

Who owns 'culture'?¹

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Abstract

*No one owns the concept 'culture'. Anthropology's long-standing proprietary claim on the concept rests on three sorts of contentions – none of which are convincing. Anthropology's overwhelmingly interpretive approaches to cultural materials have led to a preoccupation with the details of cultural materials at the expense of formulating explanatory theories. This has, among other things, rendered fieldwork experience sufficient for professional credentials. However, if the details are all that matter, then comparative and cross-cultural research, as well as most of the social sciences, make no sense. Contrary to this view, it is proposed here that theories reveal which details matter. Cognitive accounts of the sort we advanced in *Rethinking Religion* (1990) offer a firm theoretical basis for cross-cultural study of religious materials. Other types of research concerning non-human primates, early childhood development, and various social and cognitive impairments also offer insight into culture (without relying on fieldwork studies).*

1. Introduction

No one owns 'culture'.² Anyone with a viable theoretical proposal can contend for the right to determine that concept's fate. Not everyone, however, agrees with this view. Throughout its century-long struggle for academic respectability, anthropology has regularly insisted on its *unique* role as the proprietor of 'culture'. Its variety of approaches and feuding factions notwith-

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 2. In this paper we will follow the American philosopher's convention of referring to concepts by enclosing the corresponding terms for the concepts in single quotation marks. Thus, 'culture' refers to the concept of culture.

standing, it is this proprietary claim that unifies anthropology to an extent sometimes unrecognized even by its own (post-modernist) practitioners. The history of anthropology has witnessed at least three important moments in the case for autonomous cultural phenomena based, first, on traditional ontological and methodological presumptions, second, on the hermeneutic turn, and third, on post-modern analyses of discourses and their influences.

Historically, anthropologists cite two closely related bases for these proprietary presumptions. The first, which we shall not belabour here, harkens to inevitably vague discussions about culture's autonomy (with various passes at making sense of the *ontological* foundations of that alleged autonomy). Cultural anthropologists have advanced such claims for a century, but Clifford Geertz' gloss on this topic is representative both in what it endorses and in the vagueness of the grounds for the endorsement. While advancing a host of claims about culture's ontological status (for example: [1] that culture is "ideational"; [2] that it, nonetheless, "does not exist in someone's head"; [3] that it has the same status – whatever *that* is – as a Beethoven quartet; and [4] that it is "public"), Geertz insists that "the thing to ask ... is not what ... [its] ontological status is" (1973: 10-12). Unfortunately for Geertz and cultural anthropology generally, any convincing case for the autonomy of culture must account for its relations to the things that constitute it. Moreover, because Geertz never relinquishes anthropology's scientific aspirations, the issue of clarifying such ontological questions will persistently arise (McCauley, forthcoming).³

Traditionally, the second basis for anthropologists' alleged ownership of 'culture' concerns the *methodological* consequences of their presumptions about cultural autonomy. Historically, anthropologists have supposed (1) that both cultural wholes and whole cultures exceed the sums of their parts, (2) that culture enjoys a dynamics of its own that could never be reduced to the various decisions of its individual participants, and (3) that the anthropologist's job is to analyze culture's structures and functions in terms of uniquely *cultural* categories.

More recently, whether supplementing or supplanting the historic argument, interpretive anthropologists have emphasized the notion of 'culture' as publicly shared meanings that are not subject to reductive explanation, but provide the very framework in which all explanation, indeed, all human endeavour takes its shape. Explicating culture requires understanding, in any particular case, the myriad details that enrich the hermeneutic quest. While

3. We are pragmatists about ontology. Claims for the autonomy of culture fare neither better nor worse than the relative *explanatory* success of theories that confine themselves to quintessentially cultural concepts and those – addressing the same phenomena – that do not. To the extent such theories address the intellectual and practical problems that provoked such inquiries in the first place, their ontological posits merit our allegiance.

this move offers access to 'culture' to anyone with interpretive skills, legitimacy as a cultural commentator also requires a fieldworker's familiarity with the particulars.

In the hands of most interpretive anthropologists, the insistence on the critical character of the details of cultural contexts for the understanding of any particular cultural phenomenon has carried an additional implication. Not only does the interpretation of specific cultural expressions require attention to their cultural settings, all cultural phenomena are fundamentally embedded in webs of significance so extensively and profoundly intertwined that analytical treatment can neither suitably extract these phenomena from their contexts nor formulate convincing generalizations about them in isolation. The upshot of all of this, which we have dubbed "hermeneutic exclusivism", is that the central prominence accorded contexts and their details has thoroughly discouraged theorizing about what seem to be widespread cross-cultural forms (such as religion). The webs of meaning in which we find ourselves suspended are, in fact, webs in which we find ourselves so *bound* as to preclude the possibility of explaining human behaviour by means of general principles.

In the past fifteen years or so, versions of hermeneutic exclusivism have emerged that are even less friendly to explanatory theorizing. Since, on the hermeneutic view the study of culture is fundamentally interpretive, in recent years cultural anthropology has increasingly taken its inspiration from interpretive studies, instead of the sciences. Thus, post-modernism, reflexive anthropology, cultural constructivism, and the like, are less reactions *to* than apotheoses *of* the hermeneutic turn in anthropology. They permit what earlier hermeneutic exclusivists only dreamt that they could be.⁴ On these models of inquiry, all cultural forms are, finally, texts – embodying forms of discourse – in need of interpretation. But interpretation itself is a never-ending process. "The constructivist view that culture is emergent, always alive and in process is widely accepted today. . . . What all proponents have in common is the view that the meaning of the text is not inherent in the text but emerges from how people read or experience the text" (Bruner 1994: 407). Crucially, those readings and experiences depend pivotally on the creativity, the resources, and, most importantly, the circumstances of the reader.

The valuable contributions of anthropologists of this stripe have been, first, to trace how various cultural forms render some readers and *their* experiences invisible (let alone more pernicious denials of their humanity) and, second, to highlight the resulting impoverishment of cultural inquiries. Where they have

4. It follows, incidentally, that the post-modernist trappings surrounding this movement are largely incidental. All of the pivotal philosophical commitments were already present in anthropology's hermeneutic turn twenty-five years ago.

gone wrong, though, like their more conventionally hermeneutic predecessors, is in thinking that the creation and explication of increasingly wondrous webs of meaning exhausts cultural inquiry. We have argued elsewhere that this is only half of the story (see Lawson – McCauley 1990; forthcoming; and section 2 below). The resulting neglect of and disinterest in formulating systematic, empirically culpable theories on the part of post-modern cultural anthropology has created a vacuum that biological reductionists, such as sociobiologists, have been only too glad to occupy in the name of science.

It is exactly because they have failed to contest the notion that anthropologists own the concept 'culture' that scholars of religion have, for far too long, felt shy around their anthropological colleagues. By conceding 'culture' to the anthropologists, they have placed themselves either in the subservient position of passive recipients of anthropological reports or in the unenviable position of trying to match the anthropologists at their own game – by learning languages, performing fieldwork, and studying cultures' histories.⁵ In light of the considerations we have raised in the previous paragraphs, matching anthropologists includes skillfully interpreting cultures at least and probably demonstrating ample sensitivity as well both to the ability of cultural forms to oppress – frequently in ways that are nearly invisible to most observers – and to the creative dimensions of individual experience in cultural transactions. However valuable these endeavours are, when pursuing them involves – as it so often does in their post-modern incarnations – eschewing overt attention to theories about cultural systems such as religion, they trap scholars of religion into conceding (1) that they cannot fruitfully study religious materials at any high level of abstraction either from their cultural settings, or from the wielding of power, or from the intimacy of personal experience and (2) that, therefore, religion is not subject to any penetrating cross-cultural analysis. These concessions undercut the long tradition of studying religion comparatively. In effect, then, exclusively pursuing interpretive endeavours in the study of religion plays right into the anthropologists' hand. It dooms scholars of religion to the status of anthropologists' half-prepared, junior colleagues (though such confusions about the character of the study of religion is nothing new; see Lawson – McCauley 1993).

Against such claims for the ownership of 'culture', we hold that *on its own* cultural study *of this deeply interpretive sort* (whether by anthropologists or their imitators) has not enough theoretical capital to keep either the payments or the property up. Since it is too late to return 'culture' to the state of nature, we advise at least placing it in the public domain. We aim to contest the notion

5. We should clarify from the outset that we enthusiastically endorse being more informed rather than less about anything anyone seeks to study.

that anthropology either owns 'culture' or is capable of its purchase solely with coin of the interpretive realm.

In a recent article (Lawson – McCauley 1993) we criticized both religious studies and anthropology for assuming that concentrating on hermeneutics would insure both their methodological soundness and moral correctness. We argued that *cognitive* approaches to the study of religion are far more likely to achieve these goals. We also chastised scholars of religion for their confusions about the status of their own enterprise and argued that an exclusively humanistic program of religious studies relying only on interpretive techniques will render some religious phenomena virtually invisible. An exclusively hermeneutic approach is blind to certain religious phenomena that resist the hermeneuticists' textual metaphor. In actual practice, accounts of religion as "textual" are finally distorting. (Ironically, the philosophical hermeneutics of Heidegger, Ricoeur, Gadamer, and others from which these analyses, at least in part, take their inspiration emphasize some of the forms and features of human praxis to which the interpretive analyses of anthropologists and scholars of comparative religion remain, all too often, inattentive.)

By contrast, in this article we aim to reassure scholars of religion that they can escape these traps. Specifically, we hold (1) that scholars' cross-cultural insights about religion need not be suspect methodologically and (2) that religious systems can be studied comparatively. The crucial point, though, is that *these outcomes* require relaxing hermeneutic inclinations to subordinate, let alone inclinations to eliminate, explanatory theorizing.

In the second section we consider some implications for cultural anthropology of the current obsession with interpretive approaches to its materials. The anthropological search for meanings feeds on new information about ethnographic details and cultural settings. In such an atmosphere, fieldwork becomes an end in itself. *Being there* has become virtually sufficient for professional credentials. This professional focus places a premium on cultural diversity. Documenting the details of a culture that is largely like some other offers little interest. New, surprising, unexpected details are the fruit from which juicy new meanings are most easily squeezed.

But taste is a different matter. Too often interpretive anthropology neglects the theorizing necessary to distinguish the sweet fruits from the bitter. Theories of culture and culture's systems are pivotal for discriminating among the details, i.e., for deciding which details *matter*. The most important role of fieldwork is to develop anthropological imagination and judgment – not merely to formulate new theories but to formulate *improved* theories. An anthropology that subordinates the formulation and evaluation of explanatory theories to the quest for ever-deeper meaning inevitably impoverishes itself.

The third section focuses on further implications of the hermeneutic approach to culture study. The most general implication reveals an important

and suggestive asymmetry between the story *this version* of anthropology tells about itself and the stories the other social sciences tell about themselves. If the position of interpretive anthropologists is right, at least when pushed to its logical extreme, then it looks as if the other social sciences (economics, political science, linguistics, etc.) have got things mostly wrong. The disproportionate emphasis on meanings and their critical dependence on cultural context renders the cross-cultural study of various cultural forms problematic.

We, then, briefly list in the last section some alternative approaches to 'culture'. There is, perhaps, no more telling evidence against claims about the ownership of culture than the fact that other disciplines have means for investigating the human world that seem to have clear implications about what is cultural. There are more ways to gain insight about culture than interminably cataloguing the details of one place after another.

2. The hams in anthropology

In his article, "The stakes in anthropology", Ernest Gellner (1988) suggests that American anthropology especially has become addicted to the search for meanings in cultural materials. Gellner echoes Dan Sperber's (1975) declarations that supplying interpretations of symbols' meanings compounds rather than solves the anthropologist's problems about symbolism. Meaning, in short, is the *problem*, not the solution. Gellner, however, recommends against the outright prohibition of further hermeneutical pursuits, since interpretive methods, when used with moderation, play a legitimate role in anthropological inquiry. Instead, Gellner proposes establishing Hermeneutics Anonymous – an organization devoted to encouraging sobriety in all matters meaningful. This would thwart the excesses of hermeneutic exclusivism.

All hermeneuticists see themselves as inhabiting a world of "texts", in which they propose minimally, to subordinate explanations to interpretations. Perhaps not all hermeneuticists have kidded themselves into believing that everything is a text, but they have unwittingly set the stage for such extravagant post-modernist claims (McCauley forthcoming). Like Gellner, we suspect that such extravagance may undermine the very possibility of rational inquiry. At the very least, we think that hermeneuticists and post-modernists, by subordinating explanation to interpretation, overlook the productive interaction of interpretive and explanatory endeavours. We hold that the success of one *necessarily* depends upon the success of the other and, therefore, that subordinating explanation, let alone rejecting it the way hermeneutic exclusivists do, amounts to a fundamental misunderstanding of the generation of knowledge (Lawson – McCauley 1990: chapter 1). We do *not* claim that

searching for theoretical explanations of cultural phenomena that appeal to systematically related principles of general form is either the only or the premier ideal of inquiry in this domain, but we do hold that it should be subordinated to no other.

A scientific study of culture includes the search for its pervasive features, i.e., so-called cultural universals, but, in fact, a cultural form or system need not be universal to be interesting theoretically. Within an evolutionary perspective on culture, cultural forms need not be universally distributed throughout the relevant population (any more than some biological trait needs to be universally distributed throughout a species). All cultures need not have capitalist economies for capitalist economies to be proper objects for theoretical inquiry and for economies generally to be socio-cultural systems capable of isolation for analytical and explanatory purposes. Typically, "universals" simply refers to widespread cultural forms and systems, and on an evolutionary account that is all it need refer to.

From the standpoint of an evolutionary framework "[i]t is precisely the point of an explanatory theory to reduce diversity and to show in what manner it results from the encounter between general mechanisms, on the one hand, and many diverse circumstances on the other" (Boyer 1994: 7). The critical achievement is to specify the underlying mechanisms capable of generating the diversity of existing forms in interaction with assorted environments. In the biological case the central mechanisms concern the replication and mutation of the genes – as this is shaped in the process of natural selection. In the cultural case we suspect that many, maybe most, of the pivotal mechanisms are psychological.

As in all science, such hypotheses direct empirical investigations into increasingly rarefied territories where they unearth anomalies that not only will not go away, but that constitute straightforward counter-instances to cherished hypotheses and assumptions. One of the reasons that fieldwork is so central to the training and credentialling of anthropologists is that fieldwork is what turns such anomalies up.

Fieldwork is difficult and demanding. Anthropologists often spend years in settings that are inhospitable and sometimes downright dangerous. They must not only avoid offending their hosts, they must develop sufficient rapport with them to obtain esoteric information about their culture. While coping with the unusual, they must also closely observe. Then, ideally at least, they must write about these often intimate experiences as if they are detached, "objective" observers (Geertz 1988: 10).

The presumption is that deep cross-cultural *understanding* depends upon *immersion* in some foreign setting. Cultural anthropologists earn their credentials by showing that after considerable work and effort they can render the exotic understandable. From learning a completely unfamiliar language to

eating slugs and bugs, the difficulties of fieldwork exact a considerable toll. In light of that toll it should surprise no one that such immersion in an unfamiliar culture has become a necessary condition for professional authority in cultural anthropology.

What might come as a surprise, though, is that fieldwork has virtually also become a *sufficient* condition for professional authority. Attending closely to detail, admittedly integral to fieldwork, has developed a life of its own. To a considerable extent, the means have swallowed the end, the process has replaced the product. The hallmark of talks by young anthropologists anxious to demonstrate their competence is a slide show with a running commentary about invariably *small* details in the pictures that need not end up having any connections whatsoever. Reports of ethnographic details on the basis of first hand experience have not only become a central foundation of professional authority, they have also become the necessary accoutrement to *any* discussion of cultural matters – whether or not those details are at all relevant to the cultural system in question. Traditional anthropology offers ample precedent. For example, what precisely *is* the point of Evans-Pritchard's picture of "youth and boy" (plate vii in *Nuer Religion*)? (See Geertz 1988.)

Interpretive anthropology has reduced the study of *culture* (by studying cultures) into the study of *cultures simpliciter*. An imbalance favouring interpretation over explanation has in the practice of the hermeneutic exclusivists evolved into an imbalance favouring ethnographic reporting over theorizing. Increasingly, cultural anthropology, even versions with overtly scientific aspirations, has tended to sacrifice the formulation of general theoretical proposals to the celebration of the details, the exaltation of the idiographic, and the veneration of the context. This encourages high-spirited symbolic anthropology, flush with resources for divining ever deeper layers of meaning in cultural materials.

One desideratum for distinguishing top notch work from the also-ran is whether or not the details turn out to be surprising. If details are good, exotic details are better. They only seal the anthropologist's reputation as a skilled interpreter of culture. Like a good travel guide, the anthropologist renders the apparently baroque and bizarre understandable.

Ever since the discovery that some cultures do not possess the Western notion of modesty, the shock value associated with documenting cultural diversity has hardly diminished. Most anthropologists, though, are not so benighted by post-modernist excess as to have lost *all* sight of scientific possibilities. Fortunately, their interest in the unusual does not merely reflect a penchant for showmanship but their persisting, but all too often, suppressed concern with science as well. Exotic details are exotic because they challenge explicitly formulated hypotheses about general features of culture or, perhaps even more significantly, because they defy tacit presumptions we all bring to

our reflections on alien cultures. But exotic details are even more interesting when they prove just as susceptible to some theory's analysis as do far more familiar cultural phenomena. These days, though, professional fame does not ordinarily accrue to the researcher who suggests that the apparently fantastic is actually commonplace – that it is nothing but a further manifestation of cultural dynamics some theory has rendered familiar in contexts closer to home. Emerging from the bush only to report that some little known group is a lot more like us than meets the eye is not fashionable.

Roger Keesing offered grounds for hesitation about becoming entranced with bizarre details, noting that “[a]nthropologists, with their predilections for the exotic and their predispositions toward, even vested interests in, depicting cultures as radically different from ours and from one another, are prone to choose readings that fit these expectations and interests” (1987: 162). Keesing cautioned against reading too much into other peoples' conventions for talking about their experiences and mental lives. He argued that the more theoretically significant discovery would be to learn that broad cultural diversity rests on fairly mundane processes. Keesing cited, for example, Lakoff and Johnson (1980), who account for pervasive structuring of experience in terms of relatively simple metaphoric comparisons, many of which arise from basic bodily experiences that all human beings share (for example, construing anger in terms of contained heat – typically, the heat of a *fluid* in a container).

The cognitive approach to religious materials that we have pursued employs the same sort of abstemiousness concerning symbols and their interpretations. Not only do such cognitive analyses explain some aspects of cultural diversity and creativity in terms of the perfectly ordinary, but they also delineate features of the underlying cognitive mechanisms responsible for the phenomena in question. In *Rethinking Religion*, for example, we have shown how participants' representations of religious rituals piggyback on quite common cognitive means for the representation of actions generally. We also specify a relatively small collection of principles that capture the representational capacities employed (1990: chapter 5). Pascal Boyer's *The Naturalness of Religious Ideas* (1994) provides further (and numerous) arguments and illustrations of how thoroughly normal patterns of cognitive development can bear most of the explanatory responsibility for the retention, recurrence, and perpetuation of the various unusual, “counter-intuitive” commitments characteristic of religious systems.

Scientific progress always involves an on-going interaction between theorizing and attending to observational detail. *The trick is in knowing what details count*. When identifying the most prominent achievements in the history of science, the focus reliably falls on the development of particular theories and the startling observational findings and experimental results they provoked. We know that neither the ages nor the colours nor the atmospheric

contents nor the thermal properties of the planets have anything to do with either the explanation or prediction of their relative motions, because of the success of the theory Issac Newton formulated that identifies the important variables.

Excessive interest in detail for its own sake has caused anthropology to lose its moorings, because it has led it to neglect theorizing. Neglecting theory is deadly from a scientific standpoint, because *it is precisely the confrontation of competing theories that determines which details matter*. Consequently, the theories with which scientists operate, whether consciously or not, determine which details will receive attention.

Sir Arthur Eddington undertook his famous expedition at considerable expense far from the shores of Great Britain, because the concern to adjudicate rationally the conflict between two of the most important physical theories in human history, namely classical mechanics and special relativity, required the observation of a very specific celestial event which was only possible at very specific points on the globe at very specific times. The *conflict between these two theories* made the apparent positions of stars in the sky close to the sun during its eclipse *important details* for deciding which of the two better organized a vast array of physical phenomena that extend *far beyond the specific events observed* by Eddington. It is not as if in the first decades of this century scientists did not already know a great deal about the sun and its eclipses, about light and its propagation, and about stars, gravitation and a host of other related celestial and physical phenomena! All of that knowledge, though, did not include *the details that were critical for advancing knowledge at this juncture*.

Second generation fieldworkers would not have much to do, if the first generation had all the right theories and, therefore, had focused on all of the right details. Not coincidentally, the hallmark of second generation fieldwork is revisionism. Revisionists approach the previously studied culture with alternative hypotheses in virtue of which they ascertain that their predecessors either organized the details incorrectly, focused on the wrong details, missed critical details (that the new hypotheses authenticate), or some combination of these three.

A further problem with the veneration of context and the resulting neglect of explicit theory is that the theoretical perspectives informing these revisionists' judgments are not usually the objects of direct reflection and thus are often not even consciously entertained. Without open recognition of the underlying theoretical competition at stake, these disagreements look like unmotivated or (worse) ideologically-motivated squabbles about the facts. Absent the self-conscious comparison of theories, second generation fieldworkers are simply vying for the professional limelight. The stakes in anthropology are too rare to settle for mere hams.

If fieldwork and the knowledge of cultural details it fosters become the ends of anthropological research, then it will be the end of anthropological research. From the standpoint of a social *science*, celebrating contextual details is just not enough. Such details may provide the means for assessing existing theories; however, their nearly uninhibited celebration has eclipsed two fundamental tasks critical to advancing the understanding of culture.

We have already touched upon the first. As a result of this overwhelming focus on the idiographic, anthropologists too often hold their theoretical presumptions unreflectively, which is to say, although they bring biases to their fieldwork experience, they have little understanding of their genesis, rationale, or organization (if any exists). Theories organize inquiry; explicit theories organize inquiry explicitly. The problem is that, all things being equal, it is better to hold positions reflectively rather than unreflectively in order both to decrease the sort of squabbling described above and to increase the efficiency, the productivity, and the civility of anthropological discussion.

Second, and perhaps even more importantly, this proclivity of anthropology has also obscured the obligation of scientists to speculate, i.e., to formulate *new* theories. Scientists do not study the details of the world merely to assess existing claims about it. If that were the only point, such study would have ceased long ago. For as Kuhn (1970), Feyerabend (1975), and other philosophers of science have noted, every theory, from its inception, faces counter-instances. Science does not progress in any simple-minded way. Theories are more resilient than metal ducks in a shooting gallery. They do not flop over from the glancing hits of occasional counter-instances. Social science, in particular, requires the informed judgments of experienced inquirers – looking behind the appearances, sifting through the facts, marshalling their practical knowledge, considering and ranking alternatives by both judging and weighing divergent evidence, explanatory power, relative scope, suggestiveness, simplicity, and more (Thagard 1993). This is why fieldwork experience is so often helpful.

We have nothing against gathering information from the field, and we fully recognize that theoretical proposals about cultural systems must answer to the ethnographic facts. But we also subscribe to the well-worn hermeneutical insight that experience and conceptual schemes (and observation and theories) are interdependent. The point, in short, is that what facts matter and where researchers look for them is a function of an on-going negotiation between the theoretician and the ethnographer operating as *equal* partners.

Finally, the most important motive for fieldwork is not its ability to arm the ethnographer with counter-instances with which to club prominent theories, nor even its ability to corroborate preferred theories, but rather its role in educating the anthropological imagination. The progress of science turns not

on the proliferation of *mere* speculation but on the proliferation of *informed* speculation. Researchers' familiarity with the facts and their considered judgment are what inform speculations. Those speculations typically take the form of inferences to the best explanation (Peirce's "abductive inference"). From these origins more sophisticated theories take shape. The continuing goal is not only to formulate new theories but to formulate *better* theories on the basis of the comparative insights that fieldwork provokes.

Fieldworkers provide thick and intricate descriptions firmly rooted in first-hand knowledge of the details of different ways of life. The hope is that these analyses will divulge patterns of sufficiently general significance to aid understanding in other cultural settings. The danger of analyses so firmly rooted in particular circumstances is precisely that they resist generalization. Hence, as Geertz (1988) has noted, anthropologists face a rhetorical dilemma, if not a logical one. They must display their intimate knowledge of the ethnographic details while demonstrating that the analyses that emerge from that intimate knowledge do not hang on it essentially.

Another limitation of this approach is that *the details go on forever*. Most limitations on and uniformity in the details of ethnographic reports are overwhelmingly a function of common general assumptions with which virtually all anthropologists operate (largely unconsciously) about what matters in a culture (kinship, social roles, rituals, myths, legitimacy, traditions, and more). We should emphasize straightaway that *we do not begrudge them these assumptions!* On the contrary, they are the ultimate sources of most telling ethnographic comparisons. The problem is that fieldworkers who are not explicitly aware of these theoretical assumptions and their implications have no clear guidelines for determining which details count and when they can stop collecting them. The satisfactoriness of a description is always judged relative to a theory. Thus, for example, because we have proposed a theory of religious ritual (1990) that employs some assumptions at odds with those many cultural anthropologists prefer, we have found that, despite the myriad details of their ethnographic reports, they frequently do not contain much critical information that is relevant to the questions we are asking.

Although anthropology holds novices' feet to the fieldwork fires, the discipline seems considerably less vigilant about practitioners' subsequent works once they have been initiated. Comparative ethnographic studies have been known to report on groups with which the authors have had no direct encounters. Geertz, for example, notes that Ruth Benedict had no first-hand experience with two of the three groups she discussed in *Patterns of Culture* (Geertz 1988: 112). Her reputation secured, Benedict was professionally free to pursue *comparative* ethnography.

The crucial point is that we have just been sketching a case for why this is *perfectly acceptable* – if the inquiry is overtly theoretical (as opposed to inti-

mately ethnographic only). Once you know what a science of culture should do, you do not have to visit every place under the sun. Armed with a theory about patterns of culture or about the dynamics of some specific cultural system, the investigator has a clear view of the facts that matter. Objections to such projects that argue that their discussions of specific cases fail to meet the standards for description touted in ethnographic circles obsessed with context and preoccupied with details are not compelling. Where a theory has a grip, the details that matter are those that contribute to the elaboration and evaluation of it and its competitors.

By now, we assume it is clear that we are criticizing *a specific vision* of cultural anthropology that we regard as impoverished. In effect, we are suggesting that even the projects of scientifically-minded cultural anthropologists have largely been co-opted by the agendas of post-modernists and thick describers. This dominant vision of cultural anthropology neglects the formulation and improvement of theory in favour of preoccupations with the collection of ethnographic detail and the specification of cultural settings. Entranced by the never-ending search for deeper and deeper meanings, interpretive anthropology has largely devolved into a cultural freak show. Its emphasis on documenting *apparent* cultural diversity (how can we know it is *genuine* diversity without the guidance of a successful theory that provides criteria for distinguishing cultural types?) has been so single-minded that interpretive anthropology and its post-modern descendants have largely abandoned their epistemic obligations to formulate better theories.

3. The prima donna of the social sciences

We suggested in the previous section that no one owns the concept of 'culture' and that the most progressive explanatory theories of cultural phenomena available should determine that concept's fate. Just like the concept of 'heredity' in biology, accounts of contributing mechanisms and systems will constrain the fate of the concept 'culture'. That anthropology has tried to reserve 'culture' for itself is troubling enough. That interpretive visionaries have picked up on this claim is even more bothersome. In this section we shall further explore the consequences of those visionaries' views.

Insisting that cultural phenomena can only be understood as embedded in webs of meanings carries an interesting implication for the place of anthropology among the social sciences. Pushed to its extreme – which is exactly where some of these visionaries (especially some of the cultural constructionists) have pushed it⁶ – this insistence on the preeminence of the idiographic

6. Consider, for example, the claim that emotions are not merely culturally constrained but

sets anthropology apart from and ahead of the other social sciences. Ploughing through its part, anthropology of this sort hopes to hog the social scientific stage, not merely oblivious to the other members of the company but actively trying to shove them into the wings.

The central argument runs as follows. *The* key to understanding any cultural phenomenon is to ascertain its meaning(s). The particulars of their contexts determine the meanings of cultural items. Hence, every cultural matter is *inextricably* tied to the particulars of its context. Therefore, regarding particular cultural phenomena merely as tokens of cultural types is importantly misleading and abstracting general cultural forms for the purposes of cross-cultural theorizing is intrinsically wrong-headed.

We have argued at length both in the previous section and elsewhere (1990: chapter 1) against this argument's first premise. Our current goal is not to repeat or develop those arguments, but rather to provide an additional argument by highlighting what is a not-too-often-recognized and a not-too-palatable (let alone popular) consequence of this view. The view of research on socio-cultural phenomena embodied in the argument's conclusions condemns precisely what all of the other social sciences aim to do. In short, if these anthropologists are right, then virtually all the other social sciences are wrong.

Economics, political science, linguistics, and sociology (more generally) all suppose: (1) that some social and cultural systems (economies, political systems, languages, etc.) can be isolated as theoretical objects *independently* of contextual variability; (2) that the assorted examples of such systems across a wide expanse of cultural settings share various features that are pivotal to their explanation; and (3) that this fact alone is sufficient to justify their analytical abstraction from their specific cultural contexts. Psychology makes the same presumptions about human psyches. Each of these inquiries is committed to the view that the forces operating within these systems are sufficiently robust across cultures (or across individuals in the case of psychology) that many features of these systems can be described and explained in relative isolation.

Presumably, it is clear by now that *here* we side unequivocally with these other social sciences. Pushed to its logical extreme, the interpretivists' position implies that the other social sciences are wrong-headed, if not impossible (McCauley forthcoming). It would prohibit all general proposals about the dynamics of markets, the distribution of power, and the formal features of

are culturally *constituted*: "The point then is not how *much* culture matters. For culture does not constitute emotions by degree. The point is *how* culture matters. For culture is the assemblage of those discourses *within which the emotions come to be*" (McCarthy 1992: 4; some emphasis added).

languages (let alone the structures of religious systems – which interest us). If the distinctiveness of everything cultural turns on webs of culturally specific meaning⁷ in which those things figure, then attempts to isolate and generalize about such systems must prove fundamentally mistaken.

As we have just hinted, this position has direct implications for the study of religion and explains why contemporary anthropologists are often sceptical about the possibility of developing theories about *specifically religious phenomena* (Boyer 1994: 37). Ironically, prior to hermeneutics' heyday, anthropologists – as the overseers of 'culture' – had quite different motives for resisting theories of specific cultural systems such as religion. Instead of rejecting such theorizing outright, they feared that the success of such theories would shut down their show. Their worry was that the triumph of such explanatory theories – about religious ritual, for example, in isolation from larger concerns about other ritualized cultural forms – would render their peculiarly *cultural* analyses superfluous.⁸ (We hold that this worry was and is ungrounded. It underestimates the value of *any* even moderately successful proposal that gains some explanatory purchase. The ignorance about socio-cultural matters is considerable enough to tolerate multiple theoretical approaches at many different levels of specificity.)

A further motive, with which we are *sympathetic*, was some anthropologists' concern to demonstrate that there was nothing *sui generis* about either religious systems or religious experience (as manifestations of a cultural form, in particular). We have nothing against such deflationary approaches – so long as they provide their own explanations of the phenomena in question *and* provide explanations of why the religious *appears* to be so different from other cultural forms on some fronts. We conceive of our own position as one that offers a (comparatively) deflationary account of religious ritual, but one that aspires to *explain* the appearances rather than deny them.

So, whether on traditional grounds of the primacy of *cultural* analysis and deflationary views of religious phenomena or on more recent grounds concerning entangled webs of meaning, anthropologists have remained antagonistic to the theoretical isolation of specific cultural forms for the purposes of cross-cultural explanation. The current version of the argument jeopardizes the possibility of theorizing about religion in the same way that it threatens the projects of the other social sciences. If all religious materials are only properly understood in all of their cultural connectedness, then religion stands little chance of independent theoretical analysis as a recognizable cultural form.

7. Whatever *that* is. Recall Gellner's claim that meaning is the *problem*, not the solution, in the study of culture.

8. See Humphrey and Laidlaw (1994) for but the most recent expression of this view.

In *Rethinking Religion* we unwaveringly insisted that religious systems and religious ritual systems, in particular, enjoy sufficient distinctiveness and robustness across a variety of cultural settings to serve as the objects of independent theoretical analyses. We have contended that such analyses of religious systems will involve explanations carried out in the same sort of *relative* isolation from the variable details of context that pertains in any other science.

Not surprisingly, most anthropologists seem to think that no compelling reasons exist for distinguishing religious ritual from rituals of other types. By contrast, we maintain that religious ritual systems can be usefully isolated *across* cultures for the purposes of explanatory theorizing and prediction. Note, our view does not preclude the possibility that religious rituals are largely continuous with other sorts of ritualized behaviours.⁹ Indeed, we argue that on some theoretically important fronts religious rituals are continuous with *all* forms of action (Lawson – McCauley 1990: chapter 5). The important point, though, is that without a theory of ritual-in-general that matches our theory's precision, systematicity, generality, and empirical tractability, we see no reason to defer to anthropologists' unsystematic intuitions here.

Such issues are not decided *a priori*. Finally, whether theories of religious systems (and theories of any sort) deserve social scientists' respect turns on those theories' relative productivity and empirical success. In scientific contexts explanatory and predictive success are the final measures of all things. Why should anthropology not embrace a theory that brings *some* cross-cultural order to at least one recognizable subset of ritual materials? Reluctance on this front is a function of that same exaggerated reverence for detail that we have been challenging throughout this paper.

9. The problem, though, is that no one has provided an even remotely convincing theory that offers a unified account of these phenomena. Humphrey and Laidlaw (1994) advance an account of the ritualization of action that is frequently suggestive. They concede fairly openly, though, that their approach makes good sense of "liturgy-centered" rituals only, forcing them to treat "performance-centered" rituals as peripheral cases at best. (This already disqualifies their discussion as an example of a theory of ritual-in-general – a disqualification which they straightforwardly acknowledge.)

Even as an account of liturgical rituals, Humphrey and Laidlaw's position faces some nagging problems. The most important, to our minds, concerns their insistence that nothing constrains the ordering of ritual segments in liturgical rituals. As evidence they note that the ordering of ritual segments in the Jain *puja* has undergone virtually unlimited variation over time. But this is not even a sufficient defense of the claim's plausibility, let alone its truth. First, evidence concerning but one set of rituals from one cultural system hardly counts as compelling in the face of what seem to be hundreds of counter-instances from other cultures. Second, even in the case of the *puja*, from the fact that it displays variations in the ordering of ritual segments over time, it does not follow that in any given performance the order is not constrained. No doubt, over time, word order has undergone variation in (probably) all natural languages. It does not follow that at any given time word order is not heavily constrained.

Our suggestion, then, is that the study of religion will prove most appropriately and most productively situated among the social sciences (understood broadly to include the psychological and cognitive sciences). The specific theoretical strategies we are exploring take their cues from work in the cognitive sciences. Although this is pioneering research concerning religious ritual systems, analyses of other cultural systems along cognitive lines have arisen in both linguistics (Lakoff 1987; Langacker 1987) and anthropology (Sperber 1975; Boyer 1990; 1993; 1994). The study of religion, like the study of language, economy, and power, can stand as an identifiable sub-discipline within the overall social scientific enterprise. Successful theorizing in each of these sub-disciplines contributes to our knowledge of culture.

The concept of 'culture' is as notoriously vague as cultures themselves are notoriously difficult to study. Such problems are not news in science. 'Culture' is no worse off than the concepts 'mind' or 'species' or 'chemical bond'. The way scientists *always* proceed with such problems is to study their empirically tractable features and sub-systems. Not only does anthropology not own the concept of culture, the lesson of the physical, biological, and psychological sciences suggests that its development will likely depend upon progress in those social sciences concerned with culture's "constituents", i.e., the various theoretically isolable systems that make up culture.

4. Coda

Probably no consideration more clearly reveals the emptiness of cultural anthropology's proprietary claims than the fact that other disciplines have developed means for investigating the world that bear directly on how we conceive of culture. Neither anthropological suppositions nor anthropological methods are *necessary* for either collecting empirical evidence or drawing conclusions about the character of culture. These other types of biological, psychological, social, and cultural inquiry have revealed new ways to approach the topics of culture and cultural forms from angles unlike those typically employed in cultural anthropology (see, for example, Lumsden – Wilson 1981, or Tooby – Cosmides 1989). Specifically, they include drawing some empirically informed conclusions about cultural matters without documenting every little detail about each and every spot on God's green earth.

Three areas of research come immediately to mind – concerning non-human primates, early childhood development, and various sorts of social and cognitive impairments. The first two involve phenomena that enjoy some continuity with the behaviour of enculturated, adult *homo sapiens* – the first evolutionary, the second developmental. They provide perspective on both organisms' intrinsic capacities that require little (if any) cultural input and

possible biological origins of cultural forms. The third area of research exploits a well-worn strategy in the biological sciences, that is, to gain understanding about normal functioning by studying pathologies. Injuries and breakdowns offer both impetus to study and useful information about a mechanism's routine functioning. These three areas of research not only arise from sciences we have touted elsewhere, but the sorts of evidence involved spring from studies that are far more precise and controlled than most of the data available by means of conventional research in cultural anthropology.

Space limitations require that we but briefly list an example of each. When Sue Savage-Rumbaugh and her collaborators (1986) found that the pygmy chimpanzee, Kanzi, both comprehended a fair amount of spoken English and appropriately responded (by means of sign language) on the basis of mere exposure to other animals' training sessions, we learned that the spontaneous acquisition of symbols, let alone their use, was not confined to human beings.

Frank Keil's research (1979, 1989) on young children's appreciation of basic ontological distinctions strongly suggests that their representation of many concepts is subject to little, if any, cultural variability. Presumably, mastery of these distinctions is so pivotal to getting on in the world that their acquisition is either rooted in our biology or *necessitated* by circumstance.

In the course of developing an account of the unique features of what they contend is "cultural learning", Tomasello, Kruger, and Ratner (1993) examine evidence concerning autism. They predict that because most autistic persons do not conceive of others as what they call "reflective agents", they will often be incapable of acquiring various sorts of cultural knowledge. Although the criteria for diagnosing autism are by no means uncontroversial, Tomasello and his colleagues note that approximately half of the persons so diagnosed prove incapable of acquiring language.

It is, in addition to developmental psychology, various subdisciplines of the cognitive sciences that we (and Boyer) have mined for the study of religious systems, including theoretical and cognitive linguistics and social and cognitive psychology (see also Baranowski 1994.) The mutual penetration of mind and culture encourages disciplinary cross-talk. Some cultural anthropologists have begun to consult the cognitive sciences as a means of exploring the influences of culture on mind. Shore (1995), for example, considers culturally specific schemas that organize multiple areas of participants' experience. By contrast, our major interest has been in the influence of mind on culture. We have focused on what the cognitive sciences reveal both about the study of the mind generally and about the constraints the particularly human version of mentality exerts on cultural forms.

Thus, the cognitive sciences provide both methodological and substantive inspiration. For example, on the methodological front, we enlisted a host of strategic resources theoretical linguistics employs for theorizing about cultural

competencies – exploiting an analogy between the competencies of native speakers with their natural languages and of ritual participants with their religious ritual systems (McCauley – Lawson 1993.)

Substantively, research in the relevant fields suggests cognitive constraints on and contributions to those cultural competencies. Findings in the cognitive sciences concerning concept representation, memory dynamics, social attribution, and conceptions of agency – to name only some of the most prominent considerations – offer valuable hints about *why* cultural forms such as religious beliefs and religious rituals take the shapes that they do and about *how* they operate and persist as cultural systems.

Moreover, these cognitive considerations typically apply *regardless of the meanings attributed to these cultural forms*. While acknowledging the role of interpretation in advancing our knowledge of culture, such theoretical approaches as we are recommending generate systematic insights about cultural forms without preoccupation with their meanings. The point is not to silence the interpretivists but to reclaim a role for scientific theorizing in the study of culture – releasing it from *any* proprietary claims and leaving it in the hands of the most productive and penetrating explanatory schemes available. Finally, no one owns 'culture', because in science our best explanatory schemes face relentless pressure to improve.

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