Pro- and Assortative-sociality in the Formation and Maintenance of Religious Groups[1]

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Studies of evolved mechanisms and strategies supporting religious prosociality seem to dominate the experimental research agendas of many cognitive scientists of religion. Their enthusiastic and untrammeled preoccupation with prosociality would seem to predict for the human species a kind of global *kumbayah*. But in the millennia of their existence, religions have never realized this goal. For anti-sociality seems to be as well-established in our evolved repertoire of behaviors as is prosociality (Weierstall *et al.* 2013, 48; Tooby and Cosmides 2010, 192; Gat 2010; Choi and Bowles 2007; Kelly 2005), perhaps as a strategy for securing reproductive advantage (Weierstall *et al.* 2012, 1–2; Chang *et al.* 2011). Religions, especially, historically as well as currently, are recognized to be chronically implicated in this discord and violence, directed at those beyond their artificially defined boundaries of theological doctrine, and, as often, towards those claiming common religious identities but who have fragmented into sectarian factions and conflict (Mlodinow 2012, 164). Those of us with an eye towards history—or even towards current events—know that any simple congruence of religion and prosociality has never been the case.

Prosociality

Recent interest in religious prosociality among cognitive scientists of religion seems to have been especially motivated by Ara Norenzayan's and Azim Shariff's 2008 article on "The Origin and Evolution of Religious Prosociality." Here, Norenzayan and Shariff present an overview of the "empirical evidence for reli-

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gious prosociality," that is, for "the hypothesis that religions facilitate costly behaviors that benefit other people at a personal cost" (2008, 58). Norenzayan and Shariff readily acknowledge that conclusions from the empirical evidence they cite is based upon the usual array of exaggerated self-reports (Saroglou 2006: 1-2), the results of acontextual game playing (Boehm 2012, 324), of contrived experiments on priming effects, etc. and that these data allow for a diversity of interpretations (Norenzayan and Shariff 2008, 50). Nevertheless, they conclude that this evidence, collectively considered, supports rather unexceptional conclusions about *ingroup* behaviors (Norenzayan and Shariff 2008, *passim*), such as, for example, that members who are strongly committed to a group, such as to a religious kibbutz, are more committed to that group than are those who are less committed to a group (Norenzayan and Shariff 2008, 59).

Norenzayan's and Shariff's claims about religious prosociality privileges the behavioral supports and strategies of religion over those that might be extended by other social groups with which an individual might identify, such as claims of fictive kinship, political ideologies, utilitarianism, the synchronizing practices of "dance and drill," etc. (e.g. Galen 2012, 878; McNeill 1995). While religions have certainly provided widespread support and strategies for ingroup solidarity, they have done so for historically contingent reasons. That is to say, ingroup prosociality can be accounted for by general psychological mechanism and strategies (Galen 2012, 888–890; Diener *et al.,* 2011, cited by Galen 2012; Boyer 2009, 19, citing Fessler 2001 and Gintis 2000) as well as by any number of ingroup markers in addition to the religious, such as "race, nationality, computer use, or…[an] operating unit at work" (Dion 1972; Ashforth and Mael 1989; cited by Mlodinow 2012, 167; Turchin 2007, 54, 84). In fact, research has shown that the only requirement necessary for ingroup affinity is simply the "act of knowing that you belong to a group" (Mlodinow 2012, 171; Sherif *et al.* 1961).

Based on the rather self-evident data about characteristics of ingroup behaviors, Norenzayan and Shariff—and others—propose to extend these conclusions to the more problematic hypothesis that religious prosocial behavior affords an advantage for the realization of large-scale, complex societies as well (Norenzayan and Shariff 2008, 58, 62; e.g. Turchin 2007, 7; D. S. Wilson *et al.* 2009; Atran 2012, 211; Reddish *et al.* 2013; Slingerland *et al.*, 2013). Large-scale societies are usually described as complex because they are, well…..complex (e.g. Turchin 2007, 3, 338), that is, they are comprised of a diversity of separate and separatist groups, each with their own self-interested identity, a characteristic of large-scale societies already described by Alexis de Tocqueville in his "ethnography" of nineteenth-century America (Tocqueville 1900, II.2.5). Since, the evidence for religious prosociality cited by Norenzayan and Shariff and others only

supports small-scale ingroup dynamics, their proposed hypothesis of extending these dynamics to the facilitation of large-scale group formation seems to be by inferential fiat with dubious empirical support.[2] In defense of their hypothesis about a role for religious prosociality in facilitating large-scale social formations, they venture, consequently, into the murky analogies and contested arguments for what Steven Pinker has judged to be the "false allure of group selection" (Pinker 2013). For example, Scott Atran and Joseph Henrich, in their article on "The Evolution of Religion," argue for a neo-Spencerian survival of the culturally fit in which prosociality is, however, not a characteristic of religion at all but is, rather, a by-product of religious competition and conflict (Atran and Henrich 2010).

However argued, the hypothesis that religious prosociality provides a basis for large-group cooperation simply does not account for the diversity, heterogeneity and xenophobia of such human groups, especially religious groups, that are documented throughout the history of *Homo sapiens.* And, although Norenzayan and Shariff acknowledge, but only in a concluding aside, that historians might have something to contribute to discussions about human behavior (Norenzayan and Shariff 2008, 62), they, like experimentalists generally, have neglected to attend to the realities of actual human behaviors, in real-life situations, that have been, and continue to be, documented for *H. sapiens* since the beginnings of the species (e.g. Smail 2008; Galen 2012).

Assortative sociality

Whereas prosocial behavior would seem to be an evolved proclivity for small-scale groups of humans generally, religions have, from their social origins, been promoters of, perhaps the *primary* promoters of, what we refer to as assortative

2. We should like to take brief note of Ara Norenzayan's recent book, *Big Gods: How Religion Transformed Cooperation and Conflict* (2013), which was published after the writing of this paper, since it addresses one of the main themes of our critique. Norenzayan maintains that it is belief in "Big Gods" that made possible the extended cooperative behavior among total strangers and that ultimately gave rise to civilization-size human groups. The argument, however, is problematic on several levels. A close reading of his book shows, that belief in big gods is neither a necessary nor a sufficient condition for "human groups to rapidly scale up from hunter-gatherer origins to the vast societies of millions today" (75). In discussing the ambiguity of talk about "big gods" with respect to early Chinese civilization, for example, he insists "that supernatural monitoring is [not] the *only* mechanism that can push groups to expand" (134–135) and in his Introduction he admits that even though "big gods" might be a factor in such a development, they were "not the sole cause that led to large-scale cooperation" (9). Nor does the "Big-God hypothesis" correspond to the historical evidence such as that we suggest with our brief example from Hebrew Epic (p. 5). The kind of evidence needed to support Norenzayan's argument requires the kind of extensive historical analysis that one finds, for example, in Norman Yoffee's *Myths of the Archaic State: Evolution of the Earliest Cities, States, and Civilizations* (2005)*,"* an analysis that does support our *example*.

sociality. We propose that assortative sociality, whereby members of religious groups select for those like themselves and prioritize their *differences* from others, more accurately models the ethnographic and historical data for real religious behavior in the real world.

The features of ingroup sociality constitute a repertoire of behaviors that function not only as a social bond between members of each specific group but as a bond so strong as to form a barrier to those outside it, greatly minimizing contact with out-groups. The ingroup cohesiveness of religious beliefs and behaviors clearly undermines openness, individualism, non-conformity, and risk tolerance while fostering collectivism and conformity, ethnocentrism and philopatry (reduced mobility outside one's natal group), intergroup vigilance, and xenophobia. Consequently, religions function as cultural phenotypic markers that "pseudo-speciate" the human race, as Peter Munz puts it metaphorically (Munz 1985, 295–303, esp. 300**).** Thus, even though religious beliefs and practices may encourage prosocial behavior with respect to the ingroup they are anything but prosocial with respect to members of out-groups.

An explanation for this paradoxical way in which religions seem to function in human society can be found in the fact that humans, like other organisms, face two kinds of challenges, immediate and obvious threats to life and limb and long-term and unpredictable challenges to their general security. A first set of overt and immediate threats provokes an instantaneous response by the sympathetic nervous system, the so-called freeze, flight or fight reaction. A second set of threats is activated by relatively subtle cues of potential danger, which provoke a more generalized vigilance that engages a probing into and manipulation of the physical and social environment. In each case, the response to the threat may be either protective—shielding oneself from attack, or destructive—preemptively responding to eliminate the potential danger.

Recent psychological, anthropological, and cognitive science research provides considerable evidence to show that religions may well have originated as hazard-protection systems against the second kind of threat, i.e., against the unseen pathogens and parasites not endemic to their group (Fincher and Thornhill 2012; Schaller 2006; Schaller and Murray 2007, 2010). This system, subsequently, provided an expedient defense against out-group intimidations and predations. For example, virtually all religions claim to know the truth which others lack, generating, thereby, suspicion about the "false" beliefs and "immoral" practices of others and, consequently, casting them as dangerous and untrustworthy (Schaller 2003, 224). Religions, therefore, would have been adaptive in our ancestral populations by virtue of functioning as "behavioral immune systems" that actively discouraged intergroup and large-scale coop-

erative behavior. With the historical increase in economically and politically motivated cooperation among groups, however, this ancestral "immune system" came to function as a maladapted "auto-immune system," whereby religions continue their original assortative function of ingroup defense (Wiebe 2013). As Jared Diamond has similarly concluded, the "[r]eligious values" of certain "tightly communal and mutually supportive" societies "allowed them to survive for centuries….[However, they] also prevented them from making the drastic lifestyle changes…of [even more successful societies] that might have helped them survive longer" (Diamond 2005, 423). Since, these conservative religious values "tend to be especially deeply held," Diamond concludes, they are "a frequent causes of disastrous behavior" (Diamond 2005, 423).

Example

An example of the relations between pro- and assortative sociality to which we refer is exemplified from the very origins of the Western religious tradition.

Hebrew epic recounts the story of a group of late Bronze Age, Middle-Eastern Bedouin tribes that confederated into a common people. Whatever its historicity, this epic has provided the "charter myth" for virtually all Western religious formations. According to this epic account, the success and stability of the incipient Hebrew federation involved their prosocial claims to descent from a common ancestor, i.e., to the construction of (fictive) kinship and to their acceptance of a set of governing rules, the so-called Ten Commandments (Exod. 20: 1–17). These governing rules were legitimated for the federated tribes politically, by appeal to the hierarchical sovereignty of their still tenuously accepted leader, Moses, and, of course, invested with the authority of their no less still tenuously accepted common deity (Exod. 4:1). The characteristics of their socially-postulated deity are generally acknowledged by historians and biblical scholars to be derived from those of a tribal war god (Exod. 15:3; McNeill 1963, 159; Brueggeman 1997, 23). Despite the prosocial behaviors commended by the Hebrew's new social code, they nevertheless engaged, in the name of their deity, in some rather nasty assortative behaviors towards those who remained outside of the federation, i.e., towards the Canaanites, the Hittites, the Amorites, the Perizzites, the Hivites, and the Jebusites. Not only were the Hebrews commanded by their newly accepted deity to seize the land of neighoring non-Hebrew tribes and to expel them from it (Exod. 34:11) but, further, to "blot them out" (Exod. 23:23)—a god-sanctioned aggression against outgroups that recurs throughout subsequent accounts of Hebrew history (e.g. 2 Kings 8:12; 1 Samuel 15:3; Psalms 137:9; Isaiah 13:16; Nahum 3:10) (Martin 2013), and that continues to be documented experimentally today (Bushman *et al.* 2007).

And yet, research on the historical relationship between religion and violence, exemplified but not limited to Hebrew epic, has been largely neglected by those fixated on religious prosociality. The concluding but single acknowledgement by Norenzayan and Shariff that the "'dark side' of within group cooperation is between-group competition and conflict" (Norenzayan and Shariff 2008, 62; citing Choi and Bowles 2007) is typical of the disregard afforded this dimension of religious behavior. Whereas a few cognitivists have engaged the relationship between religion and violence (esp. Atran 2010), those engaged in research on how religions might "facilitate costly behaviors that benefit other people" have neglected to engage in corresponding research on this "dark side" of religious behaviors. Both assortative- as well as pro-sociality are available behaviors that Franz de Waal has argued is characteristic of "all nature" (de Waal 2013, 183). Religions, like any social association, may afford relevant cues that trigger one of these behaviors or the other (Norenzayan and Shariff 2008, 62; Boyer 2010, 379, citing Sell *et al.* 2009; Xygalatas 2013), but, we argue, no religion is characterized exclusively by either behavior alone.

Conclusions

1. We certainly agree that prosocial behavior is characteristic of ingroup cohesion and cooperation—although that insight seems to be something of a truism. We question, however, whether *religious* prosociality is any more (or less) robust than any other basis for group belonging, including arbitrarily assigned affiliations, as documented by the classic robbers cave experiment (Sherif *et al.* 1961). Rather, it would seem as though the positive effects of religious prosociality can only be assured within the context of *religious* ingroups.

In a comprehensive review published last year in the *Psychological Bulletin*, Luke Galen has critically evaluated the empirical evidence for claims to a relationship between religious belief and prosocial behaviors (Galen 2012). A psychologist of religion, Galen concludes that the religious prosociality hypothesis represents a congruence fallacy, that is, that the observed effects reflect stereotypes and ingroup favoritism, that they are due as much to non-religious as to religious psychological effects, that they are inconsistent, and that they confound those low in religiosity with nonbelievers. In brief, Galen concludes from his overview of the evidence that "the relationship between religiosity and prosociality is essentially zero, or even negative" (Galen 2012, 899, but see Saroglou 2012).

2. We question the hypothesis that religious prosociality plays any role in the formation of large-scale societies. Although the positive effects of religious prosociality are assured within the context of religious ingroups, the effects of

religious prosociality in contexts of large populations would seem, rather, to exaggerate social divisiveness. The example from Hebrew epic about tribal federations not only illustrates Galen's conclusions about religious prosociality but supports Robin Dunbar's hypothesis concerning the relationship of group size to social organization. According to Dunbar, the social-processing capacities of the neo-cortex of the human brain constrain the size of face-to-face, small-scale societies to ca. 150 or fewer, in which leadership roles are but intermittently assumed. Consequently, small-scale groups, if stable, must be organized principally on the basis of prosocial behaviors—whether religiously supported or not. Large-scale group formations, on the other hand, i.e., those having memberships greater than ca. 150, increasingly require, according to Dunbar, centralized and continuing leadership roles to neutralize assortative pressures—in our example from Hebrew epic, the claim to Mosaic suzerainty and authority. Consequently, explanations for the success, structure, and stability of complex, large-scale societies shift from the domain of evolutionary and cognitive theory proper to a historical consideration of economic and political benefit. In other words, advantages for the realization of large-scale societies, which *may* involve an exploitation of religious symbolism (Turchin 2007, 54, 84), rests largely upon environmental and economic factors and, of course, upon the development and the control of these factors by political management (Diamond 2005, 2–15). Or, a large-scale society may depend upon a coercive imposition of power whereby ingroup religious authority is replaced by political authority (Diamond 1999, 281; Gauchet 1997), or, at a minimum, whereby the religious is relegated to a function in subservient support of the political (Diamond 1999, 266, 278). In the summary of archaeologist Norman Yoffee, the development of large-scale societies was characterized by a transformation of small-scale "social relations…into relations of dominance" (Yoffee 2005, 32). For example, Jared Diamond, in his overview of the increase in large-scale societies over the last 13,000 years, only mentions religion a few times, and then primarily its role in support of military conquest (Diamond 1999). In other words, understanding the development of large-scale societies is more a matter of political science than of cognitive science.

   3. Even as we have previously recommended that historians of religion should consider the experimental findings of cognitive scientists of religion in their historiographical reconstructions (e.g. Martin 2012), we recommend that experimentalists in the cognitive science of religion include historians on their research team or, at least, consult with historians in formulating the assumptions of their experimental design and research. Any behavior that is hypothesized to be pan-human should be able to be documented from actual behaviors throughout the history of the species. Experimental researches that disregard that his-

tory are in danger of producing naïve, decontextualized, one-sided, or otherwise misleading results. With reference to the present topic, historians have universally included religious conflict as a central topic in their accounts whereas cognitive scientists and historians of religion have almost completely disregarded this issue in theirs.

4. Finally, we question why there is currently a preponderance of research on the cooperative effects of religious prosociality that neglects the fractious dynamics of religious assortative sociality? It would seem that research assumptions about religious prosocial behaviors conserves, first of all, an ubiquitous cultural bias that religion is always "good," i.e., it is associated with what Galen identifies as the stereotype "that religion is [simply] presumed to be associated with prosociality" (Galen 2012, 878, 890). This bias leads to an ingroup theological imperative for dismissing "corrupt" religious behaviors as not being representative of "authentic religion" (Kimball 2008, 8). Consequently, research that emphasizes religious prosociality appeals, especially, to funding agencies espousing religious agendas (Coyne 2012), but to secular funding sources as well as they seek to understand persisting incidents of what they consider to be "bad" religious behaviors.

In their continual—and legitimate—quest for funding, researchers in the cognitive science of religion insist that the sources of their funding, and the agendas of those sources, in no way influences their research, which they claim, they would otherwise pursue independently of those agendas. This is, of course, the same claim that is made, for example, by researchers in the development of new medications that is funded by the pharmaceutical industry, or by those investigating the benefits of "clean" coal that is funded by the mining industry.

We submit that the preponderance of experimental research emphasizing the social benefits of religious prosociality, but that neglects the asocial consequences of religious assortative sociality, aspires to the same financial benefit and, at least, gives an appearance of influence by the agendas of their benefactors (Holden 1999).[3] Such "conflict-of-interest" funding raises suspicions about the neutrality of such research and, consequently, endangers the integrity of our collective scientific enterprise generally.

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Beyond "Prosocial"

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Martin and Wiebe rightly call attention to previous work emphasizing a simple congruence between religion and prosociality. One hypothesis is that religion is a culturally-evolved mechanism that extends prosociality beyond a narrow radius of trust to include strangers via supernatural monitoring, thus allowing complex societies to emerge (Norenzayan 2013). However, this interpretation is problematic for reasons emphasized by Martin and Wiebe. No religion is characterized exclusively by either prosociality or antisociality and neither the experimental nor historical evidence indicates a uniformly prosocial role for religion beyond ingroup boundaries. There is consensus that religiosity includes what Henrich *et al.*, (2010) describe as: "prosocial behaviour towards co-religionists (and the exploitation of non-co-religionists)" (p.S88). A variety of terms aptly subsume both elements, including "assortative sociality" and "parochial altruism." Consequently, the effects or associations of religion should always be contextualized in relation to group membership.

Therefore, when the discussion regards the putative causative role of religion in cultural and societal evolution, the most relevant question is not "does religion increase prosociality among group members?" but rather "does religion extend morality towards strangers and outgroup members?" Evidence that merely supports the former is often conflated with the latter (i.e., parochial altruism is misinterpreted as general prosociality). For example, members of religious groups contribute more to their own group than do secular group members, however religious belief does not predict charity or volunteering outside the group (Galen *et al.* in press). In economic studies, when participant and partner religiosity are fully controlled, religious individuals' greater trust is contingent upon shared identity (Tan and Vogel 2008), again indicating parochial altruism.

Variation in trust, conflict and "strangers" across societies

One ambiguity in the literature regards how individuals in any given study classify anonymous partners (e.g. as presumed co-religionist versus religious

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outgroup member). For example, in some studies, targets referred to as "strangers" are actually depicted as sharing a religious identity (Fitzgerald and Wickwire, 2012). This is related to the "radius of trust" problem; a systematic variation in trust toward inferred reference groups (Delhey *et al.* 2011). There are cross-cultural variations in the extent of trust, and the religiosity of a country is negatively correlated with its radius. That is, in more religious countries, trusting "most people" connotes ingroup members (family, familiars), whereas in less religious countries this connotes religious outgroup members or strangers. Outgroup-inclusive trust is not associated with religiosity (Welch *et al.* 2007). Therefore, interpretations of studies involving interactions such as economic exchanges should consider that those from religious cultures are more likely to consider any "strangers" encountered as being within a narrower radius of trust, not as outgroup members. These concepts are relevant to cross-cultural studies, which have been used to assess the idea that religion promotes societal growth via the belief in supernatural punishers. However, although belief in watchful, moralizing gods is more prevalent in larger societies, possibly functioning to maintain cohesion, such beliefs are not associated with reduced internal conflict (Roes and Raymond 2003). Indeed, religious belief tends to exacerbate conflict (Neuberg *et al.* 2014) partially by emphasizing a group-focused morality (Cohen *et al.* 2006).

Subtypes of religious prosociality

Just as with the term "prosociality," "religion" actually consists of disparate concepts of which only a portion is linked to universal prosociality. For example, although religious group affiliation and belief in God(s) frequently coincide, at a construct level, the former is associated with ingroup preference whereas the latter is associated with outgroup prosociality (Preston and Ritter, 2013). The role of religion in promoting parochial altruism can be seen in its relationship with domains promoting the community in the face of intergroup competition (e.g. "binding" morals, including, ingroup and authority-based morals) rather than generalized prosociality (Haidt and Graham 2010). Similarly, religiosity is more related to "reproductive morals" (views on homosexuality, prostitution, abortion, divorce, drug use, affairs, casual sex) than with cooperative ones (honesty, civic behavior, non-violence; Weeden and Kurzban 2013).

As Martin and Wiebe mention, religion functions as hazard protection against threats, or as a behavioral immune system (5–6). This is consistent with Haidt's purity domain, emphasizing the prevention of contamination whether literally (avoiding ingestion) or metaphorically (mistrust and disgust toward value-violators). Religious conservatism is a reflection of such a system (Terrizzi *et al.* 2012). However, all binding morality domains are relativistic contingent upon

group identity. For example, the link between authoritarianism and sensitivity to fear/threat is contingent upon the perception of group-specific threat (Stellmacher and Petzel 2005). Thus, religious morality may have been adaptive in a specific context with intergroup conflict and high need for group cooperation, but is now only common in relatively difficult and conflictual environments (Diener *et al.* 2011). Indeed, religiously-based binding morals are viewed in stable, secular societies as anachronistic, particularistic, and immoral.

Is supernatural monitoring necessary?

Another area of contention regards whether religious effects are sui generis in fostering parochial prosociality or are equivalent, or reducible to, secular mechanisms. One example of a broader mechanism of prosociality (of which supernatural monitoring is only one manifestation) is self-awareness. The activation of concepts related to social scrutiny trigger similar effects to those activated by god concepts (Gervais 2012). The presence of a mirror has been found to increase honesty via a similar intuition (e.g. "what would others think of me"; Diener and Wallbom 1976). Both religious as well as secular "civic promoting" stimuli invoke stereotypes leading to prosocial behavior through ideomotor processes. This explains why religious priming often has equivalent effects on nonreligious individuals. Similarly, the conceptual link between bright light and honesty/generosity (and conversely, darkness with selfishness or dishonesty) also appears to be tapping into concepts of sensed monitoring by others (Chiou and Cheng 2013). Thus, supernatural monitoring can be more parsimoniously reduced to a general social monitoring mechanism.

Other, quasi-supernatural (but not "High God"-related) social intuitions also represent activation of primal evolutionary mechanisms. For example, a common intuition that inhibits the flaunting of fortune is the "evil eye"—a superstitious fear of provoking envy (Berger 2013). Like supernatural monitoring, this represents an externalization or projection of social monitoring to ensure fairness and egalitarian status in groups. Certainly, these can be activated by religious concepts and projected as supernatural agency (e.g. "God is watching me") but experimental work has demonstrated that a variety of moral intuitions are readily projected onto God (Epley *et al.* 2009). Supernatural content is not a necessary component, but one of a variety of general social monitoring intuitions.

Are "Big Gods" causal?

As Martin and Wiebe indicate, Norenzayan often uses language open to differing interpretations regarding the causality, necessity, and sufficiency of "Big Gods" for cooperation. This is problematic because supernatural monitoring is not the sole prosocial mechanism, as Norenzayan mentions. A competing inter-

pretation is that religious concepts reflect, rather than causally initiate, societal changes, such that cultural shifts have led to evolutions in religious emphases (Wright 2009). For example, as Henrich *et al.* (2010) found, market integration norms account for much of the cross-cultural variance in cooperative interactions. Therefore, the real question is whether or not religious mechanisms play a causative role as opposed to being projections of intuitions originally established by other cultural influences. As Wright (2009) puts it, "the drift of the divine can be understood only by appreciating divinity's subservience to the facts on the ground" (p. 77). Rather than secular societies having "climbed the ladder of religion and then kicked it away," as Norenzayan describes, the moral ascent may never have been causally contingent upon religious concepts.

History of assortative sociality

Martin and Wiebe also rightly recommend that historical analysis be added to the range of disciplines investigating the social functions of religion. In the present context, parochial altruism rather than prosociality can be found, for example in Israelite moral codes that were often moderated by group status (e.g. slavery). Clearly, the god of the Hebrew bible changes character as the texts shift from a patriarchal herding culture to a more cosmopolitan Greco-Roman one (Wright 2009). Characteristics necessary for existence in an inter-tribal milieu (tit-for-tat cycles of retribution) are less useful in more pluralistic environments, so God's character becomes more universalistic and less anthropomorphic.

In Biblical contexts, antisocial actions such as divinely-ordained differential punishment of outgroup members appear to be accounted for by the same group-relativistic mechanisms. Thus, the intuition of being watched by God(s) does not solely drive prosocial actions but in many cases could exacerbate conformist, antisocial actions if the target is an outgroup member, making God a "divine Milgram experimenter" rather than merely a supernatural monitor.

Conclusion

Marin and Wiebe aptly point toward an understanding of religiosity as more related to assortative sociality rather than prosociality. Therefore, future research should involve a more detailed contextualization of the group identity of involved parties. Clearly, in pluralistic societies, the most functional type of prosociality is one in which a given individual's group status is irrelevant.

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No global *kumbayah* implied: Religious prosociality as an inherently parochial phenomenon

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In a recent critique, Martin and Wiebe (henceforth MW) address what they view as several fatal shortcomings in contemporary evolutionary approaches to religious prosociality. MW present an argument that 1) begins by accusing many vibrant research programs of an "untrammeled preoccupation with prosociality" that leads to a faulty prediction of a "global *kumbayah*" (p. 1), and 2) argues that researchers with a keen eye on history and current events would be able to easily overcome an obsession with prosociality and instead recognize the clear role of intergroup conflict that is endemic to religions. Furthermore, MW stress that researchers in the field claim that *only* religious prosociality can explain large-scale cooperation and that they ignore many other historical and political causes.

We wholeheartedly agree with MW that religious prosociality has not produced a global *kumbayah*. However, we think it would be hard to find a religious prosociality researcher who actually endorses MW's description of the field. While religions may promote cooperation among coreligionists, they obviously and frequently drive conflict between groups. Just as MW suggest that researchers broaden their focus to include history and conflict, we suggest that readers of MW should similarly broaden their focus to include the claims actually made by the proponents of religious prosociality. Namely, such a reading should include the large amount of empirical research and theorizing by religious prosociality researchers regarding history and conflict that, unfortunately, did not make the final cut of MW's critique.[4] We highlight two overlapping findings characteristic of the field, that 1) religions often foster ingroup cooperation, *and* promote outgroup enmity (i.e., parochial altruism), and 2) while it isn't the only binding force, many elements of religion make it especially good at promoting large-scale (within-group) cooperation.

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4. In a footnote, MW briefly mention Norenzayan's new book, which came out after they wrote their critique. Of the ten chapters in this book, three (chapters 5, 8, and 9) explicitly deal with religious conflict and between-group competition.

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Religions practice what they preach: Parochial altruism

Which is most characteristic of religion: theological directives such as the "Golden Rule" or its opposite, "Eye for an eye"? The short answer is, both. Neither religions, nor any other factor proposed as a mechanism facilitating the rapid scaling up of human cooperation in some groups over the past twelve millennia promote universal helping behavior, nor should they be expected to (e.g. Atran and Ginges 2012; Atran and Henrich 2010; Graham and Haidt 2010).

In the evolutionary sciences, cooperation has long been seen as a particularly challenging puzzle. For cooperation to get off the ground, individuals must incur personal costs in order to provide benefits to others. Usually selfish strategies outcompete cooperative ones, except in special cases such as kinship or repeatedly interacting dyads. However, these mechanisms make poor candidates for explaining the recent diversity and flourishing of large-scale cooperation actually witnessed in *Homo sapiens* (e.g. Henrich *et al.* 2006). Just as cooperation rapidly increased in the last twelve millennia, so has the emergence and stability of religions endorsing big moralizing gods. These two puzzles may actually answer each other (e.g. Norenzayan 2013). The central premise of religious prosociality is *not* that religious people indiscriminately cooperate with others. Rather, it is that some religions may include features that were conducive to promoting *ingroup cooperation*, and as a result *outcompeted* rival religions over the course of cultural evolutionary history. In short, successful religions have had norms promoting ingroup cohesiveness and cooperation. In fact, many religious narratives instructing compassion and fairness were intended to apply only to the ingroup (e.g. "Love thy neighbor as thyself," referring only to other Israelites). Indeed, current research conducted within a framework that views religion as an inherently parochial promoter of altruism focuses specifically on conflict and cooperation as two sides of the same cultural evolutionary coin.

Within psychology, religion researchers have used a variety of methods to document how parochial altruism often leads to enmity and aggression towards outgroups. For example, participants exposed to religious priming display more implicit and explicit racial prejudice (Johnson, Rowatt and LaBouff 2010), show increased support for suicide attacks (e.g. Ginges, Hansen and Norenzayan 2009), show increased vengeful behavior (Saroglou, Corneille and Van Cappellen 2009), and show increased negative attitudes towards atheists, outgroup religions, and gays (Johnson, Rowatt and Labouff 2012). The case of anti-atheist prejudice is especially instructive, as the logic of religious prosociality was instrumental in making, testing, and ultimately supporting many hypotheses regarding this particularly puzzling form of outgroup enmity (e.g. Gervais, Shariff and Norenzayan 2011).

Importantly, a recent study has explicitly investigated the link between religiosity and discriminate prosociality (i.e., assisting the ingroup but not the outgroup). Preston and Ritter (2013) found that people were more likely to think that their religious leader would want them to help an ingroup family in need (versus an outgroup family) and, after having their religious identity activated were more likely to donate to an ingroup charity (versus an outgroup charity). This dual focus on cooperation and competition is not hidden deep within the religious prosociality literature. Indeed, the third sentence of Norenzayan and Shariff's article stresses how critical parochial altruism is to the evolution of religious prosociality: "Social science theories have long pointed to religion as a cultural facilitator of social cohesion and ingroup solidarity…*often at the expense of rival groups"* (2008, 58, emphasis added). MW point out that history reveals at least as much religious conflict as religious cooperation. A thorough reading of the current literature on the parochial nature of religious prosociality similarly reveals as much about religious conflict as religious cooperation.

It's the conflict that drives large scale cooperation

Almost everyone who ever lived has been religious, but up until about 12 millennia ago, religions lacked organized leadership, unified doctrine about supernatural agents, formalized costly rituals, and gods that proscribed behavior (Boyer 2001). However, our species' heavy reliance on intergenerational cultural learning, in combination with new ecological and social pressures, may have promoted the spread of prosocial religious norms that promoted large-scale cooperation *within circumscribed religious groups* (see Atran and Henrich 2010; Norenzayan and Shariff 2008). While the timeline of the emergence of "moralizing Gods," and the extent to which they may have facilitated large-scale cooperation can be debated (see Baumard and Boyer 2013; Norenzayan 2013), the success of some religions over others cannot.

While religion is not unique in its ability to garner ingroup cooperation, it is particularly good at promoting intergenerational transmission of its concepts by harnessing innate learning biases (Atran and Henrich 2010). These biases however, are insufficient in explaining how norms toward ingroup prosociality have become so intertwined with some successful religious traditions. *Competition* between alternative stable sets of norms across human groups creates conditions in which it may be culturally adaptive to generate and adopt ingroup prosocial norms (Atran and Henrich 2010). It's well noted that lethal and nonlethal intergroup competition appears to have been constantly present throughout human history and prehistory (Bowles 2006; Keeley 1996). Graham and Haidt (2010) argue that in this backdrop, cultures that were able to use religion to bind the

group together would have gained a significant advantage over less cohesive groups.

Roes and Raymond (2003) tested this hypothesis by examining ethnographic data from a diverse range of human societies. They found that 1) more competition between societies existed in environments rich in natural resources, 2) that larger societies tended to occupy these areas, 3) that these larger societies experienced more intergroup conflict, and, critically, 4) that they more often had religions endorsing big moralizing gods. Of course, we expect that multiple components can promote prosociality (as MW do). Henrich and colleagues (2010) conducted three behavioral experiments across 15 diverse populations, ranging from Hadza foragers to Shuar horticulturists. They found that—in addition to belief in big moralizing gods—degree of market integration predicted norms about reciprocity and fairness, and that society size positively covaried with punishment in economic games. Religious prosociality does not imply *solely* religious sources of prosociality.[5]

Shared research questions

We believe that when the findings emerging from the psychological study of religious prosociality are cast in the appropriate light, their claims are well supported by both research and history. While *ingroup* prosociality may have been central to the cultural evolution of large-scale cooperative efforts, it did not culturally evolve without (often violent) competition with other religious groups. Oftentimes, it is the conflict between groups that creates the cultural selection pressures for cooperation within groups. This, in turn, selects for a religious psychology that can both espouse the "golden rule" amongst fellow ingroup members and concurrently promote outgroup aggression, xenophobia, divisiveness, and ethnocentrism.

As a final thought, we'd like to turn readers' attention to the recently initiated *Cultural Evolution of Religion Consortium* based at the University of British Columbia and Simon Fraser University (http://www.hecc.ubc.ca/cerc/project-summary/). The consortium brings together researchers from a diverse range of fields (including history) and has a number of projects ranging from the creation of a database of religious history to addressing parochial altruism and outgroup hostility—all of which MW raise as points that religious prosociality researchers allegedly ignore. We'll leave it up to the judicious reader to critique the findings in the religious prosociality literature, but by reframing and correcting MW's brief characterization of religious prosociality, we hope that readers will

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5. That said, secular institutions and markets make poor candidates for explaining the origins of large-scale cooperation, since both institutions and markets require cooperation to exist in the first place.

see far more points of convergence than divergence in our research foci and goals. This will not promote global *kumbayah*, but perhaps it can promote a bit more *kumbayah* among researchers approaching big questions about religion, cooperation, and conflict from different disciplines.

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On the Dark Side of Religion and Other Forms of Impression Management

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Martin and Wiebe's piece (PAS henceforth) critiques a very curious constellation of problems its authors see facing "the experimental research agendas of many cognitive scientists of religion." These issues stem from an "enthusiastic and untrammeled preoccupation with prosociality." This "preoccupation" has led researchers to be all too generous in their characterization of religion and its constituents at the expense of attending to religion's uglier features. The authors contrast this view with that held by "those of us with an eye towards history" who "know that any simple congruence of religion and prosociality has never been the case" and that "no religion is characterized exclusively by either [good or bad] behavior alone." While Martin and Wiebe may, to their credit, have "an eye towards history," this piece does not make it very clear as to which direction their otherwise available eyes are pointed. As such, this piece may be a disservice to whatever points of merit it may have.

   PAS repeatedly targets Norenzayan and Shariff's review article (2008) on the basis that it "*seems* to have…motivated" the "recent interest in religious prosociality among cognitive scientists of religion" (emphasis mine). "Seems"? How widespread is this "recent interest"? Who are these "cognitivists"? Much of the cognitive science of religion focuses on relationships between mind, religious beliefs, and ritual without any obvious concern for or hint of romanticizing religion's social dimensions (e.g. Hornbeck and Barrett 2013; Kapogiannis *et al.* 2009; Legare and Souza 2012 Rossano 2009; Schjoedt *et al.* 2013; Wigger *et al.* 2013; Willard and Norenzayan 2013). Assuming that the authors take "cognitivists" to be those who engage in the cognitive *and* evolutionary sciences of religion, given the diversity of the fields and the major debates going on within them (Bulbulia *et al.* 2008; Frey 2011; Watts and Turner 2014; Voland and Schiefenhövel 2009), it remains even more difficult to take their impressionistic portrait as an approximation of something grounded in reality.

   While I make no claims to represent the field, in my reading (and production) of this work, I look for clues as to a) whether or not, b) how, and c) why people might be "nicer" (i.e. not jerks), for example, when primed with religious sym-

bolism, when engaged in religious behavior (i.e. proximate), or when dealing with social and ecological challenges to individual survival and reproduction (i.e., ultimate). Religion appears to be a well-designed tool to solve these problems. PAS does note that "the features of ingroup sociality constitute a repertoire of behaviors that function…as a social bond between members of each specific group." If the outcomes of these bonds turn out to be advantageous for individuals, then the case can be made that perhaps that suite of features could be considered adaptive or adaptations (biological and/or cultural). If religion functions to increase the strength of bonds, it would be at best an unfortunate lack of precision to claim that "religion makes people nice/nasty."

How, then, can you test the strength of religious bonds? You can measure how much people trust each other (e.g. Purzycki and Arakchaa 2013; Tan and Vogel 2008), how much people give, withhold, or steal in economic game experiments (e.g. Cohen *et al.* 2014; Soler 2012), how long institutions last (e.g. Sosis and Bressler 2003), the relationship between the strength of bond required to achieve something as awful as warfare and how costly a ritual is (including scarification, circumcision, etc.; Sosis *et al.* 2007). And this is just a handful of work conducted in the past decade. Despite the manifold conversations and spate of empirical research addressing human bonds *used for prosocial and anti-social ends alike*, Martin and Wiebe find that there is too much of an emphasis on religious *prosociality* among these nameless "cognitivists." Yes, we should not necessarily confuse "prosociality" with "treating other people equally" or "minimizing being a jerk." But in my reading of the cognitive and evolutionary research embracing the "prosociality hypothesis" (Galen 2012), this is a matter of style, not substance. The question is not about whether or not religion is best characterized as prosocial, but really about what explains *variation* in how people *use* these bonds (e.g. Atran 2003; Gibson 2011; Qirko 2009).

Nevertheless, how then are we to address the ugly side of religion (or those who study it)? For a start—as with any serious inquiry—we can abandon reliance upon haphazard intuitions, guesstimates, vague, high inference concepts, emotive language, and faulty reasoning.[6] Having commented on the allegedly

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6. For example, take the statement that "virtually all religions claim to know the truth which others lack." When we pause to consider the claim, things become a little less clear. If we set aside its anthropomorphic character and reformulate it as worthy of empirical attention, unless we find something particular about religion, the very idea that "virtually all" religious *people* claim to know the truth is about as informative as saying "virtually all" religious people breathe." The authors might therefore mean that "virtually all" religious *beliefs* are in some way inextricably anchored to the postulates "we are correct" or "others are wrong" above and beyond secular thought. Is this true? Is there something about religion that is inherently dogmatic? Direct ways to address such questions might be to: ask

obvious relationship between religion and grotesqueness elsewhere (Purzycki and Gibson 2011), I will avoid repeating myself. As PAS briefly examines, one strategy is to consider environmental variables such as the availability and distribution of resources, pathogen prevalence, degree of economic specialization, and so forth. PAS notes that this bond is "so strong as to form a barrier to those outside it, greatly minimizing contact with out-groups." It *can* do this, perhaps, but again, the question is really about *when*. Here, outside of setting up experiments that get people to do awful things, we can use scientific methods to examine the awful things that people have already done.

One recent study (Matthews *et al.* 2013; see too Storm and Wilson 2009) examines advocacy of violence among sixteenth-century Anabaptists. It found that there is more support for the prediction that local features of social and ecological context predicts advocacy of violence better than cultural transmission. In other words, religious rhetoric conforms to locally specific problems that can be addressed—using niceness or nastiness—collectively. Compare this to the Mongol Empire, which is notable for its religious pluralism. Chinggis Khan, of all people, evidently "exempted religious leaders and their property from taxation and from all types of public service" (Weatherford 2004, 69), he regularly employed people from different religious backgrounds, used religious freedom as a means to gain allies, and issued orders to maintain religious freedom for the conquered (Foltz 1999, 106). The subsequent leaders of the Empire regularly worked with and entertained debates between religious leaders as well as conveyed their endorsements of multiple traditions (see Foltz 1999; Polo 1958, 119). As utterly vicious as this empire was, religious pluralism made tactical sense in a context of maintaining wide-ranging trade networks where groups of various religious traditions from around the Old World maintained their respective traditions. These cases are not merely artifacts of historical processes or cultural transmission. Rather, they indicate how the content of rhetorical bond-strengthening shifts according to local conditions.

PAS then turns its attention to the hypothesis that "religious prosociality provides a basis for" "large-scale cooperation" or "large-scale social formations." Martin and Wiebe suggest that as a consequence of the prosociality hypothesis, researchers (cognitivists?) "venture…into the murky analogies and contested arguments for" cultural group selection. Rather than explain the reasons why, consider the many other works in the field that explicitly endorse alternative selection models (and compare it to the one they present), PAS offers no means

people if they think such thoughts, determine whether or not there is a propensity toward intellectual arrogance when primed with religious concepts, find passages in religious texts that count as "claiming to know the truth which others lack" and find the proportion of those passages to the size of the text to get an index of how salient such claims are.

for readers to assess this position or its relationship to "large-scale" sociality, thus entirely stacking the deck. Indeed, all we really have to go on are "murky," "contested," and "Pinker." These all might be true, but no reason is given to agree. There is also no case presented at all about the relationship between mischaracterizing religion as too prosocial and endorsing cultural group selection (or cognitivists). As such, the paper reads as a loosely connected list of grievances rather than a constructive critique that would strengthen the field.

The connections become stranger still when PAS manages to squeeze in the claim that ignoring religious nastiness "gives an appearance [to whom?] of influence by the agendas of" proreligious funding agencies. This "endangers the integrity of our collective scientific enterprise generally." In my view, the substantive issues lie not in how the scientific study of religion "appears," but what it does. There's little question that resources influence the way we think, talk, and act and some of us have been addressing precisely this in our attempts to understand why religious traditions take the forms they do. We're still coming to terms with how and why. Once again, the most convincing approach would be to systematically *test* whether or not there is a relationship between funding sources and the "appearance" or reality of influence (see Lyle and Smith 2012; Tyber *et al.* 2007 for examples of testing soft claims about evolutionary scientists' alleged political conservatism). Demonstrating that there is such a relationship between funding agencies and research would be a substantive and healthy reality-check for our "collective enterprise." Such a study would also remind us that we remain all too human insofar as we fashion our inquiries by virtue of the resources available. It is precisely here where the integrity of our "collective scientific enterprise" may be better maintained, namely, by actually engaging in it.

Acknowledgments

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Sound and Fury Signifying Nothing

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The mere mention of religious pro-sociality arouses strong passions in some. Given this, a constructive discussion obligates scholars to an especially high standard of circumspection and objectivity. Sadly, this article falls well short of that standard. It whines and preaches rather than enlightens.

While the authors are quick to criticize others' empirical work, their own case rests on little more than unsupported assertions, a quasi-historical anecdote, quotes from (not evidence from) Jared Diamond and Steven Pinker and accusations of money-induced bias on the part of researchers whose findings they don't like. While all these sins deserve reprimand, I'll concentrate on the unsupported assertions, since that's the only part of this article that the unsuspecting might confuse for real scholarship.

Without any attempt to cite or discuss empirically supportive evidence, we are told at various places in the paper that: "the hypothesis that religious prosociality provides a basis for large-group cooperation simply does not account for the diversity, heterogeneity and xenophobia of such human groups" (4); "religions have, from their social origins, been promoters of, perhaps the *primary* promoters of, what we refer to as assortative sociality." (4). "The ingroup cohesiveness of religious beliefs and behaviors clearly undermines openness, individualism, non-conformity, and risk tolerance while fostering collectivism and conformity, ethnocentrism and philopatry (reduced mobility outside one's natal group), intergroup vigilance, and xenophobia." (5). And on and on it goes, page after page of simply declaring as fact that which should be empirically defended and/ or demonstrated.

That readers should be skeptical of the authors' claims is born out when we take a little time to evaluate a couple of them. For example: On page 8 we are told that with the exception of a brave few (notably Scott Atran) researchers have neglected to study the "dark side" of religion. Really?

The authors have Galen's (2012) review paper in their reference list. Did they miss table 3 which summarized over a dozen priming studies showing nonsocial and antisocial effects of religion? Similarly, Preston *et al.*'s (2010) review con-

tains both a page-long section on religion's anti-social effects (7–8), and a table (Table 1) listing eleven studies with a range of findings from pro-social to anti-social. Paloutzian and Park's (eds.) *Handbook of the Psychology of Religion and Spirituality* contains chapters on religion, violence and terrorism (chapter 29) and fundamentalism and authoritarianism (chapter 21). Mahoney's (2010) review of studies on religion and family concludes that while religion often contributes importantly to domestic harmony, it can also exacerbate familial conflicts when household members adhere to discrepant religious beliefs and practices. And I could go on. One ought to at least peruse the literature before criticizing it.

Along with perusing it, one might also make the effort to understand it methodologically. Most studies showing pro-social effects of religion could have just as easily found anti-social effects; participants could have been less generous after a religious prime. But they weren't. So just because a study shows pro-social effects does not mean that it was "evading the dark side." It just happened not to find a "dark" result!

Another example: On page 1 the authors' tell us that religion is and always has been "chronically implicated" in "discord and violence." Two comments.

First: Beating the "religion equals violence and war" drum is popular in many quarters, but the only attempt I know of to empirically test this claim came to a rather surprising (and for some, a terribly disappointing) conclusion that less than 10% of wars in human history had a significant religious motive to them (google "War Audit"). A solid majority (60%) had absolutely no religious motive at all. It's only one study, but just a moment of sober reflection would suggest that the results are probably not crazy—after all, where was religion in: The Punic Wars, the Peloponnesian War, the Conquests of Alexander the Great, the Manchu Conquest of China, the Russo-Japanese War, the Franco-Prussian War, the Boer War, the Napoleonic Wars, the Korean War, etc. etc. Second: The Batak are a thoroughly peace-loving, cooperative traditional society where belief in supernatural punishment provides the very backbone of their non-violent ethic (See Endicott and Endicott 2014). For them, religion is chronically implicated in peace and harmony!

Enough cleaning up others' messes. How can we be more constructive on these issues? First, we need to be clearer about what "pro-social" means. Pro-social is still pro-social even if it is targeted exclusively at an in-group. Indeed, for as long as there have been social species, pro-social acts have almost exclusively been in-group pro-social acts. Given the way natural selection works, it simply could not have been otherwise. Those of us who ascribe to the naturalistic origins of religion find it totally unsurprising that religious pro-sociality is

largely in-group pro-sociality. Expectations of "global *kumbaya*" could arise only in the minds of those blissfully ignorant of evolutionary biology. That universalism of any sort is applied to religion is actually a complement to it, not a derogation. Other than the UN, is there any other human institution that would even be accused of harboring such wide-eyed idealism?

Two aspects of religious pro-sociality are rather surprising (1) its strength and endurance, and (2) that occasionally it extends beyond the in-group.

First on (1). On page 8 the authors question "whether *religious* prosociality is any more (or less) robust than any other basis for group belonging," Well, once again, a moment's reflection and at least a casual acquaintance with the empirical literature would indicate "yes." First, religion is a human universal (google: "Donald Brown human universals"). Largely secular societies are a very recent human phenomenon. The fact that, historically, you simply can't find a human society where religion was not integral to social life ought to at least peak one's curiosity.

Second, (the authors think we all need a history lesson, so here goes): The Moabites, Phoenicians, Hittites, Cynics, Pythagoreans, Goths, Vandals, Bourbons, Normans, Mensheviks, Bolsheviks, Whigs, Know-Nothings, Teetotalers etc., etc. have all come and gone—but the Jews are still with us! This suggests that at least some religions have found a formula for extra-ordinary staying power. In fact, I'd bet if you start listing the human groups that have sustained a continual presence across millennia they are at least disproportionally if not exclusively religious.

Third, empiricism confirms history. Rich Sosis' studies have documented how both religious communes and kibbutzim are more cohesive and enduring than their secular counterparts. Furthermore, church groups have been found to be more trusting and committed compared to secular groups such as bowling leagues or parent groups (See Rossano 2010, 163–164). Other studies have found that religious belief and practice are significant and often unique predictors of sociological factors such as: social group size and complexity (Roes and Raymond 2003), technological complexity (Peoples and Marlowe 2012); and proxy measures of commerce and cooperation (Johnson 2005); as well as personal factors such as: the number and quality of one's social relationships, the number of instrumental acts of social support one receives, and one's over-all life satisfaction (see Rossano 2010, 163–164). One need only be a scientist, not a religious apologist, to begin to wonder if this empirical data might not bear some remote relevance to Jewish longevity.

Based on this, a reasonable hypothesis is that religion emerged from the cauldron of cultural group selection. That is, different human groups with varying

intra-group cooperative norms competed with each other over the course of history and those with religious-based or religious-bolstered norms won. Religious intra-group cooperation emerging from inter-group competition provides an explanation (potentially testable) for both the in-group targeting of religious pro-sociality and the often close connection of religion and out-group antagonism.

Moreover (and now moving on to #2), if religion's origins trace back to inter-group competition, then in-group expansion would have been another important competitive tool. Any group in competition with other groups gains an advantage through numbers. Reliable and exclusively targeted religious pro-sociality can be understood as a potent expansionist tool (again another potentially testable hypothesis). Not only does this religious pro-social commitment promote high within-group fertility (Weeden and Kurzban 2013) but it can also serve as an attractive force to outsiders whose own groups have become disorganized and ineffectual. Indeed (another history lesson!), Stark's (*Rise of Christianity*, 1996) analysis of the expansion of early Christianity credits the aid and support that Christian communities offered one another during plagues and other disasters as being powerful incentives to pagan conversion. A point not lost on the reactionary pagan emperor Julian the Apostate (or Philosopher depending on one's tastes) who lamented that it was the Christians' "benevolence to strangers, their care for the graves of the dead and the pretended holiness of their lives that [did the] most to increase [their] atheism." He further complained that "no Jew ever has to beg, and the impious Galilaeans [Christians] support not only their own poor but ours as well, all men see that our people lack aid from us" (from letter 22, google "Julian the Apostate letters")

Successful expansionism, however, requires more than just attractive displays of group benefits. There must also be some willingness to accept out-group migrants. Survival could be jeopardized if the group becomes so ardently insular and unfailingly hostile to outsiders that potential wannabes are prevented from joining. This leads to another potentially testable hypothesis about religious pro-sociality—that it must have some mechanisms for allowing, and at times even encouraging, immigration. Most of the "world" religions that we see today contain universalist beliefs of one form or another (e.g. "we are all children of the One God"). Some evidence suggests that reminding people of this universalism increases out-group sympathy. Preston and Ritter (2013) found that "God" primes differentially increased people's cooperation toward out-group members, whereas "religion" primes increased cooperation toward in-group members. Yes, religious expansion has sometimes involved violent imposition. But this strategy has costs. It may be that the most effective expansion utilizes a more cost-efficient combination of attractive and credible displays of group

solidarity, promotion of high fertility, pathways to immigration, and effective deterrence against opposition and apostasy – another potentially testable idea.

The cognitive science of religion is not about bashing or promoting religion —it's about understanding it. This requires calm, clear-headed thinking. Adolescent temper tantrums should be left to the popular media.

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Selective reading and selectionist thinking: Why violence has been, and should be, important to the cognitive science of religion

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We agree with Martin and Wiebe that CSR researchers would benefit from the insights of ethnographers and historians and we commend them for drawing attention to both the prosocial and violent aspects of religion, as we think both are crucial for understanding religion's role in human sociality. Here we stress this point by drawing attention to the socioecological conditions under which we expect violence associated with religion to occur between, as well as within, groups.

We begin, however, by noting that Martin and Wiebe's reading of the CSR literature is selective; despite their protestations, violence has been a topic of considerable interest to CSR researchers for some time. In fact, major contributors to CSR including Scott Atran, Dominic Johnson, and Harvey Whitehouse have all written books and numerous articles focusing on the violent side of religion (e.g. Atran 2003, 2010; Johnson 2008; Johnson and Reeve 2013; Johnson and Toft 2014; Whitehouse 1995, 1996; Whitehouse and McGuinn 2013). The second author of this commentary has also written various pieces that aim to explain religious violence (e.g. Alcorta and Sosis 2013; Sosis 2011; Sosis and Alcorta 2008; Sosis *et al.* 2007; Sosis *et al.* 2012). And most notably, Norenzayan, who takes the brunt of Martin and Wiebe's criticism concerning CSR's alleged prosociality bias, has published several important papers on religious violence (e.g. Hansen and Norenzayan 2006; Ginges *et al.* 2009). All of this literature was curiously ignored in the target article. In light of Martin and Wiebe's concern that Templeton is leading CSR's supposed "*Kumbayah*" festivities, we should also point out that Templeton has funded all of these researchers. Ultimately, to assess Martin and Wiebe's contention, we recommend a systematic meta-analysis to determine whether a prosociality bias genuinely exists in the

CSR literature. Argumentation without the support of carefully collected data is subject to a whole host of pitfalls, not the least of which is the tendency to find support for one's ideas due to confirmation biases (Nickerson 1998).

For the sake of this discussion, however, we entertain the possibility that there is a disproportionate focus on prosociality by CSR scholars and consider the source of such a bias. Rather than machinations of Templeton, we suspect the substance of Martin and Wiebe's alleged prosociality bias may be the result of CSR's recent encounter with selectionist thinking (Bulbulia *et al.* 2008; Sosis 2009). Evolutionary scientists recognize that we live in a world of finite resources and consequently all organisms compete over those resources (e.g. energy and mates) or the means to them (e.g. territories). Conflict and competition between individuals—from mothers and their offspring, to members of opposing warring parties—are therefore inherent to all possible dyadic interactions. Conversely, cooperation in such a world is unanticipated and hence its presence and persistence are puzzling. Thus, for those within CSR who engage in adaptationist investigation, an interest in the prosocial aspects of religion derives from the fact that high levels of prosociality among non-kin are rare across species (although not absent, West *et al.* 2012), yet clearly evident among humans.

When religions are understood to confer benefits and costs to individuals within specific socio-environmental contexts, explanations for both the prosocial and conflictual aspects of religion are drawn into sharp focus. Understanding how conflicts of interest among individuals are resolved or minimized is essential to any explanation of religious prosociality. Various theorists have suggested that resource benefits available to the members of religious groups can be protected from freeriders when individuals pay costs for group membership (Bulbulia 2004; Iannacconne 1992; Irons 2001; Sosis 2003). These costs vary ecologically and are expected to increase as a function of the quality of collective resources they are protecting, and the risks of exploiting these resources via freeriding. As countless ethnographers have documented, these membership costs, typically in the form of initiation rites, often entail substantial violence inflicted by other ingroup members (e.g. Alcorta 2006; Tuzin 1982; Whitehouse 1996).

This approach, commonly referred to as the costly signaling theory (CST) of ritual, may at first glance seem to disproportionately focus on the prosocial benefits of religions. However, as many have noted, cooperation is often an effective means of competition (e.g. Alexander 1987). And indeed, much research that has applied signaling theory to religion has focused specifically on how the prosocial consequences of religious signaling facilitate intergroup violence and warfare (Ginges *et al.* 2009; Matthews *et al.* 2013; Johnson and Reeve 2013). For example, in environments with high levels of intergroup warfare, where

cooperation in defense and raiding is critical, rituals are the most violent and extreme (Sosis *et al.* 2007). High levels of ingroup prosociality, it appears, can be driven by socioecological variance in the frequency of outgroup violence. Rather than viewing religion as a cause of warfare, these findings suggest that warfare may motivate an increase in the intensity of religious commitments, including violent rituals and initiation rites.

Signaling theory emphasizes that the costs and benefits of religious displays are not equal for all members of societies. Notably, the signaling approach focuses attention on the role of environmental contexts and variables, such as economic and political stratification, in shaping the costs and benefits of ritual behavior. Quantitative ethnographic studies have shown that those at the top of social hierarchies benefit at the expense of those at the bottom in various ritual venues (Shaver 2014; Shaver and Sosis 2014). Although payoffs vary according to socioecological context, in general, there are at least four ways in which pay-offs may be influenced by variance in social stratification. First, if all individuals invest in ritual behavior to the same extent, high status individuals may receive more benefits. Second, if all members receive similar benefits, high status individuals may pay fewer costs. Third, when high status individuals manipulate ritual systems they can exclude low status individuals from participating altogether. Finally, high status individuals can manipulate religious systems so as to decrease the incentives for participation by low status individuals. These differential payoffs to ritual behavior can serve to justify and perpetuate inequalities in power and access to resources, and serve as a source of violence against ingroup members.

While there is considerable evidence of religious proscriptions contributing to violence against ingroup members, we are skeptical that religions are the *cause* of violence between groups (Purzycki and Gibson 2011). Intergroup conflict is primarily, although not exclusively, the result of resource competition (Johnson and Toft 2014). When intergroup conflicts involve religious sensibilities, religion's primary role is to motivate ingroup members to engage in outgroup violence. Religions are particularly effective in this regard as they impose a moral framework on believers which allows leaders to reframe political or economic struggles in religious terms (Sosis and Alcorta 2008). Leaders are thus able to motivate others to sacrifice themselves for a religious cause that appears divorced from material self-interest. Moreover, when benefits are cast in terms of eternal rewards, religions can alter cost-benefit calculations to help justify violence against outgroups (Sosis *et al.* 2012).

We conclude with one final point of clarification. As noted above, political and economic stratification are important for shaping the payoffs to religious behav-

ior, and these differential payoffs can explain violence that is disproportionately perpetuated against some ingroup members. We therefore strongly agree with Martin and Wiebe that political institutions and economic factors are important for understanding violence. However, separating the economic and political features of societies from other contextual factors stems from a confusion about selective processes. Evolutionary models, such as the signaling and life history models discussed above, assume that environments—that is, everything external to organisms—determine how genotypes become manifest as behavioral phenotypes, and the phenotypic variants that will be favored by selection (e.g. Sosis and Bulbulia 2011). In other words, while we are well aware that political and economic models of religious violence do not require evolutionary underpinnings to offer powerful predictions, or even coherence, political and economic determinants of religious behavior certainly are not mutually exclusive to evolutionary explanations; indeed, they are critical to them.

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Religion, prosociality, assortative sociality, and the evolution of large-scale cooperation: A few remarks on Martin and Wiebe

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In their article, Luther H. Martin and Donald Wiebe put forward three types of claims. The first type concerns current research agendas in the Cognitive Science of Religion. In this regard, they claim both that these agendas neglect religion's connection with violence by emphasizing its presumed relation with prosociality and that this neglect involves a hidden bias. The second type concerns the social function of religion. Here, they claim that religion is fundamentally tied to assortative sociality rather than prosociality. The third type concerns a possible causal link between religious prosociality and the evolution of large-scale cooperation. Here, they claim that religious prosociality did not play any pertinent causal role in such evolution. We do not address the first issue in our reply, though we would like to say that Martin and Wiebe have a partial reading of the CSR literature in this respect, which undermines the strength of their related claims.[7] We address the other two issues in turn, but, before discussing their related claims, we characterize some basic distinctions that will frame our subsequent remarks.

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7. For additional discussions of religious violence, see, for example, Blogowska, Lambert and Saroglou 2013; Blogowska and Saroglou 2013; Ginges, Hansen and Norenzayan 2009; Haidt 2012; Johnson, Rowatt and LaBouff 2010; Preston, Ritter and Hernandez 2010; Teehan 2010. We do not mention the various publications of Atran and collaborators because Martin and Wiebe acknowledge Atran's contribution to the topic. Nonetheless, given the scope and importance of Atran and collaborators' contribution, we think that it has not been duly considered by Martin and Wiebe.

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Basic distinctions

We understand prosocial behavior as behavior that follows social norms. Some social norms prescribe behaviors that indicate group affiliation without constituting any helping behavior at all (e.g. the prescription that one should use the garments specific to a social group). We shall call the related prosociality "group-marker prosociality." Some social norms prescribe behavior that constitutes helping behavior that benefits not only the recipient, but also, immediately or in the long run, the benefactor (e.g. the prescription that one should help a friend who is in need). We shall call the related prosociality "mutualistic prosociality." Some social norms prescribe helping behavior that benefits the recipient but not the benefactor, namely, the benefactor incurs a cost without receiving, immediately or in the long run, any proportional benefit (e.g. the prescription that, in a situation of war, one should sacrifice oneself for the sake of the group). We shall call the related prosociality "altruistic prosociality."[8] Mutualistic and altruistic prosociality constitute two distinct types of cooperation (see Baumard, André and Sperber, 2013). Group-marker prosociality only indirectly promotes cooperation by (sometimes costly) signaling the presence of trustworthy potential cooperators.

We characterize religion in terms of beliefs in supernatural agents and religious prosociality as behavior that follows social norms by being at least partially motivated by the belief that a supernatural agent sanctions the norm (and often by a corresponding fear of supernatural punishment in case of norm violation). Thus, religious group-marker prosociality is motivated by the belief that a supernatural agent sanctions norms prescribing behavior that simply indicates affiliation to a social group, religious or otherwise; and religious mutualistic or altruistic prosociality is motivated by the belief that a supernatural agent sanctions norms prescribing mutualistic or altruistic behavior.

Norms prescribing helping behavior have different types of scope in that they may concern different types of recipients—individuals that one is familiar with and/or strangers; individuals that are part of one's social group and/or individuals that are part of an outgroup. Accordingly, we shall say that religious prosociality, mutualistic or altruistic, may be more or less inclusive in that it may concern only ingroup-familiars, or, in addition, ingroup-strangers, or, in addition, ingroup-strangers and outgroup-familiars, or, in addition, ingroup-strangers,

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8. We should add that (i) we understand costs and benefits in terms of individual fitness (not in terms of inclusive fitness); (ii) our characterization of helping behavior is independent of the psychological motivation involved (e.g. altruistic behavior may be motivated by psychological egoism—for a discussion of psychological altruism/egoism, see Stich, Doris and Roedder, 2010); (iii) even if it is often difficult to determine whether an instance of helping behavior is mutualistic or altruistic, we deem the distinction between mutualism and altruism an important one.

outgroup-familiars and outgroup-strangers (in the latter case, the prosociality has a universalist scope).

Religion, prosociality and assortative sociality

Martin and Wiebe's claim that religion in general is fundamentally tied to assortative sociality rather than prosociality is unclear, and we couldn't find an interpretation that would make it relevant or plausible.

At times Martin and Wiebe seem to deny that religion is fundamentally tied to prosociality by focusing primarily on a notion of prosociality that is altruistic and universalistic, which is indicated by their reference to Norenzayan and Shariff's characterization of prosociality ("religions facilitate costly behaviors that benefit other people at a personal cost") and by their supposition that religious prosociality would predict "a kind of global *kumbayah*." This focus turns their critical argument into a straw man argument. Although certain religious traditions may foster more universalist attitudes, the anthropological literature clearly shows that this is not a feature of religion in general, even less so in terms of a prescription of altruistic prosociality. In other words, who in the CSR would argue that religion in general promotes such universalistic and altruistic prosociality?[9]

Occasionally, Martin and Wiebe seem to deny that religion is fundamentally tied to prosociality by focusing primarily on the role of group-marker prosociality, and by arguing that the relation between religion and group-marker prosociality is psychologically contingent:

> ingroup prosociality can be accounted for by general psychological mechanism and strategies…as well as by any number of ingroup markers in addition to the religious, such as "race, nationality, computer use, or…[an] operating unit at work" (M and W, 2)

> We question, however, whether *religious* prosociality is any more (or less) robust than any other basis for group belonging, including arbitrarily assigned affiliations, as documented by the classic robbers cave experiment. (M and W, 6)

Here again, we don't see the relevance of Martin and Wiebe's claim qua a broad critical claim. Most researchers in the CSR would agree with the stance that there is no necessary psychological connection between group-marker prosociality, or prosociality more generally, and religion (i.e., that the psychological mechanisms promoting prosociality are independent of religion), including, ironically, the principal authors they are discussing and criticizing in their article (i.e., Norenzayan and Shariff).

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9. Even if one interprets Norenzayan and Shariff's characterization in terms of mutualism (their characterization is vague in this respect) and take Martin and Wiebe's claim to regard mutualistic and universalistic prosociality, this wouldn't affect substantially our criticism here.

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   Martin and Wiebe's claim that religion is fundamentally tied to assortative sociality (more specifically, that religion is a biological adaptation whose function is to discriminate outgroup members so that to avoid pathogen contamination) faces a dilemma. Either they understand religion as independent of the occurrence of beliefs in supernatural agents, or, as in our characterization, they understand it as constituted by such occurrence (Martin and Wiebe do not provide any characterization of what they mean by "religion"). In the former case, their claim becomes totally unclear. What exactly is specific to religion that promotes an assortative disposition? In the latter case, their claim becomes quite implausible. Given the wide variety of representations of supernatural agents involved in religious beliefs, many of them completely unrelated to the sanctioning of outgroup discrimination, it is much more parsimonious to claim that the adaptations related to assortative sociality are independent of religion. In other words, in the same way that prosocial dispositions are only contingently related to religion, assortative dispositions are only contingently related to it (for a relevant discussion, see Tybur, Lieberman, Kurzban and DeScioli, 2013). In sum, in either interpretation of their understanding of religion, their claim is not tenable.

Religious prosociality and the evolution of large-scale cooperation

Martin and Wiebe's claim that religious prosociality did not play any pertinent causal role in the evolution of large-scale cooperation is plausible, though their arguments in support of this thesis are not convincing (for more poignant arguments, see Baumard and Boyer, 2013; Boyer and Baumard 2014).

   Sometimes Martin and Wiebe seem to defend their standpoint by supposing that the large-scale cooperation at stake is cooperation with outgroup members and by arguing that religion cannot sustain such cooperation, for, given its assortative nature, religion has discriminatory effects, instead of cooperative effects, in relation to outgroup members. However, under this interpretation, their argument constitutes a straw man argument because the evolutionary hypothesis advocated by Norenzayan and Shariff is primarily about the growth of social groups and cooperation within groups—i.e., about cooperation between ingroup-strangers rather than cooperation with outgroup members. Moreover, their hypothesis is a cultural-group selection one in which the growth of cohesive social groups based on Big-Gods religious prosociality outcompetes other groups by violently eliminating or absorbing them. In other words, Norenzayan and Shariff's hypothesis in fact implies that religious prosociality (or at least Big-Gods religious prosociality) has a complementary dark side concerning out-

group members (see Norenzayan and Shariff, 2008; Norenzayan, 2013).[10]

Occasionally, inspired by Dunbar, among others, Martin and Wiebe seem to defend their claim by arguing that human prosocial dispositions are effective *only* in the context of small-scale ingroup dynamics (i.e., in the context of inter-action between ingroup-familiars) and that, in the context of large-scale ingroup dynamics (i.e., in the context of interaction between ingroup-strangers), some kind of centralized and permanent political leadership is necessary to neutral-ize divisive assortative tendencies, including the assortative tendencies inherent to religion, and stabilize cooperation. In other words, according to them, while religion can enhance cooperative effects in the context of small-scale ingroup dynamics (i.e., religious prosociality might exist in such context), it can only exaggerate social divisions in the context of large-scale ingroup dynamics (i.e., religious prosociality cannot exist in such context). Thus, religion is rather a divisive force that should be controlled for the evolution of cooperation between ingroup-strangers to become stable.

We are skeptical about Martin and Wiebe's argument against the existence of religious prosociality in large-scale ingroup dynamics, namely, their argument denying that religion could enhance cooperation between ingroup-strangers.[11] First, there is a good amount of experimental evidence indicating that religion can support cooperation between ingroup-strangers (e.g. Malhotra, 2010; Ran-dolph-Seng and Nielsen, 2007; Shariff and Norenzayan, 2007). With regard to this point, Martin and Wiebe argue:

> Since, the evidence for religious prosociality cited by Norenzayan and Shariff and others only supports small-scale ingroup dynamics, their proposed hypothe-sis of extending these dynamics to the facilitation of large-scale group formation seems to be by inferential fiat with dubious empirical support. (M and W, p. 3)

However, given that much of this evidence concerns cooperation with anony-mous others, who, in the context of the studies, are probably understood as ingroup-strangers, we wouldn't say that an arbitrary inference from small-scale ingroup dynamics (i.e., cooperation between ingroup-familiars) to large-scale ingroup-cooperation (i.e., cooperation between ingroup-strangers) is involved here.

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10. We are puzzled by a passage in Martin and Wiebe's paper that apparently contrasts Atran and Henrich's cultural-group selection view (see Atran and Henrich, 2010) with Noren-zayan and Shariff's view, as if Martin and Wiebe were attributing to the latter a biologi-cal-group selection perspective (see M and W, 3). In fact, Norenzayan and Shariff adopt a cultural-group selection position quite similar to Atran and Henrich's position on the matter.

11. Our counter-argument here should not be seen as a defense of the hypothesis that religious prosociality played a fundamental causal role in the evolution of large-scale cooperation. As we indicated above, we are sympathetic to Martin and Wiebe's claim, but for other reasons.

Second, historical evidence related to phenomena such as the Crusades, for example, suggests that religion can enhance cooperation between ingroup-strangers. Martin and Wiebe claim that in such situations religion is utilized only as a symbolic device supervenient on political forces. However, what could be the reason for utilizing such symbolism in the first place, if not its possible effects in enhancing cooperation between ingroup-strangers?[12]

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12. Martin and Wiebe invoke two other arguments against the hypothesis that Big-Gods religions played a fundamental role in the evolution of large-scale cooperation. First, in a footnote, they claim that Big-Gods religions are neither necessary nor sufficient for large-scale cooperation. However, Norenzayan and Shariff's hypothesis is a probabilistic one about causal contribution, therefore this argument does not hold. Second, in the same footnote and other parts of the paper, they claim that their Hebrew-epic example counts against this hypothesis. However, besides the fact that the hypothesis at stake is a probabilistic one, their analysis of this example is too simplistic to be taken too seriously.

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Religious Prosociality, Experimental and Historical Conundrums: Continuing the Conversation

Luther H. Martin and Donald Wiebe

We thank the editors of JCSR for inviting our paper as a target article. Our paper wasn't written for a target article but as a short presentation at the annual meeting of the IACSR in Berlin 2011. Consequently, we acknowledge some of the shortcomings identified by the commentators, the need for more detailed argument, for example, or of more comprehensive citation of relevant experimental literature. We are not, however, experimentalists; we are respectively a historian of religion and a philosopher. However, since it is generally acknowledged that the interdisciplinary field of CSR was founded precisely by a historian of religion and a philosopher (Lawson-McCauley 1990), we venture to presume that our perspective might have something to contribute to this still developing but, alas, narrowing field of study.

The thesis of our presentation is that assortative sociality seems to be as well-established in the evolved repertoire of *Homo sapien* behaviors as is prosociality and that the relationship of assortative sociality to religion seems to be at least as significant as that of prosociality to religion. Yet, we have noted a disproportionate emphasis in current CSR experimental research on the latter. For those who find no "relevant or plausible" interpretation "that religion is fundamentally tied to assortative sociality" (e.g. Sousa and Prochownik, 44, 45), we have cited in support of just that interpretation the research of Corey Fincher and Randy Thornhill (2012), Mark Schaller (2006), and Schaller and Damian Murray (2007, 2010) (henceforth Fincher *et al.*). We might note at the outset that our comment about unfettered research on prosociality leading to a global *kumbayah* is an ironic remark and not a scientific prediction (as it is taken to be by Lund *et al*, Rossano, and Sousa and Prochownik). We agree with Sousa-Prochownik that had we meant it as such, our claims would be a "straw man argument," indeed (44, 45). And, our reference to violence is not a hypothesis that religion is *necessarily* associated with violence (which Purzycki, 27; Rusanno, 32; Shaver-Sosis, 39; and Sousa-Prochownik, 42ff, seem to assume of our argument) but an observation about recurrent historical correlations of religion with assortative sociality. Consequently, we did not simply ignore or were unaware of the literature with regard to religion and violence (as claimed by Purzycki, Shaver and Sosis, and Sousa-Prochownik). Rather the relationship of religious assortative sociality, as of religious prosociality, to violence is a sepa-

rate question. Our focus, rather, was on the general neglect of the relationship between religion and assortative sociality in current CSR research, which, we argue, has privileged religious prosociality. Consequently, we find the charge of "selective reading" (Shaver and Sosis, Lund *et al.*) to be curious, since it is the neglect of, or unfamiliarity with, relevant literature on religion and assortative sociality by current CSR researchers that is precisely our complaint.

Benjamin Purzycki seems to think that our major concern is that CSR researchers fail to see religion's "ugly" features and that we see CSR research as simply interested in romanticizing religion (25). What Purzycki doesn't do, however, is to recognize the fundamental problem of the assortative sociality produced by the dynamics of ethnocentric religious belief and practice (see Fincher *et al.* cited above and Wiebe's subsequent argument 2013).

According to Purzycki, we make our case by relying on "haphazard intuitions, guesstimates, vague, high inference concepts, emotive language, and faulty reasoning" (26). Although he eventually acknowledges that one of our concerns is with claims by many cognitive science researchers that large-scale cooperation rests on, or is generated by, religious prosociality (27), he asserts that, even here, we "stack the deck" in our favor against that thesis and that our paper "reads as a loosely connected list of grievances rather than as a constructive critique that would strengthen the field" (28). Overall, therefore, our essay/argument is, in his judgement, nothing more than an "impressionistic portrait" that cannot even be taken as "an approximation of something grounded in reality" (25), and is a "disservice" to the field of CSR (25).

Although Purzycki makes some good suggestions for future research, none of which, to our knowledge, is being pursued, he rejects our suggestion that current research on religious prosociality may reflect the bias of pro-religious funding agencies by defiantly acknowledging support for his own research by such an agency. While acknowledging that there's "little question that resources influence the way we think, talk, and act," he asserts that "substantive issues lie not in how the scientific study of religion 'appears,' but what it does" (28). Of course, how CSR researchers "think, talk, and act" and what they are doing with respect to their research on religious prosociality is precisely the point of our paper. He concludes that "the most convincing approach" to resolving this issue, "would be to systematically *test* whether or not there is a relationship between funding sources and the 'appearance' or reality of influence (emphasis original)." We referenced such an assessment by Wiebe (2009).

Matt Rossano, like Purzycki, also relies on *ad hominem* remarks rather than critical argument. And instead of dealing with substantive issues raised in our paper, he offers "I bet" (33) and "it may be" (34) suppositions—hardly the

"empirically supportive evidence" he accuses us of neglecting (31). Given his lack of substantive criticism and professional civility, we will proceed to a consideration of the constructive assessments raised by the other contributors to this review.

Erik Lund, Maxine Najle, Ben Ng, and Will Gervais rightly note that we see CSR researchers as being preoccupied with the prosocial character of religion. They correctly note, moreover, that we think these researchers fail to see that the ingroup cooperative character of religious prosociality constitutes a protective boundary against others and therefore cannot explain the emergence of the large-scale cooperation among strangers that made possible the growth of cities, states, and civilizations as has been hypothesized by many in the cognitive science of religion. Finally, they also point out that we think this preoccupation with religion as a prosocial phenomenon ignores historical and political realities that provide more persuasive accounts for these large-scale cooperative developments. Consequently, we agree with them that "[n]either religions, nor any other factor proposed as a mechanism facilitating the rapid scaling up of human cooperation in some groups of the past twelve millennia promote universal helping behavior, nor should they be expected to" (20).

Although much of their account of religion as fostering ingroup cooperation agrees with our analysis—including, it appears, our claims that ingroup prosocial behavior produces outgroups that block large-scale cooperative interaction with strangers (Lund *et al.* cite Preston and Ritter 2013) – they nevertheless suggest that such large-scale cooperation emerged in the Holocene, paralleled, as they put it, by the emergence of religions with big moralizing gods (20). They base that claim on Norenzayan's recent book *Big Gods: How Religion Transformed Cooperation and Conflict* (2013) (20). However, there is just no archaeological or historical evidence for the emergence of big moralizing gods or of institutionalized religions in this period of *Homo sapiens'* history (Wiebe, *forthcoming*; Martin *forthcoming*), and when such gods do emerge, Lund *et al.*'s observation that they paralleled the development of large-scale cooperation begs the proverbial chicken and egg question of causation. As they correctly note, "secular institutions and markets make poor candidates for explaining the origins of large-scale cooperation, since both institutions and markets require cooperation to exist in the first place" (22, n. 5). But doesn't this same principle of some kind of prior cooperative comportment apply for the origin of religious institutions as well?

By attempting to correct what they see as our mischaracterization of the situation, Lund *et al.* maintain that "[i]t's the conflict that drives the large-scale cooperation" (21). But this, as they admit, is to "reframe" the problem under

discussion. That "reframing" of the issue, however, amounts to a rejection of the original hypothesis put forward by Norenzayan and Shariff, which we cited, and taken up by others. As Lund *et al.* put it: "The central premise of religious prosociality is *not* that religious people indiscriminately cooperate with others. Rather, it is that some religions may include features that were conducive to promoting *ingroup cooperation* and as a result *outcompeted* rival religions over the course of cultural evolutionary history" (20, emphasis in the original). Thus, they conclude that "cultures that were able to use religion to bind the group together would have gained a significant advantage over the less cohesive groups" (22). With that speculative scenario, we have, of course, either the violent destruction of one religious group by another or the subordination and incorporation of one religious group within another, not the expansion of ingroup cooperation to include the stranger. Rather "expansion" is achieved through political power or by military force and not by cooperation. This alternative hypothesis does not, then, amount to a correction of our thesis, but rather supports it.

Shaver and Sosis at least entertain the possibility that there might be a disproportionate emphasis on religious prosociality in CSR research (37) and so take our concern seriously. But they also point out that interest in this topic should not come as a surprise given that cooperation of any sort is an evolutionary puzzle (37, so also Lund et al., 20). Although we agree with this observation, we are not altogether sure why Shaver and Sosis then proceed to spend the bulk of their response on the issues of conflict between individuals, cooperation as an effective means to competition, warfare as a possible motivator of an increase in religious commitment, and the like (37–38). We are gratified that they agree with us "that political institutions and economic factors are important for understanding violence" (39), but our claim was that such factors are important for understanding the emergence of the kind of interaction among strangers we find in the formation of large-scale societies. Furthermore, we do not disagree with their claim that "political and economic determinants of religious behavior certainly are not mutually exclusive to evolutionary explanations" (39), but we do not see this agreement as justifying their claims about religious prosociality being the foundation of the formation of large-scale societies. And, while we agree with Shaver's and Sosis's skepticism about religion being "the *cause* of violence" (38, emphasis original), we nevertheless remain skeptical about religions being the *cause* of scaled-up prosociality.

Like Purzycki, Shaver and Sosis are sensitive to our suggestion that a prosocial bias in contemporary CSR research may result from pro-religious funding agencies. Shaver and Sosis suggest that the source of this presumed bias may, rather, be the result of CSR's recent encounter with selectionist thinking. Ironi-

cally, however, evolutionary biologist Jerry Coyne has suggested that selection-ist theories of religion are just as attractive to pro-religious funding agencies as is research on religious prosociality (Coyne 2012).

Paulo Sousa and Karolina Prochownik focus their critical attention on our claims that religion is fundamentally tied to assortative sociality and that religious prosociality did not play a significant role in the emergence of large-scale cooperative behavior (42). Differentiating group-marker prosociality from mutualistic and altruistic prosociality (43), they maintain that religious prosociality can be more or less inclusive (43). The diversity of religious prosocialities, they maintain, may range in inclusiveness from ingroup-familiars to what they call ingroup-strangers, outgroup-familiars, and outgroup-strangers. Were this the case, we could acknowledge that religious prosociality could have funded, so to speak, the development of large-scale human cooperative behavior. However, their alternative proposal is itself subject to serious question. How do Sousa and Prochownik differentiate ingroup-strangers from outgroup-familiars? And what are members of outgroups if not strangers to ingroups, and members of ingroups if not familiars? This taxonomy, it seems to us, is not so much robust as overly refined.

Sousa's and Prochownik's claim that we argue that religion is of no relevance to large-scale cooperative behavior is also misdirected. We argue precisely that religion can, as they put it, be utilized "as a symbolic device supervenient on political [and economic] forces" (47). This would especially make sense should their claim be true that religious prosociality grounds large-scale cooperation on the basis of cultural-group selection, by which one big-god group out competes other big-god groups "by violently eliminating them or absorbing them" (45).

As we have noted above, we do not believe that religious prosociality is the basis for the emergence of *large-scale* human cooperation. Contrary to Sousa's and Prochownik's claim, moreover, we did not, and do not, deny that religion may play a positive role in large-scale societies; we only deny the claims that religious prosociality is the *cause* for the emergence of large-scale societies. Rather, we maintain that the primary *causes* of an expanded cooperative interaction in human populations– what Sousa and Prochonik call "large-scale ingroup dynamics"—are economic factors, political power, and military force. Furthermore, we also believe that the empirical/experimental, ethnographic and historical evidence shows that religious prosocial dispositions are far more effective in circumscribing small-scale groups and that such groups may indeed need to be in some way accommodated if they are not to become a divisive force in modern society.

We are in complete agreement with Sousa's and Prochownik's definition of religion "in terms of beliefs in supernatural agents" (as a necessary if not suf-

ficient feature) and "religious prosociality as behavior that follows social norms as being at least partially motivated by the belief that a supernatural agent sanctions the norm" (43). In Fincher *et al.*'s hypotheses, it is precisely the "wide variety of representations of supernatural agents involved in religious beliefs," that initially articulate group boundaries in order "to discriminate outgroup members."

We are pleased that Sousa and Prochownik are sympathetic to our claims and do not wish to be seen as defending "the hypothesis that religious prosociality played a fundamental causal role in the evolution of large-scale cooperation" (46, note 11), even if they offer alternative evidence for those claims. We look forward to elaborations of their "other reasons," particularly so in that this claim sets them foursquare against the claims by other CSR researchers that religious prosociality is the reason/cause for the emergence of large-scale human cooperation. Finally, we agree that "there is no necessary psychological connection between…prosociality…and religion (i.e., that the psychological mechanisms promoting prosociality are independent of religion)." Given this, our simple question then is, why then does so much contemporary CSR research focus on religious prosociality?

Unlike the other commentators, Luke Galen's assessment of our position accepts not only the conclusions we reached in our analysis of religious prosociality, but he also finds our argument sound. He cites the research of Michael Welch *et al.* showing that outgroup-inclusive trust is not associated with religiosity and sees the claim that religions extend cooperation towards strangers and outgroup members as being motivated by an assumption that parochial altruism is a form of general prosociality (13). He acknowledges that belief in watchful big gods is more prevalent in larger societies but points out that the data does not show that this is associated with reduced internal conflicts. As for big gods being important monitors of human behaviour that motivates cooperation, he shows that "[s]upernatural content is not a necessary component but [only] one of a variety of general social monitoring intuitions" (15; see now Harrell 2012). According to Galen, then, outgroup-inclusive trust is not associated with religiosity, and there is no clear evidence that big gods play a causal role in this so-called moral ascent from ingroup sociality to outgroup-inclusive cooperation.

Galen points out that our argument about religions functioning as hazard protection systems is "consistent with [Jesse Graham's and Jonathan] Haidt's purity domain" as a foundation for social binding (14, Graham and Haidt 2010). And he agrees with Robert Wright that "religious concepts reflect, rather than causally initiate, societal changes, such that cultural shifts have led to evolutions in religious emphases," i.e., that "the drift of the divine can be understood only by

appreciating divinity's subservience to the facts on the ground" (Wright 2009, 77), a historical probability we sought to illustrate, though not establish, with our "quasi-historical anecdote" (Rossano, 31).

In conclusion, we should like briefly to comment on those who wish to "give us a history lesson" (Rosanno, 33, 34). These lessons are, however, notably wanting. Rosanno's lesson, for example notes that "less than 10% of wars in human history had a significant religious motive to them…A solid majority (60%) had absolutely no religious motive at all" (32, "historical" data which he documents from his "research" on Google). Given these figures, however, Rosanno's "research" would seem rather to support than challenge our argument that historical change, much less the formation of large-scale societies, is little motivated by religion, whether pro- or assortative. Further, he asserts that a number of civilizations have "all come and gone—but the Jews are still with us" (33). He concludes from this egregious generalization that "some religions have found a formula for extra-ordinary staying power," but he does not reveal what that formula is. In fact, most of the historical groups he lists as having not survived, of course, had their own religious commitments (even the "Teetotalers"!) (33). Does he mean to imply that the Moabites, who were presumable conquered by and/or assimilated into Israel, didn't survive because of a deficient religiosity? Or might it have been because of their military inferiority? And, Rosanno emphasizes the Emperor Julians' admiration of Christian piety as documenting "an attractive force to outsiders" (this time citing Rodney Stark rather than Google). However, the good Emperor's admiration of Christian attractiveness didn't persuade him to convert, nor did it dissuade him from his anti-Christian agenda of neo-pagan revival. Rather, Julian's comments reflected a political desire to reform and strengthen Rome's own social network. Equally curious is Lund *et al*'s understanding of the New Testament dictum to "Love thy neighbor as thyself" (Mt 19:19, 22: 39; Mk. 12:33; Rom. 13:9) as referring "only to other Israelites" (20).

Purzycki's history lesson, citing Matthews *et al.* on sixteenth-century Anabaptists, concludes that "local features of social and ecological context predict advocacy of violence better than cultural transmission." In other words, "religious rhetoric conforms to locally specific problems that can be addressed… collectively" (27). Although our thesis is about assortative sociality, not violence, we agree that it is social and ecological factors that better predict religious behaviors. However, Purzycki's history lesson from the example of the Mongol Empire is puzzling. Although Purzycki emphasizes Ghengis Khan's religious accommodations as supportive of his imperial successes, the Mongol Empire was nevertheless established by unusually violent military conquests, in which

he put to death any who opposed him, including Teb Tengeri, a powerful and influential shaman who represented Ghengis's own religious proclivities and whose counsel had heretofore guided his imperial ambitions (Weatherford 2005, 75). Given the dubious "history lessons" offered by some of the commentators, Galen's endorsement of our recommendation that "historical analysis be added to the range of disciplines investigating the social function of religion" (42) is most salient.

We appreciate the critical engagement with our paper by most of the commentators and we hope to have at least shown that both the empirical and historical evidence is more complex, ambiguous, and nuanced than evidenced in the assumptions of current experimental research. Consequently, we hope that the interdisciplinary conversation our article has generated, and more like it, will continue to be a defining characteristic of CSR.

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