

# **Evaluation of “How do moral religions work? A philosophical inquiry into the cognitive science of religious prosociality”**

**by Karolina Prochownik**

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## **General evaluation**

In this thesis, Karolina Prochownik examines an ancient question, namely the relationship between good and God, or how morality connects to theism. As she specifies, although the thesis has religion as its main topic it is not a thesis in the philosophy of religion, because she does not make any claims about the veracity of religious statements. Rather, the thesis is in the philosophy of cognitive science, particularly, in the cognitive science of religion (CSR), and uses a methodologically naturalistic standpoint to evaluate the hypotheses she examines. My evaluation of the thesis will be with this in mind.

The scope of the literature that was consulted for writing this thesis is broad, going beyond the usual CSR literature. The author carefully considers models about the evolution of culture, which can be fairly technical, involving gene-culture co-evolution and multi-level selection. I learned a lot from this thesis and am impressed by the command of this very wide and often quite technical literature.

The thesis critically examines two recent types of approaches to the question of how religion connects to morality: adaptationist approaches and epidemiological approaches. The cultural adaptationist approach sees religion as a cultural adaptation to living in larger groups in the early Holocene, with the emergence of belief in big, watchful Gods as a way to solve the problem of large-scale cooperation in such groups. By contrast, the epidemiological approach puts the emergence of moral religions much later, in the axial age (between 1000 and 500 BCE) as a result of the interaction between psychological and ecological factors in elite members of communities. The author puts forward a novel thesis, the equality hypothesis: her analysis of these two approaches finds more support for the epidemiological approach, and she argues that human preferences for fairness could be an attractor for moral religious beliefs.

I believe this thesis fits the requirements for a doctoral thesis and that it should be recommended for defense. I will first provide my reasons for why I think this thesis should proceed to the defense, and then offer some specific

questions and suggestions that the author may take into account when revising parts of this thesis for publication in journals.

My first reason for recommending the thesis is that it has a well-focused question, namely to scrutinize recent theories in CSR on the link between morality and religion, and that it shows a thorough and sophisticated understanding of these theories in their broader context. To give an example, pp. 66-71 provide a detailed comparison of the gene-culture co-evolutionary theory as proposed by Richerson, Boyd and others, and compares it to the epidemiology of representations. Although both advocate a form of cultural evolutionism and use models from evolutionary biology, there are nevertheless subtle differences which will make an impact on the kind of relationship they propose between morality and religion – gene-culture co-evolution emphasizes selection, whereas epidemiology only see it as a subtype of attraction. As a result, cultural selectionist approaches emphasize context biases, such as prestige and conformist bias, to explain how cultural representations get transmitted in the face of distorting cognitive factors, whereas epidemiological approaches focus on content biases.

Secondly, I was pleased to see in this dissertation in philosophy that Ms Prochownik does not shirk away from discussing very technical issues in this literature, such as the question of multi-level selection, the use of the Price equation in modeling cultural evolution, and the factors involved in cultural evolution. A lot of philosophical literature that considers this empirical literature brushes over these fine distinctions (or is simply unaware of them), so it was rewarding to see detailed treatments of these topics in her thesis. In particular, the criticisms of cultural group selection and cultural epidemiology were sophisticated and well articulated.

Third, Ms Prochownik offers a detailed analysis of the components of cultural group selectionist accounts of religion and prosociality, teasing apart the assumptions that underlie such accounts, and then examining in detail whether the empirical evidence supports these assumptions. For example, she points out that many of the priming studies often cited to support the psychological link between religion and prosocial behavior (e.g., Shariff and Norenzayan) offer inconclusive evidence because (1) they do not adequately distinguish between social monitoring or a more specific fear of supernatural punishment as two possible mechanisms underlying the evolution of belief in big Gods and prosocial behavior, (2) they use non-agent components, such as non-agent priming words.

Fourth, throughout the thesis, Ms Prochownik clarifies concepts such as “Big Gods” and “morality”, which have been used in this literature but have not been well defined. She rightly points out that these definitions tend to oscillate, for example, Big Gods are sometimes used in the narrow sense of monotheistic creator gods, and sometimes in the broad sense of any morally concerned, watchful supernatural entity (including ancestor spirits). Morality is sometimes used in the narrow sense of harm and fairness, and sometimes in the broader sense of Haidt, which also includes norms to do with maintaining in-group cohesion, purity, authority and liberty.

One of the things philosophy can offer to the sciences is conceptual analysis, and this dissertation is a very good example. The author shows how carelessness in the use of these terms has made it difficult to draw any firm conclusions about the evidence, because different studies use different definitions, and some authors do not have a consistent definition throughout their own work.

As such, I expect that this thesis (and any publications that would be based on its chapters, especially chapter 3) will make a meaningful contribution to the CSR literature by encouraging CSR authors to make terminology more rigorous, and thus its hypotheses more testable. She is also absolutely right that the argumentation needs to be more rigorous and systematic, rather than a cherry-picking of some historical case studies, as she writes “big Gods need big arguments” (p. 178).

Fifth, if the thesis had solely consisted of the elements that I mentioned up to now, it would already have been of high enough standard to proceed to the defense. But the author goes further than this. She presents detailed novel case studies of ancient Mesopotamian religion, with a focus on the question of whether Marduk could be considered a high God, and Christianity, to evaluate cultural adaptationist accounts of moralizing religions. I was impressed with the scope of the research to test the big god hypothesis, and I think it sets a new standard of what theorizing in these fields should look like.

Sixth, at the end of the dissertation, the author proposes an extension of the cultural epidemiological approach, arguing that the increasing inequality in the axial age (note that I have some reservations about using this as a concept, see my specific comments and questions below), made egalitarian religious movements more cognitively attractive. The equality hypothesis is very intriguing, and the author provides good arguments for why it is a plausible hypothesis (e.g., the appeal of moralizing movements to poor people, because of a need of social justice that is less satisfied than in more equal societies). The author concludes that this account is at present speculative, but I think it is definitely worth pursuing. For instance, another form of support for this hypothesis is the observation that unequal countries tend to have higher degrees of religiosity (the US and the UK are for instance more religious than France or Belgium, which are more religious than the Scandinavian countries). It would be interesting to see in how far the equality hypothesis explains these GINI coefficient correlations better than Norenzayan, Shariff and others' group selectionist arguments, where religion is used as a way to enhance prosociality.

### **Specific questions and comments**

The following questions and concerns with this thesis are minor, and do not detract from its overall quality. Nevertheless, during the defense, I would hope that Ms Prochownik could address the following questions and concerns.

First, the authors mentions cultural non-independence (the so-called Galton problem, pp. 145 and following) as a potential confound for the Big Gods

cultural group selectionist hypothesis, but it is unclear to me why it would pose an insurmountable obstacle. For one thing, cultural groups selectionists are happy with cultural diffusion as a proposed mechanism for the propagation of successful traits (e.g., farming did not spread through population replacement, but mainly through cultural assimilation). It is true that people who believe in big gods (and may behave morally as a result of the sense of being supernaturally monitored, or the fear of being punished by gods) have a shared history in Abrahamic religions (and as the author says, there are also cultural spillover effects in other populations). But do these deep-shared roots adequately account for psychological responses to priming and other psychological evidence for these hypotheses? It would seem to me, if Muslims and Christians behave more morally after a prime of a bible verse or qu'ran piece, that is good evidence for the hypothesis, even though they have a shared history. I do think the author is correct that we need to treat these results with caution and we can't treat them as totally independent data-points, but they are data-points nonetheless, and have evidential value.

My second question is about the universalistic elements of religions such as Buddhism and Christianity: Prochownik rightly points out that this poses a *prima facie* problem for cultural adaptationist approaches to religion (see p. 215), because it would seem that in-group cooperation is the thing that makes for differential success of groups, and thus gets the cultural group selection going. I think of the three possible solutions to this problem the author offers, solutions 1 and 3 are problematic. But I do not think (2) can be so easily dismissed. There is ample evidence that people who adhere to universalistic religions are more cooperative with in-group members than with out-group members (e.g., Christians like other Christians best, Muslims a lot less, and atheists least).

I think that under this second explanation belief in big Gods can still have a lot of explanatory traction—as this belief is guiding actions—while nevertheless the universalistic aspects are being downplayed. For one thing, Christianity is ambiguous about its universalism. It has inclusive, universalistic elements (the good Samaritan) but it also has exclusionary elements (e.g., Jesus says that whoever is not for him, is against him, Matthew 12:30, and he explicitly says that his new religious movement is bringing division, even within households (Matthew 10:35). Looking beyond scriptural references, Christian theological doctrines are often very exclusionary: it is standard in most traditions to believe that only Christians will go to heaven, that the unbaptized will end up in limbo, and so on. In short, I think it is perfectly coherent for a cultural adaptationist to say that on balance Christian doctrine promotes within-group cooperation, at the expense of between-group cooperation and that belief in big Gods is an important element in this.

Third, I keep on being sceptical about there being something specific about population and economic dynamics around 1000-500 BCE leading to the so-called axial age. Is there a clear-cut definition of what fits within this movement? Many authors (e.g., Iain W. Provan) have expressed criticism of the concept. What distinguishes these societies from earlier affluent urban societies, such as the Harappa culture in the Indus valley? In general, I was a

bit puzzled by the author's general appraisal to the epidemiological approach to religion.

I find lots of the terminology and ideas (not a criticism to Prochownik, but to Baumard et al) a bit handwavy and not easily tested. The author does take up these potential issues, for instance, about the narrowness of defining morality in terms of fairness, later on, but I still get the sense that the epidemiologists get off the hook more easily than the cultural adaptationists.

Take for example, their invocation of concepts such as “slow strategies”, the idea that the elite, who is now freed from the everyday concerns of finding food, clothes, shelter and other primary needs can now turn to the fulfillment of less pressing needs such as aesthetics and friendship. The empirical evidence marshalled in support of this claim is, as the author argues further on, largely speculative. All the evidence shows is that people from more deprived environments behave differently in economic games, tend to do more future discounting (e.g., have children earlier), but the link between such fast strategies (and slow strategies such as taking into account the future, having children later, be more generous in economic games, delayed gratification), and axial age religious beliefs is very tenuous. Similarly, the historical evidence is tenuous (as the author herself also points out).

A main problem with the psychological evidence is not just that the link between moralising religions and affluence is not only tenuous but contestable. There is a body of empirical research suggesting that the most affluent are in fact *less* moral than others. For example, in Pfiff et al.'s 2012 PNAS paper “Higher social class predicts increased unethical behaviour” the authors found that high-socio economic status individuals are more likely to violate traffic rules, to cheat to win a prize, and to lie during negotiations. The findings have been replicated several times successfully, suggesting that high socio-economic status does not seem to correlate with a successful seeking of the good life, or trying to live an ethically sound life. Indeed, as the author goes on to explain, the relevant mechanisms seem to be those of moral self-licencing: compensating harm done to some people by for example flagellating oneself or by giving to a third party (alms).

I can thus see how some aspects of axial age religions, such as self-flagellation and giving alms could be culturally successful, given how they fit the cognitive needs of the upper class who is in search of moral self-licencing, but I do not see how this connects to the Golden rule or the radical exhortations to be good to out-group members, which, as the author has previously pointed out, cannot be entirely ignored. How can these elements of axial age religions be explained from an epidemiological perspective?

The theorizing about the role of conflict between social classes in the cultural transmission of moralizing religions—again my beef is mainly with Baumard, Boyer and others and not so much with the author of this thesis—strikes me as underdeveloped and naïve. In order to turn this into real testable hypotheses, and not just handwaving to explain post hoc observations of dynamics of the cultural transmission of moralizing religions in stratified societies, we would need databases of cultures with varying levels of social

stratification and the transmission of moralizing religions within them before we can even begin to see whether this thesis survives empirical scrutiny, or whether it is better supported by the evidence than the social control hypothesis in section 4.2 (p. 269 and following).

Finally, a relatively small remark: Haidt's liberty/oppression foundation is used to support the author's own equality hypothesis. The 'liberty' foundation is a later addition to Haidt's theory and there is some concern (I believe in Flanagan's *Geography of Morals*) that the foundation is an attempt of Haidt to fit in libertarians, who have a peculiar political orientation that focuses on liberty as the main value. The worry is that the empirical support for this moral foundation is less solid than for Haidt's other foundations, and that it was mainly added so as to make sense of libertarians. How important is it for the equality hypothesis? Could fairness be used as a foundation?

To briefly conclude, I recommend that Karolina Prochownik proceed to the defense as it more than fulfils the expectations for a doctoral thesis. I thoroughly enjoyed reading this thesis, and I look forward to the papers that will issue from it.

11 May 2017, Oxford

Helen De Cruz

A handwritten signature in blue ink, appearing to read 'H. De Cruz', with a stylized, cursive script.