3. WHY DO WE NEED COGNITIVE THEORIES OF RELIGION?

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Scores of different schools of thought have claimed to explain religion. And thousands of scholars have contributed to the ensuing debates using evidence from such diverse sources as history, archaeology, theology, sociology, ethnography, philosophy, and even astronomy and mathematics. E. Thomas Lawson, whom this volume honors, is widely and justly credited with being among the first to recognize the importance of developing a cognitive approach to the understanding of religion. But what reasons do we have for supposing that yet another set of theories will really help?

The short answer is that a great many existing theories of religion are ontologically flawed or untestable (and, in some cases, both). During the second half of the twentieth century, a drift towards hermeneutic and phenomenological approaches to the study of culture has led to declining interest in explaining religion at all. In overcoming many of these problems, the cognitive sciences offer a radically new way forward. I will argue that social/cultural anthropology, as a branch of these new sciences, is uniquely positioned to supply crucial sources of data for the development of explanatory theories of religion and could make crucial contributions to cross-disciplinary collaboration. But first, certain problems need to be overcome.

The Problem of Faulty Ontology

Thoughts and feelings consist of processes occurring in people's nervous systems, whether or not these are influenced by events in their immediate environments. Attention, perception, language, memory, dreaming, introspection, inference, empathy—in fact, every aspect of human experience and mentation is reducible in principle to organic processes and the developmental histories that shape them. The extraordinarily complex neural computations through which these effects are generated did not appear mysteriously overnight. They came into being through processes of evolution, shaped in part by principles of natural selection. Although we are very far from having the full story of how these processes unfolded, it is now possible to grasp many of the general mechanisms involved and to piece together some of the chronology. In all likelihood, we will know a lot more about neural evolution through future scientific advances. But even now it is reasonable to assume that all the complex things that go on inside human beings and in the world around them have material causes, and that ideas about cultural "forces" are part of what needs to be explained, not part of the explanation.

Because of our evolved cognitive architecture, humans everywhere are inclined to *reify* patterns of culture and social interaction, turning them into objects or even agentive forces with causal efficacy. It seems perfectly sensible to say, for instance, that most English children know the story of Goldilocks—as if "the story" has some kind of existence that is external to the children who come to know it. It is a small step from this to thinking of cultural phenomena as agent-like—imagining, for instance, that trade unions sometimes try to "bring down" governments or that terrorism can "threaten" national security. Such ideas are undoubtedly useful as a way of schematizing the complexity of the world around us, the better to determine our own responses to it, but the ontology they imply imposes heavy costs on a scientific understanding of culture and society. As Plotkin succinctly puts it (2001: 94):

> Because culture . . . is some kind of complex interlocking organization of many minds, it is the most complicated thing in the known universe. Nonetheless, culture . . . is not some kind of untouchable nonphysical essence that fills the spaces between people making up a society. Culture is what happens in people's minds when they interact in certain ways with other minds and with the artifacts that are often central to those interactions.

It follows that a story is not a "thing" even when it is written down and stored in a book (which *is*, of course, a thing). Rather, a well-known story consists of numerous recognizably similar narratives "stored" in the minds of those who know it. Even if a story is based upon a single written text, faithfully reproduced in millions of copies distributed around the world, the story itself does not exist in the patterns of ink on the pages of such books but only in the minds of readers capable of interpreting these patterns. As such, the story has no single instantiation but literally millions (all recognizably similar but almost never identical).[1] By the same token, national security is not really something that can be "threatened." True, terrorists can pose a very real threat to human life, and *people* can *feel* threatened (whether the perceived dangers are actually present or not), but as soon as we try to think about the distributed effects of terrorist actions, we run the risk of conflating the metaphorical and the literal. It is not just "ordinary" people who typically find this point rather hard to grasp, it has also proved a stumbling block for many professional social theorists.

Few would dispute that Emile Durkheim had a great and enduring impact on the sociology (and anthropology) of religion, and on the study of society and culture more generally. Durkheim was a highly original thinker. He was also a skilful teacher, blessed with a coterie of brilliant students, many of whom became great scholars in their own right. But Durkheim's work (e.g., 1933 [1893], 1964a [1896], 1964b [1915]) was tarnished by his deliberate reification of society. Although there is much to be salvaged from Durkheim's scholarship, the most unproductive thread running though the entire corpus was his insistence on the presence of an order of reality (a set of "social facts") somehow hovering above the heads of individuals. Rather than supposing that this "reality" was only a fiction, upon which the additional fictions of religion were at least partially constructed, Durkheim was preoccupied with showing that the fictions of religion were really a kind of highly coded discourse about something real (a society, greater than the sum of its parts). Unfortunately, this unnecessary, and fundamentally misleading, aspect of Durkheim's sociology of religion proved to be one of the most influential.

Nowhere has Durkheim's faulty ontology been more faithfully reproduced and extended than in the tradition of social thought known as "structural functionalism," which dominated social anthropology for several decades.[2] The central thesis of structural functionalism was that every social institution had the function of reinforcing

[1] Dan Sperber has done more than perhaps any other scholar in exposing both the problems of and solutions to sociocultural reification (e.g. see Sperber 1996a).

[2] The development and proliferation of Durkheim's faulty ontology was not an entirely straightforward matter, however. Malinowski, for instance, was an arch empiricist, whose theory of "primary functions" (1944) was explicitly driven by the desire to link cultural abstractions to concrete biological needs. Malinowski sensed the ontological confusion behind Durkheim's rhetoric and repeatedly opposed himself

certain other social institutions, thereby ensuring the stable reproduction of the society over time. This functionalist reasoning pervaded the work of some of the discipline's most brilliant practitioners: Gregory Bateson (1936), Evans-Pritchard (1940), Raymond Firth (1964), Meyer Fortes (1945), Mary Douglas (1970 [1966]), Victor Turner (1957), Max Gluckman (1963), Fredrik Barth (1964)—to name but a few. Most of these scholars also lived long enough to get caught up in the tide of anti-functionalist rhetoric that engulfed social and cultural anthropology from the early 1970s, and tried to reformulate their perspectives. The fact that institutions sometimes seemed to conflict with the conditions of reproduction of many others, and that societies seldom achieved conditions of stasis, presented a problem for functionalism. The problem was not a crippling one, however, and cannot account adequately for the more or less unanimous abandonment of functionalist reasoning. A more important factor was probably the shift from explanatory to hermeneutic models of culture. Unfortunately, these "new" perspectives often retained vestiges of the faulty ontology that had plagued structural functionalism from the outset.

An obvious example would be the interpretivist approach, championed by Clifford Geertz, which re-cast Durkheim's "social facts" as "public meanings." According to Strauss and Quinn (1997: Chapter Two), Geertz used the word "meaning" in at least three different senses and he likewise alluded to the "publicness" of meaning in a

to it. Nevertheless, according to Leach (1957: 136): "despite his advocacy of empiricism, Malinowski was really searching all the time for concepts of the middle range of generality . . . Culture is too abstract; the individual is too concrete." In the end, Malinowski's concept of primary functions proved to be a failed compromise. What Malinowski called "the principles of social organisation, of legal constitution, of economics and religion" (1935: 317) were no more readily observable or "concrete" by virtue of being linked to the satisfaction of physiological requirements. Moreover, the theory now became enmeshed in tautology: if social institutions persist, they must be satisfying biological needs and, therefore, all enduring cultural arrangements are functional (in virtue of satisfying biological needs). Partly for these reasons, the notion of primary functions had limited influence in the development of social/cultural anthropology. By contrast, Malinowski's concept of "secondary functions," which held that the function of institutions was the role they played in the reproduction of other institutions, had a much more enduring impact. This kind of functionalism, notwithstanding various attempts to "concretize" its claims in the thoughts and actions of individuals (e.g. Firth's 1964 distinction between "social organization" and "social structure"), harbored and at times exacerbated the faulty ontology pervading Durkheim's work.

number of different ways. Geertz's rhetoric against psychological views of culture, in which meanings are understood to be outcomes of interior processing, slipped between these varied concepts of meaning-as-public in ways that were often far from transparent. Nevertheless, there are several passages in Geertz's work, some of them very well known, where it is easy to pin him down. Consider, for instance, the following lines from his celebrated article on "thick description" (1973: 10):

> Culture, this acted document, thus is public, like a burlesqued wink or a mock sheep raid. Though ideational, it does not exist in someone's head; though unphysical, it is not an occult entity . . . The thing to ask about the burlesqued wink or a mock sheep raid is not what their ontological status is. It is the same as that of rocks on the one hand and dreams on the other—they are things of this world.

Here, Geertz's faulty ontology could not be clearer. For him, culture is an abstract, non-physical structure that exists *outside of people's heads*. Where it exists, however, must be a mystery, however strongly Geertz protests. As Strauss and Quinn rightly ask: "If culture (a pattern of meaning) is 'unphysical,' how can it have the same ontological status as a rock or a mock sheep raid?" (1997: 19).

Thus, the decline of structural functionalism was not necessarily accompanied by an accurate and widely accepted diagnosis of its ills, less still by appropriate methods of treatment. The real problem with functionalist theories was neither that some institutions were dysfunctional (or functionally neutral), nor that societies change. No theory should be expected to account for everything—and if only some patterns of functional integration could be identified in a given society that would have been an important insight, well worth preserving. The real defect of structural functionalism was its inability to explain how functional integration came about, and this was largely due to the poor ontological foundations of the approach. A religious dogma cannot help to bolster a system of inheritance in any meaningful sense, because dogmas and relations of descent are not exterior to mentation, but rather they are more or less convergent constructs that somehow manage to be recalled or (re-)constructed by human minds. Even this is a loose way of talking but it is a better starting point. Unless we accept that religious dogmas and relations based on common descent are located *in minds*, not in the spaces around or above them, we cannot hope to explain how relations of functional integration (or social conflict and transformation) could come into being, for instance via processes of distributed cognition. But if we are to succeed, we require some very precise, logically tenable, and reliably testable models of mind. Psychological theories of religion have at least as illustrious a history as functionalist ones and, what is more, they get their basic ontology right. Unfortunately, many of them have been untestable and, in some cases, substantially tautological.

The Problem of Untestability

The second half of the nineteenth century saw a proliferation of highly ingenious theories of religion, establishing trajectories rather different from Durkheim's (although one can readily identify areas of common ground as well). One of these trajectories came to be known as the "intellectualist" perspective on religion, championed in particular by Max Müller (1889), Herbert Spencer (1876), Edward Tylor (1871), and James Frazer (1922). In different ways, all four of these scholars envisaged religion as a response to what they saw as a basic human need for an intellectual framework capable of resolving the great existential mysteries of life. For Müller, religion addressed the mystery of infinity, for Spencer and Tylor it responded to the mysteries of dreaming, intangibility, and duality, and for Frazer religion was a rather cunning response to the failure of magic. It is not necessary to devote space to a detailed summary of this scholarship (many excellent ones have already been published),[3] but only to note some of its main strengths and weaknesses. The greatest strength of the intellectualist approach was its insistence on the right kind of ontology—religion, it asserted, could be explained in terms of properties of the human psyche. The main drawback was that none of these scholars had a sound model of what the human psyche might be like. The strategy of nineteenth-century intellectualists was to try to imagine themselves in the position of ancient populations, as religious thinking first began to emerge. What, they asked themselves, would have struck *them* as puzzling about the world, as it presented itself through the mechanisms of a newly evolved self-consciousness? Malinowski later used the weaknesses of speculative historical recon-

[3] See, for instance, Morris 1987.

struction as a way of justifying his brand of functionalism. In a later and rather more devastating critique of the intellectualist project, Evans-Pritchard described its psychological method as roughly equivalent to asking the question "if I were a horse . . .?" (1965: Chapter Two). As well as poor standards of substantiation, however, there were problems of circularity in this nineteenth-century scholarship. Religions often do supply answers to certain aspects of human experience that might otherwise seem puzzling. But the presence of answers to existential questions in the form of religious theories does not necessarily imply some pre-existing need for them.

So fundamental and obvious did these flaws appear to subsequent generations of anthropologists that the most valuable aspect of this early scholarship was overlooked—namely, the realization that the right place to look for the origins and causes of religion must be in the minds that create (and continually recreate) it. Having missed this crucial point, social anthropology became preoccupied elsewhere. But another, potentially more promising line of reasoning lurked on the peripheries of the discipline. Its instigator was the founder of psychoanalytic psychology, Sigmund Freud.

Freud's (1938 [1913]) theory of religion turned on a rather startling hypothesis about the origins of the incest taboo. Freud argued that all male children experience a deep intra-psychic conflict between desire for exclusive possession of the mother and fear that this desire will be punished by the father. The normal outcome of this dilemma, he maintained, is complete repression of the sexual drives from around age five to adolescence. According to the hypothesis, repressed incestuous desires for the mother and patricidal fantasies towards the father, amount to a fundamental syndrome in male psychological development, which Freud labeled the "Oedipus Complex." The next step was to argue that this complex was the principal *cause* of religions everywhere.

In *Totem and Taboo*, Freud speculated that all human populations were originally organized into patriarchal families, from which boys were driven out by their fathers upon reaching maturity (each son thus being required to set up his own family independently). One day, however, a group of sons joined together in a successful plot to kill their father, whose corpse they then consumed. Later, the sons were racked with guilt, which they sought to assuage by establishing an incest prohibition. By proscribing incestuous desire for the mother, the sons hoped to prevent further acts of patricide.

Nevertheless, a sense of guilt at their heinous crime was passed down through the generations and came to form the foundation of all religious thought. According to Freud, all religions were (and are) founded on the guilt occasioned by this original Oedipal conflict. Using examples from a range of religious traditions, Freud pointed to evidence of symbolic expressions, both in ritual and religious narratives, of the primordial crimes of patricide, cannibalism, and incest. What is the consumption of the Eucharistic Host, Freud asked, other than the act of killing and eating God (i.e. the father figure)? Was not Adam's crime in the myth of Genesis a cannibalistic attack on the Almighty Father (symbolized by the forbidden fruit of the Tree of Life)? That this original sin involved the taking of a life seems to be confirmed by the fact that Christian redemption required the giving of a life on the part of the son, Jesus Christ.

Taken at one level, Freud's argument is as absurdly speculative as the historical reconstructions of the origins of religion advanced by nineteenth-century intellectualists. But Freud's argument does not have to be taken that way. It is not necessary to postulate an *actual* historical act of patricide (as did Freud)[4] but merely to suppose that each new generation of male children commits this terrible crime, not in reality but in fantasy.

Freud's general theory achieved some influence among anthropologists of religion, particularly after disillusionment with functionalism had set in. Meyer Fortes, originally one of the most talented exponents of structural functionalism, was drawn increasingly to psychoanalytic interpretations of religion in later life (e.g. 1980). Barth (1987), LaBarre (1970), Leach (1958), Obeyesekere (1981), Spiro (1965), Turner (1967), and many other luminaries likewise borrowed from Freud in their attempts to explain a diversity of religious practices and beliefs. This was laudable, insofar as it was at least partly motivated by a desire to adopt the right kind of ontological stance. All these scholars had come to appreciate, at least for the purposes of certain problems of explanation, that the organization of minds played a role in the shaping of mental outputs (of which culture is constituted). Unfortunately, however, psychoanalytic psychology, even in its most advanced and recent incarnations, typically exhibits major problems of circularity and untestability.

[4] This is made clear in Freud's corresponence with Kroeber, to which the latter alluded in his review of *Totem and Taboo*.

In essence, Freud's claim was that people are driven by mostly dark and dangerous fantasies, of which they are not aware. There is nothing exceptional or even implausible about this claim, in principle: it is obvious that much of what goes on in people's minds is not conscious. The problem is how to determine whether this unconscious content is as Freud imagined it. But most Freudians do not try to *test* their beliefs about the unconscious. On the contrary, they assume that these beliefs are correct and interpret everything they see accordingly. When patients challenge the interpretations of their Freudian analysts, this is seen as repression; when they *agree* with their analysts, this is upheld as evidence that the theory is *correct*, because they have now obtained some conscious awareness of content that *used to be unconscious*. An insightful investigation into the circularity and untestability of this strategy is presented by Ernest Gellner in his erudite treatise *The Psychoanalytic Movement*. As Gellner puts it (1985: 156–7):

> The idea that certain types of infancy experiences lead to certain types of adult personality, is testable if either of the two domains (early experiences or adult personalities) is subdivided, with some reasonable degree of precision and above all of *independence*, into sub-categories. Then, *and only then*, can one say that there is some kind of functional relation, as specified in the theory, linking members of the one domain to those of the other. In fact, of course, the sub-categorization in either domain is (1) extremely loose and wooly, and (2) entirely under the control of that very theory which is to be tested, i.e. imposed on the material by it and by the privileged practioners/operators of the theory and of its therapeutic associated practices.

When structural functionalism fell into decline, anthropologists of religion were inclined to try out all kinds of alternative perspectives. Psychoanalysis only turned the heads of a minority of scholars, and at least in part because of the drawbacks I have indicated. Marxism, however, also won some influential converts.

Marx's theories of technological and economic determinism[5] had some obvious appeals for those with a taste for scientific explanations of human history. As a student I fell under its spell, like many others. Social formations and transformations, it seemed, could be

[5] A particularly succinct summary of these theories is provided by McMurtry 1978: Chapters 7 and 8.

explained in terms of material causes (forces and relations of production) which determined the content of religion, political organization, ideology, legal institutions—indeed, the whole of culture—on principles of selection. The independent variable driving selection appeared to be levels of technological advancement and the stage of development of the production relations. Everything hinged, of course, on being able to distinguish forces and relations of production and, then, to separate both (as causes) from the legal/political/religious/ideological superstructure (as effects). This is where, for me, the theory turned out to be disappointing. Once again, the problem was one of circularity.

What are "relations of production" if not relations of ownership? And what are relations of ownership if not legal/political/religious/ideological constructs? How can the one be said to cause the other if they are really just one and the same set of representations? The longer I struggled with dense and jargon-ridden Marxist literature, the less confident I became that there might be plausible answers to these fundamental questions. To argue that production relations concern distributions of powers whereas legal relations concern distributions of rights, seemed to me the ultimate tautology, since in all practical circumstances the latter were merely a (substantial) subset of the former. Moreover, the central argument of Marxism, that the plough (economic infrastructure) is mightier than the sword (political superstructure) seemed to founder on the fact that both ploughs and swords are items of technology and that the different relations of persons to these forces of production and coercion are quintessentially political/jural relations. In order for patterns of exploitation to be reproduced, clearly certain kinds of ideas (including religious ideas) had to be selected, in preference to ones that would be non-compliant with those production relations. But historical materialism could not explain *how* these compliant forms were selected—beyond tautological claims about economic determinism and vague murmurings about the supervisory prominence of the ruling class in the organs of cultural dissemination. Conspiracy theory at least had the merit of allowing individual agency into the picture—but only at the cost of obvious and gross oversimplification of processes of cultural transmission.

In some revealing autobiographical passages, Karl Popper has described a similar experience of disappointment with psychoanalytic and Marxian movements (except that, with regard to the former, it was Alfred Adler rather than Sigmund Freud who directly impinged upon Popper's formative milieu). He wrote:

> My friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to all their theories, and especially by their apparent *explanatory* power. These theories . . . seemed to have an effect of intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it . . . It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness (1963: 34–5, emphases in original).

The Problem of the Hermeneutic Vortex

The decline of structural functionalism was accompanied by the resurrection or invention of a wide range of 'isms.' Some of these, as suggested above, were ontologically flawed and/or untestable, but those that really gained a lasting grip (and remain in the ascendant today) exhibited an even more sinister trait, in the form of an avowed antipathy towards explanation in general, and the principles and methods of the natural sciences in particular. Why was this?

In a penetrating assessment of much contemporary academic research on religion, Lawson and McCauley (1993) attribute the growth of anti-scientific prejudices in anthropology to a crisis of conscience. Over the last two decades in particular, anthropologists have paid increasingly close attention to evidence of collusion between ethnographic research, at least in its early stages of development, and colonial imperialist projects. In the guise of scientific enquiry, anthropologists had (often unwittingly) sharpened the tools of colonial oppression, by providing valuable intelligence on subject populations and on the likely effectiveness or otherwise of alternative methods of imposing external control. From the 1970s onwards, scholars increasingly favored a new "hermeneutic" approach, emphasizing core values of humanism and relativism, directed towards the "understanding" (translation and interpretation) of culture, rather than the production of generalizing and falsifiable theories that might (in the wrong hands) be used to strengthen hegemonic control of relatively deprived or disempowered populations. In short, many anthropologists have sought redemption for the sins of their ancestors by sacrificing science on the altar of hermeneutics.

According to Lawson and McCauley, this hermeneutic turn has now established itself as a kind of "vortex," sucking in all those whose consciences are (rightly) pricked by the historical contributions of early anthropology to processes of political domination and economic exploitation. But they are at pains to point out that the anti-scientific rhetoric of this new generation of scholars[6] was, and is, entirely unnecessary. We all agree that precise, well-substantiated theories can be used immorally. And we probably also share a common understanding of what constitutes an immoral use of such knowledge. What seems utterly baffling is that argument that, in order to avoid the misuse of high quality information, we should instead produce information of a lower quality. Or (as is more typically proposed, if only implicitly, by the hermeneutic camp), we should produce information of *indeterminate* quality (see Lawson and McCauley 1993: 205–6).

The situation, however, is both worse and better than Lawson and McCauley suggest, at least in the case of anthropology. It is worse insofar as the hermeneutic vortex probably derives much of its current appeal from sources rather less noble than a crisis of conscience with regard to colonial oppression. It is better in that the vortex in question may be more a matter of rhetoric than a reliable guide to anthropological practice. The latter issue is discussed in the next section. The former, however, requires some swift clarification.

Why is a sense of guilt insufficient to account for the widespread anti-scientific rhetoric in anthropology? The answer is implicit in Lawson and McCauley's observation that "the connection between scientific aspirations for anthropological research and collusion with imperialism is not a necessary one" (1993: 204). Are we really to believe that our hermeneutic friends could have failed to spot this? To have exercised so much ingenuity in the propagation of such an implausible position suggests the presence of other motivations. In pondering why so many contemporary anthropologists reject the "science" label, David Shankland (2001) identifies a number of possible factors, among which the "crisis-of-conscience rationale" is not even mentioned:

[6] They mention in particular Geertz (1973) and Clifford and Marcus (1986).

> I have not conducted a systematic survey, but it seems that most friends and colleagues object to various aspects of the epithet ["anthropology as science"]: that it implies a dedication to numerical analysis; that it is based on the inherently implausible premise that the world can be analysed from a value-free viewpoint; that is encroaches upon the personal freedom to explore a sense of self in interaction with other human beings, a freedom that lies at the heart of the discipline; that it implies a sense of certainty that can never be assured, given the tools at our disposal.

Noting that these misgivings are seldom informed by an understanding of what real-world sciences are all about, Shankland proceeds to argue that such attitudes are not really reflected either in the principal concerns and activities of most anthropologists. So we are forced back to the question: why does this anti-scientific rhetoric exist at all? Part of the answer may lie in general principles of coalition-formation.

Anthropologists, like the members of many other academic disciplines, are organized into corporate groups, recruitment to which is based on a combination of successful initiation (culminating in the award to PhD degrees) and notionally open competition among initiated applicants for somewhat scarce university posts. These corporate groups are usually called "departments" (sometimes also "schools" or "institutes"). Cognate subjects, divided into departments at the same organizational level, together form larger units, usually known in the UK as "faculties," directly managed by "deans." These corporate groups are in competition for scarce financial resources. Faculties compete with other faculties for central university funds. Departments in the same faculty compete with each other for students (of graded value) and shares of other resources controlled (at least to some extent) by deans. Departments have other sources of income, however, including government research grants and (at present in the UK) special rewards based on points scored in a government-sponsored competition known as the "RAE" (Research Assessment Exercise). Although competition in the RAE is among departments in the same subject area, it is more generally the case that anthropologists are in competition with non-anthropologists, either at the level of departments or at the national level. The reason is that the most valuable resources of all (in monetary terms) are students, followed by forms of research income for which competition is encouraged across disciplines. Anthropologists formulate strategies, at the level of departments and also nationally though professional organizations, to advance their interests *as a subject area* in this competitive setting. My impression is that, although the UK system has many peculiar features of its own, this general pattern is much the same all around the world.

In such a political climate, loyalty to one's own discipline is a matter of considerable importance. Cheating or defection, on the part of colleagues, can have serious material consequences for the group, which are on the whole explicitly recognized. But even if that were not the case, the very fact of being organized into professional guilds (academic departments and professional associations), meeting at the same conferences, reading and contributing to the same journals, ostensibly sharing the same creed (e.g. Relativism Is Right, Ethnocentricism Is Wrong, etc.), and tracing descent from the same intellectual ancestors, is more than enough to engender a sense of in-group versus out-group. In short, members of the in-group (social and cultural anthropologists) implicitly (and sometimes explicitly) are concerned to protect the integrity of their discipline against corruption or invasion/colonization by out-groups (including the cognitive sciences). What evidence is there of such a concern and how is it expressed?

Among social and cultural anthropologists, whether in informal conversations, seminars, or published work, it is quite common for the term "reductionism" to be used as an insult (or at least to indicate a negative evaluation of the argument advanced). On the face of it, this is rather odd because reductionist strategies hold out considerably more promise in explaining sociocultural phenomena than do non-reductive strategies. A negative attitude towards reductionism is, of course, consistent with the sort of faulty ontology discussed earlier. According to this ontology, social and cultural phenomena have nothing to do with individual mentation, and are irreducible to it. But the crucial question still remains: why would anybody believe that?

One possible answer is that at least some anthropologists, with varying degrees of conscious awareness, are concerned that the reduction of cultural phenomena to psychological processes would threaten the integrity of their discipline—in the worst possible scenario perhaps relegating it to a rather insignificant and under-funded branch of psychology. At professional lectures and seminars I have witnessed scenes in which anthropologists who employ arguments developed in evolutionary psychology have been accused of being agents of an imperialistic cognitive science. Judging by the murmurings of assent to this view, particularly in post-conference chit-chat, such accusations are quite widely and readily endorsed.

Cross-disciplinary discussions about "human nature" provide an excellent opportunity to observe the fears and biases of anthropologists on this score. The question "what is human nature?" tends to draw a neat line between social/cultural anthropologists and the rest of the world. The former camp will usually tell you, often in ways that are hard to decipher or pin down (more about that shortly), that human nature is culturally constructed. Even those who allow some nativist assumptions to creep in, will still insist that what makes us human is mostly fashioned in radically different ways at different times and places, and thus is culturally relative. Non-anthropologists (e.g. various types of biologists, archaeologists, psychologists, and so on) will typically argue that the nature-nurture debate is now defunct and that, although there are many recurrent features of human cognitive architecture, these are outcomes of a complex combination of genetic and developmental/environmental conditions. The two camps will talk past each other and most participants will go away in a state of bewilderment and frustration. Many of the anthropologists will be reassured that they have done their bit to repel the invaders. The "invaders" will not understand that they are seen as invaders, will wonder how they managed to cause offence, and will probably make a mental note to avoid debates with anthropologists in the future.

As well as struggling to preserve a distinctive subject matter, anthropologists often display proprietary attachments to their research methodology, and many resist contamination or borrowing from other research areas. Quantitative (especially advanced statistical) methods are still quite sparingly used in the discipline, and the emphasis is on long-term, qualitative research using participant observation and interviews as the primary techniques of data-collection. A significant consequence of this approach is that the ethnographer's vision is saturated by the complexity and muddle of social life. To tackle *any* topic without some lengthy pontification on issues of gender, colonization, globalization, alterity, nationalism, and so on would be a serious *faux pas*. But the urge to cover all these different topics in the same breath, without attempting to discriminate (based on transparent evidence) the relative importance of each, is of dubious value. There are many occasions where this process of discrimination may be served in obvious ways by the use of quantitative data. Often, such data are not collected at all. At least part of the reason, I suggest, boils down once again to protectionist urges within the professional guild.

Consider first the fact that, although anthropologists proclaim the superiority of their research methods over those of quantitative social research, very few would seem to relish the opportunity to teach courses on participant observation or interview techniques (although many do so with varying degrees of reluctance). Many anthropologists avow that ethnographic research is mainly a matter of common sense and does not warrant the sort of close scrutiny demanded by the unenlightened bureaucrats who fund our fieldwork projects and the training of our doctoral students. This lack of interest in methods cannot be because we believe them to have been perfected, beyond improvement. The problem of how to gather information about the mental processes of other people, in terms consistent with the vast corpus of knowledge available from related "real world sciences," has been grossly under-investigated. If anthropologists really prized their research methods for other-than-rhetorical reasons, their efforts in both teaching and research would, I think, be allocated very differently.

Anthropological suspicions with regard to experimental methods are rationalized slightly differently. Such methods, it is often supposed, are potentially demeaning to participants (e.g. "tricking" them into delivering information that is not of their own choosing, or treating them as mere objects rather than as equals and collaborators). Many anthropologists are also inclined to assume that experimental approaches could not possibly identify all the relevant confounds and/or find ways of overcoming their effects. My impression, however, is that although such attitudes and assumptions can be widely and readily triggered among anthropologists they are seldom based on knowledge or experience of the methods in question. Again, we have to ask why and at least part of the answer might be the urge to preserve a distinctively anthropological methodology and to compete for research funding against disciplines that lack the techniques we claim as our own.

Consider now a second nexus of factors. In addition to a range of attitudes that seem to be aimed at defending the integrity and identity of the discipline, there are some rather distinctive aesthetics in social and cultural anthropology, which conflict with the methods and objectives of the natural sciences. Something needs to be said about that too, if we are to obtain a fuller understanding of anti-cognitivist prejudices. By the 'aesthetics' of the discipline, I really mean the principles (at times implicit, at others explicitly formulated) by which we judge scholarship to be "good" (e.g., richly-textured, nuanced, sophisticated, etc.) or "bad" (e.g., "naïve," "reductionist," "simplistic," etc.).

Among the principles that seem to be quite widely operative is a preference for literary flourish over clarity and precision. Non-anthropologists have often asked me why anthropologists seem to use such extravagant language and complex metaphors to convey what might be considered rather simple points. In small workshops, why do we insist on reading out (often rather pompous) prose when we could instead use overheads and informal illustrations? Why do many of us put so much store by jargon, and yet seem to resist defining it? The answer is partly that we value erudition and the arts of scholarly writing. Geertz's impact on us surely owes more to his exceptional eloquence, wit, and wide-ranging education than his theoretical and methodological ideas, which were at turns quite obscure, trivial, or simply mistaken. Much the same could be said of other heroes and luminaries. Above all, however, we like to be interpreters of what we read and hear. Opportunities to do that vary in inverse proportion to the clarity and precision of the work. The more ambiguous and uncertain the author's meaning, the less easy it is to dismiss and the more fertile it becomes as a trigger for one's own ideas. Taken to extremes in some postmodernist scholarship, this principle is the ally rather than the enemy of that most prized attribute of all in contemporary social and cultural anthropology: critical originality. To the extent that you can give the impression of saying something important and new, while saying as little as possible that could be directly tested, maximizing the possibility for others to cite (usually on principles of thematic association) what you may have said, your work is likely to be widely appreciated. More than once I have been offered the following genuinely friendly advice from internationally respected colleagues: "Harvey, the story you are trying to tell would be so much more interesting, to a wider range of your colleagues, if you could only manage to 'vague it up'." The term "vague it up" was only used once—in fact that is the only time I heard it uttered.

But the gist of this advice has been conveyed to me on several occasions. If I have applied that advice, however well meant, it has not been my intention to do so.

Another conspicuous symptom of the great store placed on literary flourish is that the anthropologists' texts are often treated as vastly more interesting than the concepts and practices of the people to whom these texts ostensibly refer. For instance, improbable comparisons are commonly made between concepts attributed to non-academic (and often non-literate) populations, on the one hand, and those adopted in essayist traditions of Western philosophy and literary criticism, on the other. Indeed, the more improbable such tropes, the more "original" and thus "interesting" they are likely to be considered. A good example of this attitude may be seen in the preoccupation of many anthropologists with the discovery (or more mystical "apprehension") of the meanings of their own terminology—as if this is likely to provide indispensable insights into the far-flung cultural representations they claim to be investigating. As Francisco Gil-White astutely observes (2001: 238):

> In anthropology it is customary to spill much ink trying to wrest control of the meaning of its terms of art from other anthropologists, as if defining a word were equivalent to providing an explanation or making an intellectual contribution. Many proceed as if the meaning of a word—itself apparently of profound importance—was something to be discovered through careful application of quasi-philosophical reflection, and the anthropologist was the professional whose task was to discover it.

I have suggested that anti-scientific prejudices among anthropologists are at least partly driven by protectionist urges within the professional coalition but that these prejudices are also consonant with a dominant aesthetic that values erudition, creative interpretation, and critical originality over precise empirical evidence and explanation. But, even if that is true, it still leaves important questions unanswered. Academic disciplines closely allied to anthropology also exhibit coalitional thinking and for much the same reasons, but not all of them reject the science label; on the whole, social/cultural anthropologists were at one time eager to portray themselves as scientists, but no longer. So what put anthropologists on a different trajectory from many of their neighbors and ancestors?

The "crisis of conscience" theory could well contain a lot of truth but it has its limitations. Archaeologists have ample reason to feel guilty over past collusions with colonial projects for the purposes of appropriating valuable artifacts. Their work also proceeds in circumstances of political contestation, often more acute than those faced by anthropologists. After all, the claims of several great religions and numerous political ideologies are potentially testable on the basis of archaeological data. Why, then, should a substantial sector of the archaeological coalition have sought to retain its identity as a science, rather than to adopt the same evasive tactics as its anthropological neighbors? The answer, perhaps, is that pricked consciences and the fear of generating potentially dangerous intelligence are not the main causes of hermeneutic escapism. The real core of the matter is this: the coalition of social/cultural anthropologists has had to face a much more serious threat than almost any other academic discipline in the post-colonial era, namely *the loss of its defining subject matter*.

Ethnography began as the study of "savages," rapidly re-described as "tribal" or "uncentralized" peoples and, later still, as "nonliterate" or "traditional" societies. But, when all is said and done, anthropology was definitively the study of *colonized peoples*. With hindsight, one might think it was foolish to build the identity of a discipline on such an historically transitory subject matter. But there it is. The *extraordinary defensiveness* of the coalition arises directly from *that extraordinary dilemma*. We are dealing with a coalition that really *has* had cause to fear dissolution, to fear being taken over by neighboring disciplines. And the greatest threat of all originally came from the real-world sciences. Why? Because disciplines like archaeology and psychology shared our most fundamental origins and aims, and provided the most relevant and commensurate principles of investigation. Anthropologists could no longer afford to identify themselves with that sector of the academy. As overwhelmingly middle class scholars, steeped in the aesthetics of the Western art cult, hermeneutics and its offshoots provided the obvious place to look for new identities. Now that the field of "cultural studies" has established itself as a major threat from *within* the place of sanctuary, it is of course hard for members of the anthropology coalition to decide where to turn next.

Much more could be said about all these problems and I readily concede that the above observations are anecdotal and impressionistic.[7] The extent to which they contain some truth may be judged

[7] Through informal discussions of this topic with anthropologist colleagues, I have

by others in the profession who have conducted participant observation among anthropologists for longer than I and, collectively, in a vastly greater range of circumstances. Moreover, two points should be added. First, even if my assessment is roughly accurate, anthropology is certainly not the only discipline to be constrained in its work by professional prejudices that both respond and contribute to the political contexts in which they operate. An honest appraisal of other fields (including the various branches of cognitive science) would undoubtedly discover comparable processes, unfolding in different ways. But it would be mistaken to infer from this that all knowledge gained through academic research is merely an expression of politically motivated maneuvers and the prevailing aesthetics of the profession. Research can certainly be of scientific importance, regardless of the political context that happened to spawn it. Second, and following from this, the research achievements of anthropology are very considerable. Although I have been focusing on shortcomings, I propose now to highlight what I see as the major contributions of the discipline, at least as far as the study of religion is concerned. In the process, I hope to show that the distinctive identity and long-term survival of the discipline (although a subsidiary concern here) is not only consistent with inter-disciplinary dialogue and collaboration, but can actually be served by that.

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come across a number of alternative (though potentially compatible) diagnoses of the current penchant for obscurity in much contemporary scholarship within the discipline. Among the more pessimistic appraisals gathered was the view that anthropological institutions recruit self-selectively: "one could say that by all sorts of subtle cues, anthropology departments indicate that you have to like a certain vague, aesthetically-driven, associative style of thinking in order to join. Having recently gone through the files of more than 160 applicants for anthropology posts at my university, I can say that about five of them described their research as addressing questions that could be answered by considering relevant evidence according to a specific methodology. All the others said that they had studied x in the context of y (e.g. 'clubbing in Martinique in the context of global images of black masculinity') without saying why on earth one should consider that rather than anything else. The way my colleagues discussed the applications ... never raised that question. What people discussed was whether the resulting associations were original (e.g. 'clubbing and black masculinity had been done before by someone else in Jamaica [that's bad] but this candidate did it by focusing on gay males with children [original = good] but I found the dissertation ponderous [bad] although he did have a lucid discussion of Foucault [good],' etc.)." As this informant went on to suggest (in keeping with the ethical requirements of such research his/her views are here anonymized): any critique of obscurity in contemporary anthropological thinking may prove pointless since people are seldom "reasoned out of" ideas they were never "reasoned into" in the first place.

Where Should We Look for a New Theory of Religion?

Of all the places we should look for tools, information, and guidance in explaining religion, social/cultural anthropology is probably one of the most important. In light of the foregoing criticisms of that area of research, how is such a claim to be justified? The first point to make is that, as noted earlier, the hermeneutic vortex is rather less powerful than one might suppose. Shankland (2001) suggests that those anthropologists who, at least until the middle of the last century, were quite comfortable with the science label, were actually doing much the same kind of field research as contemporary anthropologists:

> It is commonplace to claim to reject this earlier research as a paradigm for today's activity. However, what in fact has changed? Much less then might be supposed from the apparently sweeping rejection of the past. If Malinowski were to return and make a brisk tour around today's departments, he would be astonished, not at the differences, but at the substantial similarities with the research and fieldwork model that he proposed.

Thus, even if the rise of hermeneutics has made scientific theory building somewhat unfashionable, and to that extent has genuinely obstructed it, little has changed with regard to the empirically oriented methodologies of anthropologists. Shankland rightly points out some the disadvantages of this lack of fundamental change in approaches to field research (in particular the failure to develop more collaborative approaches to data-gathering) but the fact remains that no other discipline rivals anthropology's breadth of information on religious variation. Even if some of that information has been obscured by poor strategies of presentation and lack of theoretical substance, a huge volume of ethnographic data remains and will continue to grow, as the bedrock of anthropological knowledge. What is special about that knowledge is not only that it encompasses many populations on the margins of, and even entirely outside, the so-called world religions, but also that it is based on first-hand, intricately detailed experience.

Linked to this, my second point concerns the unique value of long-term qualitative research based on participant observation. Conducted in a systematic way, this kind of research is capable of generating a much richer picture of religious discourse and practice than even the most advanced techniques of archaeology and historiography, which after all have only physical remains and surviving documents as sources of information. But ethnographic research cannot tell us everything. We still need to know how the situations we observe came to be as they are, and so we need to work in partnership with historians and archaeologists. We also cannot rely exclusively on what people say to us, or even to each other, about their religious ideas and actions. If we are to learn about various forms of implicit learning and cognitive organization some openness to experimental techniques is indispensable. If we want to know how religious knowledge is remembered and transmitted, we should be free to borrow designs from psychological studies of recall in real-world (i.e., non-laboratory) settings. Our methodologies can be as eclectic as we like, without running the risk of polluting or replacing those upon which are discipline was originally founded.

Just as the knowledge obtained from participant observation can be enriched and extended by other sorts of data, so our capacity for obtaining and understanding those other data may depend on adequate qualitative research. Imagine what would happen if a cognitive psychologist, whose only experience of experimental research had been with American undergraduate participants, was then sent to run the same experiments in inner New Guinea. Unless this psychologist had first conducted a long period of something very like participant observation, it is almost certain that the experiments would be a disaster. Anthropological knowledge may be vital for the gathering or cultural/psychological data in all situations with which researchers are profoundly unfamiliar.

My third reason for stressing the importance of anthropological knowledge is simply that the discipline has recently spawned more than it fair share of instructive perspectives on religion. Important contributions have also undoubtedly come from archaeology (e.g., Steven Mithen 1996), comparative religion (e.g., Ilkka Pyysiäinen 2001), biology (e.g., Robert Hinde 1999), and philosophy (e.g., R.N. McCauley 2001). In some cases, collaboration between these disciplines has proven especially productive (e.g., Lawson and McCauley 1990, McCauley and Lawson 2002). But the list of scientific theories of religion advanced by social/cultural anthropologists is unrivalled by any other discipline. The works of Astuti (2001), Atran (2002), Bloch (1988), Boyer (1990, 2001a), D'Andrade (1995), Guthrie (1993), Sperber (1985, 1996a; Sperber and Wilson 1986), and Strauss and Quinn (1997), for instance, owe a profound debt to the cognitive sciences (of which they have come to form part) but the anthropological contributions *remain* distinctively anthropological. How is this to be squared with my earlier claim that the hermeneutic turn in anthropology has obstructed explanatory theories of religion?

The answer, I think, has to do with the dominant aesthetics of social and cultural anthropology. The preoccupation with critical originality has turned the discipline into a sort of intellectual red light district in which almost anything goes. To me, the squalid and depressing side to this is an overly permissive attitude towards obscurity and muddled argument. The more positive aspect to it is that anthropology seminars and conferences provide fairly safe places in which to test out really wild and imaginative ideas. Professional anthropologists seldom run the risk of destroying their reputations simply because they say a few mad things in public. When compared with the restrictive demands of more scientifically oriented neighboring disciplines, such as archaeology or psychology, one can readily see certain benefits of this. There are very few principles (apart from ordinary humanistic and liberal ones) that anthropological audiences insist upon, and no theories too sacred to challenge. The creed of relativism can be violated and even the crime of ethnocentricism (whether real or imagined) will be tolerated up to a point. Thus, to adopt a cognitivist stance may lead to unpopularity (I hope this will change), but anthropology is such a broad and tolerant church that it really does have space for everybody. This is not such a bad breeding ground for new theories, even scientific ones, as long as their creators are prepared to look for wider audiences. My point, then, is really that anthropology could have done much more (and can do more in the future) to encourage explanatory theories, by abandoning certain prejudices, out-growing its passion for obscurity, and still retaining its underlying culture of tolerance.

The other main place I think we should look in our quest for a new theory of religion is to the cognitive sciences. By that, I mean a loose conglomeration of research areas that includes cognitive anthropology (there *is* such a sub-field which, though quite small has a distinguished history), neuroscience, artificial intelligence, certain fields of linguistics, and cognitive psychology. The reason we should look there is simple. Culture (including religion) is an effect of mental activity, which arises through the interaction of people with their environments. There are, of course, aspects of the way our minds work that appear to be very much the same everywhere. Understanding these universal mechanisms holds out the promise of explaining how their activation might constrain and shape patterns of culture and social organization all around the world. The challenge is to bring together the knowledge of several disciplines. We need to understand the psychic unity of our species, but we also need to know as much as possible about the diversity of human culture and social morphology. This should be a process, not of academic invasion and colonization, but of alliance and collaboration.