Reply

Rescuing the Baby from the Bathwater: Continuing the Conversation on Gender, Risk, and Religiosity

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In this short response, we selectively address some of the key issues and criticisms raised by our esteemed commenters. First, we clarify *our standpoint vis à vis "biological" arguments, underscoring that our article is not to be read as hostile to all forms of explanations that incorporate biology into the explanation of religious behavior and belief, but only against those explanations that attempt to imply that socialization plays no role ("the it's all biology" argument). Second, we* defend *the explanatory scope of our proposal by showing that our argument is not vulnerable to the "simpler" counterexplanation proposed by Hoffmann. Finally, we* contextualize *where our contributions fit in terms of more encompassing arguments regarding the operation of gender and gendering processes as multicausal, multilevel phenomena, as well as explicitly stating our perspective on the role that "risk" should play in the explanation of religious behavior and belief.*

It is rare that authors are able to engage with their work in any substantive way after it has been accepted for publication, so we are grateful for both this opportunity and the comments of our peers.

Like Hoffmann (2009), we feel it is important to provide context for the work that stirred this most recent discussion. When we began writing this article it was not an effort to solve the nature versus nurture debate but to offer a test of the socialization account of risk preference that Stark (2002; Miller and Stark 2002) had so quickly brushed aside. We were particularly concerned, given the problematic operationalization of socialization, with a battery of gender-related attitudinal items. To redress this issue and find what we believed to be a preferable measure, we drew on Hagan's power-control theory (1987). We were unaware at the time that it was part of Miller and Hoffmann's (1995) original work and that our manuscript would be more an extension of that paper than a refutation of Stark's later work. As is common in our discipline, the debate raged on as the article moved through the pipeline with Sullins (2006), Roth and Kroll (2007), and Freese and Montgomery (2007) weighing in on matters of gender, risk, and religiosity. One will see little of this more recent work reflected in our own not because it is not important but because it moves away from our central question: *Are there valid socialization accounts for risk preference?* Unlike others, we accepted risk preference as an important source of differences in religiosity. We simply believed that there were more *sociological* explanations for such difference.

This introduction segues nicely into the comments of Bradshaw and Ellison (2009) (B & E hereafter) who suggest that we fail to adequately substantiate the claims of our article, which they interpret as "[refuting] the biological explanation proposed by Miller and Stark (2002)," because we do not measure anything biological. We never intended to rule out biological explanations or to imply that religiosity or risk aversion are "purely environmental in origin and nature." Our article was an attempt to reinvigorate socialization accounts by suggesting that Stark made a

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premature concession to biology based on too loose and imprecise of an indicator of familial socialization, given the fact that the social attitudes that an adult currently holds may have been obtained from sources outside the original familial environment. Of course, B & E's criticism is particularly ironic, given that Miller and Stark (2002) were able to publish an article arguing *for* physiology with the same lack of biological data.

We believe that we refuted at least *one version* of the biological hypothesis, which is the Miller-Stark "biology is *all* that matters" version. Clearly, the strongest version of a hypothesis is always the easiest to deal with empirically, so we commend Miller and Stark for putting their necks out scientifically. We did *not* set out to refute weaker versions of the biological hypothesis (such as the "biology matters in combination with environmental factors" version) because we actually believe along with B & E that one of those versions is probably on the right track but is also much harder to actually address empirically (Freese 2008).

In our mind, and we believe this is consonant with the beliefs of B & E even if not reflected in their comments, arguing for the inclusion of a potential environmental factor is not to assert that one has found the golden key that only works in isolation. While we do not think of the taste for risk as an inherently inborn trait, we admit that it must have some physiological aspects, and we have no problem admitting the existence of a certain predisposition for risk taking, maybe under partial genetic control that may be differentially distributed across individuals. However, we think that the plasticity of the human cognitive-emotional architecture still allows for important variation to develop in what, following the anthropologist Joseph Henrich (2008), we will refer to as the environment of ontogenetic adaptiveness or the social and physical setting within which the first 16–20 years of life are spent. Thus, the way that we wish our study to be read is not as implying that "it's all environmental" but as refuting the "it's all biological" hypothesis.

We agree with B & E that abandoning the "it's all biological" hypothesis does not imply accepting the equally misguided "it's all environmental" one. The key lesson that we want our article to convey is that if the gender/religiosity puzzle is to be cracked, one will have to seriously look at environmental influences without prematurely dismissing them; that these environmental influences will probably be connected to the *family* environment (and thus that measurable characteristics of this environment, especially parental social, economic, and educational resources) will probably matter in predictable ways and that influences of this environment that can be traced to *early* experiences will probably matter more than things that we can measure in adulthood.

However, this analytic strategy does not imply a complete blindness to the influence of biology. Clearly, the *really* hard task, as B & E point out, is, after moving beyond the simplistic positions of "it's all biology" or "it's all environment," to *partition* the variation in a given outcome that is due (a) to biology, (b) to environment, and (c) to ascertaining whether there is—as there surely will be for most interesting outcomes, including religiosity—a nonnegligible gene $\times$ environment interaction (Freese 2008) as cogently noted by both B & E and Hoffmann (2009). One weakness of the "there is a genetic component to everything" hypothesis is that while nominally true, it is analytically sterile because if biology is everywhere it is also nowhere. The key task is to move beyond the "biology" matters observation and begin to provide analytically and theoretically motivated *models* of *how* is it that biological, social, and cultural factors interact to produce phenomena of interest.

Hoffmann (2009) provides a very thorough and incisive critique of our effort to shed light on the gender and religiosity puzzle. We cannot address all of Hoffmann's points in this short response, so we will just limit ourselves to tackling Hoffmann's main objection to our findings. As Hoffmann notes, there is a simpler explanation for our results: high SES (socioeconomic status) mothers may be less religious than low SES mothers and they transmit this irreligiosity directly to their daughters. This explanation is certainly simpler than the one that postulates an unobserved process of differential risk-preference formation.

However, it is clear that this explanation does not account for the full range of facts. First, the connection between SES and religiosity is complex, but there is no evidence that higher SES persons are less religious (in fact, they tend to be more religious when it comes to over indicators of religiosity, such as church attendance). Second, even if it was the case that high-SES mothers were less religious, Hoffmann's simple counter explanation would not be able to account for the fact that the effect of mother's SES is *gender asymmetric*. If Hoffmann was correct, we should find that sons *and* daughters of high-SES mothers would be less religious (Hoffmann's attempt to explain away the lack of a father's SES effect by pointing to a relative lack of variation seems to us as ad hoc). The gender-asymmetry finding is consistent with a gender-differential risk-preference formation mechanism and not with a direct transmission of irreligiosity mechanism.

Third, if the effect was simply the one connected to daughters of high-SES mothers "role-modeling" the behavior of their irreligious mothers, why is it that the effect is stronger when using measures of *economic* occupational resources than when using measures of *educational* (or cultural) occupational resources? Out of the different components of SES, in fact, education has been the one that has been most consistently connected to declining religiosity (especially when this is measured using subjective measures of belief [Albrecht and Heaton 1984]). If Hoffmann was correct then, we should find the opposite of what we find: mothers' educational resources should have a stronger depressing effect on the religiosity of their daughters (because highly educated mothers are more likely to be less religious than high-income mothers) than mother's economic occupational resources. Once again, our finding that the gender interaction is stronger when it comes to the *economic* resources of the mother's occupation are consistent with the power-control mechanism connecting maternal material autonomy from the father with gender-egalitarian socialization practices and are inconsistent with Hoffmann's alternative explanation.

We are also glad that Cornwall (2009) takes time in her comment to situate the gender/religiosity debate within the wider context in which gender operates as physiological, interactional, institutional, and cultural phenomenon. She provides a convincing case—for which we would like to register our wholehearted agreement—for why the gender/religiosity issue has been the one that has captured the imagination of many scholars of late: it operates at multiple levels of analysis and through complex, multilayered, and interlinked pathways with (still unknown) sorts of feedback between them. Gender and gendering processes are themselves part of this multiscale, multipath complexity, since gender is itself a psychological, embodied, interactional, cultural, organizational, and institutional phenomenon, a point also made by Hoffmann (2009).

In this short response, we cannot, of course, address all the issues that Cornwall raised in her thoughtful response. We will like to limit ourselves to making a couple of observations. First, it is important to underscore one crucial point of clarification: when it comes to the gender difference in religiosity, we have no interest in "deconstructing the phenomenon." The worth of our contribution (and the future viability of this particular debate) depends on the phenomenon being robust. Here we are in complete agreement with Hoffmann (2009) in believing that the phenomenon is robust and that it constitutes a genuine scientific puzzle. We don't believe it is a by-product of social scientific measurement strategies or an epiphenomenal efflux of culturally biased ways of construing the facts. When it comes to recent attempts to deconstruct the phenomenon, suffice it to say that we have found the efforts of Sullins (2006) and Roth and Kroll (2007) to cast doubt on the robustness of the gender effect less than convincing (those particular studies actually have more methodological and conceptual problems than the literature that they attempt to criticize). In any respect, *deconstructing* a phenomenon is easy, *explaining* a phenomenon is hard. We are attracted to the harder challenge.

Cornwall notes toward the end of her response that "risk preference is not the mechanism that accounts for different levels of religiosity. Rather, gender processes allow or constrain men and women in their 'doing' of both risk and religiosity." This assertion is for us the key because it reveals two basic analytic points. First, mechanisms are nested within multiple levels of analysis, so any assertion of what *the* mechanism *is* already presumes a theoretically specified "level" at which the crucial causative factors are operating. Theory is the only source of information as to what the crucial level is. Cornwall's point depends—as she clearly noted at the beginning of her response—on taking a specific *theoretical* point of view on gender and gender differences as a multilevel, cultural, and institutional phenomenon. Our belief that risk aversion is one of the key mechanisms (and that risk aversion is one of the ways through which gendering processes manifest themselves) is also connected to taking a particular theoretical starting point regarding the sources and motivation for religious belief and behavior, is roughly in agreement with the general model proposed by Stark (1999).

Second, the key point to keep in mind is that any dismissal of risk aversion as a key mechanism is more than that, since it is really about deciding *which general theoretical approach we shall take to explain religious belief and behavior*. One cannot simply complain that the risk-aversion hypothesis is incomplete by pointing to some arbitrarily constructed set of “other factors” that might be operating if there is not a sufficiently plausible theory that places these factors or processes as important determinants of religious behavior and belief. Thus, critics of risk aversion cannot simply dismiss it without dismissing the larger theoretical structure within which risk aversion as a mechanism explaining religious belief and behavior is embedded and outside of which it does not make much sense. This also means that rejecting the risk-aversion hypothesis without postulating an *alternative set of mechanisms that can account for the same range of phenomena in the religious realm (and possibly suggest the existence of new, heretofore unobserved phenomena)* would probably not result in much progress in the way of dealing with the puzzle in a scientifically productive manner.

We would like to conclude this response by thanking our colleagues for taking the time to craft such thorough and well-thought-out responses to our article. In that respect, we feel that our article has already done what it set out to do, that is, to reopen the debate on the complex issues regarding the social sources of religious behavior—a door that we believe might have otherwise been prematurely closed.

References

Albrecht, Stan L. and Tim B. Heaton. 1984. Secularization, higher education, and religiosity. *Review of Religious Research* 26(1):43–58.

Bradshaw, Matt and Christopher Ellison. 2009. The nature-nurture debate is over and both sides lost! Implications for understanding gender differences in religiosity. *Journal for the Scientific Study of Religion* 48(2):241–51.

Cornwall, Marie. 2009. Reifying sex difference isn’t the answer: Gendering processes, risk, and religioisty. *Journal for the Scientific Study of Religion* 48(2):252–55.

Freese, Jeremy. 2008. Genetics and the social science explanation of individual outcomes. *American Journal of Sociology* 114(Supplement):S1–S35.

Freese, Jeremy and James Montgomery. 2007. The devil made her do it: Evaluating risk preference as an explanation of sex differences in religiousness. *Advances in Group Processes* 24:187–230.

Hagan, John. 1987. Class in the household: A power-control theory of gender and delinquency. *American Journal of Sociology* 92(4):788–816.

Henrich, Joseph. 2008. A cultural species. In *Explaining culture scientifically*, edited by Melissa J. Brown, pp. 184–210. Seattle, WA: University of Washington.

Hoffmann, John. 2009. Gender risk and religiousness: Can power control provide the theory. *Journal for the Scientific Study of Religion* 48(2):232–40.

Miller, Alan and John Hoffmann. 1995. Risk and religion: An explanation of gender differences in religiosity. *Journal for the Scientific Study of Religion* 34(1):63–75.

Miller, Alan and Rodney Stark. 2002. Gender and religiousness: Can socialization explanations be saved? *American Journal of Sociology* 107(6):1399–1423.

Roth, Louise Marie and Jeffrey C. Kroll. 2007. Risky business: Assessing risk preference explanations for gender differences in religiosity. *American Sociological Review* 72(2):205–20.

Stark, Rodney. 1999. Micro foundations of religion: A revised theory. *Sociological Theory* 17(3): 264–89.

———. 2002. Physiology and faith: Addressing the “universal” gender difference in religious commitment. *Journal for the Scientific Study of Religion* 41(3):495–507.

Sullins, D. Paul. 2006. Gender and religion: Deconstructing universality, constructing complexity. *American Journal of Sociology* 112(3):838–80.