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Big Gods, historical explanation, and the value of integrating the history of religion into the broader academy

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This article discusses critiques raised by historians of religion concerning Ara Norenzayan's *Big Gods: How Religion Transformed Cooperation and Conflict* (Princeton: Princeton University Press, 2013), offering some defense of Norenzayan's position, but also discussing in detail the more substantive challenges. It concludes with some reflections on the current position of the history of religion within the Academy, and an argument for why large-scale explanatory projects and interdisciplinary collaboration should be part of the future of our field.

KEY WORDS Ara Norenzayan; Big Gods; evolution of religion; cognitive science of religion; Religious Studies; interdisciplinarity

Since its publication, Ara Norenzayan's *Big Gods: How Religion Transformed Cooperation and Conflict* (Norenzayan 2013) has received a great deal of scholarly attention. In addition to several reviews, two review symposiums have been dedicated to comments on the book, the first set in *Religion, Brain and Behavior* (2014 pre-print online), the second in this journal (*Religion* 44.4, 2014). In this article, I take on the task of responding to the latter set of comments on Norenzayan's behalf. I step in partially because the majority of these comments are from my colleagues in the history of religion, and their strengths and weakness have, I believe, much to say about the state of Religious Studies in the modern Academy. In addition, the hypotheses originally advanced by Norenzayan in *Big Gods* have since become part of a team-based, integrated position – developed in coordination and dialogue with myself, Profs. Joseph Henrich and Mark Collard, and our former and current students – that forms the basis of a major research initiative (CERC; http://www. hecc.ubc.ca/cerc/project-summary/), as well as a target article forthcoming in *Behavioral and Brain Sciences* (Norenzayan *et al*. Forthcoming-a, Forthcoming-b) that responds to many of the arguments and concerns expressed by various scholars, including the commentators in *Religion*.

Norenzayan and I have come to agree that "Big Gods," although a catchy title for a trade book, in hindsight was not the best choice, focusing too much attention on merely one aspect of a broader, prosociality-enhancing religious package. This has

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resulted in numerous misunderstandings of his position, many of these reflected in the *Religion* commentaries. For instance, Norenzayan does not, in fact, assert that Big Gods alone do all of the work he is hypothesizing (*pace* Stausberg 2014; Wiebe 2014; see Norenzayan 2013, 9, 134–135, Ch. 10 "Cooperation Without God"), and in fact acknowledges the important functions performed by fictive kinship (*pace* Martin 2014; see Norenzayan 2013, 116–117, 138) and ritual (*pace* Geertz 2014; see Norenzayan 2013, 98, 103, 114–115, 130–131, 138, 142, 160, 166). Similarly, he never claims that Big God religions make people "nice" in any indiscriminate sense (*pace* Martin 2014; Pyysiäinen 2014; Wiebe 2014; see Norenzayan 2013, Ch. 8 "The Gods of Cooperation and Conflict" or Ch. 9 "From Religious Cooperation to Religious Conflict"): Norenzayan's "prosociality" is a decidedly parochial one, its flipside being outgroup hostility and distrust. "For all its virtues in binding strangers together, religious cooperation is born out of competition and conflict between groups," Norenzayan (2013, 140) notes. "It is therefore expected that religious cooperation in turn fuels the very conflicts, real or imagined, that are perceived to threaten it." Indeed, the main thrust of the Big God hypothesis is that forms of religiosity including moralistic gods, costly rituals, and supernatural monitoring spread partly because they more effectively bound individuals into integrated, internally cooperative societies that, in a variety of ways, outexpanded, out-competed, assimilated, or simply wiped out other cultural groups.

In an ideal world, *Big Gods* would have instead emerged bearing what Norenzayan refers to as its proper, Victorian title:

> On the cultural co-evolution of parochial prosocial religions and large scale cooperation, that with escalating intergroup competition, often turns hostile towards outgroups, driven by increasingly potent and diverse supernatural punishment beliefs, karma, extreme rituals in the form of CREDs and other commitment signals, fictive kinship, tribal instincts, moral emotions, self-control, and practices and traditions that suppress selfishness and promote high fertility.

To be fair to the *Religion* commentators, the breadth of Norenzayan's hypothesis, which has in fact evolved over the last year or two, was not always highlighted in his book. Below, I will discuss in more detail a few key challenges raised by these comments, arguably none of which were given adequate space in Norenzayan's original formulation of his hypothesis. I would then like to turn to what I see as a more important and relevant topic for both myself and readers of this journal: the use of historical evidence in substantiating or falsifying broad claims about human cultural history. This second topic, I will argue in my conclusion, has important implications for how we view our jobs as historians and scholars of religion, as well as the place of Religious Studies in the broader academic community.

Karma, norms, and secular institutions

Several challenges to Norenzayan's thesis point to important shortcomings or weaknesses. Some of these have been corrected in more recent formulations; others require more attention.

Karma and other non-theistic "watcher mechanisms"

In her helpful commentary, Ann Taves (2014) notes that impersonal "watcher mechanisms" of various sorts, rather than "watcher gods," might be doing

important work in some religious traditions, a theme echoed in the commentaries by Martin (2014) and Schlieter (2014), who point to supposedly non-theistic Buddhism as a counter-example to Norenzayan's Big God hypothesis. As Schlieter argues:

> An overwhelming majority of Buddhist traditions and practitioners do not conceptualize the Buddha (and the Buddhas, respectively) as a "God" possessing "agency" in regard to humans, but as a perfect being, a superhuman teacher … The Buddha (and the Buddhas) is never said to monitor humans "supernaturally." It would not make sense, because the workings of karma are fully independent of any supernatural observer. (655)

This criticism has also been leveled by other commentators on Norenzayan's book, with regard to other world religious traditions. For instance, Hagop Sarkissian characterizes early China as "one of the largest and most enduring civilizations on the planet, yet one also lacking a rich tradition of belief in Big Gods or supernatural monitoring" (Sarkissian 2014).

Any response to these critiques must begin by acknowledging that Norenzayan' (2013, 75, 124) *does*, in fact, at least suggest that impersonal mechanisms, such as karma, could in some cases be doing the same work as agentic supernatural monitors, a point that has been expanded upon in our recent updating and refining of the central hypotheses in *Big Gods* (Norenzayan *et al*. Forthcoming-a, Forthcoming-b). As a historian of Asian religions, I would also urge extreme caution in taking Schlieter's comments as incontrovertible, although they do reflect a common view – especially in the West or among Western-educated scholars – of Buddhism as a supposedly "philosophical" or "humanistic" tradition. It is important to avoid the fallacy of attributing theological correctness (Barrett 1998; Slone 2004) to adherents of karmic – or any other – religion. Although the official theology of some forms of Buddhism, for instance, might postulate a completely impersonal karmic order, as practiced on the ground they typically involve devotion and sacrifice to enormous pantheons of rewarding and punishing anthropomorphic deities, as well as conceptions of heavens and hells so elaborate and vivid that they would put Dante to shame. As I have argued in a piece on "neo-Orientalism" in the study of China, the idea that early Confucianism, or Indian Buddhism, is somehow uniquely humanistic, philosophical, and free of "superstition" bears little resemblance to the lived folk-religious traditions in these cultures, and has much more to do with the needs of Western Enlightenment thinkers and their later Chinese and Indian followers (Slingerland 2013; cf. Lopez 2005 for a discussion of this trope in Buddhist studies).

In a similar vein, both Thomassen and Schlieter correctly point out that, from what I would call the theologically correct point of view, the eyes on the Nepalese stupa mentioned by Norenzayan are meant to symbolize wisdom rather than surveillance (Schlieter 2014, 655; Thomassen 2014, 668). This, however, is entirely beside the point. When it comes to the hypothesized psychological effect of these symbols, the empirical literature cited by Norenzayan suggests that eye-spots – even in quite stylized form, and even when not consciously noticed at all – induce a feeling of being observed in human beings, along with a corresponding uptick in prosocial behavior. Thomassen dismisses this body of work by declaring that "psychological experiments cannot replace historical evidence" (669), but does not spell out how he thinks doctrinal statements produced by a literate elite can tell

us anything about implicit human psychological processes. There *are* methods for discerning the patterns of implicit cognition functioning in the background of historical texts and artifacts, but simply taking declarations of theological faith at face value is not one of them.

Thomassen gets more to the heart of some of the deep problems in contemporary psychological research when he observes that experiments made on American college students cannot stand in for humanity as a whole (669), but this is now – thanks in no small part to Norenzayan and his colleagues in the Psychology department of UBC (Henrich, Heine, and Norenzayan 2010) – a recognized issue in the field, and more recent empirical work is being conducted in a wide variety of small and large-scale societies around the world. Moreover, as I have argued in more detail elsewhere (Slingerland 2014a), the problem of using WEIRD people as our exclusive model for human psychological processes should motivate historians not to dismiss psychological research, but rather to get involved in it by drawing upon what Luther Martin has called the "data from dead minds" (Martin 2013) that has left its traces in texts and artifacts from the past.

Finally, with regard to the Nepalese stupa iconography, we can also concede the point made by both Thomassen and Schlieter that enormous eyes are not a common feature of Buddhist iconography, while still recognizing that a visitor to almost any Buddhist temple will be confronted by enormous statues of anthropomorphic, punishing, and rewarding deities that would produce the same hypothesized effect. As a scholar of Asian religion, I would, in fact, reach the opposite conclusion than that voiced by Schlieter. From the perspective of the common folk, it is arguably the case that the "overwhelming majority of Buddhist traditions and practitioners" tend to focus on the veneration of particular deities, who are typically portrayed in anthropomorphic form, and the Buddha himself is quite commonly treated as a Big God (see, e.g., Purzycki and Kulundary under submission). The forms of Buddhism that are most widespread in East Asia, for instance, center on personal devotion to Buddhas capable of rewarding one with rebirth in an eternal Pure Land (Payne and Tanaka 2004). Ultimately, claims about what the "overwhelming majority" of Buddhists do or do not believe or practice are ones that need to be subjected to careful, broadly based historical substantiation – a point I will return to below.

The case of early China may present a better counter-example to an emphasis on anthropomorphic gods. In Shang and Western Zhou sources, Shang Di (the "Lord on High," or simple High God) or "Heaven" (*tian* 天) is portrayed as a full-blooded, anthropomorphic Big God, experiencing anger, sending down punishments, and abandoning immoral followers (Eno 2000; Keightley 2004; Clark and Winslett 2011). Indeed, the original graph for *tian* 天 is clearly a pictograph of a large person – you cannot get more anthropomorphic than that! On the other hand, Shang Di/Heaven is, even in these earliest sources, significantly less personal than, say, the God of the Hebrew Bible. Heaven never directly speaks, instead revealing its will through indirect signs such as portents or disasters, and never appears in corporeal form to its worshippers. In early Warring States (5th–3rd c BCE) thinkers such as Confucius or Mozi, *tian* continues to be portrayed in anthropomorphic terms, but gradually becomes less personalized as we enter the 3rd century BCE. For late Warring States thinkers such as Zhuangzi or Xunzi, *tian* has become so impersonalized that some have argued for switching to "Nature" rather than "Heaven" as a translation (Machle 1976; Ivanhoe 2007), although *tian*

arguably regained some of its anthropomorphic force in the Han and later dynasties.

Whatever its degree of anthropomorphism, however, *tian* continued to serve many of the same functional roles postulated by Norenzayan for a "Big God," providing cosmic warrant for the ruler's political position, enforcing moral norms with punishments in the form of natural disasters, droughts, and popular uprisings, and lending a sense of moral realism to societal norms, which were seen as decreed by Heaven or grounded in the Heavenly order. Norenzayan probably did not adequately address in *Big Gods* the idea that this functional role could be – and perhaps in many Asian religious traditions is – filled by an impersonal cosmic principle. Moreover, whether we are talking about *tian* in China or karma or the figure of the Buddha, it is probably the case that within the same tradition we can find varying degrees of personification of the cosmic order, depending upon on the individual's level of education or socioeconomic status and other factors. (For the record, Norenzayan does discuss the psychological consequences of personification of gods in Chapter 2). Degree of personification is also almost certainly subject as well to individual, genetically based psychological differences, such as theory of mind ability (Crespi and Badcock 2008; Norenzayan and Gervais 2009; Norenzayan, Gervais, and Trzesniewski 2012; Willard and Norenzayan 2013). In any case, the degree to which karma, as it actually functions in the psychology of real-life humans, is impersonalized or contributes to prosocial behavior is ultimately an empirical issue that has been, until recently, overlooked by researchers in the behavioral sciences, but is a topic currently under investigation by our research team and others (e.g., White, Sousa, and Prochownik forthcoming; Banerjee and Bloom 2014).

The role of self-monitoring and internalized norms

In addition to concerns about the extent to which the role of the "Big God" is in fact filled by impersonal cosmic forces, another critique of *Big Gods* has been its relative neglect of the importance of moral self-monitoring and internalized norms. This charge is not entirely unfair. Norenzayan does observe that

> just as religion is not the only source of prosociality, supernatural monitoring is not the only source of prosociality in religion. No doubt, the moral emotions such as deep feelings of empathy and compassion are at play in religions for at least some believers some of the time. (2013, 75)

However, it is certainly the case that external supernatural sanctions loom much larger in *Big Gods* than what Schlieter refers to as "prosocial selfmonitoring." Schlieter correctly notes that such self-monitoring plays a central role in Confucian morality, an observation echoed in Hagop Sarkissian's critique of *Big Gods* (Sarkissian 2014). Taves also takes Norenzayan to task for emphasizing extrinsic "watchers" over "intrinsic commitments," an emphasis that Donald Wiebe finds puzzling in light of my own work concerning the role of value internalization and trust in large-scale cooperation (Wiebe 2014; see Slingerland 2014b, Ch. 6).

Focusing attention on individual-level and internal processes is an important corrective to the idea that cooperation is all about external situational forces, which is the dominant impression that Norenzayan's account gives. There is certainly a great degree of variation in how individuals respond to external incentives and

pressures, as well as the degree to which they will depend upon them. It is important, however, to distinguish between pro-social self-monitoring and norm internalization, for they rely upon different proximate psychological mechanisms and – in my view, at least – the latter is probably much more important when it comes to securing large-scale cooperation.

Prosocial self-monitoring does involve a degree of internalization of certain broad, prosocial commitments, such as maximizing social well-being (for utilitarians) or conforming to the categorical imperative (for Kantian deontologists) or the Confucian Way (for early Confucians). In terms of proximate psychological mechanisms, however, this sort of monitoring involves conscious reasoning and what psychologists refer to as "cognitive control" – the replacing of automatic, "hot," or "System 1" behaviors with those dictated by "cold," "System 2" processes (Stout 2010). Cognitive control is a powerful human cognitive ability, but it has severe limitations, slowing reaction times, inducing fatigue and becoming rapidly depleted (Baumeister *et al.* 1998; Inzlicht, Schmeichel, and Macrae 2014). For these reasons, scholars working at the intersection of cognitive psychology and ethics (Haidt and Kesebir 2010; Slingerland 2011a) have argued that securing reliable prosocial behavior on a large scale will necessarily involve the full internalization of values – that is, the rewiring of "hot" cognitive tendencies in accord with normative ideals. This is the model of prosocial training long advocated by virtue ethicists, from Aristotle to Confucius, who have as their goal cultivating new, self-activating, and effortless dispositions to act in a way that accords with prosocial desiderata (Casebeer 2003; Flanagan 2009; Slingerland 2011b). It also fits with recent experimental data suggesting that unconscious, emotional reactions are the driving force in most ethical behavior (Damasio 1994; Haidt 2007), as well as with the finding that, at least when it comes to the inhabitants of large-scale societies, "hot" automatic reactions tend to be prosocial (and often "irrational" from a classical economics perspective), whereas subjects given time to reflect or plan often revert to more "rational," selfish strategies (Rand, Greene, and Nowak 2012).

So, while it is true that self-monitoring and norm internalization is neglected in Norenzayan 2013 (and not highlighted perhaps as much as it should be even in Norenzayan *et al.* Forthcoming-a), it is important to realize that the latter – and much more important – of these processes is dependent on many of the social mechanisms that *are* emphasized as components of the cultural complex described in *Big Gods*: CREDs (credibility-enhancing displays), collective rituals involving costly sacrifices and synchronous movement, monumental architecture, and other cultural technologies designed to facilitate the "hot" embodiment of socially desirable norms and values.

Institutions vs. Gods

The potentially most interesting counterargument to the broader hypothesis in *Big Gods* is the claim that, as a matter of historical fact, secular institutions played a larger causal role in the rise of large-scale societies than supernatural monitors or moralizing high gods (Geertz 2014; Martin 2014; Levy 2014; Thomassen 2014). Norenzayan does note that religion is not the only path to large-scale sociality; the topic of Chapter 10 of Norenzayan 2013 is, after all, "Cooperation Without God," where Norenzayan spells out in some detail how secular societies have replaced many of

the cooperative functions arising from prosocial religions. That said, the issue of relative importance of secular institutions, warfare-driven pressures and literacy in the rise of large-scale sociality is a crucial one for any theory that hypothesizes a prosocial function to religious beliefs or practices. Moreover, Norenzayan does portray strong secular institutions as late-comers on the scene, "kicking away the ladder" created by Big God beliefs and practices only relatively recently in human cultural history. Thomassen and Martin correctly take him to task for failing to acknowledge the rich institutional heritage of early empires such as Rome or China.

The crucial issue, though, is whether or not it was these primarily secular institutions, such as contracts and courts, which played the major causal role in facilitating large-scale cooperation in ancient empires. Both Thomassen and Martin suggest that this is, in fact, the case. Geertz similarly fingers the non-religious pressures of external threats of warfare as the main driver of large-scale cooperation, and both Geertz and Levy feel that the role of cultural technologies, such as literacy, are neglected in *Big Gods*. Other commentaries on *Big Gods* have argued that the same forces – literacy, strong secular institutions – played the major causal role in facilitating cooperation in early China (Sarkissian 2014). If it could be demonstrated that many, or most, early large-scale societies managed to ramp up the scale of cooperation without relying on elements of the prosocial religious complex, this would be a major problem for both Norenzayan's (2013) formulation and the more recent theoretical position laid out in Norenzayan et al. (Forthcoming-a, Forthcoming-b).

To begin with, it is important to recognize that disentangling "secular" from "religious" in the premodern world is extremely problematic. So, although Martin is correct in noting that "'secular' contracts" (630) have a long history in the Mediterranean world, the "secular" qualifier needs to be taken with a rather larger grain of salt than Martin's scare quotes provide. Roman contracts were bolstered with dramatic oaths before the gods, often involving significant (costly) sacrifices (Rauh 1993), as were important "covenant" (*meng*盟) documents in ancient China (Poo 2009). Similarly, while it is true, as commentators such as Sarkissian (2014) have noted, that comprehensive and well-designed legal codes, backed by efficient bureaucracies, have characterized China since at least the 5th century BCE in the state of Qin, it is not at all clear that they functioned entirely without the support of the supernatural beliefs and practices that make up Norenzayan's "Big God" complex. Even in highly legalistic states such as Qin, official religious rituals, sacrificial cults, and claimed supernatural endorsement still played a major role at all levels of society (Pines et al. 2013). It is very much an open question as to what sorts of beliefs and practices are doing the most "work," in the sense of expanding the scope of cooperation, and the extent to which supposedly purely "secular" legal codes or bureaucracies can be separated from the broader, prosocially functioning religious background in which they were embedded.

In the end, it is this issue – the role of institutions versus a specific hypothesized set of religious beliefs and practices – that is most important for deciding the plausibility of the latest version of the hypothesis developed in *Big Gods*, as expressed in Norenzayan *et al*. (Forthcoming-a). The existing historical evidence is not at all clear, and even among area specialists there remains intense disagreement. How to deal with such disagreement, as well as how to better integrate the work of

theorists and empirical scientists with scholarly work in the history of religion, will be the topic of the remainder of my commentary.

Crossing the methodological abyss

Luther Martin takes Norenzayan to task for his "neglect of history" (Martin 2014, 633), and such a criticism presumably underlies Jörg Rüpke's offhand comment that *Big Gods*, though well written, is "badly researched" (Rüpke 2014, 645). In his introduction, Michael Stausberg claims that "the historians of religion writing in this issue … make it clear that the proposed thesis does not live up to the historical evidence as understood by professionals" (Stausberg 2014, 604).

As a professional historian myself, I beg to differ. More importantly, though, I was struck by the fact that my colleagues, having set the bar so high in their critique of Norenzayan, came nowhere near to clearing it themselves. The use of historical data by the professional historians in this review symposium was, without exception, anecdotal and almost completely devoid of scholarly rigor or documentation. Thomassen, in a representative passage, simply asserts that "India, China, and Japan have all been able to develop fairly cohesive societies without the help of monitoring, Big Gods" (Thomassen 2014, 670), without a single citation or acknowledgement of the massive amount of scholarly disagreement on this topic. Despite his dismissal of Norenzayan's research ability, Rüpke does no better than Thomassen, making similarly undocumented, unsupported and yet sweeping claims about the historical record (e.g., "[a moral conception of religion] is … not prominent in the large empires of the 1st millennium BC and CE, including China and Rome, with Achaemenian Iran being a possible exception" [Rüpke 2014, 647], with no citations or evidence presented).

When confronted with the spectacle of good scholars behaving less professionally than one might expect, one must delve beneath the surface for deeper explanations. Perhaps it is the case that, in these historians' own minds, the evidence is so obvious as to not require reference or defense. Perhaps they were simply pressed for time. I think, however, that there is a deeper scholarly prejudice at work here, one that it would be helpful to uncover and discuss. In his introduction, Stausberg at one point notes in passing that "historically minded scholars *are not willing* to subsume their evidence under potential macro-schemes, which *threaten* to hand evolutionary theory back to models of evolutionism" (604; my italics). I think this comment goes a long way toward explaining the otherwise baffling dearth of evidence from the professional historians. Grand explanatory schemas – especially ones that seem to suggest a directionality to history – have become anathema throughout the humanities. Partly this is a result of justifiable concerns about 19th- and early 20th-century teleological accounts of world religious history by some of the pioneers of our field, some of whom arguably portrayed the history of human religious thought and practice as an inexorable process of groping toward that obvious pinnacle of spiritual perfection, Anglicanism. The specter of "evolutionism" continues to haunt any large-scale, explanatory account of historical processes.

I would argue, though, that our fear of evolutionism has caused us to throw out the explanatory baby with the colonialist bathwater. The damage was compounded by the "cultural turn" in religious studies, and the humanities more generally, in the 1960s and 1970s, which introduced a postmodern suspicion of all grand

explanatory frameworks. As I have argued in detail elsewhere (Slingerland 2008; Slingerland and Collard 2012), this postmodern antifoundationalism was grafted on to older views, built into the very structure of the modern university, whereby the object of study of the *Geisteswissenschaften* is seen as ontologically distinct from the *Naturwissenschaften*, and therefore not subject to mechanistic *Erklären*. The conviction that the *Geist* is above causal explanation, following only its own impulses and reasons and being amenable only to individual acts of *Verstehen* on the part of a sympathetic scholar, provides another justification for dismissing large-scale explanation as a project irrelevant to historians. If the particularity and uniqueness of human-level phenomena need to preserved if they are to be grasped at all, the only sort of explanation possible will be local, grounded in the individual scholar's qualitative intuitions. Moreover, tying these local explanations into some broader historical narrative will be a fool's errand, because there is no deeper level of functional or causal principles that would provide the foundation for such a synthesis.

What I found most encouraging about the *Religion* review symposium was the fact that top figures in the history of religion agreed to respond to Norenzayan's arguments, to take his claims about the historical record at least somewhat seriously, and to offer correctives and alternative interpretations. This suggests a willingness on the part of historians of religion to get back into the "big question" game, even if only to debunk, in their view, the historically naïve views of a trade-book psychologist. I see this as a positive development, both in terms of a return to considerations of large-scale explanatory projects and an engagement with scholars outside of our field. Other commentators in the symposium also seem to share my cautious optimism on this front. "We certainly also need more empirical investigations into the effects of religion on prosocial behavior and social trust (which are not necessarily the same thing)," Thomassen concludes. "We especially need historical studies on this topic that are informed by recent work in the social sciences and experimental psychology, work that itself as a rule is uninformed by historical evidence" (673). It is true that the vast majority of work in experimental psychology has occurred, and continues to occur, in blissful ignorance that anyone other than American college students has ever existed. However, it should be recognized that in multiple venues, such as the American Academy of Religion and the International Association for the History of Religion, we are beginning to see genuine engagement between empirical researchers and historians, in a process that has the potential to enrich work on both sides of the humanities–science divide.

My reading of the review symposium left me, however, with the feeling that we, as historians, still have some work to do before we can be fully effective interlocutors in such a dialog. Specifically, it seems to me that the *ad hoc* nature of the historical evidence marshaled against Norenzayan indicates that we do not, as yet, have effective ways of bringing our scholarly knowledge to bear in a systematic manner when it comes to evaluating broad claims about the historical record. This is where the issue of methodological innovations becomes relevant, because if we are to communicate with scientists we require methodological tools that would allow us to do so effectively.

In dismissing the possibility of explanatory historical work that would be able to substantiate, or undermine, contemporary functionalist accounts of religion, Stausberg observes that

> [s]cholars of religion/s whose work is primarily ethnographic or historical would never dare to pose questions such as whether "religion," or some aspects or factors held to be part of this conceptual package, might have contributed to the scaling-up of human cooperation, for the simple reason that such a question will not be answerable with the help of their extant concepts, data and methods. (605)

He is right about this, but the key word is "extant." At another point, Stausberg observes that "there is a methodological abyss dividing historical and experimental work and so far it seems impossible to replicate the experimental findings on historical data" (604). This "methodological abyss" is, I would argue, linked to the fact that we historians have yet to develop rigorous methods for substantiating generalizations about the historical record. Scholarly argumentation, which we engage in constantly, too often consists of exchanges of cherry-picked examples, with no real hope or expectation of being able to resolve differences of interpretation.

I would here like to suggest two potential methodological bridges across this abyss. These are techniques for supplementing our indispensable and primary training in deep, textured qualitative readings of historical sources with quantitative data in a way that would help us resolve interpretative disagreements, or at least conclude that one side should be favored over another with a specific degree of statistical probability. The first of these methods borrows techniques from the natural sciences (large quantities of data coupled with statistics), the second merely takes advantage of new technologies – the internet and open-source database techniques – to systematize and make more easily accessible the synthetic judgments of experienced historians.

Large-scale textual analysis

A long-held commonplace in my field is that the early Chinese (or "the East" more generally) have a "holistic" view of the self, with any kind of mind-body dualism being a completely alien concept (Jullien 2007; Rosemont and Ames 2009). All parties in the debate agree that, if there is a word for "mind" in classical Chinese, it is *xin* 心, variously translated as "heart," "heart–mind," or "mind," but originally referring to the physical organ in the body. Scholars arguing for the holist position claim that the *xin* is viewed as no different than other organs in the body, such as the liver or lungs, and that it is therefore inappropriate to equate it with concepts in Western languages denoted by *mind*, *Geist*, or *esprit*.

There are a variety of reasons for being dubious about this claim (see Slingerland 2013 for a review), but debates on the topic, to the extent they occur, are hampered by the methodological limitations mentioned above. Scholars such as Ames or Jullien can produce passages from classical Chinese texts that seem to imply a holistic view of the human body-mind; critics can identify others that call such holism into question. Although every scholar feels that his or her preferred evidence is more representative of the early Chinese tradition, we have had no way of actually demonstrating this quantitatively.

Motivated to try to fill this methodological gap, several years ago I ran a project that took advantage of the fact that the received early Chinese corpus is now almost entirely digitized, in fully text-searchable form. We pulled from a representative sample of this corpus every passage containing the word *xin*, and then had teams of coders – graduate students skilled in reading classical Chinese, but

blind to the purpose of the study – characterize the manner in which *xin* was being used in the passages: what its functions were, if it was being contrasted with the body, and so on. Intercoder reliability measures gave us a sense of how consistent these qualitative judgments were. The results, reported in Slingerland and Chudek (2011b) and discussed in more detail in Slingerland (2013), suggest that the early Chinese did, in fact, view the *xin* as qualitatively different from the other organs in the body, with special powers that map on quite well to the powers attributed to *mind* or *Geist*. Moreover, in almost 10% of the passages in which it occurs, *xin* is being explicitly or implicitly contrasted with one of the three main words referring to the body, suggesting a conception of mind–body dualism that seems to grow stronger over the historical period studied (from earliest received texts to the end of the Warring States in 221 BCE).

There are numerous limitations to this study (see, e.g., the critiques by Klein and Klein 2011; and response in Slingerland and Chudek 2011a), but it gives some sense of how we could potentially start to back up generalizations with some quantitative data about the historical textual record. Moreover, historians now have access to potentially much more powerful, and easy to implement, automated textual analysis techniques. Topic modeling, for instance, can crunch through enormous amounts of textual material and produce "topics," or clusters of words that hang together in the corpus in a manner that is statistically unlikely to be the result of chance. These topics, qualitatively analyzed and labeled by experts, can then be used to trace themes, conceptually categorize, or even date individual texts or portions of texts. Collocation analysis allows one to measure the degree to which certain target words tend to appear together in a textual corpus, which may allow one to discern semantic relationships. For instance, we are now extending our original *xin* study to a much larger corpus – one far too large to analyze using human coders – to see if *xin* and the words referring to the body appear in close proximity more than other common organ words do, which would arguably indicate that *xin* is being preferentially singled out for contrast or comparison with the body.

The unifying feature of these techniques is that they are stupid and crude when compared to a properly trained scholar engaging in the qualitative analysis of individual passages. Their sheer stupidity, however, is also their strength. Statistical patterns too subtle for human analysts to notice can be picked up, and implicit biases corrected. I myself found that some of my qualitative intuitions about the early Chinese philosophical corpus were incorrect, and that, for instance, *xin* is much more disengaged from emotion in the late Warring States than I would have said before we conducted the study. They also, as Luther Martin has observed (personal communication), have a long pedigree in Religious Studies, in many cases merely representing faster and more accurate ways of deploying analytic techniques developed by New Testament scholars, particularly in Germany.

In any case, the great virtue of all of these techniques is that they allow us to begin to quantify our understanding of historical texts, which in turn can help us to resolve, or at least tip the balance in, long-standing hermeneutical disputes. Relating this to the methodological concerns surrounding *Big Gods*, they also give us a way to evaluate "big history" claims with an unprecedented comprehensiveness and quantitative rigor. For instance, one current project (Nichols, Logan, and Slingerland, in progress) is looking at the degree to which words denoting high gods in early China (*tian* 天, *di* 帝) tend to preferentially collocate with

punishment and reward terms, as opposed to terms referring to minor or "low" gods (*shen* 神, *gui* 鬼). We are also looking at how these collocations change when the focal terms refer to mythical or historical human rulers, rather than supernatural beings. Because of the speed and power of these automated techniques, we can scan the entire received and archeological textual corpora from the earliest times until well into the medieval period, giving us an overview of the early Chinese corpus that would be impossible for any single scholar to match. Our claims about the semantic significance of any particular collocation pattern will have to be validated with qualitative checks of a sub-sample of the passages analyzed. Automated techniques are also clearly a blunt instrument when compared to the intuitions of a trained scholar. However, these methods – harnessing the power of computers and statistics, and taking advantage of the digitized form of many of our historical corpora — clearly give us a powerful new tool for discerning broad patterns in the history of human belief and practice.

Quantitative encyclopedias of cultural history

A similar desire to get a more rigorous, comprehensive overview of the historical record motivates various projects, all currently in the very early stages of development, to construct databases of cultural history. The project most relevant to the topic at hand is the Database of Religious History (DRH; http://religiondatabase. org/), based at the University of British Columbia. (See Slingerland and Sullivan, forthcoming for an overview.) The DRH is essentially an on-line, quantitative encyclopaedia of religious history, focused on identifiable religious groups, with the "data" gathered directly from relevant historical experts. Data gathering takes the form of historians completing on-line questionnaires concerning a particular religious group, with the questions involving a series of binary decisions concerning the presence or absence of specific features of religious thought or practice (see Table 1 below for examples). Comment boxes allow qualifications of answers, or critiques of the questions or assumptions behind the questions, that are subsequently employed to improve the structure of the questionnaire itself.

The DRH was originally conceived of as a way to rigorously evaluate large-scale explanatory theories of religion – like those outlined in Norenzayan 2013 or Norenzayan *et al.* (Forthcoming-a) – against the historical record, allowing quantitative approaches to cultural evolution while remaining firmly grounded in humanities expertise. In other words, it was designed to allow scholars to pose the sort of large-scale questions about the functionality of particular beliefs or practices in human cultural history that have hitherto been difficult to approach responsibly. In addition, it also allows historians who have no interest in large-scale theories to quickly and efficiently check their qualitative intuitions about the historical prevalence or distribution of beliefs and practices against those of their colleagues, and serves as a center for the documentation of scholarly disagreement. With powerful built-in visualization and analysis tools, the DRH also doubles as an engaging pedagogical tool. We are hoping that other, as yet unforeseen, uses for the DRH will emerge as historians of religion explore its functionalities and suggest new features.

Other database projects are dedicated to exploring more narrow but potentially revealing aspects of the historical or archeological record, such as Roman trade routes as revealed in amphorae seals (Remesal et al. 2014), Carolingian coin

Table 1.   Some relevant entries and variables from the database of religious history (DRH; http://religiondatabase.org/).

Question	Amarna Religion	Ancient Egypt	Early Zhou Religion	Pre-Imperial Chu Religion	Late Shang	Chinese State Religion (Shang and Western Zhou)	Chinese Folk Religion	Warring States Religion	Warring States Confucian Thought	Mithraism	Religion in Attica (600-300BCE)	Israelite Religion	Mesopotamia	Zoroastrianism	Lowland Maya
A supreme high god is present:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		Yes		Yes	Yes	No
Supernatural monitoring of prosocial norm adherence in particular:		Yes			Yes	No	Yes	Yes						Yes	No
Supernatural rewards are bestowed in the afterlife:		Yes		Field Doesn't Know	Yes	Yes			Yes		Field Doesn't Know			Yes	No
[Supernatural punishment] through impersonal cause-effect principle:		No											No		
[Supernatural punishment] to enforce group norms:		Yes			Yes				Yes			Yes		Yes	Yes
[Supernatural punishment] to inhibit selfishness:		Yes			Yes				Yes					Yes	Yes
Are general social norms prescribed by the religious group:	Yes	Yes	Yes	Field Doesn't Know	Yes					Yes				Yes	Yes

Entity Name	Author	Date Range	Region
Amarna Religion	Thomas Schneider	1350 BCE - 1320 BCE	Amarna
Ancient Egypt	Joseph Manning	2000 BCE - 30 BCE	Upper Egypt
Early Zhou Religion	Constance Cook	1046 BCE - 771 BCE	Western Zhou (based on Li Feng)
Pre-Imperial Chu Religion	Constance Cook	790 BCE - 223 BCE	Chu (655-585 BCE)
Late Shang	Keightley & Ashton & Slingerland	1250 BCE - 1045 BCE	Middle and Lower Yellow River Valley
Chinese State Religion (Shang and Western Zhou)	Lothar von Falkenhausen	1750 BCE - 850 BCE	Middle and Lower Yellow River Valley
Chinese Folk Religion	Poo Mu-chou	350 BCE - 200 CE	Qin Dynasty boundaries
Warring States Religion	Armin Selbitschka	481 BCE - 206 BCE	Middle and Lower Yellow River Valley
Warring States Confucian Thought	Edward Slingerland	450 BCE - 221 BCE	Middle and Lower Yellow River Valley + Qin Dynasty boundaries
Mithraism	Roger Beck	100 CE - 400 CE	Roman Empire (Greatest Extent)
Religion in Attica (600-300BCE)	Roger Beck	600 BCE - 300 BCE	Attica
Israelite Religion	Shawn Flynn	1200 BCE - 515 BCE	Levant
Mesopotamia	Karen Sonik/Beate Pongratz-Leisten	3200 BCE - 612 BCE	Mesopotamia
Zoroastrianism	Michael Stausberg	1500 BCE - 500 BCE	Middle East
Lowland Maya	Chris Carleton	600 CE - 900 CE	North America

hoards, or shipwrecks in the Mediterranean (see list of databases at http://darmc. harvard.edu/icb/icb.do?keyword=k40248&pageid=icb.page601659). One particularly important project for evaluating the Big God hypotheses, the Seshat database spearheaded by Peter Turchin and Harvey Whitehouse (https://evolution-institute. org/project/seshat/), focuses on gathering data concerning political, military, cultural, and economic variables, which will prove crucial in resolving the questions raised above concerning the relative importance of secular institutions, warfare-driven pressures and literacy vs. religion when it comes to the rise of large-scale social cooperation. What all of these projects have in common is a desire to harness the power and flexibility of the internet and personal computers to provide cultural historians with a supplement to our traditional qualitative tools, allowing us to try to answer big questions more confidently, and in any case providing a quantitative check to our qualitative intuitions about any aspect of the historical record.

How could these methods help us with evaluating Big Gods?

Thomassen's critique of Norenzayan cites one textbook writer's opinion concerning the non-moral nature of the Greek gods (Garland 1994), and on the strength of this single citation concludes: "So much for supernatural monitoring. Observations of a similar kind can no doubt be made regarding a wide range of ancient polytheistic

religions" (671). One scholar's opinion about one particular culture settles the matter for all time? Similar observations can "no doubt" be made? How are we to evaluate this flat assertion against the evidence Norenzayan presents in *Big Gods* concerning supernatural monitoring in ancient Egypt, China, and Rome? The source Thomassen cites is a basic textbook on Greek religion, and presumably he assumes that one "textbook" answer is sufficient. In most fields, however, textbooks can disagree on important points, as illustrated by the ability of Nozenzayan *et al*. (Forthcoming-a) to cite another (and more recent!) textbook on Greek religion (Mikalson 2010) to support the claim that commitment to the Greek gods played an important role in enforcing prosocial behavior. It would be much more satisfying, for both us as historians and our intended audience, if we were able to demonstrate rather than merely assert claims such as these. This, in turn, requires developing methods for both documenting and assessing the nature of scholarly disagreement in our field.

The DRH has nowhere near enough data at the moment to be entirely useful in this regard, but even the very preliminary information we have gathered is enough to call into question overly confident or quick dismissals of the importance of moralistic gods in early large-scale societies. Table 1 provides a snapshot of DRH answers to *Big God*-relevant questions for several early large-scale societies around the world.

Drawing any kind of strong conclusions at this point in the DRH's development would be premature. As the DRH grows, however, its accuracy and usefulness will increase, as will its ability to quantify scholarly disagreement on particular topics. Unlike any given textbook, or even edited volume, on a topic like "Greek religion," the DRH can potentially provide a fully comprehensive, and constantly updated, snapshot of the state of scholarly opinion. Its ability in this regard has been enhanced by recently developed features such as the "Challenge" button, which allows a scholar browsing another expert's entry to provide an alternative answer to a given question about a religious group without having to create an entirely new entry. Maybe it is the case that the historical community disagrees entirely about whether "supernatural monitoring of prosocial norm adherence in particular" is a feature of Greco-Roman religion, with scholars splitting more or less 50–50 on this question. Perhaps, as Thomassen suggests, Garland 1994 actually represents a broad consensus (say, 90–10) in the field that the answer to this question – all things considered – is "no," or perhaps it is Mikalson's textbook that is more representative of the state of the field. Any truly confident and responsible stance on such questions, however, requires precisely the sort of new methodological tool that the DRH, and projects like it, provides.

A consilient approach to religious studies: back to the future

In his commentary, Armin Geertz observes:

> It is indeed a remarkable fact that the cognitive science of religion (CSR), despite its weaknesses, nevertheless has made it possible to address once again the big questions that had been abandoned by frustrated scholars of religion 100 years ago. (Geertz 2014, 610)

Despite whatever shortcomings in *Big Gods* that we historians of religion might perceive, I agree with Geertz that the proper response is to get back in the big-picture

explanation game, not to simply dismiss it. This would, of course, have the function of radically increasing the quality of the work being done in these areas, much of it spearheaded by non-historians. Additionally, it would help us to demonstrate the importance of what we, as historians, do – something that is, in my view, increasingly urgent as the humanities come under pressure in universities and colleges across the world. It would also allow us to regain a broadened sense of collegiality: that our intellectual interlocutors include not just colleagues in our particular, narrow field, but a broad scholarly community extending across the humanities and into scientific disciplines.

In his commentary, Luther Martin at one point observes of CSR that, if it

> is to move beyond its circle of experimentalists to include a larger faction of religious studies scholars, as I believe it must if it is to make a lasting contribution to the study of religion, its practitioners need more actively to seek collaborations with these traditional scholars. (2014, 636)

I heartily agree, but the obligation goes both ways. Although Martin found *Big Gods* disappointing on this front, at least Norenzayan made, and continues to make, an effort to engage with historians – no one needs to convince a scientist that hypotheses require data. Based solely on the evidence provided by the *Religion* forum on *Big Gods*, it is less clear to me that the historical community is exerting a corresponding effort.

CSR desperately needs engagement by humanities scholars, and this is not just because historians are custodians of "the data," as Martin suggests. In a properly "consilient" (Slingerland and Collard 2012) approach to cultural history, humanities scholars would be in on the ground floor of theory formation, pointing out problems with the scientific method (philosophy of science being bizarrely almost entirely unknown outside of the humanities), debunking culturally parochial analytic concepts (e.g., clearly Protestant models of religiosity, Abrahamic-based conceptions of religious affiliation), and highlighting the challenges of reducing qualitative textual and archeological material to quantitative data. At the same time, responding to work in CSR forces historians of religion to begin engaging again with "big" explanations of cultural history. Doing so not only helps to re-establish a dialog between religious studies and the broader world of empirical and intellectual inquiry, but also incidentally does much to defuse the supposed "crisis" of the humanities by demonstrating the relevance of humanistic, scholarly knowledge to modern scientific questions as well as broader contemporary concerns.

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