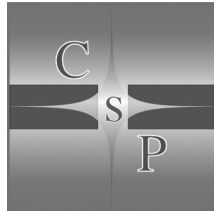


How Do We Know?

How Do We Know?
Evidence, Ethnography, and the Making
of Anthropological Knowledge

Edited by

Liana Chua, Casey High, and Timm Lau



Cambridge Scholars Publishing

How Do We Know? Evidence, Ethnography, and the Making of Anthropological Knowledge,
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TABLE OF CONTENTS

Acknowledgements	vii
Introduction: Questions of Evidence <i>Liana Chua, Casey High, and Timm Lau</i>	1
Old and New Reflections <i>Marilyn Strathern</i>	20
“We Sing About What We Cannot Talk About”: Music as Anthropological Evidence in the Venda Region of South Africa <i>Fraser McNeill</i>	36
Audio-Visual Evidence and Anthropological Knowledge <i>Carlo A. Cubero</i>	58
<i>End of the Spear</i> : Re-imagining Amazonian Anthropology and History through Film <i>Casey High</i>	76
Pragmatic Evidence and the Politics of Everyday Practice <i>Ann Kelly</i>	97
The <i>Vodou</i> Priest Who Lost His Spirit: Reflections on “Modernity” and “Tradition” in a Beninese Village <i>Anna-Maija Aguilera Calderòn</i>	118
Enmities and Introspection: Fieldwork Entanglements and Ethnographic Reflexivity <i>Emma Varley</i>	133
Understanding Tibetan Shame and Hierarchy through Emotional Experience in Fieldwork <i>Timm Lau</i>	157

Virtual Returns: Fieldwork Recollected in Tranquillity <i>Lisette Josephides</i>	179
Afterword: Malinowski's Heirs <i>Keith Hart</i>	201
List of Contributors	210
Index	213

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INTRODUCTION: QUESTIONS OF EVIDENCE

LIANA CHUA, CASEY HIGH, AND TIMM LAU

Introduction

Questions of evidence have become increasingly prominent within anthropology in the last few years. Once an invisible pillar of the anthropological enterprise, “evidence” is now becoming an object of scrutiny in its own right. In the inescapably reflexive climate of much contemporary anthropology, such a move may seem surprisingly belated, for the accrual of evidence through fieldwork and its presentation through written ethnography have long been instrumental to the discipline. Regardless of their theoretical persuasion, anthropologists have invariably relied implicitly on the quality of their evidence as the basis of their epistemological and rhetorical authority. That evidence is central to the production of ethnography should thus be beyond debate. Yet, its virtually unquestioned centrality has arguably also been its veil. As we suggest below, evidence was for a long time treated simply as the factual basis of ethnographic analysis: the “object” that stood outside the “argument” (Hastrup 2004), the means to a more prominent end. Even as the postmodernist debates of the 1980s paved the way for a more reflexive approach to ethnography, and rubrics like “writing culture” and “knowledge practices” directed anthropologists’ attention towards the *conditions* in which anthropological knowledge arose, evidence *itself* remained a non-topic.

In recent years, however, the situation has altered considerably, with the contours of a new discussion taking evidence as its central theme being shaped by a growing number of anthropologists. During this time, a number of articles (e.g. Csordas 2004; Hastrup 2004) and a special issue of the *Journal of the Royal Anthropological Institute* (Engelke et al. 2008) have yielded analytical, theoretical, and methodological insights into its place and significance within anthropology. Evidence, in these works, takes multiple forms: more than merely “facts” or “data”, it can equally be a trope (Csordas 2004) or an experience (Hastrup 2004), a tool or a condition (Engelke 2008, S5). The intellectual genealogies of these

approaches extend much further back than their thematic novelty suggests, however, for the questions they raise strike at the very heart of anthropological practice. Interrogating the concept of “anthropological evidence” concomitantly demands interrogating what anthropology itself is all about: its objectives, methods, parameters, ambitions, doubts. If evidence, as Csordas notes, has to be “of and for something” (2004, 475), then those who would discuss it must inevitably work out what that “something” is.

That process of working things out is what interests the contributors to this volume, as well as the participants in the conference from which it derives. “Questions of Evidence: Ethnography and Anthropological Forms of Knowledge” was held over two days in January 2007 at the Centre for Research in the Social Sciences and Humanities (CRASSH) and King’s College at the University of Cambridge. Focusing on ethnography and the processes by which anthropological evidence and forms of knowledge are created, it featured a diverse range of papers that explored “evidence” in and through the British broadband industry, films of the Caribbean, dreams and emotions, Cucapá indigenous curse-words, *vodou* goddesses, and pragmatic clinical trials, to name but a few. Inherent to them all was a strong reflexive impulse, whether personal or disciplinary; an ambition to not simply talk *about* evidence, but to examine the circumstances that give rise to it and make it recognisable as such.

The wide-ranging response to our call for papers was, serendipitously, unanticipated. Our initial intention had been to hold a one-day workshop for PhD and postdoctoral anthropologists to critically evaluate the use of encompassing explanatory models, and the role of small-scale ethnography in engaging with them. From this we planned to meander down various evidence-related paths, asking where it might be located, what forms it might take in an age of multimedia and globalisation, and how it might be rethought with the aid of interdisciplinary approaches. The proposals and comments we received, however, markedly widened the conference’s scope and interests.

Significantly, the participants came from both ends of the anthropology career spectrum: while most were young anthropologists and graduate students, many others were experienced academics with long-standing reflexive interests in the discipline. The paper-presenters were connected by the fact that they had either recently done fieldwork or were writing about fieldwork and processes of anthropological knowledge-making. Interpreting “ethnography” in its broadest sense, these participants—many of whom feature in this volume—were grappling with issues of immediate import to the practice and ethos of anthropology. Their papers are

accordingly contemplative, but also tinged with rawness; a sense of having only just worked through the ethnographic process, with all its flukes and foibles.

Placed in dialogue—as they were during the conference—with Marilyn Strathern’s keynote address and Keith Hart’s afterword, these papers comprise a volume which is grounded in anthropological experience but also methodologically, analytically, and theoretically ambitious in exploring evidence in anthropology. While they share the epistemological and philosophical concerns of recent writings on “how evidence works in and for the discipline in its generation of knowledge” (Engelke 2008, S3), they address them through specific case studies, exploring how evidence is located, elicited, and utilised in ethnography. In the process, they remind us that more than being a topic for philosophical-scientific discussion, evidence fundamentally emerges in and through what ethnographers *do*. In this respect, the composition of our contributors is particularly apt: the majority have just begun to feel their way through the “doing” process, and the others have “done” and reflected on it for much longer. The result is a volume which extends the nascent debate on evidence in anthropology in the direction of *ethnographic* practice and the generation of specifically anthropological forms of knowledge.

Evidence and the changing object of anthropology

Before moving on to some key themes in the volume, it is worth addressing what we mean when we talk about evidence. To unequivocally define it as an object with specific parameters and features would be futile: as the following papers reveal, evidence is highly contingent on the contexts in which it is generated. Our point in this section is thus to trace the outline, however faint, of the concept of evidence vis-à-vis existing debates on the topic and in anthropology more generally. It is important to note that although evidence is our explicit focus, the questions we raise here are not new. In exploring them, we encounter our disciplinary ancestors who, by interrogating and redefining anthropology’s methods, epistemologies, and interests, also reshaped our modes of apprehending evidence. This has resulted in a proliferation of evidentiary conventions both at the core of the discipline and along its borders, rather than a clear consensus on what they should entail. Studying these multiple and sometimes competing conventions—with or against which anthropologists have had to work—provokes questions of great reflexive import. These do not merely centre on what evidence *is*, but on how its very nature and guise have evolved along with the objects of anthropology.

A glance at the history of anthropology is instructive in this regard. During the formative years of the discipline, “primitive societies” became the objects of anthropological study; whether as evidence of earlier stages in human evolution, or of social and cultural variability around the world. As various forms of functionalism came to dominate early-twentieth century anthropology, seemingly exotic practices discovered through long-term fieldwork in far-off places became evidence of how particular societies operated as distinct wholes (Kuper 1988). Over time, anthropology moved away from both its original object of study (“primitive society”) and its founding theories (evolutionism and functionalism), while retaining its dominant methodology of participant-observation, advocated most prominently by Malinowski and Rivers (Grimshaw and Hart 1995). But while “fieldwork” has lingered at the core of contemporary anthropological research and disciplinary identity, anthropology’s objects, theories, and scope have undergone multitudinous changes. In the last century, anthropologists have greatly expanded and diversified their objects of study, which have come to include the structuring tendencies of the human mind, symbolic meanings, historical processes, and ethnographic knowledge production itself. These developments, as well as the repertoire of methods accompanying them, have significantly (if sometimes silently) reshaped our understandings of evidence as both a tool and a concept, while also influencing our evidence-gathering practices.

One theoretical development which has had a particularly significant impact on anthropological engagements with evidence is the emergence of reflexivity as an intrinsic component of ethnography. Its entrance into mainstream anthropology in the 1980s took place simultaneously with the rise of postmodernist approaches, which issued significant challenges to the authority with which previous generations of anthropologists had appeared to speak. Such challenges have highlighted the role of ethnography as both producer and form of anthropological evidence, thereby turning ethnographers themselves into part of the object of study. The reflexive nature of much postmodernist anthropology has on more than one occasion come under fire for turning “self-conscious authorial positioning [into] an authenticating device” at the expense of “scientific standards” (Roth 1989, 557). In the context of this discussion, however, some circumspection is necessary.

The growing prominence of reflexivity within anthropology has in no way diminished the conceptual importance of evidence to contemporary anthropological practice (Engelke 2008, S7). Regardless of the prevailing theoretical and methodological climate, the anthropologist’s ability to

marshal convincing arguments depends upon the construction of a body of evidence to support them. The *idea* of evidence thus remains foundational to the organisation and dissemination of anthropological knowledge as a crucial and expected feature of good analysis. What has changed, or at least widened, is an understanding of the possible forms that evidence can assume—including those which blur the ostensible distinction between “self” and “science”. As some of the chapters in this volume reveal, personal engagements in ethnography may become forms of evidence no less rigorous than those produced by a supposedly “detached” observer. Instead of collapsing the distinction between self and other or subject and object, they show that we may take epistemology and the production of anthropological knowledge *themselves* as objects of enquiry and evidence.

In light of such developments, it is unsurprising that the nature of evidence within the discipline has been undergoing increasing reflexive scrutiny. As mentioned earlier, recent anthropological writings have approached the topic from different directions, variously exploring evidence as an epistemological phenomenon inseparable from anthropological experience, a rhetorical and discursive device, or a tool. All, however, are united in emphasising its relational quality, the fact that it does not stand alone as an objective phenomenon, but must be recognised, generated, and evoked within a larger network of connections. And yet this network does not branch out infinitely; at some point it has to be “cut” (Hastrup 2004, 456, after Strathern 1996) in order to separate potentially limitless “data” from what counts as “evidence” (Csordas 2004, 477). But if not all data become evidence, not all evidence merely derives from data. As the following chapters reveal, evidence of and for ethnographic analysis does not only arise from the ostensibly “objective” information amassed in the physical and temporal space of fieldwork, but also from memories, reflections, emotions, writing, reading, creative and documentary processes, and institutional demands. Ethnography, in this sense, is multi-sited (Marcus 1995) in more ways than one.

It is also in this sense that we suggest the papers in this volume make their strongest contributions to discussions of evidence in anthropology. While most voices in the “evidence debate” have thus far addressed the philosophical, analytical, and methodological implications that a focus on evidence might have for anthropology, the contributors to this volume concentrate on the *processes* through which they found, created, and used evidence during their fieldwork and writing. To claim that evidence is what they make of it is not a statement of impudent ambiguity, but a reminder of the situatedness of their accounts. “Evidence” was not simply an abstract ideal that guided our contributors’ ethnographic explorations,

but something that emerged amid the exigencies and contingencies of fieldwork and post-fieldwork experiences. In the process, many of them came face-to-face with evidentiary conventions and expectations within and without anthropology. The papers they have produced are, in that regard, explorations of the role of evidence in their attempts to “get on” with anthropology (Metcalf 2002). In the following pages, we examine these contributions more closely through three key themes around which the volume coalesces: the evidentiary forms that lie “beyond text”, the relation between ethnography and theoretical models, and the role of emotions and reflexivity.

What counts as evidence?

In anthropology, the question of what counts as evidence has been significantly complicated by the expanding variety of methods and sources on which anthropologists might draw. The chapters in this volume attest to this point, engaging directly with the epistemological implications of the often diverse and contrasting forms of evidence that they bring to their ethnography. Rather than treating music, film, history, and personal experiences as raw data, they critically examine how these sources can be used to advance particular ethnographic claims that may both contradict and corroborate our informants’ everyday discourses as well as dominant anthropological models.

Carlo Cubero’s chapter on ethnographic filmmaking on the Caribbean island of Culebra explores how film and the process of filming provide distinctive forms of ethnographic evidence. On the one hand, film footage as an end product can be seen as material evidence of social relations observed in fieldwork—an argument advanced by Margaret Mead and other advocates of ethnographic film who focused on audiovisual media’s capacity to create an objective and durable record of “facts”. On the other hand, Cubero examines how the actual experience of filming during fieldwork became an important “purveyor” of ethnographic knowledge. He describes how the personal, even physical, process of recording musical performances challenged his ideas about Culebra in important ways that informed his subsequent ethnographic writing: even footage which he initially shot as a “visual record ... for future historical reference” eventually led him to understand an unfolding controversy about development on the island in new ways. His paper reveals the dual role of film in recording events and processes as well as allowing anthropologists to reformulate and rearticulate the questions they bring to ethnographic writing.

Cubero's paper raises questions, fundamental to visual anthropology, about the relationship between visual and textual forms of representation (Hastrup 1992; MacDougall 1998). In particular, he explores how these forms of ethnography constitute, rather than simply represent, different kinds of evidence in his research. By focusing particularly on the subjective experience of his dual role as filmmaker and ethnographer, Cubero demonstrates how visual and other non-textual media allow anthropologists to redefine what they take as evidence and how they use it. Most importantly, he suggests that audiovisuals do not merely constitute evidence of "historical facts" in the positivistic sense implied by previous generations of ethnographic filmmakers. Rather, his experience of filming during fieldwork created an equally if not more important form of ethnographic evidence. As with other chapters in the volume (Josephides, Lau, Varley), we see that such experiences and the knowledge they create are no less important or rigorous types of evidence than formal interviews, fieldnotes, or video recordings.

Whereas Cubero explores the experiential and material value of film as evidence, Fraser McNeill's paper suggests that performances themselves are also valuable forms of ethnographic evidence which enable the anthropologist to deal with practices and knowledge normally kept hidden from the public sphere. In this context, music becomes a mode of evidence, revealing knowledge otherwise unavailable to anthropologists who rely solely on discursive and textual sources. McNeill examines the role of music as an important medium through which HIV/AIDS educational messages are communicated in the Venda region of South Africa, where AIDS is "bound to webs of secrecy and suspicion that prohibit its inclusion in open conversation". Since the disease's transmission is enmeshed within witchcraft accusations, silence and avoidance of the topic in everyday life protect individuals from threats of guilt by association with a death. Music, however, has become a key site of HIV/AIDS education due to its wider cultural role as a legitimate and safe medium for the communication of ritual knowledge.

McNeill's paper illustrates that music and other performances beyond everyday interactions and speech can become fruitful objects of anthropological enquiry precisely where other evidence is inaccessible. For McNeill, music allows a new perspective on local attitudes about the disease, revealing "the hidden dimensions that remain elusive in public, and often private, arenas". More than a supplementary source of ethnographic evidence that allows anthropologists to get closer to the "truth", music in the Venda context reveals a distinct form of knowledge production not normally accessible to local people or ethnographers. Like

Cubero's exploration of film and filmmaking as ethnographic evidence, McNeill's paper suggests that diverse media beyond everyday discourse may provide evidence to question previous ethnographic assumptions and contribute to new anthropological knowledge. However, the paper also cautions researchers not to assume music-based evidence to be representative of a particular group. This raises the issue, also addressed by High and Calderòn, of how encompassing models generated by anthropological research are as much a result of what the researcher adopts as evidence as they are representative of "social reality" among the group studied.

Casey High's paper broaches the question of how expanding the scope of ethnographic evidence allows us to rethink dominant analytical models in regional studies. In particular, his paper draws from historical sources and popular cinema as evidence for reassessing anthropological claims about contemporary indigenous Amazonian ontologies and regional history. By juxtaposing his ethnographic research on historical narrative in Huaorani communities in Ecuador with a recent North American feature film about past Huaorani violence, High suggests that while indigenous Amazonian ideas and practices bear certain elements of a generic model of "Amazonian" cosmology, they are equally constituted within a wider social and historical context that extends well beyond indigenous communities.

High describes how past violence is central to both indigenous oral histories and Western characterisations of the Huaorani during much of the twentieth century. Partly because of their famous history of spear-killing raids, the Huaorani have attracted much attention from Christian missionaries since the 1950s. This culminated in the widely-publicised killings of five North American missionaries while they were attempting to establish a mission in Huaorani territory in 1956. Fifty years after this event, and in the aftermath of an intensive missionary campaign in the region, a Hollywood film was released based on the history of Huaorani violence. High explores how the imagery of Christian martyrdom suggested in the film and in missionary texts can also be found in historical narratives told by Huaorani people. His paper engages missionary discourses and images as a form of evidence that allows him to place Huaorani notions of victimhood within wider historical and intercultural relations. He argues that the dominant Huaorani self-identification as essential victims not only reflects the generalised Amazonian model of "ontological predation" advanced in the regional literature (Viveiros de Castro 1996), but must at the same time be seen as the product of a particular history of interaction between indigenous people and missionaries.

High concludes that combining evidence generated in fieldwork and the archives with less localised sorts of evidence, such as popular cinema, can benefit anthropologists who seek to place their ethnographic accounts within a wider social and historical frame. As with McNeill's chapter, we see that engagement with multiple forms of evidence holds significant potential for questioning dominant analytical models within anthropology. The contributions of Cubero and High illustrate in different ways the important potential of visual media in ethnographic and anthropological projects. Both papers point beyond antecedent conceptions of film as an objective representation of "reality" and demonstrate that visual media can be combined with more conventional sources of ethnography to produce new anthropological knowledge. In their own ways, they illustrate that film and other visual media are more than mere aids to ethnographic texts (MacDougall 1998).

More generally, these papers reveal how anthropological arguments, whether they concern claims about ethnographic "realities" or abstract theoretical models, are pivotal in determining what counts as ethnographic evidence. As Hastrup has argued, anthropological knowledge is "relational" insofar as it is "defined as much by epistemology as by the ontology of its object of interest" (2000, 456). Evidence thus does not exist as an objective "substance" or "raw data" independent of the ethnographer's questions and arguments. New kinds of argument necessarily require new kinds of evidence—but also, as Josephides (this volume) reminds us, different ways of (re)interpreting it. The ethnographic cases explored in this volume attest to this relational quality of evidence, recognizing how new and multiple forms of evidence consequently lead to new forms of anthropological knowledge.

Evidentiary conventions and encompassing models

As Engelke notes, evidence is not an easy topic for discussion amongst anthropologists, not least because of the lack of a "well-developed language" with which to do so (2008, S3). Allied to this is the fact that most "evidentiary protocol[s]" (*ibid.*, *passim*) within the discipline have remained implicit despite their decisive role in defining and generating what is recognised as evidence. The work of Engelke and others mentioned earlier, however, reveal a growing disciplinary reflexivity which has not only focused our analytical attention on the concept of anthropological evidence, but also on the conventions by which it is framed, validated, and articulated. More than asking what constitutes evidence, we must also consider how it is generated and recognised as

such—and more intriguingly, how its very definition might shift over time and space.

Marilyn Strathern's keynote address deals with these questions by exploring two comparative methods through which anthropologists and others "reduce, digest and otherwise summarise information in such a way as to yield a yardstick or measure by which other information can be judged, proven or verified." On the one hand, she describes a "mathematics of encompassment" frequently found in Euro-American (and anthropological) settings, whereby persons, examples, and arguments, among other things, are drawn together and aligned as equivalent (comparable) entities within a single framework. On the other, expanding on her earlier work (2006), she explores "analogic reasoning" as a mode of comparison which involves "elucidat[ing] one thing by reference to another", thereby maintaining each element's distinctiveness. She illustrates the difference between these two modes through an array of case studies, from British university promotions exercises to courts of law in Papua New Guinea, using these particular instances—themselves analogies of each other and of anthropological practice—to elucidate our understanding of how anthropological evidence may be constituted.

The resonance of Strathern's chapter derives partly from the fact that like the analogy, it offers no resolution to the incommensurability of different evidentiary conventions. For her, such divergences are better to think with than to solve problems with. For other contributors, however, working with or against certain evidentiary conventions engendered epistemological problems which sometimes demanded difficult solutions. In some cases, this involved balancing the demands of different representational forms, such as film and text (Cubero, High). Others encountered dilemmas over how to reconcile pressing but relatively unconventional forms of evidence that emerged through—and as—personal experience, with mainstream organisational and rhetorical tropes in ethnographic writing (Lau, Varley). In explaining how they worked through these dilemmas, each contributor simultaneously lays bare the often unspoken evidentiary allowances and expectations underpinning anthropological writing and practice.

These examples reveal the sheer difficulty of defining a coherent set of anthropological evidentiary conventions. Yet in a world of increasing interdisciplinarity and "applied" research, anthropology, like many other disciplines, is coming under mounting pressure to articulate, if not define precisely, the epistemological and methodological qualities that set it apart. Whether or not anthropological responses to these recent demands have been productive—and indeed whether the boundary-crossing

impulses behind them have entirely desirable consequences (cf. Strathern 2006)—any discussion of evidentiary conventions within anthropology would be incomplete without some reference to their status vis-à-vis other parties. Indeed, Engelke (2008, S12) posits that anthropology’s growing interdisciplinary and political connections may be causally related to the intensification of anthropological interest in evidence. No longer concerned exclusively, if it ever was, with studying the differences between “us” and “them”, anthropology has now had to extend its attention to the “commons and borderlands” (Strathern 2004) of disciplines, institutions, and other groupings.

Almost inevitably, questions and controversies over evidence surface at these junctures. Evidentiary conventions and expectations are not everywhere the same, even if they often overlap and intersect. This is especially true in many Western knowledge economies, of which anthropology is historically part, and in which the concept of evidence possesses significant, if varied, import. In an age of “evidence-based” policy and medicine, for example, what counts as evidence can influence anything from the allocation of public funds to new environmental laws for reducing carbon emissions. Squaring such conceptions of evidence with anthropological ones can be both trying and illuminating. While anthropologists have long been used to dealing with and even eliciting difference in their engagements with “other” cultures, working with(in) disciplinary or institutional settings which have their own distinctive evidence-making structures can generate other sorts of tensions.

Ann Kelly’s chapter, which derives from her research with a UK-based arthritis research charity on a trial osteoporosis screening programme, brings some of these issues to the fore. Reflecting on her fieldwork, she admits to feeling “woefully inadequate” upon realising that her ethnographic findings would not produce the “rich empirical detail” that she expected would benefit clinical practice. Instead, “the phenomena on which [she] was reporting belonged to a different order than routine clinic research procedure and its practical effects.” The evidence she produced as an anthropologist and the evidentiary conventions of the clinical trials she studied, in other words, simply lay on different planes. One could not feed constructively into the other in a “real-life” clinical setting.

Kelly’s response to such apparent epistemological and methodological incommensurability is instructive when juxtaposed with Strathern’s keynote address, which recounts several similar instances of mismatching conventions. Like Strathern, she turns to analogy rather than encompassment as a means of addressing the tension between divergent sets of conventions. Clinical trials and ethnography, she suggests, might be

regarded as “analogous methodological strategies”, each reliant in different ways on concepts of “everyday practice”. Rather than leaving them thus as mutual illuminators, however, she further argues that “the efforts made by the designers of clinical experiments to transform evidence *of* ‘everyday practice’ into evidence *for* ‘everyday practice’” hold analytical and methodological lessons for anthropologists working in any field. By adopting this perspective, she adds a new dimension to understandings of both anthropological evidence and anthropology itself, thereby provoking a pressing question: what if we were to admit a form of ethnographic evidence that does not simply *represent* reality, but actively shapes and becomes part of it? Put differently, what happens when model and reality are collapsed?

Kelly’s ruminations occupy familiar territory for many anthropologists. The relation between anthropological models and frameworks and the experiences and discourses of the people we work with has variously been a source of consternation, intrigue, and inspiration. Every “gift” (Mauss 1990) or “exemplary centre” (Geertz 1980) has its ethnographic roots; but it only takes a glimpse at their numerous offshoots to realise how quickly they too become the theoretical basis for further ethnography (see Josephides, this volume). On this point, we return to a central theme of our original call for papers: the relation between encompassing explanatory models and ethnographic evidence from “small places” (Eriksen 1995).

As suggested earlier, evidence is only recognised as such *in relation* to other things: questions, theories, disciplines, experiences, persons, and epistemological frameworks, all of which provide the conditions for transforming data into evidence. In the process, these conditions are shaped and reshaped—even created anew—by what is identified and harnessed as evidence. But if ethnographic evidence generates analytical and theoretical models, the reverse is also true, for it may also be used to legitimate and perpetuate them in a variety of contexts quite dissimilar to the ones from which they stemmed. Underpinning this, to use Strathern’s phrase, is a certain logic of encompassment, by which specific persons and events become incorporated and flattened within a broader analytical framework. To a degree, this reflects anthropology’s perennial predilection for comparison, which prevents the discipline from getting mired in innumerable vignettes too particular to be of analytical use. But how far can we go before the opposite occurs, and our ethnographic data only gain salience as evidence for the validity of broader analytical models?

This is the question with which Calderon grapples in her paper, which provides an in-depth account of the rise and fall of an ambitious *vodou* priest in a village in Southern Benin. Eschewing prevailing models of

“tradition” and “modernity” which have generally depicted *vodou* worship as “a convenient study of the effects of globalisation on a local level”, Calderòn presents the story of Sofò as a situated counterpoint to what she sees as an anthropological approach too generically tied to a “meta-narrative” of modernity (Englund and Leach 2000). In the process, she attempts to “render analytical” the categories and discourses used by her informants to discuss the practice of *vodou*, and more specifically, relations with its spirits. Rather than being “subjective and partial representations” (Viveiros de Castro 1998, 478) of what some anthropologists might see as the objectively “real” phenomena of globalisation or modernity, such categories become the very basis of her ethnography: they *are* the real thing. Calderòn thus elides the difficulties inherent in balancing models with ethnographic realities, not by recourse to analogy, but by creating her own micro-narrative which places her ethnography in constructive and critical dialogue with anthropology’s broader concerns and models.

Emotions, experience, and anthropological knowledge

As noted above, one may be justified in saying that reflexivity has grown on anthropologists: reflexive approaches have played an important part in redefining the discipline’s methods, strategies of representation, and theories. We also note that the very concern with evidence constitutes a reflexive moment in both anthropological endeavours and the discipline as a whole. Amidst these wider processes stands old-fashioned social-scientific reflexivity as defined by *The Oxford English Dictionary*: “that which turns back upon, or takes account of, itself or a person’s self, esp. methods that take into consideration the effect of the personality or presence of the researcher on the investigation”. In this volume, three chapters in particular make significant contributions to discussions of reflexivity in anthropology, which we might define more narrowly as recognition of the ethnographer’s subjectivity in the formation of anthropological knowledge. In combination, they address the question of what may constitute evidence. Rather than only envisioning evidence in the form of “fieldwork data” recorded in fieldnotes, these contributions demonstrate that evidence is created relationally and contextually through subjective processes that constitute fieldwork and ethnographic writing.

Emma Varley’s contribution argues that much avowedly reflexive anthropology has remained “constrained by a quiet political correctness” which obscures the more problematic aspects of our selves in fieldwork. Varley argues that if our biographical background, personal interests, and

values influence our ethnographic production, then “our work inevitably reflects or integrates not only positive aspects of our selfhood but also the more problematic or morally uncertain”. Yet, anthropological tropes of reflexivity have been purposefully incomplete, “smoothing away many of our own rough edges”, stressing instead early misunderstandings and *faux pas* which in the long run lead to more enlightened ethnographers and ethnography. Her account of her own fieldwork experience serves as a contrast to what she critically describes as “being generous to ourselves” in only partially reflexive accounts.

Varley carried out fieldwork in Gilgit, Northern Pakistan, where she researched the impact of Shia-Sunni conflicts on the provision of health services to local women. Her research took place amidst two simultaneous zones of strife: communal fighting between local Sunni and Shia factions which escalated heavily during her fieldwork, and communal infighting between herself and a neighbouring Gilgiti Sunni family. Varley’s account does not shy away from those moments and dynamics of fieldwork that include “personal wrongdoing or malintent”, and reveals how her involvement in interpersonal battles shaped relationships with her informants during fieldwork.

Significantly, these circumstances and her actions partially formed her ethnographic perspective and the position from which she was able to produce insights into Gilgiti sociality. On the one hand, her own conflict with members of the Sunni community, centring on her locally contested marital position, elicited insights into how antagonistic relationships are conducted within the community. On the other hand, in the context of Varley’s doctoral research, her own story as an embattled wife brought out related stories from her informants. These provided evidence to question her initial assumption about the communal conflict’s centrality, emphasising instead the importance of domestic relationships in determining whether and how Gilgiti women took up local health services. Varley demonstrates that her ethnographic insights were centrally enabled by her personal tribulations, and thus shows that the challenging events of her fieldwork carried highly significant evidentiary potential for anthropological analysis. She stresses that, if allowed their space as potential evidentiary platforms, such dilemmas within the experience of fieldwork can be important sites of cultural understanding. Her chapter, then, makes an important and original contribution to discussions of reflexivity in anthropology by urging us to include its—and hence our own—more problematic sides.

Where Varley’s chapter suggests the importance of personal fieldwork experiences, however emotionally troubling, to the generation of

ethnographic evidence, Timm Lau's contribution treats emotions as specifically sensitising experiences in the making of anthropological knowledge. Lau presents a key episode during his fieldwork with Tibetans in the diaspora in Northern India, during which he crossed local hierarchical boundaries. He shows that this event engendered an intense emotional experience produced by the gaze of others which emphasised relative social positions. Lau's argument could be understood as an exercise in analogic reasoning (Strathern, this volume), in which his own emotional experience provided a bridge or heuristic relation that made it possible to understand the importance of shame (*ngo tsha*) in his informants' lives. The resulting awareness of local contexts of shame facilitated his analysis of the importance of public visibility and *ngo tsha* for the socialisation of children, behaviour in public gatherings, and the constraints on romantic relationships inside Tibetan refugee settlements. Lau demonstrates that Tibetan *ngo tsha* is centrally connected to the gaze of others, leading to a powerful "emotional aesthetic of shame" for his Tibetan informants. His resulting analysis suggests that "the processes by which Tibetan social actors draw upon *ngo tsha* in social interaction at the same time reproduce the structural features of the social hierarchy".

The intrinsic connection of Lau's emotional experience during a key fieldwork episode to awareness of his Tibetan environment and hierarchical social position is reflected in his analysis of the importance of the gaze of others, and the way in which meaning is attached to notions of shame vis-à-vis hierarchical superiority. Lau's exploration of Tibetan concepts of shame addresses how Tibetan social hierarchy is ingrained on the ground, in Tibetan sociality. His chapter demonstrates that new kinds of evidence in ethnography, in this case elicited by the sensitisation of emotional experience, can be highly productive and significant in answering lingering anthropological questions. As with Varley, the starting point of Lau's exploration is his own emotional experience, which is structurally produced through its local setting, thus making it possible to "see and connect pieces of ethnographic evidence in new ways". Lau advances the notion that in anthropological analysis, the ethnographer's own emotional experience can be of significant evidentiary value through its sensitising capacity, as a portal for evidence of the meaning carried by local emotions.

Both Varley's and Lau's chapters demonstrate that attention to reflexivity and emotional experience is not about "studying ourselves", but about how anthropologists' own subjectivity counts as one of many sources of evidence upon which they may draw in fieldwork. Lisette Josephides' chapter adds to this a longer term perspective on ethnographic

fieldwork and evidence. First, she discusses what happens after fieldwork, by way of what she calls “virtual returns”, when evidence is produced away from the field through processes of contemplation and recollection that involve fieldnotes, emotions, and other elements. Secondly, she reflects on her own long-term trajectory as a social anthropologist, demonstrating shifts in how she collected and used evidence in two books from distinct stages in her career. Her paper gives a nuanced and in-depth discussion of reflexivity and subjectivity which reveals how her individual research prerogatives influenced or determined what she took as evidence.

Josephides introduces the concept of virtual returns through Wordsworth’s notion of “recollect[ions] in tranquillity”, in which memories of events are contemplated in such a way that the essence of the experience remains in one’s consciousness as other aspects of it fall away. She demonstrates that recollection in times away from the field can be enriched in numerous ways—whether through memories and dreams of emotional importance, or books read after fieldwork. Josephides makes an important contribution in showing that these influences “in fallow periods” may lead to different ways of conceptualising evidence, long after the initial fieldwork is done. In this context, her own material suggests with great openness that fieldnotes are an edifice we construct in fieldwork which has the potential to contain evidence outside and beyond our initial reading. Like the ethnographic film rushes described by Cubero, such entities contain more information than that which they were originally intended to convey. For Josephides, fieldnotes, while being constructed in the phenomenological context of the “world of the ethnography” and its expanding horizons, can transcend as well as contain the emotional context in which they are created.

The chapters by Varley, Lau, and Josephides share common ground in working to integrate the ethnographer’s subjective experience into the evidentiary realm—the realm of that which is admissible to “think with” in conventional anthropological analysis. All three are also highly reflexive pieces for anthropology itself. Varley admonishes anthropologists to own up to their shortcomings and in turn realise the latter’s evidentiary potential. Lau ends his chapter on an analogy between anthropological and Tibetan social practice, pointing out that in both cases the treatment of shame secures present hierarchies. Josephides’ contribution goes even further by turning the epistemological itself into an object of anthropological enquiry.

Conclusion

The contributions to this volume collectively raise the question of how we might conceptualise evidence in fieldwork, ethnographic writing, and the processes of knowledge production within anthropology. The chapters provide ethnographically grounded examples of how multiple forms of evidence—including film, music, emotions, and the very processes of creating ethnography—allow and indeed demand a rethinking of disciplinary conventions and analytical models within anthropology. The inevitable corollary of this process, however, is the need to rethink our understandings of anthropological knowledge and practice. The often unspoken evidentiary protocols which determine what and *how* anthropologists know (or claim to know) are inextricably tied to shifting conceptions of the very object and scope of anthropology. As discussed earlier, what counts as evidence has been the very locus of change within the discipline. We suggest that by the same token, future shifts in anthropological theory and methodology will be informed by changing conceptions of the nature of anthropological evidence. In making visible and giving analytical purchase to the shifting evidentiary basis of the discipline, the papers that follow offer glimpses of the mutable and multifarious shapes that anthropological knowledge takes.

A fundamental contention of this introduction is that discussions of evidence are inexorably linked to the ethnographer: “how we know” is deeply entrenched in “who we are”. By this we do not mean to (re)invoke a navel-gazing reflexivity that turns the anthropologist into the object of study. Instead, we make the case that ethnography is ultimately the product of a historical, social, and personal assemblage which includes not only the ethnographer’s person, but also one’s intellectual background, institutional demands, conceptual genealogies, and relational quirks within and beyond the field, to name but a few. Exploring the importance of evidence in current and future anthropologies thus means critically considering the role played by anthropologists in *creating* rather than simply uncovering it.

This notion is not alien to contemporary anthropology. Indeed, to the extent that anthropology is now widely understood as a written mode of representation (Clifford and Marcus 1986), it has become somewhat axiomatic. In critically thinking through the concept of evidence, however, the chapters in this volume provoke new insights into other aspects of anthropological knowledge-making in which the anthropologist is invariably entangled. Such insights can both supplement and nuance contemporary debates on the nature and practice of anthropology, by

highlighting the ways in which ethnography holds new promise for questioning the potentially procrustean power of abstract models in anthropology and beyond.

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OLD AND NEW REFLECTIONS

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Let me begin with an anthropologically obscure corner of the world, and with one of its practices. It is a practice that summons a model of encompassment, for it purports to give a measure of a person by adding up the elements of the person's accomplishment (cf. Hoskin 1996); indeed, literally added, since the result is a score at the end of the day, I want to suggest that ways of measuring persons yield insights into how anthropologists might take the measure of the societies and cultures they study, that is, arrive at some kind of summative or summary typification of the nature of social life. Summaries may be global, totalising, definitive, while variably encompassing or not encompassing in their effect.

My example comes from the University of Cambridge and its procedures for academic promotion. What is interesting from our point of view is the role given to evidence. The adjudicators have to score the academic's research performance—originality, reputation, and so on. However, what they score is not what they think of the candidate's originality, reputation, and such, but whether or not there is *evidence* in the paperwork. That is, there needs to be mention of these qualities in the references and other supporting documents. It makes sense—the adjudicators may not be specialists, and attention on the paperwork eliminates anecdote and makes other kinds of knowledge extraneous.

An observer—an ethnographer in the anthropological sense perhaps—might remark that this notion of evidence embodies procedure in a documentary form (Riles 2006) with its own codes of presentation. Like an oracle, it gives the procedures an authority—you might not know if the scholar has done original work, but should one of the referees mention the term, there is the evidence, strong or weak as it may be. Moreover, its own scale—C, S, D: “Clear evidence”, “Satisfactory evidence” and “Doubt” [about whether criteria are met]—offers the adjudicators a formula through which to express their opinions without having to elaborate them. In effect their opinions take statements in the paperwork as the evidence. “Evidence” as an explicit focus of attention substitutes for other kinds of factors that might contribute to a decision.

Is this not a rather odd use of the term? We live at a time when “evidence” carries huge politico-cultural freight—“evidence-based” medicine is not the only mantra that rings in our ears. Much of it is to be welcomed; that is not the point. The point is that however anthropologists might wish to deploy the term in research settings, it has other lives elsewhere, including no doubt for the university academic elsewhere in the university, and takes diverse forms. On my desk, weighing far too much from its thick paper, is a mammoth volume of glossy coloured photographs of the natural world, presenting the arguments for Creationism in a form heightened into medieval splendour with all the lasciviousness of modern imaging technologies. The *form* is that of scientific evidence.

Dividing evidence from evidence

Our observer has reminded us of the world in which anthropologists operate. Relying on his or her language of exposition as a source of analytical terms, the anthropologist’s refinement is to draw a guiding distinction between different contexts. On the one hand is a specific cultural form: in English “evidence” as a special mode of presentation is also a technique for sifting data from data, the contours of a file into which some but not all information will fit. There is an old insight here, namely that only certain presentations of information will count as evidence. For English speakers this is familiar from the law and is encountered wherever you hear the term “evidence-based”. On the other hand, evidence can be taken by observers for their own purposes as the grounds for knowledge, the unfabricated, pre-constructed facts of a case always available for further scrutiny. Here the relative rawness of what is a greater or lesser fact is of course open to infinite regress. In either case, to talk about evidence will be to establish a line, create a division, between what is/is not recognisable or admissible as such.

So what is going to qualify for analytical usage? The English speaker has no problem in managing multiple connotations in his or her own language but to make the term do analytical work is another matter. How explicitly, for example, does there have to be a corresponding concept in a local repertoire? In his classic comparison of Merina and Zafimaniry in Madagascar, Bloch (1975) found that he could analyse the economics of both in terms of “labour”, but for only one of them could he say that they made labour an explicit grounding principle for themselves. What, by analogy, might we wish to make of the concept of evidence?

One of anthropology's preoccupations is about just where the ethnographer is going to make divisions between information presented as having undergone minimal processing ("data") and ethnographic knowledge pressed into modes amenable to exegesis in order to specify the context of explanation ("analysis"). This is a working problematic rather than a problem to be solved, for the Euro-American scholar shifting between contexts is ubiquitous to knowledge-making. The construction of an explanation involves distinguishing contexts by typifying or summarising sets of information. In short, "evidence" requires separating kinds of information from one another. Leaving aside the notion of "context", let me adapt this formulation to at least make it possible to see corresponding forms elsewhere.

I propose taking "evidence" as a construct pointing to practices—whether undertaken by the anthropologist or his or her subjects—that imply the ability to reduce, digest and otherwise summarise information in such a way as to yield a yardstick or a measure by which other information can be judged, proved or verified. It thus merges the two senses sketched above in the general process by which matching pieces of information to one another selects or privileges such information as is amenable to being summarised. This draws attention to the mathematics entailed in the process.

Those promotions protocols deploy evidence in a specific way through a mathematics of encompassment—the notion that a person's worth [for these purposes] can be added up as the sum of his or her parts, that lots of little scores taken together will yield a bigger score, as one might imagine adding together 3, 4 and 5 making a sum of 12, or 3*, 4* and 5*, coming out with a summative 4*. I use "encompassment" for its connotations of an embrace, of everything (of importance) gathered together.¹ It used to be thought that societies and cultures were amenable to such gathering, that the more bits one described the more one knew, in the sense of having enough material to arrive at some synthetic typification of their fundamental characteristics. Indeed this was one way in which the very idea of "societies" or "cultures" as singular, definable entities was created.

With this in mind I want to see how far our anthropologist would get in situations where people do not add up their accomplishments in quite the same way, where there is no sum of parts. Local mathematics is relevant. For example, if "one" is the largest number there is, all multiples being fractions of one, then it is division that creates magnitude, while each specification of magnitude will always fall short of the totality (one).² In that kind of situation one has to ask what it means to typify or summarise, and whether people look for anything like "evidence" in one another's