

Conjectures, Refutations, and Verification: towards a testable theory of ‘modes of religiosity’

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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.

(Popper 1963: 34-5, emphases in original).

Summary

Most of the contributors to this review forum share with me certain fundamental assumptions concerning the general nature of explanation in the social and cognitive sciences. Explanatory theories, we agree, should be explicit, precise, consistent with the weight of available evidence, generalizable in principle, and testable in practice. Speculations, assuming they can be tested, may pay off in the long run but the chances of success are greatly increased if speculation can be founded upon assumptions parsimonious enough to be supported by what we already know. So we have license to be ambitious but there are limits to how high we can fly.

On the whole, the contributors to this review forum have judged *Arguments and Icons* to measure up reasonably well by these criteria. But, as they also point out, the book could have been better on a number of fronts. I welcome their critical observations and suggestions for improvement just as warmly as their generous praise. It is only through such processes of critical engagement, and ultimately of collaboration, that we can hope to test and develop many of the specific predictions advanced by the theory of modes of religiosity. Some of the criticisms raised in this review forum will be directly countered, others accepted and used as a basis for further modifications and refinements to the theory. I hope, however, that this will be only part of a wider and more enduring dialogue, through which systematic cross-disciplinary collaboration in the study of the cognitive causes of religion might spread and flourish.

Psychological Explanations of Religion

Contrary to certain intellectual traditions in social and cultural anthropology,¹ I assume that cultural meanings in general (and religious ideas in particular) are located only in human minds, and do not migrate above or between them. Society is not greater than the sum of its parts, nor is culture external to individual minds. Both are merely distributions of (countless and enormously complex) mental events, responding cumulatively to present and prior interactions between individuals and their environments. These mental events and interactions together seem to form patterns that we, as a species, have specialized capacities for recognizing and interpreting. It is helpful to assign labels to these patterns, for instance to describe certain forms of political association as 'hierarchical' or 'centralized'. People everywhere do this kind of thing, regardless of whether or not they have been trained in an academic tradition of social theory. People talk of political offices (the 'monarch', the 'chief', etc.) and items of culture ('the 'painting', the 'book', etc.) as if they constituted meaningful categories outside the minds of individuals. They don't. The office of king or chief is a mental concept; paintings and books are just arrangements of colours and marks on pieces of paper or canvas. Even clouds in the sky are not really 'clouds' but only distributions of moisture in the air. What makes these distributions into shapes, with the meaning of clouds, are the highly structured and complex operations of perceptual and cognitive equipment in human brains. Although distributions of certain things (whether social, meteorological, or some other) can be translated into patterns, the act of translation is a mental process and the patterns themselves are mental products. Insofar as these mental processes and products become distributed in ways that we can recognize, they appear to us as varieties of social morphology and cultural form. But they remain distributions of mental phenomena and nothing more.

As long as we remember this, we can use conventional (including specialist) labels for the cultural patterns we perceive and cognize. But, because they are really just distributions of mental events and processes, they can be explained in terms of general psychological properties. Most mental events have little effect on other people. Some, however, become recognizably recurrent (not as duplicated mental states but at least as relevantly similar ones across populations). In many cases, it is not a matter of similar but of *complementary* schemas becoming widespread – such that differently distributed knowledge produces varied 'divisions of labour'.² Patterns of distribution are best envisaged as outcomes of selection, and the task of explanation is really one of identifying the factors driving selection (see Sperber 1996). In *Arguments and Icons*, as well as in subsequent publications (Whitehouse 2001a, 2001b, 2002a, 2002b, 2002c), I have suggested that two variables are of particular importance in this process of selection: memory and motivation. To be transmitted successfully, actions and ideas must be capable of being recalled but people must also be motivated to repeat them, and so to pass them on.

Where some of the contributors to this review forum take issue with the details of my argument, if not with the general epistemology outlined above, is in relation to my account of the sources of motivation that drive transmission. For me, religious motivation derives from the activation of various forms of revelation. Pascal Boyer argues that it derives instead from evolved mechanisms of contamination avoidance. Fredrik Barth suggests that it may derive at least in part from forms of unconscious processing, discussed in some of his earlier work with reference to psychoanalytic theory (e.g. Barth 1987: Chapter 9). Sjoblom urges us to look instead at a range of emotionally-driven effects suggested by a theory of 'somatic markers'. The only contributor directly challenging the wisdom of searching for intra-psycho explanations in the first place is, perhaps, Michael Houseman. Let's begin by addressing these viewpoints in turn.

Of all the above contributors, Boyer has done the most to advance our knowledge of the causal role of cognitive mechanisms in the selection of religious concepts. In a major corpus of research in this area

(Boyer 1990, 1993, 1994, 1996, 2001a), Boyer has shown how a limited number of evolved (and therefore universal) predispositions to process information in certain ways, accounts for the cross-cultural recurrence of precisely those representations of extranatural agency that are found in the ethnographic record. The cognitive predispositions with which Boyer is concerned are exceptionally well understood within cognitive and development psychology. The claim that they confer a transmissive advantage on religious representations actually documented, as opposed to a much larger variety of possible representations, has been supported experimentally in studies by Boyer and his collaborators (see Boyer 2001b: 87, fn7 and Boyer and Ramble *forthcoming*). It is a further strength of Boyer's approach that he has recently enriched and extended his account of the evolutionary foundations of the selectional mechanisms that explain certain aspects of religion everywhere (Boyer 2001a).

The central thrust of Boyer's critique of *Arguments and Icons* is that there is no need to go beyond some very parsimonious evolved cognitive constraints in explaining the divergent modes of religiosity with which my book was concerned. Boyer agrees with me that the two modes constitute real distributions, requiring an explanation. He agrees that the explanation lies in the organization of cognition, which favours certain distributions rather than others, operating on the principle of selection. And he endorses many of the specific hypotheses I advanced with regard to the divergent effects of alternative systems of memory, on 'patterns' of codification, transmission, and political association. Where Boyer disagrees with me is on the question of what *motivates* the forms of ritual action through which divergent modes of religiosity are reproduced.

For Boyer, the reason people perform rituals is because they 'activate, albeit in a heightened and grotesque way, perfectly normal cognitive systems specialized in the detection of and protection against contaminants in the natural environment' (p 10). This parsimonious explanation, developed convincingly by neuropsychologists in the study of obsessive-compulsive disorder, is all we really need to explain the global recurrence of organized rituals. Asking people why they perform rituals is a fruitless exercise. According to Boyer, people seldom have ready-made answers to such questions but, even if they had, these would explain very little. Contamination avoidance – which *really* explains the global recurrence of rituals – is not, on the whole, consciously recognized. So any reasons people make up, or pass on, to explain their behaviour are really just *post hoc* rationalizations, rather than explanations (see also Boyer 2001a: 262-3). In light of this, Boyer regards as potentially misguided my attempt in *Arguments and Icons* to show that: (a) rituals produce revelations; (b) these revelations help to motivate subsequent transmission; (c) revelations are valued because of the social bonds they create. In relation to all three points, however, I think my position can be effectively defended.

Why should rituals ever provide occasions for revelation? The answer has to do with the nature of ritual itself. A number of properties may be associated with ritualization,³ but the most salient of these for the purposes of my argument is that ritual actions are always non-technical actions, in certain respects. In part, what enables us to recognize an action as ritualized is a lack of 'technical motivations' (Sperber 1975) with regard to at least some details of the prescribed action scheme. Religious people in various parts of the world do a huge variety of strange things in the context of rituals that participants and observers intuitively recognize to be entirely superfluous to any technical purpose. Alternatively expressed, rituals lack any *intrinsic* intentional meaning (Humphrey and Laidlaw 1994). The inferential processes entailed in ordinary communication become ineffective in the ritual context, and judgements of relevance insufficient to drive shared interpretations (see Sperber and Wilson 1986). This makes rituals potentially *puzzling* forms of action. They allow an exceptionally wide range of possible interpretations or 'exegesis'.

As a kind of shorthand, we might refer to the process of inventing ritual meanings as one of 'spontaneous exegetical reflection' or SER (Whitehouse 2002b, 2002c). Under certain conditions, the

process of SER can be inspirational, moving, and epiphanic. This is one route to what we might call 'religious revelation'. But people do not always respond to rituals in this way. Another possibility is that people are told explicitly the one and only authoritative meaning of a ritual, and obstructed in various ways from speculating further. This too can involve forms of revelation, but of a very different kind. In certain fundamentalist religious movements, for instance, people may be told that the original (and thus most authentic) meaning of a given ritual has been discovered and, in the process of being persuaded of this, may adopt attitudes of righteous dogmatism that have an intense (some might say 'fanatical') character. Inspiration and revelation are part of this process, even though the psychological mechanisms involved may be rather different (but not completely different) from those entailed in SER. A further possibility is that people entertain no exegetical knowledge with respect to the rituals they perform.

In *Arguments and Icons*, I developed the 'frequency hypothesis' to account for these three scenarios, based on ideas originally advanced in earlier work (especially Whitehouse 1992 and 1995; see also McCauley 2001). SER, I predict, will be most virulent in circumstances of rare, climactic ritual transmission. A good example would be the agonies of ritualized septum-piercing experienced by Ankave boys in Papua New Guinea (Bonnemere 2001). The boys are told little or nothing in the way of exegesis, although they know that the ritual actions are not technically-motivated. The experience is remembered for the rest of their lives and it remains profoundly puzzling. In the course of life, many subsequent experiences (especially their involvement in the initiations of other boys) remind them of their traumatic ordeal and clues as to its possible symbolic properties gradually mount up. Knowledge of this sort is constructed slowly over the course of a lifetime at increasingly higher levels of explicitness (eventually available as cross-domain analogic processing). The generation of SER in such circumstances is an intensely revelatory experience. Barth's (1975) account of of this kind of religious experience still stands out as the fullest and most penetrating ever published.

Consider, by contrast, processes of revelation among followers of Martin Luther in sixteenth-century Europe. For them, novel ideas concerning (among other things) the exegesis of Christian liturgical rites constituted a major source of revelation (e.g. McGrath 1993). These revelations were not, however, generated via processes of SER, but delivered through a highly persuasive corpus of religious teachings transmitted orally, and sustained through extremely frequent rehearsal and repetition, primarily in the form of sermons. Routinization, however, opens up other possibilities, including the third scenario identified above in which exegesis is lost altogether. For instance, most contemporary Lutheran worshippers in Finland could not tell you why the minister faces away from his congregation at various points during the church service. Frequent repetition can result in such deeply habituated forms of action that its performance requires no explicit knowledge at all. And lack of reflection on *how* to participate can inhibit reflection on *why* one participates, consequently restricting the production of SER. This makes people susceptible to authoritative exegesis, and routinization provides a favourable climate for its transmission. But what if this potential is not realized? We end up with empty procedures, aspects of the ritual process that hold no meaning for most participants.

So, while rituals *can* be associated with revelatory knowledge, this is not always the case. One aim of the theory of modes of religiosity is to explain when, how, and why these processes of revelation are activated. The next question, of course, is whether these revelatory processes can be among the motivations driving ritual participation and transmission. Boyer says they cannot, because the 'real' causes must be psychological mechanisms that are both universal (cross-culturally invariable) and outside conscious awareness. There are two reasons to resist this conclusion.

First, we cannot explain variables in terms of constants. Some religious adherents give up all their rituals voluntarily, while others would lay down their lives to preserve them. Boyer's contamination-

avoidance mechanism, construed as a pan-human disposition towards ritual participation, cannot explain the kind of variable commitment which is at issue.

Second, I can think of no good reason to suppose that all cognitive causes of religion (or of any other cultural domain) reside exclusively in implicit mechanisms. Consider the case of language (as does Boyer, pp 9-10). Many grammatical rules are undeniably applied via implicit mechanisms but it is also perfectly possible to be conscious of the process. For instance, until about seven years ago, I used the word 'however' quite freely as a conjunction, until it was pointed out to me by a colleague that this was a literary *faux pas*. For some time afterwards, I was conscious of adjusting my speech to accommodate this 'new' rule, just as I have, since my schooldays, tended quite deliberately to anticipate and to avoid split infinitives and other such verbal or written transgressions. Clearly, most of my grammatical knowledge is applied implicitly, even if some of it is in principle capable of being formulated explicitly. But I am aware also that my speech is often guided by the conscious application of minor grammatical conventions. The fact that the latter are entertained explicitly does not in any way diminish the fact that they exercise a causal role in the syntactical organization of my utterances. Why should it be any different in other cognitive domains, such as ritual action?

When people tell us that they participate in rituals *because* successful harvests or personal salvation depend upon it, why should we disbelieve them? When people tell us that they ceased to perform rituals *because* of a crisis of faith or a loss of belief, why should that not be true? The answer cannot be because the causal efficacy of such beliefs is untestable. The theory of modes of religiosity proposes the following falsifiable prediction: *any religious tradition composed entirely of rituals for which no religious justifications are thought to exist will rapidly become extinct*. Boyer's contamination-avoidance theory would predict the reverse. I think the weight of evidence from both ethnography and historiography supports my hypothesis over his. But I admit that we need much more data before we can reach confident conclusions on this matter.

Boyer's final point is that revelations (and cultural knowledge more generally) are not valued because of their contributions to social cohesion and identity. He points, for instance, to the fact that people readily learn local dialects without intending to establish localized social bonds (Boyer, p 10). My response to this has two aspects. On the one hand, I can certainly recall making a considerable effort to learn and adopt the dialectal norms of my peers at the widely differing schools I attended in childhood. And my more recent experience of living in Belfast, and observing the conscious efforts made by those around me to accentuate and display their religious and political allegiances, has helped to persuade me that the high value placed on certain cultural markers can have everything to do with their identity-conferring properties. On the other hand, however, this was not a major plank of my thesis in *Arguments and Icons*. What I was suggesting was that the scale of communal identities and the intensity of social cohesion were outcomes of divergent patterns of transmissive frequency and cognitive processing via differing systems of memory. In this model, social identity and cohesion were treated as consequences rather than causes, even though (once established) those consequences could have further ramifications of their own. For instance, many of the markers of Loyalist identity in Belfast refer in complex ways to highly schematized historical and religious knowledge somewhat patchily transmitted in Protestantism via the more or less routinized practices of the doctrinal mode. But, regardless of what the natives tell you, these referents are not particularly relevant to explaining the special valence of such identity-markers in Northern Ireland. This valence derives primarily from the imagistic ritual practices of localized Loyalist groups, such as ritualized violence and its psychological consequences for both victims and perpetrators, rites of initiation within paramilitary cells, and so on. In other words, Loyalists in Belfast value their identity markers principally because of their participation in imagistic practices, not the other way around. But, having accorded high value to them, these markers are then quite deliberately used to intimidate

enemies, to celebrate in-group solidarity, to legitimate the punishment of defectors, and (yes!) to *motivate* future ritual performances. The relationship between ritual and revelation becomes one of mutual reinforcement but the causes and origins of the motivational states at issue ultimately reside in the way cognition is organized and distributions of cultural markers stabilized through selection.

Like Boyer, Barth has suggested that the motivations driving participation in rituals may not be consciously accessible to the participants themselves. In *Arguments and Icons*, I suggested that the use of Freudian models in attempting to reconstruct such unconscious processes ran against the grain of Barth's (and my own) empiricist instincts. I am less sure of that now, because I have since learned of attempts within psychoanalytic psychology to construct a body of experimental evidence relating to the operations of a dynamic unconscious, formed through deep motivational conflict (Luborsky 1976, 1977, Luborsky and Crits-Christoph 1990, Barber and Crits-Christoph 1993), and I have been persuaded of the potential relevance of these findings for our understanding of religion (for a fuller discussion, see Nuckolls 2001 and Whitehouse 2001a). What we do not yet know is whether unconscious motivational conflict has a causal role in the success of all religious traditions or just some, and there is at least some anecdotal evidence that the master-plan of Freudian theories of religion, the Oedipus Complex, may not be applicable cross-culturally (e.g. Strathern 1992). But even if such a complex turns out to be as ubiquitous as Boyer's predisposition to acquire contamination-avoidance measures, we still have to ask whether this is a sufficient motivation for participation in rituals. As it stands, the theory of modes of religiosity predicts that it is not: without explicit revelations, people will drop out.⁴

Sjoblom suggests yet another possible psychological approach to explaining divergent modes of religiosity. Like Boyer and Barth, he suggests that we focus our attention on *unconscious* processing, this time guided by Damasio's (1994) theory of 'somatic markers'. Although not elaborated at length, Sjoblom's proposal is that explicit recollection of past ritual performances may play a lesser role in subsequent transmission than people's unconscious responses to cues in the unfolding process of ritual performance. These responses, he suggests, are shaped by emotionally-laden intuitions regarding 'how the ritual should be done' as opposed to conscious recall of 'how it was done in the past'.

Sjoblom raises an interesting point, one that has (as he notes) been developed further in the cognitive study of religion and ritual by Pyysiainen (1996, 2001).⁵ Such an approach, it seems to me, has great potential to build upon and extend the general perspective I have been proposing (and vice-versa). The same, as I hope to have made clear, would be true of arguments from evolutionary psychology concerning contamination avoidance (as suggested by Boyer) or from psychoanalytic psychology (as suggested by Barth). But these different approaches are likely to explain different things, and should not (at this stage of development in our knowledge) be seen as rival theories. Just as contamination avoidance might explain some constants in the human disposition towards ritual participation, so theories of a dynamic unconscious or of somatic markers might explain other recurrent aspects of ritual transmission and transformation over time. But none of the above approaches can account for the clustering of divergent suites of variables that the theory of modes of religiosity hopes to explain. Such projects are compatible, not mutually exclusive.

The only review in the current batch that *seems* to advance a conflicting perspective is that of Houseman (although I think the conflict may be more apparent than real). On the face of it, Houseman appears to be suggesting that the causes of divergent modes of religiosity do not lie in what he calls 'internal, psychological states', 'mental operations', or 'interior, intra-psychoic mechanisms' (p 18). Rather, the causes lie in 'relational forms' (p 19) which, largely by implicit contrast, we are encouraged to suppose are somehow 'external' to mentation. Interpreted this way, it seems to me that Houseman's argument is founded on faulty ontology. Where are social relations to be located if not in the minds of those who entertain and act upon them? But Houseman's argument may be read another way. His

objection might be, not that the causes of modes of religiosity are non-psychological but that the theory (as set out in *Arguments and Icons*) focuses on the *wrong sorts* of psychological causes. Interpreted in that light, Houseman's argument looks rather more like Boyer's, Barth's, and Sjoblom's. In particular, he suggests that symbolic meanings and revelations may not be terribly important outcomes of ritual, and may not indeed help to motivate subsequent transmission (cf. Boyer's critique, discussed above). Rather, the cognitions generated by ritual action are principally concerned with the re-ordering of social relations, and these are the schemas that motivate future ritual performances.

I agree that the initiation rituals mentioned by Houseman mark a change in each initiate's vantage point on the world, and that they often do so by dramatizing and thereby announcing a change in his or her social relations. What is less clear to me is why this outcome should be 'more important' than the revelatory knowledge that is typically generated through such rituals. I can, however, think of reasons why the reverse is probably true – that the transformation of social relations is a *less* important aspect of ritualization than the transmission of revelations. The main reason is this: rituals are not necessary for the transformation of social relations, whereas they are essential for the construction of revelatory processes such as those associated, in the imagistic mode, with SER. Graduation ceremonies and puberty rites, for instance, are ways of transforming social relations. Both can be dispensed with, however, without jeopardizing these transformations. Graduates can be sent their certificates in the mail, and children can gradually acquire adult status without this being ritually marked. The reason why rituals are not strictly necessary to effect changes in social relations is that the latter are capable of becoming evident of their own accord, simply through their routinization in everyday life. Rituals can mark such transformations as more sudden and dramatic but the claim that they *effect* such transformations is mere ideology, rather than a natural 'given'. By contrast, one cannot set in train psychological processes of revelation, of the sort described for instance by Barth in his study of Baktaman initiations, *without* the traumatic ordeals that novices are obliged to endure.

Transmissive Frequency: some clarifications

In *Arguments and Icons*, I proposed that transmissive frequency constituted a crucially important variable affecting the activation of systems of memory, and a range of sociopolitical features consequent upon this. Several of the contributors to this review forum have commented critically on the way this hypotheses was constructed and expressed – most notably McCauley, Barth, Houseman, Malley, and Sjoblom. Many of their comments on this matter are highly instructive and some have important theoretical and/or methodological implications.

R. N. McCauley persuasively identifies the dangers of talking too loosely about transmissive frequency (p 27-8). If we are seeking to ascertain the frequency of a particular ritual, do we mean by that: (a) the frequency with which it occurs in a particular population (if so, which?); (b) the frequency with which individual worshippers participate (if so, does this vary and is any variation random or is it 'by category of participant'); (c) the frequency of the ritual over a particular time period (if so, what do we do if it is performed ten times in a given month but never again for ten years?); (d) the frequency of rituals performed or rituals observed, or both? These questions (and others are possible) are clearly of paramount importance, and McCauley is right to call me to account for not being more explicit about the relevant measures of frequency.

As regards the activation of those memory systems essential to the crystallization of modes of religiosity, measures of frequency should pertain only to individual participants (not populations, and not mere observers). As far as the activation of 'Flashbulb Memory' (FM) is concerned, it appears to be crucially important that a given traumatic episode is surprising and novel. Long-term repeated exposure to

the same ordeals appears to have mnemonic effects very different from those associated with FM (see Terr 1991), and would not be adequate to account for the coalescence of imagistic features. Subsequent involvement in such ordeals, in the role of co-ordinator, helper, or observer (rather than victim) is an entirely different matter (see below). Likewise, when it comes to highly routinized rituals, the frequency measure must be individual participation. Mere observation would not be sufficient to activate the implicit procedural knowledge entailed in the reproduction of frequently-performed rituals. A more passive role is possible when it comes to the learning of explicit ritual scripts, exegesis, doctrine, narratives, and other verbally-articulated religious knowledge. As with all forms of language-based learning, active rehearsal can be a useful means of consolidation but it does not appear to be essential.⁶ Dutiful listening can be enough. But since the stable reproduction of an orthodoxy depends in part on the SER-suppressing effects of implicit procedural learning, high-frequency ritual *participation* is an essential element of the doctrinal mode (see Whitehouse 2002b and 2002c).

Barth and Houseman make some very valuable observations concerning the relationship between participation and frequency in the imagistic mode. Both point out that the FM-effects of one-off initiatory ordeals may not be sufficient to provide the sorts of recall necessary for the re-enactment of these rituals in the future (Barth, p 15; Houseman, p 20). Rather, they both suggest, it is subsequent and *repeated* involvement in the co-ordination of initiation rites for fresh cohorts of novices that provides the richly schematized knowledge of the ritual process needed for confident orchestration of initiations as experienced elders. On current evidence, this seems a plausible corrective to the argument presented in my book. But if the one-off ordeals of novices in initiations are not sufficient to allow subsequent transmission, why should they be necessary at all? Given my hypothesis that massively elevated negative arousal in one-off ritual ordeals is a reliably recurrent feature of the imagistic mode *in general*, there had better be a good answer to this question.

The problem is not, to be fair, entirely new to me. Indeed, in my most recent publications I have tried to make a start at resolving it (Whitehouse 2002a, 2002b, 2002c). FM, I now suggest, does not provide a sufficient set of reference points to which those responsible for subsequent re-enactments refer (which is not to say it has no importance in that regard). The most critical contribution of FM is rather to generate SER, and thus *motivation*, via subsequent primes and cues in the environment. In some cases, this may take the form of intrusive flashbacks of the sort widely associated with ‘post-traumatic stress disorder’ (Conway 1995: Chapter Four). In others, recollection may be rather less emotionally intense, as Barth has suggested in his review (p 15). But the act of recall for intrinsically very puzzling episodes provides ample ‘food for thought’ through years of cautious and tentative contemplation. SER-production may take the form of a highly retarded process of ‘representational redescription’ (Karmiloff-Smith 1992, Whitehouse 2002b, 2002c). The victim of a ritual ordeal initially recalls only ‘a few vivid but cryptic flashbulb moments’ (Barth, p 15). These recollections are associated, at first, with rather vaguely formulated intuitions about the possible symbolic motivations of the ordeals one has endured – much as described in Barth’s (1975) pioneering study of Baktaman initiation. Over time, these partially explicit inferences come to be entertained more confidently and perhaps more elaborately. Future involvement, in the role of ritual co-ordinator or observer, could certainly play an important (perhaps vital) role in this process of representational redescription. Very experienced ritual experts and elders achieve such an advanced stage of SER-production that their exegetical insights are largely available to cross-domain, off-line processing with all the properties that Barth associates with ‘analogic’ codification. The knowledge constructed in such a fashion is not only constitutive of the traditions most valued ‘revelations’ but, I would argue, plays a hugely important role in *motivating* inter-generational transmission. The full case for such a line of interpretation is set out elsewhere (Whitehouse 2002b, 2002c) but I admit we need more data, especially of an experimental kind,⁷ in order to understand these processes more fully.

If our understanding of production of SER and motivation in the imagistic mode remains incomplete, this may be said also of the way orthodox exegetical knowledge in the doctrinal mode serves to motivate worship (and religious commitment more generally). With regard to the latter, some important questions are raised by Brian Malley. The doctrinal mode, he points out, presents special problems of motivation. The revelatory quality of its teachings may well be especially intense during periods of reformation and spread – when doctrines appear novel and inspirational, problems of religious motivation are kept at bay. But what happens when the same religious teachings have been heard thousands of times, and the content of sermons becomes entirely predictable? Like me, Malley recognizes the potential for *tedium*, in conditions of religious routinization (p 6):

The tendency, in discussions of religion, has been to emphasize the sacredness, profundity, passion, ecstasy, and danger of religious experience. To be sure, such sentiments are important. Yet in fact the experience of practising religion is more often dull, boring, or even irritating. (It is not without reason that the ‘world’ religions have some notion that discipline is required.) Rather than neglecting such sentiments as somehow non-genuine or spurious, Whitehouse suggests that the tedium of the doctrinal mode is an inherent destabilizing factor, creating openings for more exciting splinter movements and requiring constant investment for the preservation of doctrine.

If tedium is a predictable outcome of excessive routinization, the ‘Achilles heel’ of the doctrinal mode wherever we may find it, then how do we account for the long-term success of certain very ‘pure’ examples of the doctrinal mode in American Protestant Christianity? In short: ‘why doesn’t the tedium effect take hold?’ (Malley, p7). Malley supplies a plausible answer to the question.

Repetition, Malley suggests, can have the effect of increasing the relevance of religious teachings in people’s lives. The teachings themselves may be thoroughly familiar but their continual reiteration, in proper ways, allows endlessly novel applications of this knowledge to the ever-shifting contexts of daily experience. In other words, as suggested in *Arguments and Icons*, the doctrinal mode can evolve in such a way that its techniques of persuasion have enduring motivational force. Of these techniques, I suggested that rhetorical, dialogic discursive practices are particularly important (e.g. 2000: 93-4). Styles of sermonizing that ‘draw the congregation in’ to a kind of conversation founded around questions and answers provides a template for the interpretation of seemingly all aspects of daily life. The template used in the pulpit can be used also in the shopping mall, the work place, and the living room. This is at least one aspect, I think, of the ‘relevance’ of some forms of Christian teaching that Malley is referring to. But like all forms of evolution, this one operates on principles of selection. Persuasive forms of rhetoric did not become widespread in certain American Protestant churches simply because they had exceptionally talented preachers. Rather, countless acts of experimentation happened to lead cumulatively to the techniques we can now observe (and which have since spread to many other parts of the world). Meanwhile, other forms of Protestantism, as found for instance in the Church of England, have unwittingly developed less successful forms of codification and transmission. The lesson appears to be that it is not only the *novelty* of the message that motivates participation in the doctrinal mode, but also the extent of its *relevance* and *applicability* in everyday life. Again, however, we need more empirical information on patterns of religious involvement and spread in the doctrinal mode, and the psychological mechanisms driving them.

Sjoblom, by contrast, presents the frequency hypothesis with a problem, which, if valid, would be much more serious than those addressed above. He focuses his discussion on four kinds of Finnish celebrations, associated respectively with Christmas, Easter, the start of the New Year, and the marking of midsummer. According to Sjoblom, the first two (Christmas and Easter) are constitutive of a doctrinal mode of religiosity; the second two (New Year and midsummer celebrations) sustain an imagistic mode

of religiosity. The problem is this: all the above celebrations have the same frequency (all occurring annually), calling into question my hypothesis that variations in frequency are *necessarily* implicated in the activation of alternative systems of memory, which in turn drive selection of the two modes of religiosity. To challenge the frequency hypothesis is to strike at the very heart of my theory.

If Sjoblom's cases really present the kind of problem he suggests, then they must do so in relation to the theory I am actually advancing and not some other. The crunch question is whether the various celebrations considered by Sjoblom are constitutive of modes of religiosity in the way he suggests. There are several reasons to suppose that they are not (although the information provided to Sjoblom is insufficient to say with any certainty).

To begin with the Christmas and Easter celebrations, it should be emphasized that these are probably *part* of the doctrinal mode rather than being constitutive of it. In order for any Christian tradition to be preserved in a recognizable form, it must successfully transmit certain teachings and liturgical rites on a basis *much more frequent than once-a-year*. The intricate and extensive bodies of knowledge that make Christmas and Easter specifically Christian celebrations (rather than, as for many of us, merely pseudo-religious holidays), derive from very frequently-rehearsed beliefs and practices. Even the most infrequent⁸ rites of Christianity (e.g. weddings, baptisms, and funerals) are cannibalized from weekly liturgy (cf. McCauley 2001). Thus, celebrations of Christmas and Easter in Finland (or anywhere else) do not provide evidence of a doctrinal mode of religiosity, founded upon forms of ideological and ritual transmission with a frequency of once-a-year. Insofar as these are distinctively religious occasions, they are based on forms of transmission with a very much higher frequency than that.

New Year and midsummer celebrations present a different set of problems. Above all, it is not clear to me why these should be constitutive of an imagistic mode of religiosity. In order for that to be the case, it would be necessary to show: (a) that these activities generate widely different FM-effects and (b) that the FM-effects drive processes of SER that motivate subsequent transmission. With regard to (a), I suspect that these annual celebrations unfold in ways that accord with substantially schematized knowledge available in semantic memory. Elements of particular celebrations that are recalled episodically (and thus survive the effects of alcohol-induced amnesia!) are probably not of the FM-type. More importantly, with regard to (b), there may be no special religious valence attached to details encoded episodically. Why? Because it is likely to be only the non-ritualized, technically-motivated occurrences that survive in episodic memory (e.g. 'hilarious' and/or titillating drunken escapades). By contrast, ritualized aspects of the event, conforming as they do to schemas in semantic memory, are less likely to be recalled in the long run as distinct episodic moments. Since I have never been to Finland on New Year's Eve, and unfortunately just missed the midsummer celebrations on my first visit to the country, I cannot attest to any of this with confidence. But, on the strength of my limited knowledge, I suspect that midsummer celebrations resemble rather more closely the 'turning of the Minister's back' in Lutheran church services, than the revelatory acts of Baktaman initiation. Midsummer celebrations, Sjoblom tells us, once had a widely-known, verbally-transmitted exegesis, which people no longer recall. These annual parties, however, are unlikely to generate SER of the sort that prevails in the imagistic mode of religiosity. Again, this is a matter that could only be resolved through systematic empirical investigations.

Testing and Applying the Theory

The key predictions of the theory of modes of religiosity can be broken down into twelve basic scenarios for religious transmission which can only have four kinds of outcomes: doctrinal effects, imagistic effects, survival without doctrinal or imagistic effects, and extinction of the practices in question. Justin Barrett, a

psychologists at the University of Michigan,⁹ has summarized these key predictions in the form of a series of flow-charts (Fig. 1). The predictions represented here are essentially those advanced in *Arguments and Icons* but Barrett's mode of presentation is very much more concise. The starting point, with regard to each set of predictions, is the combination of arousal level (high/low) and modality of codification (doctrinal/non-doctrinal), allowing four possible combinations (low arousal/doctrinal, low arousal/non-doctrinal; high arousal/doctrinal; high arousal/non-doctrinal). The consequences of any one combination depend upon transmissive frequency (high/low). The resulting eight scenarios have distinctive implications for the processing of religious and ritual knowledge in *memory*, and for the *motivation* of future participation via alternative modalities of revelation. This produces at least five scenarios that should lead to the non-selection/extinction of religious and ritual practices:

- (i) low arousal/doctrinal practices, with low-frequency transmission and no external mnemonics (e.g. written guidance, visual representations, electronic recordings, compositionality¹⁰, etc.) to support them;
- (ii) low arousal/non-doctrinal practices, with high-frequency transmission (see the prediction presented above, contra Boyer);
- (iii) low arousal/non-doctrinal practices, with low-frequency transmission (ditto);
- (iv) high arousal/doctrinal practices, with high-frequency transmission but poor encoding of verbal material (due to elevated arousal);
- (v) high arousal/non-doctrinal practices, with high frequency transmission.

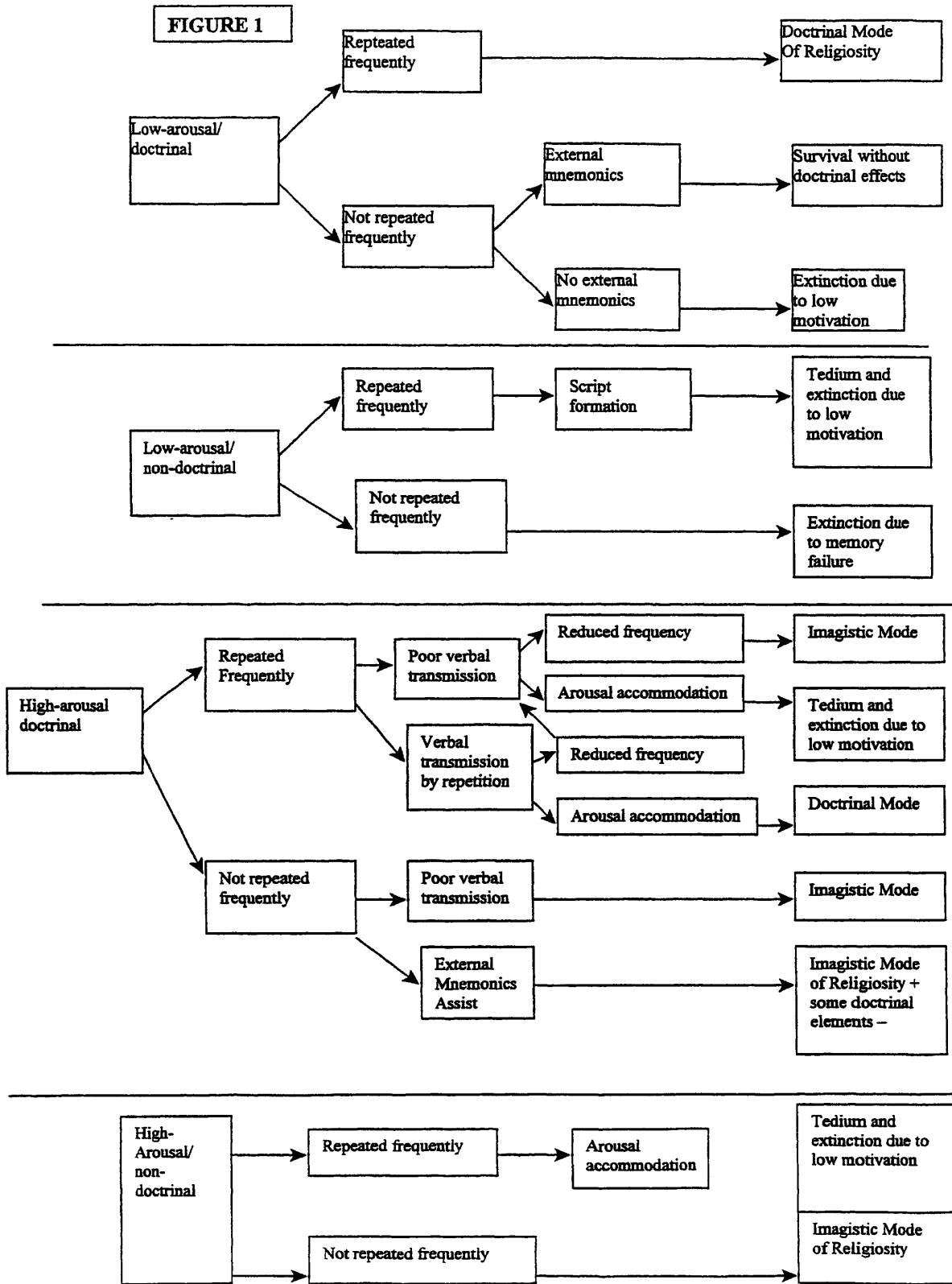
What would count as a refutation of my model would be clear ethnographic or historiographical evidence of the survival of religious traditions founded exclusively or primarily on *any* of the above scenarios. Likewise, we need evidence to support or challenge those predictions (in Fig. 1) that relate, more positively, to the production of both doctrinal and imagistic effects. In light of the data I have so far encountered, I am tentatively optimistic that the modes of religiosity theory might (one day) be verified, perhaps in a modified or refined form.

Martin's review highlights both the challenges and potential rewards of empirical verification. One challenge is to find out whether the predictions summarized in Fig.1 are supported by what we already know, or can find out, about religions everywhere. Martin recognizes that this challenge is laid, in the first instance, on the doorstep of social and cultural anthropology, as the main repository of *detailed* knowledge of religious variation. Martin argues, however, that an ethnographically well-substantiated version of my theory might have considerable benefits for the construction of historiographical knowledge (pp 31-2). We stand to gain from an opportunity to 'fill in the gaps' of our understanding of religions long since abandoned or transformed. Martin's recent use of the theory of modes of religiosity to explain the remarkable success of early Christianity is exemplary in this regard (Martin *forthcoming*).

In view of the above, I must confess to being a little disappointed by Trompf's contribution to the review forum, which seems neither to characterize my hypotheses accurately, nor to respond to them in any systematic way. Trompf's considerable knowledge of Melanesian religions (which is at least partly ethnographically-based) has been acquired over several decades and might even be described as 'encyclopaedic' (Trompf, p 41). As such, it is of a sort that *could* be immensely valuable to us. But in that case it would have to be used to respond to specific hypotheses rather than to impressionistic feelings about them. Trompf's lament, 'ah, the complexities of Melanesia', challenges us (well, it challenges me) to ask how we might carve up that complexity at the joints, rather than merely to bask in awe of it.

'Pure' examples of modes of religiosity will be hard to find. This was brought home to me, even as the idea of 'modes' first began to crystallize in my mind, through the experience of carrying out detailed ethnographic research on a single religious tradition in Papua New Guinea (Whitehouse 1995). In writing up that material, I was acutely aware of the difficulties of balancing the model against the

FIGURE 1



complexity of social life 'on the ground'. It was obvious, for instance, that 'imagistic' phenomena were not outcomes *simply* of episodic recall for low-frequency, high-arousal rituals; nor was the doctrinal mode, coalescing around more or less routinized religious activities, founded *exclusively* on highly schematized procedural and propositional knowledge. Sociopolitical variables were, if anything, even harder to pin down, and much of my ethnography was preoccupied with the extraordinary complexity and flux of communal identities and of relations of power, authority, and influence (see also Whitehouse 1996). In consequence, the modes story turned out to contain many digressions and sub-plots. The reason this did not put me off, however, was that the modes story still seemed to me the *main* plot, in explaining religious transmission, and it was only in relation to this that the other bits of the story could be recognized as relatively *peripheral* details and embellishments. Thus, the theory provided a sense of scale and structure, impelling me deeper into the ethnography as well as forcing me to develop some generalizing ambitions.

The two modes are nothing more than basins of attraction around which certain patterns of codification, transmission, and political association tend to coalesce. But degrees of coalescence might vary quite dramatically across space and time, exacerbated by temporary, historically contingent factors, such as droughts, famines, disease, genocide, invasion, and so on. A crucial question for my theory is whether massive interferences in the coalescence of modes are indeed only ever temporary, such that they are counteracted in the long run by the underlying psychological constraints proposed by the model. Despite storms and tempests, does the tide always tend to carry our variables back to their respective attractor positions? To test this out really requires a broad canvas, depicting long-term patterns of variation across wide regions, and taking into account entire traditions (rather than bits and pieces of one, or of several ones). *Arguments and Icons* tried to initiate such a project, for the region of Melanesia, but it was only a limited start.

As should now be clear, I believe the way forward is through inter-disciplinary collaboration. Thanks to a major grant from the British Academy, three specialist research teams in the fields of anthropology, history/archaeology, and cognitive science have now been assembled,¹¹ with the aim of testing the theory of modes of religiosity to the limit. From humble beginnings, and thanks to a mushrooming of interest in the project that none of us anticipated, we now have over sixty scholars contributing data and ideas to this enterprise. Numerous collaborative publications are expected to follow. If *Arguments and Icons* was a useful first step in this process it will, I hope, prove to be just one of many.

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Endnotes

¹ Particularly well-known examples include Durkheim's (1962) functionalism, which rejects psychological explanations of 'social facts', and Geertz's (1973) interpretativism, which postulates a notion of culture as 'public' (external to the mind).

² Hutchins' (1995) work on 'distributed cognition' currently constitutes the most thoroughgoing examination of such processes.

³ A classic review may be found in Lewis 1980: Chapter Two.

⁴ Before moving on, two points may be noted. First, Barth may not be suggesting that a Freudian unconscious provides either necessary or sufficient motivations to participate in rituals. His original publications merely suggest that such mechanisms as primary association, displacement, condensation, etc. may be implicated in patterns of ritual innovation. Second, the postulation of a dynamic unconscious plays only a minor role in Barth contribution to this forum and in his work more generally. This is one of the few areas in which I have (rightly or wrongly) expressed criticisms of his

approach, to which he has now responded. His more substantial counter-critique is addressed below.

⁵ Pyysiainen's (1996: 81) main argument, in this connection, is that extrovertive mystical experience 'closely resembles or is even identical with' what Sjoblom here describes as 'flow'.

⁶ For a striking example of this, see my description of the possession of a young girl in Papua New Guinea who, despite no prior experience as a religious teacher, showed remarkable skill as an orator when the ancestors spoke 'though' her (Whitehouse 1995: 146-8).

⁷ For a discussion of some preliminary attempts to gather relevant experimental data, see Whitehouse 2002a.

⁸ Frequency, as noted above, means 'per individual participant'. Clearly, Christian rites of passage may be frequent events for priests and ministers but for most lay participants they are relatively infrequent.

⁹ Unlike most cognitive and developmental psychologists, Barrett takes seriously the study of cognitive foundations of religious transmission, and has conducted a number of pioneering experimental studies of the way concepts of extranatural agency are processed at an intuitive/implicit level (see in particular Barrett and Keil 1996, Barrett 2000, and Barrett and Lawson 2001).

¹⁰ Compositionality, or what McCauley (2001: 138) has called (by analogy with natural language) a 'compositional hierarchy of ritual', refers to the embedding of comparatively simple rituals in a more complex and elaborate ritual process. Thus, it sometimes happens that a major ritual, which is performed quite rarely, is largely composed of a set of shorter rituals that are performed much more frequently, thus obviating the need for written guidelines to support transmission.

¹¹ Under the auspices of the British Academy Networks Project, three conferences will be held in the following areas: anthropology (Cambridge University, 2001), archaeology/history of religion (University of Vermont, 2002), and cognitive science (Emory University, 2003). The leaders of the research teams form a steering committee for the project, which first met in December 2000 at the Queen's University of Belfast, to finalize plans for the implementation of the research. Interim meetings of the steering committee are also being funded by the Templeton Foundation, as part of the International Culture and Cognition Program at the University of Michigan.

Biographical Sketch

Harvey Whitehouse carried out two years of ethnographic field research in East New Britain, Papua New Guinea from 1987 to 1989, as a doctoral student at the University of Cambridge. From 1990 to 1993 he was a Research Fellow and Director of Studies in Archaeology and Anthropology at Trinity Hall Cambridge. Since 1994, he has taught at the Queen's University of Belfast, where he is currently Reader in Social Anthropology.



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