

The Road Not Taken: Possible Paths for the Cognitive Science of Religion

Richard Sosis

Introduction

In the summer of 2014, the cognitive science of religion (CSR) celebrated its twenty-fifth year at the fifth biennial conference of the International Association for the Cognitive Science of Religion (IACSR). The conference was held in Brno at Masaryk University, home of LEVYNA, one of CSR's premiere research labs. The conference was entitled "Religion Explained? The Cognitive Science of Religion after 25 Years." Yet, the question posed by the conference title was never seriously addressed, nor do I suspect there was any expectation that it should be. Nonetheless, a veritable army of young researchers, who presented the results of sophisticated and exciting new studies, indirectly answered the question. Religion explained? Not yet: we are just getting started, for the best is yet to come.

There is much to praise about what CSR has accomplished in twenty-five years and there is reason to celebrate. Here I aim to gauge that success by comparing CSR to two other young academic fields: human behavioral ecology and evolutionary psychology. Through these comparisons I seek to draw out lessons for CSR that I hope will help the field forge a successful future.

Assessing the cognitive science of religion

The marriage of religious studies and cognitive science

The obvious place to begin an assessment of any field is at its emergence, and there is no reason to diverge from that well-worn convention here. While the title of IACSR's biennial conference would suggest a precise founding date of CSR, such precision is hardly evident. I am not privy to the conversations of the conference organizers who determined the founding date of the field, but the organizers did reveal that there was substantial debate. And, indeed, there should have been. "Twenty-five years" was

based on the publication of Lawson and McCauley's pathbreaking *Rethinking Religion* (1990). There are, however, seminal CSR papers that were published much earlier than 1990. What distinguishes these earlier works, although admittedly not all (e.g., Lawson 1967), is that they are anthropological.

For example, the founding date of CSR could be considered a full decade earlier with the 1980 publication of Stewart Guthrie's "A Cognitive Theory of Religion" in *Current Anthropology*. Or we could push the clock back even further and argue that Dan Sperber's *Rethinking Symbolism* (1974/1975) marks the founding of CSR. But Guthrie and Sperber would probably both argue that their works are built on a long tradition in anthropology that aims to understand the cognitive underpinnings of religious thought. For instance, prominent cognitive and psychological anthropologists Melford Spiro and Roy D'Andrade published a cross-cultural analysis of supernatural beliefs in *American Anthropologist* in the late 1950s. Their discussion seems to augur CSR's later interest in the role of cognition in the cultural transmission of religious ideas. In an illustrative passage, they write:

Though analytically conceived as culturally-constituted fantasy, belief systems are not created de novo by the fantasy of each individual or of each generation; rather, they are transmitted as cognitive structures from one generation to the next as part of a group's cultural heritage. (Spiro and D'Andrade 1958: 456)

And of course the writings of Levy-Bruhl in the early twentieth century push the anthropological interest in religious cognition even further back (Levy-Bruhl [1910] 1926, [1922] 1923). Nonetheless, CSR is fundamentally a product of the so-called "cognitive revolution," beginning in the 1950s, so it rightfully does not draw upon such early works as those of Levy-Bruhl, or consider itself as the intellectual heir of this anthropological tradition.

If we accept the conference organizers' founding date and assume that CSR began in 1990, then we are not celebrating a twenty-fifth birthday of the field. For it is clear that the anthropological interest in religious cognition predates this time. Rather, CSR is celebrating an anniversary that marks the marriage of two prominent academic fields: religious studies and cognitive science. This is not to deny that anthropologists have played a critical role in the founding and development of CSR. Indeed, anthropologists Pascal Boyer, Scott Atran, Dan Sperber, Harvey Whitehouse, Stewart Guthrie, Armin Geertz, and others are considered leaders in the field; and frankly, CSR would be unrecognizable without their formative contributions. But to appreciate why CSR researchers are interested in the questions they pursue and why the field is institutionally structured as it is, it is necessary to recognize that CSR developed out of a very unlikely exogamous marriage that crossed not just disciplinary lines but also the science-humanities divide as well. By linking the founding of CSR with the publication of *Rethinking Religion*, the conference organizers were acknowledging that the primary direction and concerns of CSR have been defined by those trained in religious studies, even if much of the theoretical development and empirical research in CSR has been pursued by those outside religious studies.

Fundamental principles of the cognitive science of religion

To assess the health of an academic field, it is informative to examine how the field is upholding its core principles. In the case of CSR, this turns out to be more challenging than perhaps anticipated. Despite the fact that CSR has become a recognizable and firmly established subfield of religious studies, there have been surprisingly few attempts to articulate its core principles. Review articles of the field instead focus on delineating core topics pursued by CSR researchers, such as HADD (Hyperactive Agency Detection Device) and MCI (Minimally Counterintuitive) theory (e.g., Barrett 2000, 2007a Barrett and Lanman 2008; Boyer 2003). Incidentally, conference presentations by Robert McCauley, Armin Geertz, and others followed this approach precisely; core principles of CSR were never discussed until I raised the topic in the last lecture of the conference. It is an interesting state of affairs for a young field to offer polythetic, rather than essentialist, self-definitions. It suggests the lack of a common theoretical or methodological core, or possibly a field in flux. There is of course general agreement that CSR consists of research aimed at understanding the cognitive underpinnings of religious thought and behavior, but this is hardly a blueprint for an academic discipline.

Nonetheless, at risk of being accused of misrepresenting the literature, it must be acknowledged that there has been some limited discussion in the literature concerning fundamental principles of CSR. Consider a recent example by Dimitris Xygalatas (2014a: 345–6):

CSR scholars by and large agree on a set of basic overarching assumptions. First of all, religion is not a *sui generis* domain of the human existence and therefore can and should be subject to explanatory scrutiny just like any other cultural expression. Second, a scientific study of religion must necessarily adopt a position of methodological naturalism; religious explanations of religious phenomena cannot be taken to have any explanatory value in themselves. In line with evolutionary psychology, it is accepted that cultural forms are subject to the biological constraints of the human brain and the universal mental capacities of the human species, as they have evolved through natural selection. In line with Cognitive Science, it is also accepted that the mind is neither a blank slate nor a general-purpose computational machine but comes pre-equipped with a host of specialized mechanisms, each with a specific function. Based on these premises, cognitive scientists of religion are interested in exploring the causal mechanisms that might account for the recurrent patterns of religious beliefs and practices found around the world.

While this summary is certainly not the final word on CSR's core principles, it does provide a helpful window into the field. There are three observations worth underscoring. First, these principles emphasize the point that CSR emerged out of religious studies. Indeed, these principles seem aimed precisely at distinguishing CSR from religious studies. For example, whether or not religion is *sui generis* is a long-standing debate in religious studies (Proudfoot 1985; Taves 2009), but it has received little attention from those outside of this discipline. Second, and following from the first, these core

principles might be shared by CSR researchers, but they are shared for diverse reasons. Scientists interested in the study of religion would not pause to consider that religion might be beyond explanatory scrutiny. And methodological naturalism is such a basic assumption for scientists that it is rarely explicitly stated. In other words, for CSR researchers coming from the sciences, all of Xygalatas's assumptions will appear obvious and banal, but for those traditionally trained in religious studies, these principles are likely to appear revolutionary, or at least they did in the early years of CSR. Third, these principles are not distinctive to CSR, as those who claim to engage in the evolutionary psychology of religion also share them (e.g., Rossano 2010; Weeden and Kurzban 2013).

If Xygalatas's core premises of CSR are roughly correct, then the field is wide open to nearly anyone who wishes to study religion scientifically. This is intellectually exciting since it means that CSR can grow and adapt and will not be limited to particular methods or theories that are likely to be disproven or out of date any time soon. However, although this theoretical and methodological omnivory is exciting, it is institutionally challenging. Specifically, if CSR is unable to distinguish itself from other psychological and behavioral sciences studying religion, such as evolutionary psychology, what exactly is its *raison d'être*? The original motivations for the development of the field, which distinguished CSR from religious studies, will soon become irrelevant. The marriage of religious studies and cognitive science has given birth to a young generation of researchers who, unlike their predecessors, are not trained in traditional religious studies programs and do not share the concerns and perspectives of the previous generation of CSR researchers.

CSR is indeed in the midst of a transition and I believe it is at a pivotal juncture in its development as a field. To gain perspective on its growth and potential trajectories, I compare CSR to two other young fields that I have been intimately connected with in my training, research, and teaching: human behavioral ecology and evolutionary psychology. Both of these fields emerged at roughly the same time as CSR and thus serve as useful comparators.

Comparing human behavioral ecology and the cognitive science of religion

Behavioral ecology is the application of the theory of natural selection to the study of behavioral adaptation and design in an ecological setting (Winterhalder and Smith 1992). Human behavioral ecology (HBE) extends the theoretical perspective and methodological tools of animal behavioral ecology (Krebs, Davies, and Parr 1993) to the study of human populations. Chroniclers of the field (Winterhalder and Smith 2000) situate its beginnings with optimal foraging studies, conducted in the 1970s, and notably collected in an edited volume published in the early 1980s (Smith and Winterhalder 1981). By the mid-1980s human behavioral ecologists began to extend their research beyond foraging behavior, applying optimality models to understand fertility decisions, mating tactics, childcare practices, life histories, and other aspects of human behavior.

There are various similarities between CSR and HBE, but two in particular are worth highlighting. First, similar to CSR, HBE emerged from two larger disciplines: biology

and anthropology. Whereas CSR originally consisted primarily of religious studies scholars employing the theories and tools of the cognitive sciences, HBE was founded by anthropologists who borrowed the methods and models of evolutionary biologists and applied them to humans. In other words, both fields drew inspiration from a discipline that offered more rigorous methods and generalizable theories than were currently available in their home disciplines. Second, both CSR and HBE have benefited from the affections of larger academic fields. While archaeologists maintain a close relationship with behavioral ecology, using their models to interpret their data and advance theoretical development, theologians and philosophers of religion pay close attention to nearly every finding in CSR, scrutinizing them and reflecting on their significance for their own theories and theologies. The interests of archaeologists and theologians respectively should not be underestimated. CSR and HBE are both small fields; without the sustained and enthusiastic interest of larger disciplines, their survivorship as academic areas of study, especially in the early years, would have been much more precarious.

Despite these similarities there are considerable differences between the fields. First, whereas CSR researchers are trained and employed in various disciplines, including psychology, neuroscience, anthropology, religious studies, and history, HBE is primarily limited to one academic discipline: anthropology. HBE researchers collaborate with economists, biologists, psychologists, and others, but anthropology is the home discipline to the overwhelming majority of researchers who consider themselves human behavioral ecologists. Second, unlike CSR, HBE never developed its own academic organization. HBE researchers participated in the founding of the Human Behavior and Evolution Society in 1988, but they are just one subfield in this organization. Likewise, similar to CSR's section in the American Academy of Religion, HBE researchers were instrumental in creating the Evolutionary Anthropology Society (EAS) within the American Anthropological Association, but like the Human Behavior and Evolution Society, EAS does not consist exclusively of human behavioral ecologists. Third, HBE researchers never established their own journal, as CSR has, but rather publish in various evolutionary and anthropology journals. The publishing situation in HBE is somewhat similar to CSR before the founding of *Religion, Brain and Behavior*, and the *Journal for the Cognitive Science of Religion*. Specifically, *Human Nature* plays the role for HBE that the *Journal of Cognition and Culture* (JCC) played for CSR; *Human Nature* does not exclusively publish HBE research, but it does serve as an informal "home" for HBE researchers, similar to JCC's status since its founding in 2000. Fourth, HBE researchers have not published any trade books. And the books they have published, with a few exceptions (e.g., Kramer 2005; Marlowe 2010), have been published primarily by Aldine de Gruyter, a respectable press but not of the stature or visibility of the university presses of Oxford, Cambridge, or Harvard. Fifth, one reason that human behavioral ecologists have not published trade books, in contrast to CSR researchers (e.g., Barrett 2012; Bering 2011; Boyer 2001; Slingerland 2014a), is because human behavioral ecologists employ research methods (e.g., testing of optimality models) and pursue research topics (e.g., foraging and life history theory) that lack mass appeal. In other words, HBE is understandably of little concern to lay people. The study of religion, in contrast, is of great interest to many religious, as well

as nonreligious, laypeople. Sixth, HBE lacks the private financial support that is currently a significant engine in the growth of CSR. Specifically, HBE research is primarily funded by the Anthropology Section of the National Science Foundation (NSF), and the private agencies that are interested in HBE, such as the Leakey Foundation, are small. In contrast, the John Templeton Foundation (JTF) is directly or indirectly funding the majority of work in CSR and the amount of money that JTF has invested in CSR is remarkable. There are single projects being funded by JTF whose budget is larger than the entire annual NSF budget for anthropology. Quite simply, JTF has unquestionably transformed CSR and spurred its growth. While much has been made of JTF's possible religious and political influence on the field (e.g., Bains 2011; Martin and Wiebe 2014), JTF's greatest impact may be its preference for high-profile, news-worthy research.

Given the previous description of HBE, it is fair to ask whether HBE is a successful academic field. Of course, the answer to such a question would depend on the measuring instrument employed. If the number of headline-grabbing results or TED-talks were the assessment tool, then HBE would fall short. However, HBE researchers do not view themselves by these standards, and, importantly, neither do others. HBE has indeed been successful, but it is a quiet form of success. This is a field that has not sought publicity but has rather relied on the strength of its research to build its reputation. During the past fifteen years HBE research has been overrepresented (i.e., articles per capita) in *Current Anthropology*, which maintains the highest impact factor of anthropology's flagship journals. NSF also funds HBE research projects well out of proportion to what would be expected by the relative number of active HBE researchers. Moreover, anthropology departments have recognized the funding and publishing accomplishments of HBE, and despite inherent conflicts with the evolutionary and reductionist approaches of HBE, the job opportunities for HBE researchers continue to grow in an otherwise very difficult academic job market.

What can CSR learn from HBE? The message is simple: the key to success is high-quality research and not necessarily more journals, bigger organizations, media attention, or trade books. That said, in my estimation CSR cannot follow the path of HBE. First, CSR already has a thriving academic organization (IACSR) and successful specialized journals (*JCSR* and *RBB*). Moreover, CSR has already grabbed too much media attention to achieve "quiet" success. And given that JTF funds the majority of significant research programs in the field, CSR is likely to gain considerably more public attention in the coming years. Second, CSR's topic of study, religion, is inherently interesting to laypeople. Thus, even without the encouragement of JTF, CSR research will always have an attentive lay audience. Nonetheless, the importance of high-quality research is a message worth embracing, regardless of the path a field takes.

Comparing evolutionary psychology and the cognitive science of religion

Evolutionary psychology (EP) likely provides a better comparison with CSR than HBE, or at least they appear to have taken similar paths. The goal of EP is to uncover the selective pressures that have shaped human psychological mechanisms (Gaulin and McBurney 2001). The field itself views its roots in Darwin's own writings, but it was

not until the publication of Donald Symons's *The Evolution of Human Sexuality* (1979) that the field genuinely began to take shape. The publication of *The Adapted Mind* (Barkow et al. 1992), however, launched EP into an exceptional growth pattern that, if not unique, is certainly rare among academic fields.

The similarities between EP and CSR are abundant. Here I focus specifically on similarities relating to the fast growth rate and high visibility of both fields. First, EP and CSR have their roots in the cognitive sciences and thus benefit from the sustained success of the cognitive sciences over the past several decades. Second, while CSR results often grab headlines, EP's research focusing on mate choice (e.g., Buss 1994) has resulted in extraordinary media attention. Sex is a topic, understandably, that interests nearly all humans, and consequently EP sells magazines and newspapers and attracts viewers and listeners in the visual and audio media. Third, while EP never developed its own independent centralized international organization, the Human Behavior and Evolution Society, which EP researchers dominate, essentially serves this role. It should be noted that despite their numerical prominence, evolutionary psychologists have equitably shared the power structure of HBEs with other fields, specifically HBE and cultural evolutionists. Rather than founding a centralized academic organization, EP has established various regional (e.g., NorthEastern Evolutionary Psychology Society, European Evolution and Human Behaviour Association) and topical (e.g., Feminist Evolutionary Psychology Society) organizations. Fourth, EP maintains several journals that exclusively publish evolutionary psychological research (e.g., *Evolutionary Psychology*, *Evolutionary Psychological Science*, and *Evolutionary Behavioral Sciences*). Fifth, EP has fostered an industry of trade books, some of which have become international best sellers (e.g., Buss 1994; Pinker 1997, 2002; Wright 1994).

The extraordinary growth of EP, however, has not been without costs. First, EP was stigmatized early in its development, with claims of shoddy research and the more persistent accusation of offering nothing more than "just-so-stories" (Buller 2005; Plotkin 2004). Many of these critiques were easily dismissed as genuine misunderstandings of EP research. And some critiques were based on fundamental disagreements about human behavioral sciences that were not particular to EP but were leveled against EP because concerns about determinism and ethical implications made EP an easy target (Buss 2005). But most objective observers would agree that EP research, especially in the early stages of its development, exhibited high variance in quality. As the field matured and became confident and stable enough to be self-critical, overall research quality improved, although EP's development of new journals may be outpacing the relative growth rate of its practitioners, which will inevitably put research quality at risk.

Second, the extraordinary success of EP has not only resulted in an increase in self-identified evolutionary psychologists but also transformed social psychology itself over the past several decades. It is now commonplace in psychology journals, possibly essential, to posit some evolutionary rationale for research findings. This trend, it should be emphasized, was never the goal or intention of EP. Evolutionary psychology offers a theoretical foundation that can generate, through informal inference, a priori hypotheses about psychological adaptations (Gaulin and McBurney 2001). Post hoc evolutionary interpretations of data—in other words, claiming that findings are consistent with what we would expect natural selection to produce—are a far cry from the

methods envisioned by early proponents of EP. Ultimately, EP's success has meant that there are many psychologists who lack rigorous training in EP, yet apply evolutionary interpretations to their research. Again, whether fair or unfair, this situation has made EP an easy target for criticism.

There are several lessons that can be learned from the growth and development of EP. First, maintaining the high quality of research that was on display at the 5th biennial IACSR conference in Brno, and is generally manifest in CSR, will not be easy. With regard to EP's initial reputational problem, much of it was solved by an extraordinary husband and wife editorial team, Martin Daly and Margo Wilson. In 1997, when Daly and Wilson became editors of *Evolution and Human Behavior* (EHB), not only did they transform the journal but also their editorial work impacted the entire field. Daly and Wilson set the bar high and rejected papers from authors who had grown accustomed to regularly publishing in the previous incarnation of the journal, known as *Ethology and Sociobiology*. It did not take long for researchers to get the message: only the highest quality research would be published in EHB. Daly and Wilson's editorial decisions were undoubtedly difficult, but they ultimately placed the health of their field above long-term friendships and collegiate loyalties. Their efforts were ultimately appreciated, but it took such efforts to raise the bar in EP. I've described this history in some detail because CSR may find itself in similar circumstances, requiring someone to make comparable sacrifices. Moreover, it is worth recognizing that individuals, or in this case a couple, can have a lasting impact on a field.

Second, because EP is like CSR insofar as it is compelling to many outsiders, some researchers have been inclined to loosely draw upon CSR theories to interpret their data. The situation in CSR may be even more challenging than that faced by EP. Evolutionary psychologists may be concerned about how social psychologists use their theories for post hoc data interpretation, but social and evolutionary psychologists are at least part of the same family of disciplines, with both seeing themselves as pursuing human behavioral science. CSR, however, is a science that is attracting the wider attention of humanities scholars. These scholars recognize the power of CSR's theories and wish to apply them to their historical and ethnographic data. However, effectively using CSR's theoretical advances in the humanities will generally require some retraining (Slingerland 2008; Justin Barrett's former Cognition, Religion, and Theology project at Oxford University provides an excellent model).

Third, part of EP's success has been its ability to avoid splinter groups, especially in its earliest and obviously most fragile years of development. Splinter groups already seem to be sprouting in CSR, such as the emerging concentration on cognitive biblical studies (e.g., Luomanen et al. 2007), which maintains its own section (Mind, Society and Religion in the Biblical World) in the Society of Biblical Literature. Moreover, some of the evolutionary camps, such as D. S. Wilson's Evolutionary Religious Studies group, have not been fully integrated into CSR. The Human Behavior and Evolution Society might serve as a valuable model for CSR, as it represents the interests of diverse groups who share a common interest in employing evolutionary approaches to understanding the human condition. It would seem that IACSR could, and should, function similarly, developing into an umbrella organization that serves diverse groups interested in cognition and religion.

This suggestion leads to a fourth lesson that can be learned from EP: do not fear a name change. To keep evolutionary researchers, social psychologists, cultural anthropologists, and biblical studies scholars all in the same organization, CSR might need to consider an alternative name that broadly captures what all of these researchers have in common, which in truth, is probably not cognitive science. As noted, HBE went through this process with its flagship journal, changing its name from *Ethology and Sociobiology* to *Evolution and Human Behavior*, which reflected the reality that researchers in the organization no longer saw themselves as ethologists or sociobiologists, but rather as evolutionary anthropologists, psychologists, economists, political scientists, and so forth.

A fifth lesson that can be learned from EP is the importance of nurturing relationships with scholars who have a broad academic reach. *The Adapted Mind* (Barkow et al. 1992), as noted earlier, marked the initial ascent of EP, but its extraordinary growth was fostered by a number of highly popular trade books, such as *The Moral Animal* (Wright 1994), *The Evolution of Desire* (Buss 1994), and *The Origins of Virtue* (Ridley 1996). Stephen Pinker's conversion to EP and his subsequent writings (e.g., Pinker 1997, 2002) were also certainly not trivial. In contrast, CSR was marginalized, or notably absent, in several widely read and influential works on religion (e.g., Dawkins 2006; Dennett 2006; Wilson 2002). And most recently, CSR was summarily dismissed as irrelevant by the late, eminent sociologist Robert Bellah (2011), a point I return to later in this chapter.

EP and HBE offer very different models of how a field can develop. Lessons can be drawn from both of them, but most importantly, CSR needs to decide what its primary goals are. What kind of field does CSR want to be? What does CSR hope to accomplish? Once these questions are settled, the path that should be taken to reach its goals will be much clearer. Since IACSR has already been firmly established as the central institutional organization of the field, it would seem that it is the responsibility of its leaders to clearly articulate specific goals for the field and provide a compelling vision for achieving those goals.

Future directions and friendly advice

In this section I offer several recommendations concerning how CSR might develop. Whether or not my advice is heeded, if nothing else, I am hopeful that my comments can at least initiate several conversations that I believe are important for the health of CSR.

First, CSR needs to study religions. For the uninitiated, this recommendation might sound absurd, but CSR researchers, in fact, will recognize it as controversial. Many CSR researchers claim that religions do not really exist and therefore to study them, as religious studies scholars have always done, is misguided. The in vogue approach among CSR researchers is "fractionalization," as was discussed by both Whitehouse and Lanman at the IACSR conference. Fractionalization refers to the reductionist research strategy that seeks to understand elements of religion by breaking them down and isolating their features for scrutiny. For example, it is argued that to study ritual

we need to isolate (in experimental labs) the various common components that constitute ritual, such as body movement, synchrony, singing, attire, and so forth. This is an approach that I have been sympathetic to in the past, and I continue to pursue research in this vein, but I have become increasingly concerned about its ability to explain religious experiences and lives. To be clear, fractionalization has its value and such an approach has already (and will continue to) advance our understanding of religion and the human condition. Admittedly, I am an unrepentant reductionist. But reductionism is just a first step in any analysis of complex phenomena. Ultimately, the pieces must be put back together, and therefore, drawing implications about religious systems from the results of analyses that focus on one corner of the system can be misleading. Even more problematic for fractionalization, however, is the strong possibility that religious systems exhibit emergent properties (Sosis 2012). If so, then religion cannot simply be broken down and studied in isolated fragments with the hope that by understanding the constituent parts we are gaining insight into the workings of the whole system (Purzycki et al. 2014; Sosis 2009; Sosis and Kiper 2014).

The fractionalization approach has been justified by arguing that religion is an illusion. As Whitehouse described at the IACSR meeting, "religion is like the big dipper." In other words, when we look at the Big Dipper, we believe we are seeing a pattern in which the sum of the parts constitute a meaningful and functional whole, but of course, classical constellations like the Big Dipper are arbitrary sets of stars. It is only human cognition that imposes a pattern on what is otherwise a capricious arrangement. Likewise, it is argued, religion looks like a "complex whole" only because human cognition detects such a pattern. But the analogy falls short. While the spatiotemporal relationships between stars in a constellation are independent, that is certainly not the case for religion. Indeed, one of the most interesting and pressing questions faced by CSR researchers concerns the interrelationships between the various elements, such as ritual, myth, and supernatural agent beliefs, that constitute the core of religions (Sosis 2009).

In *Religion in Human Evolution*, Robert Bellah (2011) recognized this problem within CSR, which is why he dismissed the field as irrelevant to his project. He singled out the two most widely read books in CSR, *Religion Explained* (Boyer 2001) and *In Gods We Trust* (Atran 2002), as "unhelpful." Bellah ignores CSR in his treatise because of its "lack of insight into religion as actually lived" (Bellah 2011: 629). Some may be inclined to write off Bellah as an outsider who failed to understand CSR's approach and achievements; nonetheless, the onus would still be on CSR to better articulate its value. I, rather, suspect Bellah understood CSR all too well. Indeed, many within CSR readily admit that they are not studying religions and thus Bellah was correct that his historical focus on the developmental trajectories of religions is an anathema to CSR.

Second, CSR may have emerged as a marriage between religious studies and cognitive science, but it did not remain a monogamous marriage for long. Anthropologists and philosophers were quickly seduced, and by the late 1990s the evolutionists had followed suit (Bulbulia et al. 2008). These were strange bedfellows, but CSR's religious studies scholars welcomed their new partners and overall it has been a fruitful polygamous arrangement. Nonetheless, the initial concerns that motivated disaffected religious studies scholars to break from their traditional discipline and develop CSR still linger in CSR discourse. It is time to recognize that CSR has extraordinary

disciplinary diversity; the majority, or near majority, of the field no longer originates from religious studies. Rather than a subfield of religious studies, the next generation of CSR would be better conceived as a subfield within the behavioral sciences. As a subfield of the behavioral sciences, CSR will be in a position to reengage with religious studies in a healthier and more productive relationship as equals, rather than as parent and wayward teenager, which seems to characterize current relations.

Third, CSR has a responsibility to understand how it is impacting religious communities. It is clear that CSR is influencing the discourse in some religious communities, especially among religious intellectuals. But it remains unknown if media and other popular accounts of our work affect the beliefs and behaviors of those we seek to study. Admittedly, if it turns out that our research is genuinely impacting human lives, I am not sure what should, or could, be done with such information. Nevertheless, we cannot even begin to have such a conversation until we assess the possible impact of our research. Such a venture is not unusual: most scientists want to know how their work is influencing the populations—whether penguins or people—they are studying. And to be clear, I am not suggesting that if our work turns out to be impacting religious lives that we therefore alter our research to minimize effects. All I am saying is that a conversation about the impact of our research needs to occur, and such a conversation will only be useful if it is informed by accurate assessments.

CSR also has a responsibility to educate the public about religion. Indeed, we are in a unique position to help resolve the increasing frequency of secular and religious conflicts that plague our world. CSR cannot adjudicate such conflicts, but our research may be useful in diffusing such conflicts. One causal factor that pervades most conflicts between the religious and secular, as in most disputes, is a lack of understanding and appreciation for the beliefs and behaviors of the opposing side. Suggesting that observant Jews, for example, should light one less candle during Hanukkah, as proposed by Israeli conservationists ("Green Hanukkah Sparks Criticism," *United Press International*, December 5, 2007), indicates a complete lack of understanding and appreciation for the conviction of religious beliefs and practices. And disputes like this are commonplace (Sosis and Kiper n.d.). If CSR can offer anything in such disputes, it is the explanation of religious beliefs and behaviors in materialist terms. Of course, CSR theories and data are unlikely to be satisfying for many religious adherents, because they fail to capture the depth of meanings in their convictions. CSR research, however, may help religious adherents understand how even secular values can be sacralized by secularists and evoke strong commitments (Tetlock 2003). More importantly, CSR interpretations of religion are likely to provide satisfying materialist explanations for secularists. CSR should not aim to justify such beliefs and practices, but with an appreciation for why humans possess such strong convictions for religious beliefs, it can hopefully open up a fruitful dialogue to facilitate conflict resolution.

Conclusion

I conclude with a suggestion that CSR think deeply about its identity. To claim that CSR has an identity problem is a gross understatement. Consider the simple fact that

I am writing this chapter, not to mention that I serve on the executive committee of IACSR. My credentials as a cognitive scientist are simply nonexistent. Indeed, trained as a behavioral ecologist, I have largely been agnostic regarding cognitive mechanisms throughout my career (Sosis and Bulbulia 2011). It is a remarkable fact that I have been welcomed into the CSR community—no doubt, a testament to goodwill, but also surely evidence of CSR's lack of self-identity. My guess is that I, and those with similar backgrounds, have been welcomed into CSR because (1) we study religion scientifically as a natural phenomenon, and (2) we recognize that sociological, political, and economic models are insufficient for explaining religious thought and behavior. This makes for a stimulating, albeit amorphous, collection of scholars. Many of the researchers engaged in CSR are not actually doing cognitive science. In fact, at the IACSR conference in Brno, about half of the research papers presented could not be classified, even by the most liberal definitions, as cognitive science. It is not surprising, as previously discussed, that CSR defines itself by the research it pursues rather than a set of core principles.

The IACSR conference was not an aberration, but rather emblematic of changes brewing in CSR. Consider the recent research of IACSR's president, Joseph Bulbulia, and acting past president, Armin Geertz. In my mind they have published two of the most important, yet neglected, articles on religion in the past few years: "Charismatic Signaling" (Bulbulia 2010) and "Brain, Body, and Culture: A Bio-Cultural Theory of Religion" (Geertz 2010a). These papers, I suspect, have been relatively ignored because they offer theoretical models that do not fractionate religion, but rather point toward a holistic science of religion. Their models, therefore, are not easy to evaluate through experimental methods, although both Bulbulia and Geertz support their models by drawing upon many experimental results that are consistent with predictions from their respective models. Genuine evaluation of their models is likely to require the slow and labor-intensive methods of detailed ethnography. Ethnographic research requires more time and deeper commitment, and produces fewer headlines than experimental studies; it is therefore no surprise that their theories are not spurring evaluation and that even anthropologists are now frequently turning toward experimental methods. Ethnography, however, must complement experimental studies for CSR to remain a credible voice in the academic study of religion. The bottom line is that we cannot ignore the study of religions.

I highlight Bulbulia's and Geertz's articles because they offer, in my mind, a more compelling science of religion than the current dominant theoretical and methodological approaches in CSR. In some ways, these papers also characterize the direction in which CSR seems to be moving, although these papers are emblematic rather than causative of this movement. Self-identified CSR researchers are no longer just studying the cognitive mechanisms that underlie religious beliefs and behaviors. Neither Bulbulia nor Geertz, for example, focus on what is happening in the head, but rather their attention is drawn to the dynamic interaction between mind, body, and environment. Had Bellah been aware that CSR researchers were pursuing such approaches, I suspect he would have appreciated their efforts and given CSR the attention it deserves.

Moreover, it is clear that Bulbulia and Geertz are well aware that they are moving beyond the traditional CSR paradigm. Notably, neither draws on cognitive science to label their models. Bulbulia turns to sociology, specifically Weber's notion of religious charisma, and Geertz describes his model in anthropological terms as bio-cultural. Geertz was writing for a more traditional religious studies audience, and thus he was concerned that "[t]he term 'biocultural theory of religion' may strike some as too much hard science" (2010a: 313), although this is unlikely to trouble CSR researchers. I conclude with the vision for CSR of our acting past president, not only because he is better positioned to offer such a perspective, but because I endorse this vision and cannot say it any better myself:

What I envision is a combination of theories from neurobiology, social psychology, anthropology, cognitive science, archaeology and comparative religion. I am convinced that nothing less can do the job and am therefore arguing that traditional disciplinary boundaries are in need of modification and change... I hope that I have shown that the intimate interrelations between body, brain, culture and society, demand a "biocultural turn." (Geertz 2010a: 313)

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