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Against interpretive exclusivism*

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Interpretive exclusivism is the dogma that we can only understand cultural systems by interpreting them, thereby ruling out causal explanations of cultural phenomena using scientific methods, for example based on measurement, comparison, and experiment. In this article, I argue that the costs of interpretive exclusivism are heavy and the benefits illusory. I make the case instead for an interactionist approach in which interpretive and scientific approaches work together on an equal footing. Although such approaches are neither easy nor cheap, I argue that they are necessary to improve the intellectual ambition, comparative breadth, and practical relevance of anthropology as a discipline. In all these ways, incorporating rather than excluding scientific methods would improve the long-term prospects of anthropology as a flourishing field of research and teaching.

This article concerns a serious problem with contemporary social and cultural anthropology (hereafter simply 'anthropology'). The problem is 'interpretive exclusivism': the widely accepted dogma among professional anthropologists that the *only* way to study cultural systems is to interpret them rather than to explain them scientifically. Interpretive exclusivism can take many forms: for example, the interpretation of webs of significance (Geertz 1973), of experiential processes (Ram & Houston 2015), of states of being (Jackson 2005), or even of forms of 'enwindment' (Ingold 2007). One thing these otherwise disparate interpretive strategies have in common is that they repudiate and exclude core features of scientific method and therefore the possibility of explaining human culture.

In November 2023, a meeting of the American Anthropological Association was held in Toronto. In the online conference programme, the word 'memory' was mentioned 174 times. However, the abstracts revealed a striking neglect of the voluminous literature in psychology analysing how human memory works and

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unpacking the complex causes and consequences of remembering in social life. This reluctance to engage with the methods and findings of neighbouring sciences stems from the exclusion of explanatory frameworks that are based on hypothesis testing and causal inference, along with other features characteristic of the scientific method. In 2010, the American Anthropological Association made this principle explicit by announcing that it intended to exclude science from its long-range plan. But while the informal exclusion of science in the day-to-day business of anthropological research usually attracts less attention, it is arguably a much more serious problem than furores over mission statements and long-range plans.

Perhaps the most vaunted claim of interpretive exclusivism is that it gives voice to oppressed groups. In this context, the exclusion of science from the conversation may be justified on the grounds that it is an instrument of imperialism whereas interpretivism is grounded in the more noble desire to critique various forms of domination, originating in colonial and patriarchal histories. I would argue to the contrary, however, that to combat racism, gender asymmetries, persecution or exploitation of minorities, and other forms of social injustice, there is every reason to recruit the methods and findings of the sciences as well as the humanities, and that an isolationist strategy is counterproductive. On this view, anthropology can best contribute to liberal objectives by working collaboratively with allied sciences in an inclusive spirit. One of the rationales for embracing scientific methods is that understanding the causes of cultural phenomena is necessary for developing effective interventions. By contrast, interpretive exclusivism produces a runaway inflation of meanings, providing no principled method of adjudicating between them, thereby degrading the value of theory and reducing the wider practical relevance of anthropology. As a result, public policy experts often find more fruitful collaborators in fields like development economics, psychology, and behavioural ecology in designing and implementing interventions.

The rise of interpretive exclusivism to hegemonic status in anthropology is indebted particularly to Clifford Geertz but owes its origins also to a longer-term drift towards unconstrained relativism (e.g. Clifford & Marcus 1986), most commonly expressed in the 'metatwaddle' of postmodernism (Gellner 1992). Once established, it became a pervasive feature of the ramifying anthropologies of the ontological turn (Erickson & Murphy 2017). To rescue ourselves from this interpretive vortex, we need to reconnect with much deeper historical roots in anthropology, which I will argue are exemplified by the works of scholars such as Fredrik Barth.

The problem with interpretive exclusivism

Towards the end of the last century, philosopher Robert N. McCauley and religion scholar E. Thomas Lawson (1996) published a compelling critique of anthropology entitled 'Who owns culture?'[1] Their answer to this question was nobody. And yet, they argued, anthropologists frequently claim otherwise, resting their case on three dubious assumptions. The first is that culture is an ontologically autonomous domain that can only be understood with reference to itself rather than with reference to factors outside it – such as the shaping and constraining effects of our evolved psychology, or the winnowing effects of cultural group selection. The second is methodological: that only cultural categories can be used to analyse culture, and consequently the only way to study culture is to conduct long-term immersive fieldwork by means of participant observation. The third is that cultural phenomena cannot be disambiguated into

discrete units for the purpose of conducting comparisons or reaching generalizations about their origins.

As a result of these three assumptions, anthropology has come to presume ownership of the concept of culture, denying the legitimacy of other people's efforts to study it. And in the same fatal stroke, it has come to reject the possibility of explaining cultural phenomena scientifically. Interpretive exclusivism – the effort to understand cultural meanings exclusively in terms of other cultural meanings – has made cross-cultural comparison seemingly pointless while sanctifying ethnographic fieldwork as intrinsically worthwhile rather than merely a means to an end. As McCauley and Lawson put it:

> The anthropological search for meanings feeds on new information about ethnographic details and cultural settings. In such an atmosphere, fieldwork becomes an end in itself. *Being there* has become virtually sufficient for professional credentials. Documenting the details of a culture that is largely like some other offers little interest. New, surprising, unexpected details are the fruit from which juicy new meanings are most easily squeezed (1996: 175).

However, as they went on to argue, this modus operandi provides no coherent way of distinguishing sweet fruits from bitter ones. The exercise becomes equivalent, I would argue, to studying the nature of mirrors by recording all the things that mirrors can reflect. There is no way of establishing which image in the mirror should be of particular interest other than to note that some images are new or surprising or to construct fanciful narratives about how the reflections make one feel or what associations they trigger. The value of the observation can then easily come to be seen merely as a function of the perceived originality or aesthetic appeal of the interpretive exercise, encouraging ever more elaborate interpretations that are increasingly distant from the original observation. This may be of interest from a literary perspective and has certainly proven to be a useful device for gaining attention professionally by generating ever more outlandish and flamboyant juxtapositions of unforeseen imagery. However, it is far from clear why it should be seen as the only way of doing anthropology, any more than recording lists of the images reflected in a looking glass is the only way of appreciating the properties of mirrors. Why exclude the possibility of investigating how mirrors reflect light in predictable ways under different conditions, why distortions occur, or how, at a more advanced level, diffuse reflection differs from specular reflection or incident light is converted into reflected light? Even more interesting still, from a practical perspective, are questions about how mirrors could be used to enhance the power of telescopes, the functioning of cameras, or the ability to insert fillings in your teeth. To interpret what your mirror reflects in a field setting is only one rather trivial thing you can do with a mirror – and to forbid all other lines of inquiry and innovation is needlessly straitjacketing. And yet that is exactly what interpretive exclusivism requires us to do.

The case for interpretive exclusivism is weak and the benefits of abandoning such an approach seemingly boundless. It is not that all the painstaking discoveries of past anthropology are suddenly devalued, but the very opposite: they now become genuinely useful as a way of opening up questions of a comparative kind. We can then begin to ask if there are any fundamentally recurrent features of what we observe in widely differing environments and what the main recurrent patterns might be. This in turn would naturally prompt questions of causality, encouraging us to adopt an increasingly

sophisticated array of methods for pursuing answers. Ethnographic fieldwork would then be restored to its rightful place as a means to an end rather than an end in itself.

So what should we call this alternative to interpretive exclusivism? It may be tempting to call it interpretive *inclusivism*. But there are some good reasons for not doing so. For a start, interpretive inclusivism – as practised by some anthropologists – still accords interpretation a privileged position over science. Indeed, the clue is in the phrase – which mentions interpretation but not explanation. Interpretive inclusivists may tolerate scientific insights but they see them as peripheral in the study of culture. Instead, Lawson and McCauley (1990) recommend an *interactionist* view of interpretive and scientific approaches: placing the two on an equal footing in a continual back and forth between the methods of qualitative research and causal inference.

Interpretive and scientific interactionism in New Guinea and beyond

Anthropology wasn't always such a hostile environment for science or preoccupied by parochial matters. In fact, once upon a time it was widely regarded as the Queen of the Social Sciences (Atran 2024). In Britain, Bronisław Malinowksi famously sought to establish what he called a scientific theory of culture, and many of those who came after – Mary Douglas, Raymond Firth, Meyer Fortes, Max Gluckman, and Edmund Leach, among others – were animated by much the same explanatory ambition. In America, this creative explosion in the discipline was fuelled also by figures like Marshall Sahlins, Leslie White, and Eric Wolf at Columbia University, who were likewise committed to interpretive and scientific interactionism. Unfortunately, with the takeover of Geertz's brand of interpretive exclusivism, and even worse the rise of postmodern obscurantism, this legacy was rapidly overshadowed by a new ruling elite in anthropology. Those still sympathetic to interactionism dwindled in number.[2] Fredrik Barth, however, was one of those admirable few. Barth's ethnographic research in Inner New Guinea triggered at least two scientific theories that have resulted in decades of empirical research involving collaboration across numerous disciplines and in scores of cultural groups world-wide. The rich mixture of interpretations, questions, and hypotheses that this process generated illustrates amply the case for combining interpretive and scientific approaches.

In 1968, Barth went to live among the Baktaman – one of a number of small tribal groups living in Inner New Guinea (Barth 1975). At the time, the Baktaman numbered 183 individuals after six people had died and three had been born by the end of his fieldwork. Prior to this, direct contact with cultural groups outside the region had been very limited and relations between the Baktaman and their neighbours were frequently hostile, roughly a third of all deaths being attributable to raiding and warfare. Barth's primary interest was in the way Baktaman religious knowledge was constructed through successive grades of initiation into a male cult. His account of this process was remarkably rich and detailed – an extraordinary feat of painstaking ethnographic description given the highly secretive nature of the rituals and the taboos on talking about them and reflecting explicitly on their meanings.

Although there are many respects in which the details Barth uncovered are of comparative interest, there were four main features of his conceptual framework which have proven to be especially fruitful for the scientific study of culture. The first was a general point, more fully elaborated later by Dan Sperber (1985), that the transmission of cultural knowledge is an epidemiological process (McCauley & Lawson 2002: 69). That is, Barth recognized that cultural representations spread in any given population

through a process of imperfect copying – which implies, of course, that rates of innovation and fidelity of reproduction are potentially measurable. Second, Barth observed that among the key mechanisms in this process of transmission were memory and the frequency of transmission. In the case of the Baktaman, he hypothesized that the comparatively low frequency of participation in male initiation rituals – in particular, the fact that each novice underwent induction into each successive grade only once in a lifetime – meant that the knowledge transmitted was subject to forgetting, garbling, and incremental change. Third, Barth proposed a link between shared suffering in rituals (which in Baktaman initiations included being beaten with stones, whipped, dehydrated, and forced to dance to the point of exhaustion) and strong bonds of solidarity among participants. Fourth, he proposed that undergoing such experiences triggered processes of reflection on problems of meaning and exegesis based on analogic principles, providing the core of cosmological knowledge among not only the Baktaman but also other groups in this region of New Guinea (Barth 1987).

Some two decades later – between 1987 and 1989 – I undertook ethnographic research among another group in Papua New Guinea: the Mali Baining of East New Britain Province (Whitehouse 1995; 2000). Like Barth, I underwent elements of the initiation process myself. After returning from the field, I was struck by many similarities in our observations and I began to wonder whether there was a causal relationship between the frequency of rituals, the way they were remembered and reflected upon, the group bonding effects we had observed, and the patterns of intergroup conflict that were typical of both Baktaman and Baining groups. In 1992, I published an article in *Man* (as the *JRAI* was then called) outlining a general theory of how these dimensions were related – drawing on both Barth's writings and my own observations (Whitehouse 1992).

The starting point for this theory was that rarely performed rituals involving intense emotions such as pain or fear (so typical of male initiations in many cultures) produce long-lasting episodic memories. Drawing on psychological studies of 'flashbulb memory' (Brown & Kulik 1977), I argued that recall for ritual ordeals had a distinctively vivid and visceral quality that in turn motivated processes of reflection on problems of meaning and significance, along the lines of what Barth referred to as an 'analogical code' in which multiple layers of associations lend salience, depth, and gravitas to the tradition. I contended that the sharing of these memories and experiences with other group members generated strong forms of social cohesion, but also set strict limits on group size – since the memories only pertained to those who were co-present during the rituals. Consequently, the way in which Baktaman and Baining initiations were organized in memory strongly influenced the scale and structure of ritual groups, encouraging members to support each other in battle when defending their groups.

As such, low-frequency ritual, shared suffering, episodic memory, reflection, group bonding, and intergroup raiding and warfare were hypothesized to form part of a single nexus of features – what I dubbed the 'imagistic' mode of religiosity, in reference to the enduring images that these rituals 'seared' into the memories of participants (Whitehouse 1995). All these hypotheses sprang from a combination of description and interpretation, but it became increasingly obvious that the only way to test them was to turn to science.

In practice, this process was characterized by three features. The first feature was qualitative generalization and comparison, characterized by interdisciplinary dialogue. I began by asking whether the clustering of imagistic features observed in Baktaman

and Baining groups could be observed in other descriptions of rarely performed, emotionally intense ritual. This led me to write a book comparing such practices across numerous other groups in Papua New Guinea and in the documented religious history of Europe (Whitehouse 2000). This book prompted interest from scores of anthropologists and historians who, in a series of edited collections, began to explore the applicability of this theory to detailed case studies around the world and across the span of human history and prehistory (Martin & Pachis 2010; Whitehouse & Laidlaw 2004; 2007; Whitehouse & McCauley 2005*a*; 2005*b*; Whitehouse & Martin 2004*a*; 2004*b*; 2005).

The second feature was *quantitative* generalization and comparison, characterized by interdisciplinary *collaboration*. One reason this was necessary is that comparison based on purely qualitative case studies is vulnerable to selection bias. It was impossible to establish whether those interested in the imagistic mode of religiosity were drawn to the theory because it fitted with their own ideas and observations. What if those who had data contradicting the theory were less inclined to join the conversation? Likewise, there was a risk that qualitative descriptions of imagistic traditions overemphasized features that conformed to the theory and downplayed those that did not. But another reason why a quantitative approach was necessary was that qualitative case studies did not define the variables relevant to testing the theory in a way that could be measured, enabling us to analyse statistically the extent to which features like frequency, emotional intensity, cohesion, group size, and warfare were correlated in a representative sample of the world's rituals.

To do that, we need to build a series of databases capable of being analysed statistically: an ethnographic database coding roughly 100 variables pertaining to 645 rituals from 74 cultures around the globe (Atkinson & Whitehouse 2011); a series of archaeological databases, ranging from fine-grained comparison of data over thousands of years at a single site (e.g. Whitehouse, Mazzucato, Hodder & Atkinson 2014) to coarser-grained comparison of data over scores of sites across a much wider region and longer timespan (e.g. Gantley, Whitehouse & Bogaard 2018); and, finally, a global history databank comparing vast numbers of rituals across hundreds of societies over thousands of years (e.g. Turchin *et al.* 2020; Whitehouse *et al.* 2023). These approaches required not only interdisciplinary dialogue, as when comparing qualitative case studies, but something far more challenging still: the design of datasets that met the highest standards of ethnographic, historical, and archaeological accuracy but in a way that would also enable data scientists to analyse the material statistically. This was more than a matter of mere dialogue – it required prolonged collaboration necessary to repackage the observations of qualitative researchers in a form that could be directly compared and quantified.

A third feature of this interactionist approach was the design of studies aimed at drawing causal inferences based on data. The ethnographic observations that Barth and I had made at our respective fieldsites had generated testable hypotheses about the relationship between various features of ritual and social organization. For example, the theory of imagistic transmission predicted that rarely performed, emotionally intense rituals would generate more exegesis than frequently performed, less intense rituals, and the only way to test such a prediction was to manipulate the independent variables of interest and measure the effects of doing so on relevant dependent variables. Our first experiments along these lines had undergraduate students perform collective rituals borrowing much of the symbolism of Baktaman and Baining initiations but assigning

half the participants to a low-arousal condition and half to a high-arousal one, and then measuring the effects of this on the analogical richness of their interpretations of the experience afterwards (Richert, Whitehouse & Stewart 2005). In these studies, arousal was manipulated using dramatic music and lighting effects as well as minor ordeals such as reaching into a box without knowing what might lurk inside. Later, we developed innumerable studies in populations that had naturally undergone imagistic experiences of a more intense kind. These studies typically took three forms: correlational designs in which we measured the extent to which infrequent emotionally intense ritual produced group cohesion and willingness to fight and die in intergroup conflicts (Kavanagh, Jong, McKay & Whitehouse 2018); priming studies in which we had half the participants remember an imagistic experience and half the participants not do so (as a control condition) and then measured the effects on the same outcomes variables (Jong, Whitehouse, Kavanagh & Lane 2015); and, finally, longitudinal studies in which we measured the effects of varied intensities of imagistic experiences on longer-term psychological and behavioural measures (Buhrmester *et al.* 2018; Muzzulini, van Mulukom, Kapitány & Whitehouse 2022).

As a result of many studies along the lines described above, we now have a detailed picture of how the variables that Barth and I were interested in are causally related – not only among the Baktaman and the Baining but also among many other groups all around the world (for an overview, see Whitehouse 2018; 2021). Even more interestingly, we are now able to test hypotheses about the role that imagistic practices have played in the evolution of sociopolitical complexity over the course of world history.

By way of illustration, here are four examples of our main take-home discoveries. First, we now have compelling evidence from a host of empirical studies that sharing memories for life-changing rituals fuses together our personal and group identities and motivates extreme forms of pro-group action (Whitehouse *et al.* 2017). Second, we have found that this form of group bonding is very widespread in groups that have to stand together in the face of grave risks: for example, in military or paramilitary units (Whitehouse 2018). Third, we have found that such imagistic practices are less prevalent in the ethnographic record as agricultural intensity increases (Atkinson & Whitehouse 2011). Fourth, during the Neolithic transition, as human populations settled down and began to farm, imagistic practices faded (Gantley *et al.* 2018; Whitehouse & Hodder 2010). As state formations and empires emerged and spread, imagistic practices were either outlawed and forced underground or restricted to tightly controlled elite institutions, especially in the military (Whitehouse 2021).

What began as a set of very narrow observations in a couple of tiny and remote communities set off a train of scientific research that has provided novel and testable insights into the way human societies have evolved over the millennia (Whitehouse 2023; 2024). All this was only possible by adopting an *interactionist* approach to interpretive and scientific research.

Modes of religiosity and the ritual form hypothesis

The parallels between Barth's observations among the Baktaman and mine among the Baining are not limited to ideas about imagistic experiences and analogic codes in traditional initiation rites. They extend also to an interest in processes of missionization. Both Barth and I were struck by the fact that the indigenous modes of cultural transmission we had recorded at our respective fieldsites contrasted starkly with more

doctrinal belief systems. Barth was interested particularly in differences of leadership type (Barth 1990). On the one hand were the 'gurus' who led the doctrinal traditions of the transcendentalist religions of Asia, which Barth knew well from his fieldwork among Indonesian Hindus in Bali. On the other hand were the indigenous leaders: the elders and ritual experts whom Barth described as 'conjurers' because they helped their juniors to conjure up their own interpretations and insights into cosmological mysteries rather than telling them what to do or what to believe. I was struck by a similar idea but initially drew the contrast between varieties of missionary Christianity and indigenous imagistic cults. It seemed to me that the contrasts between doctrinal and imagistic modes of religiosity were expressed not only in distinct patterns of leadership and revelation but also in starkly divergent patterns of transmission, emotional arousal, reflection, social cohesion, group structure and scale, and intergroup relations. I called this the theory of 'divergent modes of religiosity' (Whitehouse 2004).

The central idea was that whereas imagistic practices were associated with low-frequency, high-arousal rituals, the opposite was true of the doctrinal mode of religiosity, whose prototypical rituals were high frequency and comparatively low arousal. According to this theory, frequency and arousal impact group bonding, scale, and structure as a result of the way they are processed in memory (Craik & Jacoby 2023; Tulving 1983). If the rituals are rare but intense – as in the imagistic mode – they may be recalled as life-changing events in *episodic memory*, shaping personal identities as well as defining tightly circumscribed groups. By contrast, if the rituals are comparatively frequent and dull – as in the doctrinal mode – they are recalled in *semantic and procedural memory* as general scripts and schemas which are more detached from autobiographical experience. But although cohesion generated by the doctrinal mode is more depersonalizing, it is capable of unifying large populations with remarkable speed. This is partly because guru-like leaders (as Barth described them) codify their teachings in speech, enabling them to proselytize and spread their ideas rapidly and efficiently. But it is also because frequently repeated beliefs and practices are less vulnerable to garbling and decay. Deviations are not only less likely to occur, but they are also easier to spot. The emphasis on the oratory of inspired leaders and their orthodoxies therefore encourages the growth of religious hierarchies, capable of policing the tradition. In other words, routinization and hierarchy work together to enable detection of and punishment for unauthorized innovation. These are all testable hypotheses which have been the focus of much empirical research over several decades (Whitehouse 2021).

At the start of this process, McCauley was working with his long-term collaborator, E. Thomas Lawson, to develop a theory of why rituals take consistently recurrent forms across widely differing cultural systems. This led them to propose the ritual form hypothesis (or RFH). One of the main predictions of the RFH was that rituals tend to be either relatively low in frequency and high in emotional intensity or more frequent and low key. Since both the RFH and the theory of divergent modes of religiosity were clearly making overlapping predictions, it became inevitable that the relationship between the two theories would need to be clarified (McCauley & Lawson 2002; Whitehouse 2004).

The RFH proposes that all religious rituals are mentally represented as actions – just like any other – in which, for example, an agent may act on a patient by means of an instrument. For example, just as a policeman may strike a protester with a truncheon, a priest may sprinkle holy water on an infant. But religious rituals differ from everyday actions by inviting us to imagine that a supernatural agent is involved in some way,

for example by acting through the agent (e.g. through the priest performing a wedding ceremony), or through the patient of the ritual (e.g. where the god is seen as the recipient of an offering), or through the instrument (e.g. blessings with holy water). According to the RFH, religious rituals therefore take three distinct forms: special agent rituals (such as rites of passage), special patient rituals (such as those petitioning the gods, spirits, or ancestors), and special instrument rituals (such as the sprinkling of sanctified substances over the patient). McCauley and Lawson propose that depending on which form the ritual takes, we can predict the frequency with which a ritual is performed and its emotional intensity – two features that are fundamental to the theory of modes of religiosity. Specifically, they propose that special agent rituals are non-repeatable and non-reversible because we intuitively suppose that what the gods have done cannot be undone – thus making the rituals once-in-a-lifetime events and therefore low in frequency – and also occasions of exceptional gravitas because of the high status of the gods – requiring us to crank up the sensory pageantry of the ritual, amplifying its emotional impact. By contrast, both special patient and special instrument rituals are repeatable since the appetites of the gods can never be assuaged, and this in turn leads to more frequent performances with lower sensory pageantry, given that the acts of mere humans require comparatively little fanfare.

In their exposition of the RFH, McCauley and Lawson drew extensively on Barth's ethnography of the Baktaman as well as my later fieldwork among the Baining. For example, they devoted many pages to a detailed summary of Barth's observations on the frequency of participation in Baktaman initiations, both from the viewpoint of the ritual patient and from that of the organizers. They focused particularly on Barth's ideas about the relationship between frequency, memory, emotion, analogical thinking, innovation, and transmission. By reflecting on the potential relatedness of these variables, Barth was more or less explicitly generating hypotheses about the causal relationships between them – hypotheses that were not only testable in theory but have also indeed been tested in practice through research in allied disciplines. McCauley and Lawson emphasized this point especially in their discussion of how the RFH related to the theory of modes of religiosity.

Recall that the RFH set out to explain why some rituals are low in frequency but high in arousal while others are high in frequency and low in arousal – which was also the starting point for the modes theory. McCauley and Lawson depicted their synthesis of our two theories by means of a diagram resembling a peacock with a resplendent tail (see Fig. 1). In this diagram, the head of the peacock – and in that sense the driving force behind its behaviour – corresponds to the predictions of the RFH, determining how frequently the ritual is performed. However, the body of the peacock corresponds to the *consequences* of ritual frequency (at least according to the modes theory) for the way the ritual is remembered, thought about, and codified. In other words, the modes theory predicts the downstream consequences of high-frequency versus low-frequency rituals – represented by the many feathers of a peacock's tail. These are the effects of performing a ritual on various aspects of the social system in which the ritual occurs, such as group structure and scale, levels of cohesion, leadership type, stability of the ideology over space and time, and so on.

Integrating the RFH and the modes theory in this way generates a raft of empirically tractable predictions. For example, we would predict that all imagistic religious traditions would have at their heart some type of special agent ritual – such as a rite of initiation – in which supernatural agents effect permanent changes on those undergoing

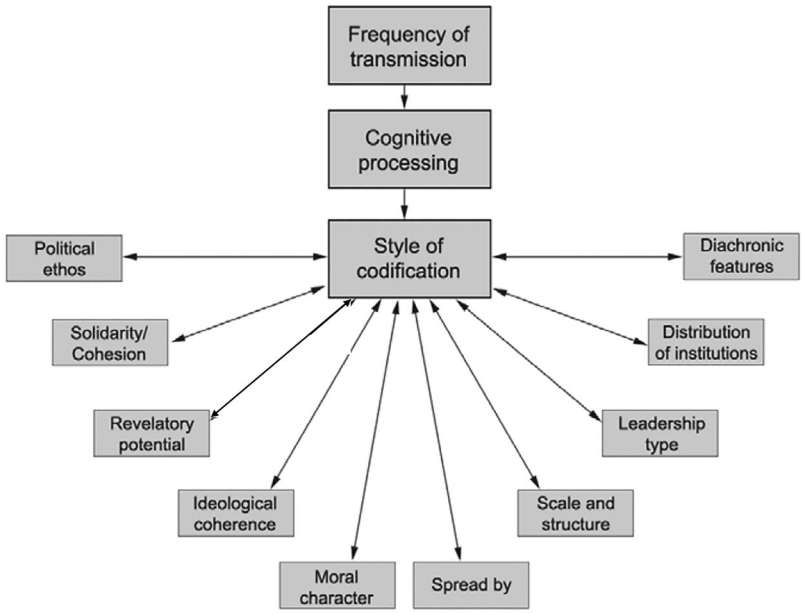

Figure 1. Direction of influence among Whitehouse's thirteen variables (adapted from McCauley & Lawson 2002: 106; reproduced here with the permission of the authors).

it. And we could predict that all doctrinal religions would be constructed around special patient or instrument rituals in which adherents routinely carry out rituals to please, placate, or petition supernatural agents. Among the alternative predictions is that ritual frequency varies somewhat independently of people's intuitions about the appropriate form of the ritual, based on how supernatural agents are thought to intervene (if at all). If the tendency is for collective rituals to cluster into low- and high-frequency poles in the 'morphospace' of possibilities (Atkinson & Whitehouse 2011; Kapitány, Kavanagh & Whitehouse 2020), then this may be due to the winnowing effects of cultural selection explainable by the modes theory rather than the intuitions proposed by the RFH.

These are all empirical questions. Ryan Hornbeck and Justin Barrett (2020) have argued that it is precisely its empirical tractability that makes the RFH and the predictions relating to imagistic and doctrinal modes different from most anthropological theories, even those aspiring to adopt an interactionist approach. Hornbeck and Barrett provide as examples of potentially promising scientific approaches Durkheim's (1912) theory that rituals generate cohesion, Malinowski's (1948) theory that rituals satisfy biological needs, and Rappaport's (1999) theory that ritual facilitates adaptive co-operation in response to existential crises. However, each of these theories remains untestable unless specifying precisely what feature of ritual should have the predicted outcomes and unless the outcomes themselves are measurable. Hornbeck and Barrett suggest various ways in which these criteria could be met, but they praise McCauley and Lawson for stipulating their theory in a manner that is amenable to testing in its current form.

This is something that Fredrik Barth also clearly appreciated. Although he may not have published directly on the RFH, he did give serious consideration to the modes theory while he was at Boston University (Barth 2002). As he noted, the core arguments of my book *Arguments and icons* (Whitehouse 2000) took the form of testable hypotheses, and in the spirit of testing them, he set out various pieces of evidence from his own field observations. Barth acknowledged that he and I were advancing competing hypotheses regarding the role of dysphoric emotions in Baktaman initiations. He had proposed that fear and pain were evoked as part of a process of 'stage management' – emphasizing the gravity of the secrets being revealed. Whether or not he was aware of this, his argument on this score was consistent with the RFH which maintains that special agent rituals amplify sensory pageantry to demonstrate that the gods or ancestors are in command, instilling durable memories of the experience for that reason.[3] But as he pointed out, the modes theory offers an additional explanation: namely that the emotional intensity contributing to the formation of enduring episodic memories was necessary to create strong forms of enduring group cohesion as well as motivating intergenerational transmission of the tradition.

To begin to adjudicate between these possibilities, Barth appealed directly to various forms of evidence – albeit anecdotal. First, he observed that many of his own most durable episodic memories were not necessarily associated with strong emotions. Second, he noted that on at least one occasion an initiated Baktaman man had argued persuasively that his memories for the most painful element of initiation were somewhat hazy and confused.[4] Nevertheless, Barth also explicitly recognized that more systematic evidence on this topic was needed to draw more robust conclusions. This is precisely the motivation behind interactionism: the effort to test interpretive hunches against data, based on scientific methods.

Barth clearly appreciated the value of an interactionist approach to interpretive and scientific endeavours. He realized that it is only by couching hypotheses in a testable form that we can hope to reap the benefits of explanatory pluralism,[5] interdisciplinary collaboration, and incremental theory building. And when we can get all these things working together at once, we reap yet another reward: the capacity to impact the world to achieve more positive outcomes. This is the theme to which I now turn.

Towards a more relevant anthropology

If one of the objectives of anthropology – or of the social sciences more generally – is to be relevant not only to surrounding disciplines but also to society at large, then the case for an interactionist approach to interpretive and scientific endeavours becomes even stronger. It may be hard to imagine how field observations in societies as remote as the Baktaman and the Baining of Papua New Guinea could lead to scientific theories capable of impacting large-scale societies and even strategies for global co-operation. However, that possibility is opened up by adopting an interactionist approach. Consider three examples.

First, consider the problem of intergroup conflict. The rituals of the Baktaman and the Baining led us to hypothesize that rarely performed but emotionally intense experiences produce long-lasting episodic memories and processes of reflection that in turn produce strong forms of social cohesion capable of motivating heroism in warfare. This theory has inspired a small industry of research into the processes driving warfare and violent extremism in a wide variety of different groups. This is partly due to the fact that the theory of imagistic group bonding has been augmented

by the development of a psychological construct known as 'identity fusion' (Swann, Jetten, Gómez, Whitehouse & Bastian 2012) – a visceral sense of oneness with the group – which allows researchers to measure the effects of low-frequency, high-arousal rituals on group cohesion and willingness to fight and die for their comrades. Scores of studies have shown that when personally self-defining experiences are shared with other members of the group, our personal and group identities become fused together. Once this happens, any attack on the group is taken personally, and so highly fused individuals will stop at nothing to defend the group, even to the point of making the ultimate sacrifice. These conclusions are based on studies of imagistic group bonding in a very wide variety of groups: from deadly clashes between rival football fans in Brazil (Newson, Bortoloni, da Silva, Buhrmester & Whitehouse 2018) to farmer-herder conflicts in Cameroon (Buhrmester, Zeitlyn & Whitehouse 2022), and from armed revolutionaries in Libya (Whitehouse, McQuinn, Buhrmerster & Swann 2014) to military veterans in conventional armies (Whitehouse *et al.* 2017). One of the practical benefits of this research is that we are now able to develop techniques of early detection of terrorism threats based on the signals of imagistic group bonding that violent extremists inadvertently provide in their writings and speeches (Ebner, Kavanagh & Whitehouse 2022). All these discoveries and their applications came about because of hypotheses generated by field research in remote regions of Melanesia when coupled with an interactionist approach to interpretation and explanation.

Second, consider the problem of crime in the United Kingdom. Once again, it may seem far-fetched to imagine that research among the Bakataman and the Baining could possibly bring anything useful to the table when tackling that problem. However, research among prisoners and probationers suggests that imagistic group bonding could play a crucial role both in reforming ex-offenders and in improving reintegration efforts when released into the community. In the United Kingdom, an organization known as A Band of Brothers undertakes initiation rites for groups of young men coming out of prison, offering mentoring as part of what they call 'a rite of passage journey' which fosters a search for meaning and purpose in life (Whitehouse 2024). Building on similar ideas, another UK initiative, known as the Twinning Project, connects football clubs throughout the country to their local prisons, providing intensive coaching for inmates. My colleagues and I have been administering measures of imagistic group bonding with large numbers of prisoners participating in this programme – just as we have been doing in our studies of West African initiation cults, military groups, fraternities, and other tight-knit organizations that carry out personally transformative rituals. And we are finding that the same psychological processes may be at work among Twinning Project participants (Newson & Whitehouse 2020). Our research on prisons and reoffending increasingly suggests that the fusion mechanism – which lies at the heart of imagistic group bonding – may hold the key not only to desistance among ex-offenders (Whitehouse & Fitzgerald 2020), but also to the willingness of prospective employers to offer second chances (Reich, Buhrmester & Whitehouse 2024).

The idea that imagistic bonding could lead to peaceful forms of prosocial commitment and co-operation has also inspired a raft of new research on global problems, such as the climate crisis. Just as the sharing of personally transformative experiences in Baktaman and Baining initiations led to strong forms of pro-group action among initiated men, we have found similar bonding processes at work in a variety of world religions (Kavanagh, Kapitány, Putra & Whitehouse 2020). There is

great potential to harness the psychological mechanisms driving religious cohesion to motivate action on a range of environmental issues. A simple example of what can be achieved is the EcoSikh movement, which set out to plant a million new trees world-wide to celebrate the five hundred and fiftieth anniversary of Guru Nanak's birth (Whitehouse 2021). Not only religion but also shared human experiences of all kinds have the potential to motivate transnational forms of co-operation. For example, we have found that perceived sharing of the pain of childbirth creates very strong bonds among mothers (Tasuji, Reese, van Mulukom & Whitehouse 2020) – and that bonds of motherhood can be used to motivate willingness to support other women as part of a global sisterhood (Reinhardt & Whitehouse 2024). Building on these findings, we have shown with a sample of 1,000 UK participants that merely reminding people of imagistic experiences shared by humanity at large has a significant effect on people's willingness to act on climate crisis arguments (Reinhardt & Whitehouse in preparation). Further research is clearly needed to explore the power of beliefs in shared experiences to increase prosocial commitment and improve co-operative outcomes.

A fruitful way of understanding the contrasting orientations of interpretive and explanatory approaches – and why they should be integrated – flows from the differences between thick and thin descriptions of cultural systems. This point is made very compellingly by McCauley (2024), who observes that the goal of thick description is to document connections between the local and the particular in a cultural system. This is upheld by interpretive exclusivists as the only legitimate goal of anthropology (e.g. Geertz 1973). And yet many of the cultural constructs that people use in local and particular settings have much wider currency. For example, we have found that initiation rituals such as those of the Baktaman and the Baining have many features that are common to societies as far away as Amazonia and West Africa (Whitehouse & Laidlaw 2004). Recognizing these similarities is only possible by distilling from the local and particular somewhat 'thinner' descriptions of behaviour that can be generalized to a wider range of examples. It is precisely our capacity to do so that allows us not only to formulate and test generalizable hypotheses about the origins and spread of cultural phenomena, but also to engage in even the most modest form of cross-cultural comparison.

The study of culture is greatly enriched by the ability to utilize thin description for the purposes of testing general theories. However, thin description in turn is ideally distilled from thicker description – which is to say that the promise of the explanatory enterprise often depends on the quality of the interpretive one. An example of this is to be seen in efforts to analyse statistically large repositories of sociocultural data, such as the Seshat Global History Databank.[6] The Seshat team has attempted to establish whether generalizable features of religion helped to drive the rise of social complexity, and this required a method of measuring potentially relevant features of religion in ways that were directly comparable across widely differing world regions and historical periods. But because research on, say, precolonial West African history takes place in a different silo from the research on the Basin of Mexico or big-island Hawai'i, we had to establish a bridging mechanism that would allow the team to compare like with like across the world regions in the Seshat database. This meant coding for a large number of features of religious belief in exactly the same way across hundreds of past societies as well as coding data on changing levels of social complexity during the histories of each of those societies (Turchin *et al.* 2023; Whitehouse *et al.* 2023). In this way, thin

Table 1. The costs of interpretive exclusivism and the benefits of interactionism.

Costs of interpretive exclusivism	Intellectual benefits of interactionism
Scientific theories, methods, and findings become inaccessible	Ethnography becomes a source of hypotheses that can be tested empirically
Research limited to participant observation	Research informs controlled experiments
Generalization and comparison impeded	Data can be analysed statistically facilitating comparison and causal inference
Obscurantism rife	Variable and recurrent patterns of culture and their evolution become potentially explainable
Elitism hampers diversity of participants	Science belongs to everyone
Practical relevance of research diminished	Potential for evidence-driven policy recommendations improved

descriptions of historical features provided the bridges between distinct political and cultural systems in the database.

We currently have around 100 actively collaborating historians working with us on the Seshat project to accomplish this feat – which means drawing on thick descriptions in historiography while also converting that information into thinner descriptions that allow comparison across widely differing cultural systems. Combining thick and thin descriptions is thus a necessary part of the scientific process. Championing the value of *thin* description (in this case, recurrent features of religion that are not unique to particular cultures but can be compared across them) also means championing the value of reliable *thick* descriptions derived from more parochial and time-bound sources (McCauley 2024). In this example from the Seshat project, *thin descriptions* had to be distilled from a corpus of *thick descriptions* to enable us to carry out comparative analysis. Interpretive exclusivism would make such an enterprise impossible.

Conclusions

Interpretive exclusivism deprives anthropology of the many benefits of interactionism. By limiting the methodological repertoire to immersive fieldwork and the pursuit of thicker descriptions and ever more distant interpretive strategies, it closes the door on generalization and comparison. But that is not the only downside of interpretive exclusivism. There are also strong moral arguments against it. In fact, it is ironic that defenders of interpretive exclusivism so often position themselves as champions of social justice, given that their strategy is inherently elitist. In effect, the approach turns anthropology into a privileged club open only to those who adopt its narrow-minded dogmas. Drawing in a greater diversity of members of the club may involve inviting small numbers of people from marginalized groups to join the elite but only at the expense of the people they leave behind and whose interests are not served by interpretive exclusivism. Indeed, except perhaps for those who rise to the top of the anthropology profession, nobody's interests are served by interpretive exclusivism since the practical relevance of the discipline for society at large is diminished behind a veil of obscurantism, lacking in explanatory power.

In short, the case against interpretive exclusivism is overwhelming (see Table 1). The fact that it continues to enjoy such high status in anthropology is a consequence of the way in which clubs reproduce their elitist orthodoxies. This is the case even when the discipline is haemorrhaging members and failing to replenish them (Piot 2023).

It is hard to imagine why anyone would recommend interpretive exclusivism as a strategy for anthropology if it were not already endorsed by the discipline's power structures. What anthropology needs is not an *ontological* turn but an intellectual *U-turn*. By rediscovering its scientific roots and embracing interpretive and explanatory interactionism, ethnography would become a means to a palpably worthwhile end. Rather than sucking us into an interpretive vortex, it would open up new vistas of intellectual inquiry. By generating precise and testable hypotheses, the value of the enterprise to neighbouring disciplines would naturally become more readily apparent. The tools of those neighbouring disciplines would become available too, enabling anthropology to contribute collaboratively to cross-disciplinary research, including the design of experiments, surveys, longitudinal studies, and database construction. By strengthening the scientific foundations of the discipline, anthropology would become more relevant to society at large, to the policy community, and to the marginalized and oppressed groups it has a duty to defend. Since the methods of science are generalizing and inclusive, they are also practically useful and empowering, and so the real challenge is to make the interactionist approach to anthropology – one that combines interpretive and scientific goals – accessible to all.

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NOTES

[1] The article was republished as chapter 4 of McCauley and Lawson's *Philosophical foundations of the cognitive science of religion* (2017). This book should, in my view, be core reading on all introductory courses to social and cultural anthropology.

[2] Although its influence has greatly diminished, the interactionist approach has never been entirely expunged from anthropology. The Society for Anthropological Sciences continues to meet at the American Anthropological Association and research in cognitive anthropology is still in evidence, especially studies focusing on language and spatial cognition (e.g. Dasen & Mishra 2010; Levinson 2003) and on cultural models (e.g. Bennardo & de Munck 2014; Strauss & Quinn 1997). Nevertheless, such research – and the interactionist approach at its core – is now generally regarded as a peripheral feature of the discipline.

[3] Barth's commentary was published around the same time that McCauley and Lawson's book *Bringing ritual to mind* (2002) appeared, but it seems clear that he had not read the latter at the time of writing.

[4] A study of another high-arousal ritual – firewalking – reported similar responses (e.g. Xygalatas *et al.* 2013).

[5] The phrase 'explanatory pluralism' appears to have been coined by McCauley (1986) and used repeatedly in subsequent papers (e.g. McCauley 2014).

[6] *https://seshatdatabank.info*; *https://seshat-db.com*.

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Contre l'exclusivisme interprétatif

Résumé

L'exclusivisme interprétatif est le dogme selon lequel on ne peut comprendre les systèmes culturels qu'en les interprétant, excluant ainsi l'explication causale de phénomènes culturels par des méthodes scientifiques telles que la mesure, la comparaison et l'expérimentation. L'auteur affirme ici que le coût de l'exclusivisme interprétatif est considérable et son bénéfice illusoire. À la place, il plaide pour une approche interactionniste associant, dans une égale mesure, interprétation et méthode scientifique. Bien que cette approche ne soit ni facile ni économique, il estime qu'elle est nécessaire pour améliorer l'ambition intellectuelle, l'ampleur comparative et la pertinence pratique de l'anthropologie en tant que discipline. À tous ces égards, l'incorporation des méthodes scientifiques, au lieu de leur exclusion, améliorerait les chances de l'anthropologie d'être, à long terme, un champ florissant de recherche et d'enseignement.

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