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Losing faith in the intelligence–religiosity link: New evidence for a decline effect, spatial dependence, and mediation by education and life quality

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ABSTRACT

Research has shown negative intelligence–religiosity associations among both persons (Zuckerman, Silberman, & Hall [*Personality and Social Psychology Review 17* (2013) 325–354]) and countries (Lynn, Harvey, & Nyborg [*Intelligence 37* (2009) 11–15]). Nevertheless, it remains unclear if these associations are stable over time or explained by education, quality of human conditions (QHC), or spatial dependence. In Study 1, we re-analyzed Zuckerman et al.'s meta-analysis, and after controlling for sample differences, the negative intelligence–religiosity link declined over time. The intelligence–religiosity link was non-significant among samples using men, pre-college participants, grade point average, and those collected after 2010. Education also partially mediated the intelligence–religiosity link. In Study 2, we re-analyzed Lynn et al.'s data from 137 countries and found that QHC positively moderated and partially mediated the positive relation between IQ and disbelief in God; this link became non-significant after controlling for spatial dependence (i.e., the extent to which adjacent countries reflect statistically non-independent observations). Although the negative intelligence–religiosity link appears more robust across people than countries, multiple variables moderate or mediate its strength, and hence, limit its generalizability across time, space, samples, measures, and levels of analysis.

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1. Introduction

Are intelligence and religiosity related? A recent meta-analysis of 63 studies (70,647 people) showed a significant negative intelligence–religiosity association (Zuckerman, Silberman, & Hall, 2013). In addition, a recent study of country-level data from 137 nations showed a strong positive association between IQ and disbelief in God (Lynn, Harvey, & Nyborg, 2009). Although these associations linking intelligence to religiosity at both the person and country level appear robust and are largely consistent with the notion that belief in God is unintelligent (Dawkins, 2006), they may be weaker in strength and less generalizable than believed. Specifically, key variables related to intelligence, religiosity, or both were left unexamined or not adequately modeled in these studies. For example, the country-level analysis ignored spatial dependence (statistical non-independence in geographical data), the meta-analysis ignored changes in the intelligence–religiosity link over time (decline effect; Schooler, 2011), and neither study formally tested mediation of this effect by individual differences in education or country-level differences in quality of life. In the present research, we re-analyzed data from M. Zuckerman et al. (2013) and Lynn et al. (2009) to test whether the intelligence–religiosity link is moderated and mediated, and the extent to which it generalizes across time, samples, measures, and levels of analysis.

1.1. Meta-analysis

M. Zuckerman et al.'s (2013) meta-analysis was groundbreaking because it was the first comprehensive quantitative synthesis of the intelligence–religiosity association. Specifically, they found that the random-effects unweighted-mean intelligence–religiosity correlation across 63 studies was −.16, whereas the fixed effects weighted-mean correlation across 62 studies (one study gave no sample size) was −.13, 95% CI [−.14, −.12]. Although small in magnitude (Cohen, 1988), these average effect sizes were robust and did not include zero. Correlations were also heterogeneous across studies. M. Zuckerman et al.'s meta-analysis represented a good faith effort to systematically quantify the intelligence–religiosity link across studies; however, it had at least five limitations: overreliance on both fixed effects and unweighted means, and no formal tests of publication bias, the decline effect over time, or meta-analytic mediation. In our re-analysis (Study 1), we address each of these concerns and show that the intelligence–religiosity link may be weaker and less generalizable than believed.

First, although M. Zuckerman et al. (2013) reported that unpublished (vs. published) studies did not moderate effect sizes, they included no statistical tests and provided no formal examination of publication bias (e.g., funnel plot, Egger's regression). Testing publication bias is important because it can help establish whether the published

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intelligence–religiosity literature suffers from a file-drawer problem (Rosenthal, 1979).

Second, M. Zuckerman et al.'s (2013) meta-analysis is limited by its overreliance on unweighted (vs. weighted) mean effects sizes, which treat all studies alike regardless of their sample sizes, and hence, precision (e.g., Ns of 20 and 14,277 had equal weight). The result is that imprecise effect sizes from small, underpowered studies have undue influence on the estimated mean effect size. For example, one study of 72 people produced an extreme intelligence–religiosity correlation of −.75 (Southern & Plant, 1968). In M. Zuckerman et al.'s unweighted meta-analysis, this study had the same weight as all other studies (including the 10 studies with over 1000 participants that yielded far more modest effect sizes). Indeed, using regression-based outlier analyses (see Judd, McClelland, & Ryan, 2009), our re-analyses of these data showed that this −.75 correlation was a significant outlier because it explained 23% of the unweighted mean effect size, despite that its sample of 72 people represented only 1% of the total sample ($N = 70{,}647$). Excluding this outlier decreased the unweighted-mean correlation from −.16 (M. Zuckerman et al.) to −.14. In contrast, using a weighted approach, this 72-person study was no longer an outlier using the full sample; it received relatively little weight because it was smaller than 87% of the included studies. In sum, weighted (vs. unweighted) approaches provide more precise and accurate mean effect size estimates (Card, 2012; Hunter & Schmidt, 2004).

Third, M. Zuckerman et al.'s (2013) meta-analysis is limited by its overreliance on fixed (vs. random) effects estimates. Specifically, random (vs. fixed) effects results are more conservative because they take between-study variance (τ^2) into account, and thus, mean effect size estimates have wider CIs. Random (vs. fixed) effects do not make a strong assumption regarding the homogeneity of effect sizes, and focus on estimating the true population parameter of all studies (or effects sizes), not just those sampled in the meta-analysis. Thus, random (vs. fixed) effects meta-analysis is the more general and less-restricted technique and has fewer assumptions. For these reasons, leading meta-analysts discourage using fixed effects meta-analysis simply because its assumptions are rarely met in practice (Card, 2012; Hunter & Schmidt, 2004; National Research Council, 1992; Schmidt, 2010). Moreover, random-effects meta-analysis can also be combined with meta-regression to examine study-level moderators in mixed-effects meta-analysis.

Fourth, M. Zuckerman et al.'s (2013) meta-analysis neglected to examine change in effect sizes over time (publication year). This is important because some meta-analyses have shown decline effects (Schooler, 2011), where effect sizes diminish over time (e.g., Fischer et al., 2011; Jennions & Møller, 2001; Webster, Graber, Gesselman, Crosier, & Schember, 2014; Wongupparaj, Kumari, & Morris, 2015). To their credit, M. Zuckerman et al. did identify and test several other study-level variables that partly contributed to the heterogeneity in intelligence–religiosity effect sizes, including gender (proportion of males), methodological differences, various intelligence and religiosity measures, and age-related sample type. Nevertheless, whether the intelligence–religiosity association is stable over time or shows a decline effect remains unexamined.

Fifth, although M. Zuckerman et al. (2013) correctly caution that their meta-analytic data are purely correlational, they test other researchers' theoretically driven causal models involving intelligence, education, and religiosity. Specifically, some researchers suggest that education may mediate the intelligence–religiosity link (Hoge, 1974; Reeve & Basalik, 2011); others suggest that intelligence may mediate the education–religiosity link (S. Kanazawa, January 2012, personal communication cited in M. Zuckerman et al.). Nevertheless, the methods used by M. Zuckerman et al. were informal and relied on unweighted analyses, which can be problematic (as noted above). In our re-analyses, we use formal multivariate meta-analytic procedures (Card, 2012) along with 95% CIs (Funder et al., 2014) to test these proposed mediation models.

1.2. Country-level data

In their study of country-level data from 137 nations, Lynn et al. (2009) showed that country-level IQ related positively to disbelief in God ($r = .60$, 95% CI [.48, .70]), a strong effect size (Cohen, 1988). Lynn et al.'s study was itself a secondary analysis of country-level data, combining intelligence data (IQ) from Lynn and Vanhanen (2006) with religiosity data (disbelief in God) from P. Zuckerman (2007). The possible country-level relations among IQ, wealth, and inequality remain a controversial and politically polarizing issue for multiple reasons (e.g., Kanazawa, 2008), including the validity of country-level IQ measures (Hunt & Wittmann, 2008; Wicherts, Borsboom, & Dolan, 2010; Wicherts, Dolan, & van der Maas, 2010; Wicherts, Dolan, Carlson, & van der Maas, 2010). These problems notwithstanding, because our goal was to re-analyze (vs. collect) data, we have taken their data at face value, however imperfect they may be.

Lynn et al.'s (2009) analyses, however, present their own limitations. First, Lynn et al.'s (2009; Zuckerman, 2007) atheism or disbelief-in-God data were highly positively skewed. Because correlation/regression analysis assumes error distributions to be normal, homogeneous, and independent (Cohen, Cohen, West, & Aiken, 2003; Judd et al., 2009), analyses based on positively skewed country-level disbelief-in-God data likely violated all three assumptions. We believe that Lynn et al. should have corrected this skew by log-transforming P. Zuckerman's atheism data prior to analysis, which addresses both normality and homogeneity concerns. Second, because country-level data often show non-independence (Ward & Gleditsch, 2008), we examined and controlled for spatial dependence in spatial regression analyses. Third, although Lynn and Vanhanen (2002, 2006) collected country-level data on a potentially key variable—the quality of human conditions (QHC) index—Lynn et al. chose not to examine it as a moderator or mediator in their analysis of the IQ–disbelief-in-God association. We do both in our re-analysis of these data while using log-transformed disbelief-in-God data.

1.3. The present research

Re-analyzing data from both studies is important for multiple reasons. First, from a societal standpoint, a majority of Americans believe in God, are members of religious communities, and view religion as important in their lives (Gallup, 2015). This makes understanding the intelligence–religiosity link a hot-button topic, and it is unsurprising that at least one of these study's findings received international media attention, and both studies together have garnered 169 citations to date (Google Scholar, December 1, 2015). Second, from a scholarly standpoint, understanding *how* and *for whom* these variables relate are necessary steps toward constructing and advancing broader theoretical models. Fortunately, both studies provided ample data to conduct these additional analyses.

2. Study 1: Meta-analysis

2.1. Method

Using summary data from M. Zuckerman et al.'s (2013) Table 1, we conducted the first formal tests of publication bias in these data by inspecting funnel plot symmetry and using Egger's test (Egger, Smith, Schneider, & Minder, 1997); both examine effect sizes as a function of their sample size (or precision).

Next, we re-analyzed M. Zuckerman et al.'s (2013) meta-analytic data using weighted random- and mixed-effects meta-regressions with maximum likelihood estimation (see Card, 2012, pp. 245–249; Hadden, Smith, & Webster, 2014) in Mplus 6.1 (Muthén & Muthén, 2010). We used planned contrast codes (see Cohen et al., 2003; Judd et al., 2009) to examine and control for study-level moderators of interest. We coded the three ordinal sample categories—pre-college, college, and non-college—into two age variables using linear (−0.5, 0.0, 0.5) and quadratic (0.33, −0.67, 0.33) contrasts. Similarly, because some

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