anthropological fashion, that if Gordon wanted to understand Pirahã numeracy, he needed to collect 'evidence about "everything"' (Stafford, p. 127, this volume).

Stafford's chapter raises the important issue of how the scale of a discipline has a direct but often overlooked effect on how practitioners make judgements about the validity of others' claims. Stafford is prompting us to consider these effects. He wants us to realize not only what we so easily can – that there are dangers in the abstracting reductionism and eurocentrism of experimental methods – but equally that our traditional focus on 'everything' leaves us in something of a muddle. 'By holding that all things are interconnected', Stafford argues, 'we tend to make falsification of our claims (e.g. via experimentation) more or less impossible' (p. 132).

Knight and Astuti's chapter, coming from a similarly appreciative view of what the cognitive sciences and psychology can teach us, also raise issues of scale. Their focus here is on how anthropologists ascribe certain properties and dispositions to social groups. Ascription, they stress, is not a problem in itself for anthropology. Indeed, it is on the basis of ascription, and the intuitive judgements that anthropologists are able to make through the in-depth nature of their personal engagements, that we produce much valuable evidence. The problem, they argue, is that anthropologists do not always pay enough attention to the different kinds of ascription they make. It is one thing to

say, on the basis of repeated observations, that the Dobuans are bad sailors; it is another to say that the Vezo think bodily properties are socially transmitted. This second kind of ascription, which has to do with questions of cognition, is, they point out, much harder to scale properly. When it comes to cognitive processes, anthropologists have to tread cautiously in claiming to have evidence on a large scale.

It is, of course, standard practice for anthropologists to avoid generalizations of most sorts, especially when it comes to what 'the natives' in question believe or think. There are not many anthropologists who would claim, after having worked in, say, a Zulu village for eighteen months, that 'the Zulu believe ...'. As other of the chapters in this volume attest (Holbraad; Keane) – adding voice to a long-standing debate – 'belief' is a particularly explosive word when employed analytically. It is one of the most difficult attributions for us to scale. For Knight and Astuti, however, the general awareness that exists within anthropology about the dangers of collective ascription is unsatisfactory. They are not satisfied with the caveats and qualifications one often finds ('most Zulu believe ...' or 'many Zulu say ...' [thus shifting the object from inner thought to outer expression]), and still less with the penchant for peppering ethnography with reference to the 'contests' and 'negotiations' in play. These are for them vague hedgings that downplay or ignore some of the important insights cognitive studies have produced on the constitution and social transmission of knowledge: namely that knowledge is not evenly distributed within any 'cultural group'. Perhaps the most valuable fruits of research along these lines have come from anthropologists working on children and learning (but see also the classic work on knowledge by Frederik Barth, discussed by Keane in his chapter), research which is forcing us to recognize the need to disaggregate our objects of investigation along the lines not only of gender, power, and other important concepts, but also of cognitive development in relation to age.

Sharad Chari raises questions of scale and the problems it creates for ethnography from a different perspective. In one section of his chapter, he focuses on life histories, both offering an investigation of their qualities as objects of evidence and presenting details from several he collected in the field. The range of Chari's work gives us an excellent sense of how life histories serve as evidence in a variety of ways, and according to a range of protocols (personal, social, and professional). In his research in Durban, Chari has been struck by the extent to which political and social activists use their life histories as a way of mobilizing evidence of discrimination (both during and after apartheid) and for, in some cases, claims to specific entitlements such as land. The activists he knows invest their life histories with a faith in what I mentioned at the outset of this section as the power of metonymy – that is, the power of part for whole representations.

Like ethnographers, it seems, these activist-residents are self-conscious about the extent to which their personal stories and experiences can serve as evidence of or for something greater. In an effort to bolster their objectivity, the residents turn to a host of objects – from deeds and other official documents, to newspaper clippings, photos, and other externally validating things. Chari's chapter suggests that life histories and other forms of activists' self-presentation that he describes can be valuable precisely because of their unstable nature as social scientific objects. While they may not be as paradigmatic as their authors (and sometimes anthropologists) can imply, these formations of political evidence are valuable precisely because they do not resolve the tension between subject and object.

Quantity and quality

This second theme I want to raise is closely related to the first. As Stafford's concern with scale suggests, for example, anthropologists face a peculiar burden of balancing the quality and quantity of their evidence. When everything is potential evidence, how do you make it legible as such? In one sense you cannot have too much evidence for something, but you do not *necessarily* need 'a lot' of evidence – however one wants to quantify it – to have a compelling argument. This point is also made by Holbraad in the discussion of his friend Jorge, a Santería follower in Cuba. In a different way, Chari's observations in Durban suggest that people (from activists in Durban to the ethnographers studying them) often have the impulse to horde as much 'stuff' ('data') as they can in the (vain) attempt to be exhaustive. This is an impulse that tends to kick in for anthropologists about half-way through a field trip, an impulse I have felt and which has been confirmed to me by several Ph.D. students in the last few years: how do we know when we have enough? Should we try to stay a few months longer? Surely that could make all the difference.

In many cases, we learn from Ecks's chapter, medical doctors seem to suffer not from the lack of evidence, but from too much of it. Issues of quantity and quality stand at the heart of his chapter. He relates these issues to both his field research in Kolkata and the intersections between 'evidence-based medicine' (EBM) and medical anthropology.[5] For doctors in Kolkata, quantity and quality are defined in large part by specialization. Ecks found that when it comes to making sense of depression, general practitioners (GPs) are more willing to take 'social factors' (living in a slum, for example, with no chance of escape) as evidence for a rise in rates of depression, a rise in which many GPs are convinced. Psychiatrists, on the other hand, while willing to accept that life in a slum is difficult, are less willing to accept social factors as evidence, in large part because they do not have 'proper' studies that transform their anecdotal experiences into reliable evidence of a causal link.

Ecks notes that most anthropologists reading an account by GPs in Kolkata citing the link between depression and social factors would accept it as reliable evidence 'without blinking an eye' (p. 76). As with activists in Durban, there seems to be an affinity between the folk-level evidentiary protocols of Indian GPs and our profession. But Kolkatan GPs are not in the majority view when it comes to the medical profession, in India or elsewhere. Or, at least, their understandings of evidence are not the dominant model. That honour belongs to EBM, a movement within medical science that is shifting the sources of knowledge and the justification for action from practitioners to 'the literature', a kind of quasi-object that some hope in the future will be available to all doctors on their hospital rounds as internet downloads.

Proponents of evidence-based medicine argue that it helps preserve and promote best practice by shifting the profession away from 'ego-based' and 'eminence-based' medicine, a state of affairs in which the good and great might act on their own authority, rather than the findings of science. Critics of evidence-based medicine caution that the utopian vision of doctors with instant on-line access to the gargantuan EBM database robs practitioners of alternatives and concomitantly of agency. It is important not to overstate the pro and con positions; Ecks does not suggest that opponents of EBM want doctors to throw away their books, refuse to read some percentage of the 5,000 articles published every day, and work on impulse and intuition. Especially when viewed in light of his thought-provoking section on developing an 'evidence-based medical anthropology', however, Ecks brings to the surface

important issues about the disciplinary pressures and regimes to which I referred in the previous section in the discussion of Collingwood. The vibrancy and potentials of any discipline or profession can often hang in a delicate balancing act between rigorous quality controls of what counts as 'good' evidence and a recognition that these controls must remain flexible lest they blinker a professional vision.

Certainty

In the previous section I highlighted the point that anthropology can be reliable but not certain. In this it is like any proper science. Certainty is one of the issues that recurs in discussions of evidence, although there are not many professional communities that will insist on its delivery. Evidence-based medicine, for example, is not about certainty, but about a kind of 'certain reliability' amassed in a shelf-load of studies. Even in many religious worldviews certainty is not the point, or at least it is a certainty conditioned by the productivity of doubt. So why does this issue resurface?

Holbraad's chapter on Santería in Cuba can be read as an answer to this question, or, at least, an exploration of it. He begins the chapter with a discussion of his friend Jorge, a Santería follower who accumulates 'proof' of the gods, a process that Holbraad likens to the anthropologist's accumulation of social facts, which are subsequently turned into ethnographic evidence. In each he sees a metaphysical longing for certainty at work and, on the basis of this claim, tries to show how the Santería follower and the anthropologist are versions of one another – how 'proof' (*prueba*) in Santería functions like 'evidence' in anthropology, and vice versa. Picking up on a point by Lakatos, Holbraad suggests that the concern with evidence looks more theological than scientific and that, as a result, we might well consider abandoning any 'faith' in evidence as that which confirms or produces knowledge (cf. Miyazaki [2004] on anthropological investments in hope as a method).

Holbraad's argument is an exercise in figure-ground reversal of the kind we find in the works of Roy Wagner, Marilyn Strathern, and Eduardo Viveiros de Castro. It is, in part, an effort to dislodge anthropology from the 'art or science?' discussion altogether. Holbraad is not convinced we need gather 'evidence' in the pursuit of reliability – much less certainty. It might be more productive to align ourselves closer to philosophy, a discipline committed to 'conceptual analysis'. Holbraad is well aware this is a contentious claim, and it is not the only strand in his chapter, but as I suggested near the outset of this introduction, it raises an important point we ought to consider.

Keane's chapter, like Holbraad's, raises important issues about how certainty is a concern not only among religious subjects but the anthropologists who study them. He does not call for an end to evidence, and he limits his discussion to the anthropology of religion (a discussion which includes an insightful set of remarks on the difficulty of bounding such a sub-field in the first place). In Keane's analysis certainty manifests itself in part through the prism of belief, a cognitive state that is often contrasted with certainty. What we cannot be certain of – what we cannot know, prove, see, or point to – gets cast as belief. We know about horses; we believe in unicorns. Keane challenges this contrast because of the burdensome work it makes the concept of evidence perform, work that 'may obscure certain dimensions of that which we want to understand' (p. 105). Turning instead to the materiality of religious phenomena, and in particular the materiality of religious language, he gives a detailed explanation of how these semiotic forms can stand as evidence of something as seemingly immaterial as belief. Far from being concerned with the airy realm of the ideal, semiotics (especially

after Peirce) is well grounded, so to speak. 'Semiotic practices can therefore both furnish evidence of something that is not directly found in experience, and, as components of experience, give rise to new inferences and serve as evidence in new ways' (Keane, p. 119, this volume).

Finally, Good's chapter makes clear the problems in entertaining Holbraad's call for 'conceptual analysis' for anthropologists who act in the courts as expert witnesses. In the courts there is no room for this. Good's chapter thus serves as a useful complement to the more analytical interventions on the problem of certainty by Holbraad and Keane (although there is a practicality in the latter's argument, too), giving us a sense of how in legal contexts anthropologists might well employ a strategic certainty, or at least be pressured to. Anthropology, Good notes, tries to preserve ambiguity and complexity. The results of this can be disastrous in the legal system, as the case of the Mashpee Indians on Cape Cod makes clear. In the 1970s, the Mashpee were pushing to be recognized as a tribe and thus gain certain rights to the land. In the Mashpee trial, as Clifford (1988*b*) shows, the anthropologists (and historians) who acted as expert witnesses on behalf of the Mashpee were unable to give the kind of confident answers the judicial system seeks. The existence of the Mashpee tribe boiled down to a series of yes and no answers. As Good tells us, however, legal analysis will always 'prune away "extraneous" details' (p. 47). 'Experts are therefore under pressure to profess greater certainty than they really feel' (p. 45).

Intention

This is an appropriate theme with which to end, if only because it is a main concern of the first two chapters – Bloch's more explicitly, but no less so in Pinney's. Intention has already surfaced in several of the discussions, both in the preceding section and in the sketches offered as to what we can expect from this volume. In a classic sense – and certainly when it comes to the word's legal uses – human intention ought to be absent from evidence. The historian of science Lorraine Daston (1994: 244) clarifies this point. A bloody knife might serve as evidence in a murder trial, but if we learn the knife was planted by someone, it is no longer evidence (at least of the murder). As Daston also tells us, the concern with human intentions corrupting scientific experiments led to 'methodological precautions' such as double-blind trials. For anthropology, as we have discussed, pattern recognition has served as a similar kind of methodological precaution. When found in patterns, the evidence we offer can be trusted as reliable (if not certain). In a sense this means it has to have an agency of its own.

Bloch's short chapter builds a case for the important link between sight and truth, a link he then relates to the concept of evidence. 'I saw it with my own eyes' becomes the tell-all phrase. What is interesting about this phrase is the appeal to objectivity that stands behind it. Prompted in part by his conversations with the Zafimaniry (after administering the 'false belief task' to them), and in part by his knowledge of the ethnographic record, Bloch suggests that we (i.e. humans) privilege seeing over hearing as a window onto the truth because the former is not mediated by language. The Zafimaniry he spoke with all agreed that language is what allows humans to be deceptive and lie, an observation that Bloch ties to larger currents of human thought. This makes any description of social experience suspect. There is, then, an experiential side to generating evidence in which 'being there' and 'seeing it for yourself' becomes the *sine qua non* of authority and, in a sense, certainty. This is not so different from the logic of anthropology! (Compare it as well with Schaffer [1994] on 'self-evidence' and

Scott [1994] on the evidence of experience.) So, what we say is subjective, while what we see is objective. As Bloch argues, sight is 'verification which avoids the treacherousness of language used in social life' (p. 25). What we see, in other words, is not, like language, 'vitiated by Machiavellian social intentionality' (p. 25).

Bloch's chapter can be read alongside Pinney's, which takes up the interrelated themes of intentionality and certainty in a more restricted frame, but with equal analytical rigour. Pinney's focus is not human nature but colonial India. In the chapter he traces the rise of photography in the second half of the nineteenth century as the ultimate visual medium; whereas lithographs depended on the skills of the lithographer, there was a sense in which the photographic image was not so much representation as presence. 'Its *positivity*', he writes, drawing from the semiotics of Peirce, 'lay in its indexicality' (p. 32, original emphasis). Throughout much of the nineteenth century, then, Pinney says that photography was perceived as the 'cure' for the weaknesses of other kinds of visual representation.

For the colonial state, however, photography as cure slipped all too quickly into photography as 'poison', as those critical of the state used its positivity to their own advantage. The key example on this point is drawn from the historical records of the massacre at Amritsar in 1919, in which hundreds of people were killed by the authorities. After the massacre, the photographer Narayan Vinayak Virkar took a series of photographs where the massacre took place, including a bullet-riddled wall against which locals stand, pointing to the bullet holes ringed in white chalk. Virkar's documentation of the Amritsar massacre showed what photography could do: serve as 'eyewitness' (Pinney, this volume). It was direct evidence, stripped of human intention and free, in a sense, from the subjectivity of its producer. Not long after, 'images in India were at war' (p. 37), a state of affairs, Pinney goes on to suggest, that threw the innocence of photography's positivity into doubt. In this chapter of the history of seeing, intention makes a rude return. As evidence, the photograph is neither certain nor free from the interpretative frame in which it is offered.

Because all anthropologists have to grapple with questions of evidence in their research and writing, it behooves us to take them seriously. Evidence is a concept not far from the heart of our discipline's internal debates over epistemology and the nature of anthropological knowledge. As such, evidence cannot be separated from our concerns with truth, certainty, and reliability; facts, social and otherwise; the dynamics of objectivity and subjectivity; intention and agency; and, most generally, in Hastrup's succinct phrase, getting it right. The chapters here do not provide one answer to how we get it right, and still less an exhaustive list. What they offer are several productive avenues into understanding the techniques of persuasion, illumination, and objectification that stand behind our contributions to the human sciences.

NOTES

[1] The only anthropologist in the volume is Jean Comaroff, who provides a commentary on Ian Hacking's article on multiple personalities and questions of evidence in psychology.

[2] The partial exception here is work in legal anthropology (e.g. Jeffrey 2006; Just 1986). In most of this literature, however, evidence is presented as an ethnographic object rather than an epistemological tool. It is important to note as well that the situation is somewhat different in anthropology's other sub-disciplines, even linguistic anthropology, which is probably the most relevant sub-discipline for the chapters here. For some important discussions on the role of evidence in linguistic analysis, see Chafe & Nichols (1986) and Hill & Irvine (1993).

³ Within Anglo-American anthropology, and especially the dominant tradition of American cultural anthropology, strong versions of positivism – those which proffer universal laws of history, or causation, for instance – have been consistently eclipsed (Fabian 1994; Keane 2005; cf. Bowman 1998). In George Steinmetz's estimation, anthropology's 'nonpositivist' commitment 'represents something of an extreme in the episte-mological space of all the human sciences' (2005: 9). I would not dispute this argument. But in my reading, Kelman's idea of closet positivism is not incompatible with anthropology's nonpositivism. What it captures is the hesitancy or even embarrassment that accompanies any 'clarity of vision' (Kelman 1994: 188) we might express – and often feel we have – when advancing a point of analysis or interpretation, even as and when that vision is explicitly recognized as momentary and partial.

⁴ Not everyone may be comfortable with such a materializing metaphor as 'scale'; in reading an earlier draft of this chapter, Dominic Boyer suggested we might also talk about the 'tuning' of our ears, for instance, since 'anthropological evidence is almost all absorbed from words and images'. This is an important point, and one that was explored in a workshop at the London School of Economics and Political Science in March 2008 on 'The Pitch of Ethnography' co-organized by Rita Astuti, Olivia Harris, Michael Lambek, Charles Stafford, and myself.

⁵ This hyper-explicit shift to the value of evidence is happening in other fields, too, such as the move to evidence-based policy (Riles 2006; Strathern 2006) and evidence-based crime prevention (Sherman 2006). In some of my most recent research, I have even come across literature on evidence-based spiritual healthcare (South Yorkshire NHS Trust 2003).

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