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The Negative Intelligence–Religiosity Relation: New and Confirming Evidence

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Abstract

Zuckerman et al. (2013) conducted a meta-analysis of 63 studies that showed a negative intelligence–religiosity relation (IRR). As more studies have become available and because some of Zuckerman et al.'s (2013) conclusions have been challenged, we conducted a new meta-analysis with an updated data set of 83 studies. Confirming previous conclusions, the new analysis showed that the correlation between intelligence and religious beliefs in college and noncollege samples ranged from −.20 to −.23. There was no support for mediation of the IRR by education but there was support for partial mediation by analytic cognitive style. Thus, one possible interpretation for the IRR is that intelligent people are more likely to use analytic style (i.e., approach problems more rationally). An alternative (and less interesting) reason for the mediation is that tests of both intelligence and analytic style assess cognitive ability. Additional empirical and theoretical work is needed to resolve this issue.

Keywords
intelligence, religiosity, meta-analysis, analytic thinking

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Are intelligence and religiosity negatively related? This question has been asked in many studies, as early as 1928, and has been the subject of a meta-analysis of over 60 studies (Zuckerman, Silberman, & Hall, 2013). Zuckerman et al. (2013) found a negative correlation between intelligence, measured with conventional psychometric tests, and religiosity, measured in terms of religious beliefs. For college students and the general population, the mean correlation between intelligence and religious beliefs ranged from −.20 to −.25; for precollege youth and when religiosity was assessed by religious behaviors and religious affiliation (as opposed to strength of beliefs), the corresponding correlations were weaker. Zuckerman et al. (2013) also addressed several potential mediators and moderators of the overall effect and discussed several possible explanations for why the intelligence–religiosity relation (IRR) is negative.

It is not surprising that evidence suggesting that more religious people are not as intelligent as less religious people met with mixed reactions. Comments in the media ranged from expressions of surprise and curiosity to skepticism or even disdain about what intelligence tests actually measure. Although it is only 6 years after the original work was published, we thought it is worthy of another careful evaluation. Our main purpose is to expand the evidence base to retest the validity of Zuckerman et al.'s (2013) conclusions and to evaluate their explanations. Collecting new data to ascertain the validity of previous findings is crucial for science anytime, but especially when the subject matter is socially relevant and emotionally fraught.

We addressed four major questions in the present article. First, the previous finding of a correlation of −.20 to −.25 between intelligence and religiosity was established for a subset of 35 studies in the previous work (following data cleaning procedures and various exclusions, to be explained later). Compared with many meta-analyses, this data set is small. The smaller the data set, the greater the chance that the findings will be overturned when new studies appear. Expanding the data set may put doubts about the size and direction of the IRR to rest.

Second, Zuckerman et al. (2013) discovered clear boundary conditions for their main findings; for example, the negative IRR is more true for religious beliefs (as opposed to religious behaviors) and for college and noncollege samples

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(as opposed to precollege samples). It is important, we believe, to retest the validity of these conditions.

Third, Zuckerman et al. (2013) examined two possible mediators of a negative IRR: education and analytic style. There was no empirical support for mediation by education but this null result was based on only six studies. There was support for mediation by analytic style but this result was based on only two studies. We thought it was imperative to put these findings on a firmer empirical base because mediation by either education or analytic style can provide a convincing explanation for why the IRR is negative.

Fourth, Webster and Duffy (2016, Study 1) argued that a reanalysis of the Zuckerman et al. (2013) data showed that a negative IRR was obtained only in samples of women and only in studies dating before 2010, and that education did in fact mediate the IRR. They were also critical of the statistical models that were used in our previous work. We will address these claims as they pertain to the Zuckerman et al. (2013) meta-analysis, but especially in light of the expanded data set.

Method

The original 62-study data set[1] was supplemented by 21 additional studies that for the most part date from 2011 to 2018. We used this entire data set ($k = 83$) to examine the IRR in terms of magnitude and possible interpretations.

Collecting 20 New Studies

Search. As Zuckerman et al. (2013) collected data up to 2012, the present search covered the period from 2012 to 2018, using methods similar to those of the earlier work. We searched for articles in PsycINFO and Google Scholar using two sets of terms, one that covered intelligence (intelligence, IQ, and cognitive ability) and one that covered religiosity (religiosity, religion, and religious beliefs). This search yielded 2,470 records (there were no duplicates). As articles on intelligence sometimes include religiosity as a control variable, we also inspected 605 articles that appeared in the journal *Intelligence* from 2012 to 2018. Finally, reference lists in articles that were identified by any of these methods were also searched.

Inclusion/exclusion criteria. The primary inclusion criterion was that a study must have examined intelligence and religiosity *at the individual level* and reported Pearson r or some other statistics from which Pearson r could be computed. Authors were contacted for relevant information if it appeared that intelligence and religiosity were measured but their relation was not reported (asterisks in the relevant tables denote information that was obtained by personal communication). Studies that reported correlations between intelligence and religiosity at the aggregate level (e.g., countries) were not included.

The studies and their characteristics are presented in Table S1 (supplementary materials) in a format that is similar to the corresponding Table 1 in Zuckerman et al. (2013). If results for more than one independent sample were reported in a particular source (e.g., article, unpublished dissertation), they were considered as separate studies. Altogether, there were 20 studies from 19 sources. Those sources are marked by asterisks in the "References" section.

Coding

The coding process was identical to Zuckerman et al. (2013). We coded the proportion of males in each study, the type of intelligence measure, and the type of religiosity measure. As in Zuckerman et al. (2013), there were too few studies associated with a particular intelligence measure to allow meaningful analysis of particular measures. Religiosity was coded as religious belief, religious practice, membership, and mixed. The "mixed" category implies that the study provided correlations between intelligence and both belief and practice/membership measures; in such cases, Table S1 provides both the average mixed correlation and (in footnotes) the separate correlations.

Studies were classified as investigating precollege, college, or noncollege samples. Precollege participants were aged younger than 18 years; college participants were undergraduates; noncollege participants were recruited outside academic contexts and tended to be older than the college participants. The prevalence of online studies in recent years greatly increased the percentage of studies with noncollege samples (27% in Zuckerman et al., 2013; 65% of the newly collected studies).

We also coded studies for two potential sources of bias: time gap and extreme groups. A third potential source of bias—restricted range—was coded in Zuckerman et al. (2013) but did not occur in any of the 20 new studies (except for studies with college students who presumably score higher on cognitive ability tests than the rest of the population—more on this topic in the "Results" section). Time gap implies that the intelligence and religiosity measures were not administered at the same time; contrary to expectations, however, neither Zuckerman et al. (2013) nor the present analysis found that time gap moderated the IRR. Studies that we call "extreme groups" compared participants very high in intelligence (or religiosity) with participants low in intelligence (or religiosity); studies in this group inflated the IRR in the Zuckerman et al. (2013) analysis.

Finally, we coded whether the data originated from published or unpublished studies. The latter category included one study (Clark, 2004) in an undergraduate journal.

An effect size r was extracted from each study. When multiple correlations were available due to the use of different intelligent measures, the average was computed and presented in Table S1.

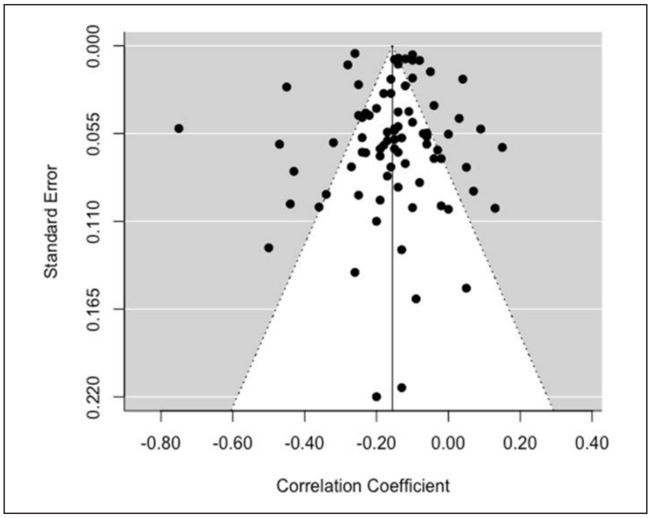

Figure 1. Funnel plot of the 83 correlations between intelligence and religiosity.

Corrections to the Zuckerman et al. (2013) Data Set

In going over the studies in Zuckerman et al.'s (2013, Table 1) meta-analysis, we determined that we erred in designating Sherkat's (2011) scientific literacy scale as an intelligence measure. The reason is that 10 of the scale's 13 items assess science knowledge (e.g., understanding that antibiotics do not kill viruses). Recently, McPhetres and Zuckerman (2018) found that religiosity is negatively related to science knowledge and that the relation is partially mediated by negative attitudes toward science. In other words, scientific literacy is at least partially reflective of attitudes toward science.

The remaining three items in Sherkat's (2011) scientific literacy scale can be divided into two parts—understanding the need for a control group in medical studies (one two-part question) and understanding probabilities (two questions); both can be viewed as components of an intelligence measure. In addition, the 2006 General Social Survey (GSS)—the source of data for Sherkat's (2011) study—included a 10-word test (Wordsum) of verbal intelligence. Accordingly, we *z* scored the control group question, the probabilities questions, and the Wordsum test, and combined them into a single intelligence measure. We also constructed a two-item religious belief measure (assessing belief in God and whether respondents consider themselves religious) and another two-item religious practice measure (assessing frequency of praying and frequency of attending religious services). The line before last in Table S1 presents the information relevant to these data.

Finally, the last line in Table S1 presents data for Wahlig (2005), which was cited in Zuckerman et al. (2013) but mistakenly excluded from the analyses.

Data Analysis

To ensure that our analysis is comparable to that employed in Webster and Duffy's (2016, Study 1) critique, we used their meta-analytic model, namely, meta-regressions based on weighted random/mixed effects model with maximum likelihood estimation. We used several software packages. In the first part of the "Results" section, we used the *p* curve Web App, Version 4.05 (Simonsohn, Nelson, & Simmons, 2015) for *p* curve analysis. Computations of other types of publication bias, estimations of meta-analytic means, and both simple and multiple moderation analyses were conducted with R (Version 3.3.1), using the metafor package (Viechtbauer, 2010).

For mediation analyses, we used the R package metaSEM 1.1.0 (Cheung, 2015), which uses random-effects meta-analytic structural equation modeling (MASEM; Cheung, 2007; Cheung & Chan, 2005; Cheung & Hong, 2017). The models were constructed in two stages. First, a pooled random-effects correlation matrix was estimated. Second, the weighted least square estimation method was used to fit structural equation models based on the pooled correlation matrix and its covariance matrix. The mediation was derived from the SEM parameters. This method takes into account covariance among correlations and it avoids using ad hoc sample size (e.g., arithmetic mean and total of individual sample sizes) to estimate *SE*. Importantly, we used likelihood-based confidence interval (CI) to estimate indirect effects (Cheung, 2007), thus capturing the asymmetric nature of the distribution of the indirect *ab* effect (e.g., intelligence → education → religious beliefs) in small samples. Thus, the estimated value of the effect is not necessarily at the midpoint of the estimated CI.

Results

The overall data set, before data cleaning, included 83 studies: 61 studies from the previous meta-analysis (all the studies except Sherkat, 2011), 20 new studies, replacement data for Sherkat (2011), and the Wahlig (2005) study. First, we present analyses involving these 83 studies and then describe the data cleaning that resulted in a final data set of 62 studies.

Overall Data Set (83 Studies)

Publication bias. The funnel plot (Figure 1) appears symmetric, indicating that publication bias was unlikely. Egger's regression, a more formal test of publication bias, was not significant, $p = .74$. The *p* curve analysis produced no evidence of *p* hacking (running studies until they reach significance) as the distribution of *p* values did not show an increase in frequency just below the .05 level (see Figure 2). More formal tests also supported this conclusion. First, the

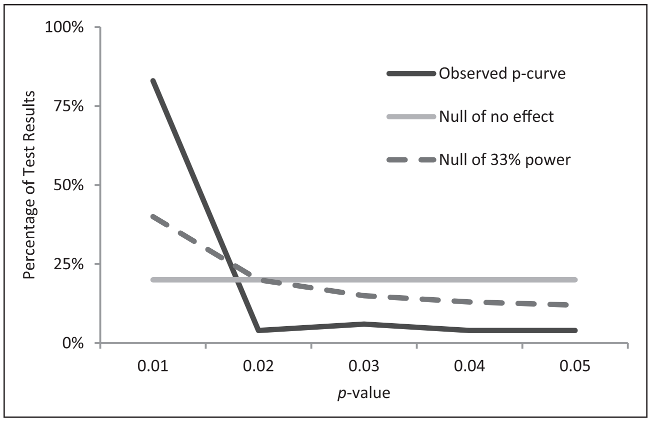

Figure 2. *p* curve plot.
Note. The *p* curve includes 53 statistically significant ($p < .05$) effect sizes (out of 83 total) of which 48 are $p < .025$.

distribution of *p* values was right-skewed ($z = -25.28$, $p < .001$), indicating that the effect is real. Second, the distribution of *p* values was not flatter than what one would expect at 33% power ($z = 17.73$, $p > .99$), again supporting the evidential value of the studies. Third, the power leading to an expected *p* curve that most closely resembles the observed *p* curve was .99.

Finally, the difference in the IRR between published studies ($k = 70$, $r = -.15$, [−.18, −.12], $p < .001$) and unpublished studies ($k = 13$, $r = -.19$, [−.24, −.14], $p < .001$) was not significant, $b = .05$, [−.04, .13], $p > .25$. Note that the direction of the difference (unpublished studies yielded a more negative IRR) is opposite to the direction of a possible bias in favor of publishing results supporting a negative IRR.

Overall IRR (83 studies). Preliminary analysis, before data cleaning, showed that the 83-study data set yielded a significant negative IRR, $r = -.16$ [−.19, −.13], $p < .001$, and significant heterogeneity of effect sizes, which largely reflects true variability ($Q = 1{,}437.0$, $p < .001$; $I^2 = 97.02\%$); 73 (84%) effect sizes were negative and 10 were positive. Of the 20 new studies, one (Clark, 2004) predated 2010 and the remainder came out in 2011 or afterward. This latter group also yielded a significant IRR ($r = -.18$ [−.22, −.15], $p < .001$, $Q = 711.4$, $p < .001$, $I^2 = 95.04\%$). It is not the case that the IRR no longer exists, as was suggested by Webster and Duffy (2016, Study 1).

Data cleaning of the 83-study data set. We first tested those moderator effects that can be viewed as biases; these were the effects of using an extreme-groups design (which might inflate the magnitude of the IRR), using grade point average (GPA) as an intelligence measure (which is not a valid measure of the construct; see Zuckerman et al., 2013), and using religious membership/religious practice as a religiosity measure (because they are subject to influences that are extrinsic to religious belief). In simple moderation analyses, we found a significant difference between studies using the extreme groups design ($k = 6$) and the remainder of the studies, $b = -.14$ [−.19, −.09], $p < .001$; simple effects analysis showed a much more negative IRR for extreme groups ($r = -.42$ [−.52, −.32], $p < .001$) than for the remainder ($r = -.14$ [−.16, −.11], $p < .001$).

The moderator effect for GPA was examined in two simultaneous contrasts, linear (GPA = −1, mixture of GPA and more conventional intelligence measures = 0, and non-GPA= 1) and quadratic (−1, +2, and −1, respectively). The linear contrast was significant ($b = .09$ [.02, .15], $p = .007$), whereas the quadratic contrast was not, $p > .25$. Simple effects analysis showed that the IRR became more negative as a function of moving from GPA ($r = -.00$ [−.12, .11], $p > .25$), to mixed ($r = -.09$ [−.15, −.03], $p = .004$), and then to non-GPA ($r = -.18$ [−.23, −.12], $p < .001$).

The moderator effect for the religiosity measures was also examined with two contrasts, linear (behaviors = −1, mixture of behaviors and beliefs = 0, and beliefs = 1) and quadratic (−1, 2, and −1, respectively). The linear contrast was significant ($b = -.06$ [−.10, −.01], $p = .009$), whereas the quadratic contrast was not, $p > .25$. Simple effects analysis showed a linear decrease (IRR becoming more negative) from behaviors ($r = -.07$ [−.14, .00], $p = .06$) to mixed ($r = -.13$ [−.16, −.09], $p < .001$), and then to beliefs ($r = -.19$ [−.23, −.15], $p < .001$).

Additional insight can be gained from inspection of studies in the mixed category as those reported the IRR separately for religious beliefs and religious behaviors. Orthogonal to the linear contrast of measure reported above, a multilevel (within-studies) analysis of the mixed studies showed that correlations between intelligence and religious beliefs were significantly more negative than correlations between intelligence and religious behaviors, $b = -.17$ [−.19, −.16], $p < .001$. Simple effects for beliefs and behaviors, respectively, were $r = -.21$ [−.25, −.16], $p < .001$, and $r = -.04$ [−.08, .00], $p = .12$.

The moderators were not independent of one another (see intercorrelations in Table S2 in supplementary materials). Still, testing the multiple moderation of all five contrasts (extreme groups, linear and quadratic GPA, and linear and quadratic religiosity measures) simultaneously showed that the effects that were obtained in simple moderation analyses remained significant: extreme groups, $b = -.12$ [−.17, −.08], $p < .001$; linear GPA, $b = .08$ [.03, .13], $p < .003$; and linear religiosity measure, $b = -.05$ [−.09, −.01], $p = .01$. This indicates that the effects were independent of one another.

Owing to these results, we deleted from analyses all studies comparing extreme groups, all studies using GPA as a sole measure of intelligence, and all studies using membership/behavior as a sole measure of religiosity. We retained studies that used other cognitive abilities beside GPA and measures of religiosity other than religious behaviors. This procedure resulted in a 62-study final data set, markedly

Table 1. Effect Sizes of the Relation Between Intelligence and Religious Beliefs for Precollege, College, and Noncollege Samples.

Sample	k	r	95% CI	Q	I^2 (%)
Precollege	11	−.07**	[−.12, −.03]	41.6	68.1
College	29	−.18***	[−.22, −.14]	85.3	67.7
Noncollege	22	−.20***	[−.24, −.17]	609.7	94.7

Note. CI = confidence interval.
**$p < .01$. ***$p < .001$.

larger than the 45-study data set that Zuckerman et al. (2013) analyzed after an identical data cleaning procedure. Note that it is coincidental that the k of studies in the older 2013 data set (62 before data cleaning) is identical to the k of studies in the final data set (62 after data cleaning). All references below to the 62 studies or the 62-study data set refer to the total, cleaned data set we have just described.

Final Data Set (62 Studies)

Using the final 62-study data set, we repeated some tests of publication bias: Egger's regression was not significant, $p >$ .25, and the difference in the IRR between published and unpublished papers was also not significant, $b = .04$, [−.02, .11], $p > .22$ (as in the uncleaned, 83-study set, unpublished papers reported a more negative IRR than published papers). As studies whose design distorted the size of the IRR have been removed, it was now appropriate to screen for outliers both for the overall sample and for the moderation analyses that followed. We defined outliers as effect sizes that were identified by metafor as having standardized residual values exceeding 2.5 and Cook's distance values exceeding .05 (Cohen, Cohen, West, & Aiken, 2003). This is a conservative estimate as Cohen et al. (2003) suggested using Cook's distance value of > 1. No outliers were identified in any of the searches.

The final 62-study data set included 59 (95%) negative correlations, two positive correlations, and one correlation of .00. The overall IRR was significant, $r = -.17$ [−.20, −.14], $p < .001$, $Q = 989.2$, $I^2 = 93.1\%$. The extremely large I^2 (75% or greater is considered large; Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006) indicates that most of the heterogeneity among the effect sizes reflects study-level factors (moderators) rather than sampling error. Below, we examine factors that might account for this heterogeneity.

Tests of Moderation

Moderation by age. The final 62-study data set was comprised of precollege, college, and noncollege samples. We examined moderation effects of two age contrasts, linear (precollege = −1, college = 0, and noncollege = 1) and quadratic (−1, 2, and −1, respectively). The linear contrast was significant ($b = -.06$ [−.10, −.03], $p < .001$), whereas the quadratic contrast was not, $p = .13$. However, rather than present the simple effects for each sample, we calculated the overall IRR for each sample of studies independently (see Table 1). Our rationale is that the the three groups of samples differ qualitatively from one another and, as such, do not really form a coherent theoretical continuum; of course, the results in Table 1 are very similar to what was obtained in simple effects analyses.

The IRR at precollege was relatively small ($r = -.07$). Zuckerman et al. (2013) noted that that religious beliefs are not fully formed in precollege ages, and that religiosity at this time of life is a weak predictor of religiosity in adulthood (O'Connor, Hoge, & Alexander, 2002). The IRR was stronger in college samples ($r = -.18$) but, as intelligence in college samples is range restricted, the empirical result is an underestimate. To get a more accurate representation of the actual IRR, we used Thorndike's (1949) Case 2 formula, which relies on the ratio between the unrestricted and restricted *SD*s of the variable (intelligence) under consideration.[2] Sackett, Kuncel, Arneson, Cooper, and Waters (2009; P. R. Sackett, personal communication, May 2012) arrived at a 1/.67 ratio for students who applied to but did not attend college and students who applied and attended. The ratio was derived from three cohorts (1995-1997) of students who applied to 41 colleges and universities in the United States. Note that the ratio may be conservative as it is based on samples of SAT test takers rather than the entire population.[3]

We first used the Thorndike (1949) formula to correct the correlation between intelligence and religious beliefs in each college sample, and then used the corrected correlations to estimate the overall IRR: $r = -.23$ [−.27, −.14], $p < .001$. As the meta-analytic mean r for the noncollege samples was −.20 (see Table 1), we estimate that the correlation between intelligence and religious beliefs for college plus noncollege samples ($k = 51$) ranges from −.20 to −.23. The comparable estimate in the earlier meta-analysis (Zuckerman et al., 2013), also for college plus noncollege samples ($k = 35$), was −.20 to −.25. Having increased the earlier data set by 16 studies (46%), it was gratifying to find an almost identical result, leading us to the conclusion that the negative IRR is now firmly established.

Is the IRR moderated by gender and year? Recall that Webster and Duffy (2016, Study 1) asserted that the negative IRR is limited to female samples and to studies dated before 2010. A close inspection indicates that neither claim has merit.

In a simple between-studies moderator analysis, Zuckerman et al. (2013) had already found that the IRR was more negative in studies with more females. Webster and Duffy (2016, Study 1) identified the same effect and reached the conclusion that the IRR was negative only in females. However, because a meta-analytic *between-studies* moderation effect can be confounded with other study characteristics, meta-analysts are routinely cautioned not to be misled by moderator effects that do not mean what they appear superficially to mean (e.g., Hall, Tickle-Degnen, Rosenthal, & Mosteller, 1993; Lipsey & Wilson, 2001).

Sensitive to this issue, Zuckerman et al. (2013) proceeded to test for gender moderation in studies that offered *within-studies* gender comparisons, which control much better for potentially confounding variables. Analyses of two studies (Kanazawa, 2010; combined $N = 21{,}437$, about 30% of the entire population of the data set) showed no difference in size of negative IRR between males and females. It was thus concluded that the between-studies gender moderation effect was an artifact of variables that were confounded with the proportions of men and women in the samples.

In the final 62-study data set, we were able to conduct within-study comparisons between males and females for two additional large studies: the 2006 GSS data ($N = 2{,}379$; see Table S1 under “Correction to Sherkat, 2006”), and the Zuckerman and McPhetres (2016) data ($N = 1{,}477$). In the former, the intelligence–religiosity correlations were −.12 for males and −.16 for females; in the latter, the correlations were −.26 for males and −.24 for females. Across the two new and the two previous Kanazawa studies (combined $N = 25{,}293$), the correlations for males range from −.12 to −.26 (weighted $M = -.17$, unweighted $M = -.14$), and the correlations for females range from −.11 to −.24 (weighted $M = -.16$, unweighted $M = -.13$). These data offer no support for the notion that the negative IRR is limited to, or stronger among, females.

Webster and Duffy’s (2016, Study 1) assertion that the negative IRR is limited to studies dating before 2010 was also based on moderation analyses with year of study as the moderator. However, the year moderator effect was not significant when tested alone ($p > .25$) and became significant ($p = .004$) only when gender was added as another moderator. An effect that is significant only when controlling for a spurious effect is itself spurious (e.g., if only because the moderator effect of year was obtained after gender has moved variance from the unexplained to the explained column for unwarranted reason).

It is the nature of spurious effects to disappear or weaken when new data are added. We, therefore, predicted that both gender and year moderator effects would weaken or disappear when examined in the new data set. Simple moderation analyses of the 51-study data set (the basis for our conclusions that correlations between intelligence and religiosity range from −.20 to −.23) indicated that studies with higher proportions of males and studies published more recently tended to produce less negative IRRs, but that the effects were not significant, $b = .08$, $p = .41$, and $b = .00$, $p = .81$, respectively. In multiple moderation analyses, testing the effects of age (college vs. noncollege), year, and gender, the effect of age was negative but not significant, $p = .15$; the effect of gender was not significant, $b = .15$ [−.06, .35], $p = .16$; and the effect of year approached significance, $b = .0015$ [−.0001, .0031], $p = .06$.

Why did the multiple moderation produce stronger effects for year and gender? Year and gender were both positively related to IRR in simple moderation but negatively related to each other ($r = -.31$). Thus, each acted as a suppressor, augmenting the effect of the other factor when both were entered into the equation. In view of this latter result of the between-studies moderator analysis, and particularly because within-studies comparisons showed no evidence of gender differences, we can safely conclude that neither gender nor year of study moderated the IRR.

Tests of Mediation

Mediation by education. Webster and Duffy (2016, Study 1) showed that education significantly mediated the IRR, but that controlling for education reduced the association between intelligence and religiosity from −.13 to only −.12, leaving room for skepticism about this claim. In addition, the analysis was based on only seven studies (one of which should have been excluded, see rationale below).

The final, 62-study data set included eight new studies with relevant data on this question, allowing us to reexamine this mediation with greater statistical power. Recall, however, that our conclusions about the IRR concerned religious beliefs, not behaviors. Furthermore, the logic we used to explain why intelligence is more strongly related to religious beliefs than to religious behaviors might apply also to education. Of the 15 studies with relevant data, nine provided separate correlations between education and religious beliefs and between education and religious behavior (see Table S3). One of the studies, however (Blanchard-Fields, Hertzog, Stein, & Pak, 2001, Study 1), was conducted with college students and, therefore, does not provide meaningful data for the education variable; unless noted otherwise, this study was not included in the following analyses.

We first examined whether education is more highly related to religious beliefs than to religious behaviors. A multilevel analysis yielded a significant difference ($b = -.10$ [−.14, −.06], $p < .001$), and simple effects analysis showed that the education–religious beliefs association ($r = -.10$ [−.15, −.05], $p < .001$) was more negative than the education–religious behavior association ($r = .00$ [−.05, .05], $p > .25$). Testing the difference between the two types of correlations, but now including Blanchard-Fields et al.’s (2001) Study 1, produced highly similar results, $b = -.09$, $p < .001$. Accordingly, we proceeded to test two mediation models, intelligence → education → religious beliefs and education → intelligence → religious beliefs.

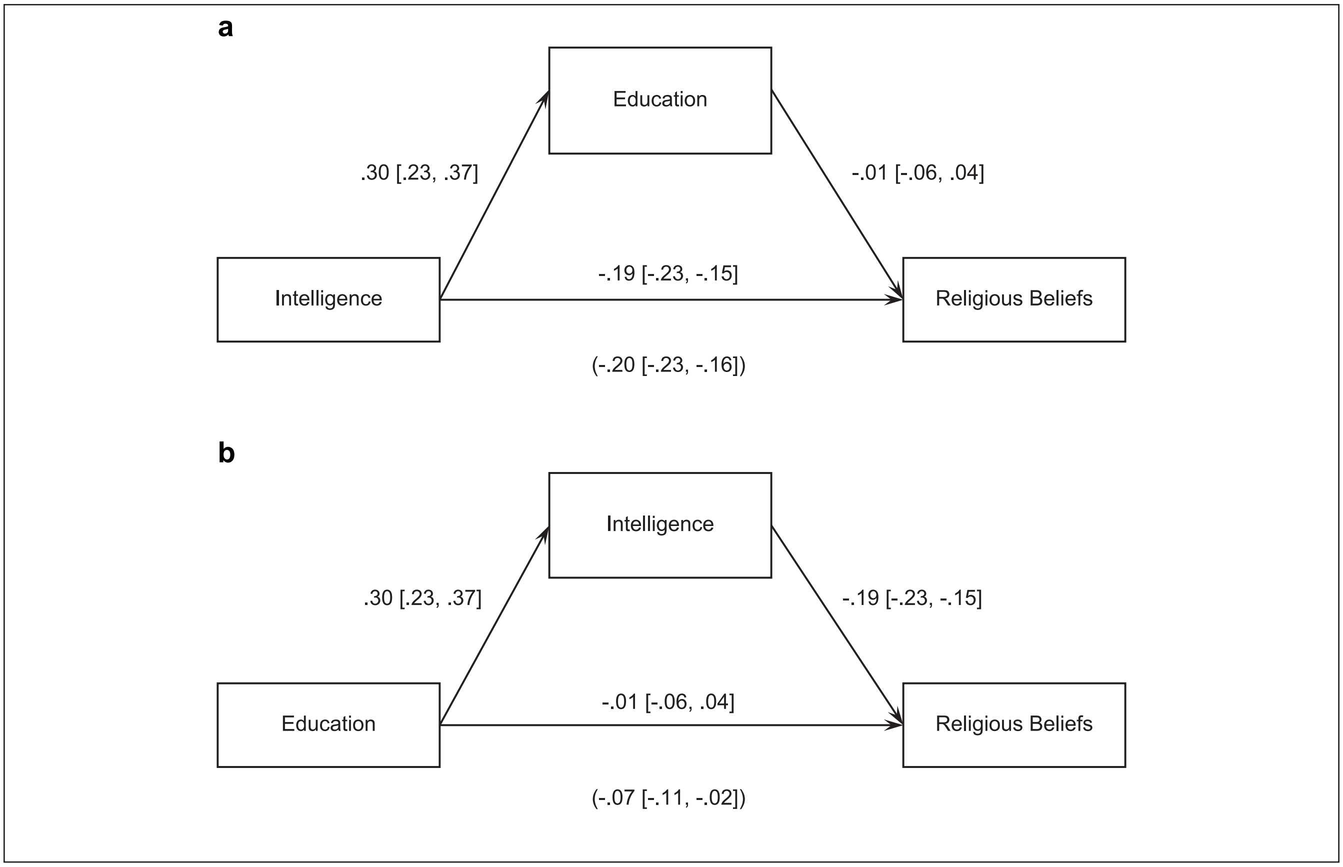

Figure 3. Mediation paths from metaSEM ($k = 14$).
Note. The intelligence → education → religious beliefs mediation is shown in "a," and the education → intelligence → religious beliefs mediation is shown in "b." Numbers are standardized correlation coefficients. The unmediated direct effects appear in brackets.

We first calculated the meta-analytic correlation between intelligence and religious beliefs, and the corresponding partial correlation (controlling for education); both were significant, $r = -.20$ [−.23, −.16], $p < .001$, and $r = -.18$ [−.22, −.14], $p < .001$, respectively (Table S4 in the supplementary materials provides data for each of the 15 studies). The small drop from the zero-order to the partial correlation suggests that education did very little in terms of accounting for the IRR. In contrast, the meta-analytic zero-order correlation between education and religious beliefs was significant, $r = -.07$ [−.11, −.02], $p = .003$, but the corresponding partial correlation (controlling for intelligence) was not ($r = -.00$ [−.05, .04], $p > .25$). This pattern suggests that the weak but significant relation of education with religiosity is fully mediated by intelligence. As could be expected, intelligence and education were positively correlated, $r = .30$ [.23, .37], $p < .001$.

The formal mediation analyses confirmed these suggestions. There was no support for the intelligence → education → religious beliefs model; the indirect effect was not significant, $b = -.00$ [−.02, .01]. Indeed, controlling for education only minimally reduced the relation between intelligence and religious beliefs from $b = -.20$ to $b = -.19$ (see upper part of Figure 3). Conducting this mediation analysis with all 15 studies (including the Blanchard-Fields et al., 2001) produced an identical indirect effect and identical conclusions.[4]

Testing the education → intelligence → religious beliefs model showed that intelligence fully mediated the education–religious beliefs association; the indirect effect was significant ($b = -.06$ [−.08, −.04]), and that controlling for intelligence reduced the direct relation between education and religious beliefs from $b = -.07$ to $b = -.01$ (the former coefficient was significant but the latter coefficient was not; see lower part of Figure 3).

Mediation by cognitive style. Another possible mediational path involves cognitive style, which refers to the distinction between analytic and intuitive thinking (also referred to as System 2 and System 1, respectively; Epstein, 1994; Kahneman, 2003; Stanovich & West, 2000). Analytic or System 2 thinking is controlled and systematic, whereas intuitive or System 1 thinking is reflexive, mostly unconscious, and heuristic based. Frederick's (2005) three-problem cognitive reflection test (CRT) was the first performance test of analytic style; each problem requires respondents to choose between a correct but intuitively unattractive solution and an intuitive incorrect solution (a number of alternative CRTs are now

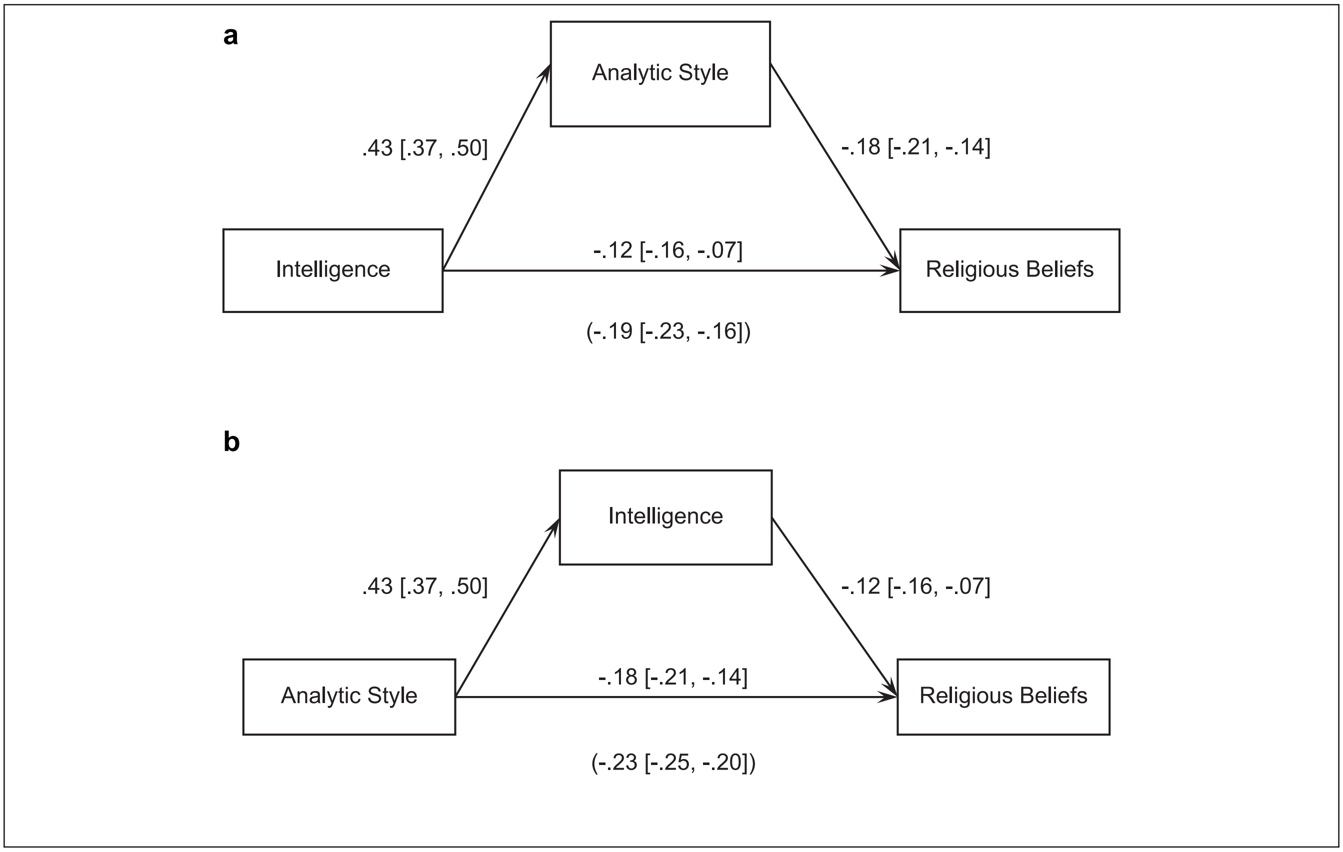

Figure 4. Mediation paths from metaSEM ($k = 13$).
Note. The intelligence → analytic style → religious beliefs mediation is shown in "a," and the analytic style → intelligence → religious beliefs mediation is shown in "b." Numbers are standardized correlation coefficients. The unmediated direct effects appear in brackets.

available, see note in Table S6 in supplementary materials). The prevalent scoring system is to consider the number of correct answers as an indicator of analytic style. There is strong empirical evidence that analytic style is negatively related to religiosity (see meta-analysis by Pennycook, Ross, Koehler, & Fugelsang, 2016). Building on this relation and assuming a relation between intelligence and higher CRT scores, Zuckerman et al. (2013) suggested that analytic scores on the CRT account for the IRR. They presented empirical evidence from two studies in support of this prediction. The final, 62-study data set included 13 new studies with relevant data on this question, allowing a mediation testing with greater statistical power.

First, however, we again examined whether we should target general religiosity or religious beliefs as the dependent variable. Seven of the 13 studies provided correlations of analytic style with both religious beliefs and behaviors (see Table S5 in supplementary materials). A multilevel analysis of these studies indicated that analytic style–beliefs correlations were more negative than analytic style–behaviors correlations, $b = -.09$ [−.14, −.04], $p < .001$); simple effect analyses yielded $r = -.24$ [−.27, −.20], $p < .001$, for the analytic style–beliefs association, and $r = -.15$ [−.19, −.11], $p < .001$, for the analytic style–behaviors association. We, therefore, tested two mediation models, intelligence → analytic style → religious beliefs and analytic style → intelligence → religious beliefs.

The meta-analytic zero-order correlation between intelligence and religious beliefs was significant, $r = -.20$ [−.23, −.16], $p < .001$, as was the corresponding partial correlation (controlling for analytic style), $r = -.11$ [−.15, −.07], $p < .001$ (Table S6 in supplementary materials provides data for each of the 13 studies). The zero-order and partial correlations of analytic style with religious beliefs were also significant, $r = -.23$ [−.26, −.21], $p < .001$, and $r = -.17$ [−.21, −.13], $p < .001$, respectively. That the partial correlations were smaller than the zero-order correlations in both cases suggests that intelligence and cognitive style each partially mediates the other's relation with religious beliefs.

Formal mediation analyses confirmed these suggestions. The indirect effects for intelligence → analytic style → religious beliefs and analytic style → intelligence → religious beliefs were both significant: $b = -.08$ [−.10, −.06], and $b = -.05$ [−.07, −.03], respectively. Controlling for analytic style reduced the direct relation between intelligence and religious beliefs from $b = -.19$ to $b = -.12$, but the association remained significant (see upper part of Figure 4). Controlling for intelligence reduced the relation between analytic style and religious beliefs from $b = -.23$ to $-.18$, but the association remained significant (see lower part of Figure 4).

The results support the notion that analytic style partially mediated the IRR. This finding has interesting implications but they are not as clear-cut as what Zuckerman et al. (2013) proposed. We will return to this issue in the "Discussion" section.

Discussion

Meta-Analytic Models

Meta-analysts are routinely faced with choices about what statistical models to use. These decisions affect both statistical power and the kind of generalizations that are possible from the analysis. After discussing the advantages and weaknesses of a fully random-effects (i.e., unweighted effects) model in which studies are the sampling units versus a fixed-effects model in which participants within studies are the sampling units, Zuckerman et al. (2013) used both methods. Webster and Duffy (2016, Study 1) used a different weighted random-/mixed-effects model, aiming to retain the advantage of wide generalizability (a feature of the random-effects model) but still weighting studies by their sample size (a feature of the fixed-effects model). However, weighting studies by sample size makes the weighted random-effects model more limited in generalizing to future studies than is the unweighted approach (Hall & Rosenthal, 2018). Weighting by sample size amounts to weighting by all methodological features that distinguish between large and small studies—an approach that can lead to misleading interpretations. Furthermore, weighting by sample size might trigger assumptions (common to fixed-effects approach) that are hardly ever met; those are the existence of one true effect size and sampling error being the only source of variation among effect sizes.

Given the limitation of the meta-analytic model that we used, it is of interest to see how the current results would compare with results obtained by pure fixed-effects and random-effects models. We, therefore, repeated some key analyses using these two alternative models. Table S7a in the supplementary materials shows estimates of mean effect sizes and CIs for the entire sample ($k = 83$), the sample after data cleaning ($k = 62$), and the three age groups (precollege, college, and noncollege). Table S7b shows the significance levels of the three comparisons/contrasts that were used to clean the data and the contrast comparing the three age groups. For each estimate, comparison or contrast, the two tables display from left to right the results obtained by the unweighted random-effects model, the weighted random-/mixed-effects model (the method used in the present analyses), and the fixed-effects model.

Surprisingly (or unsurprisingly, depending on one's point of view), the three models produced extremely similar results. In fact, if we were to substitute the model we used with either the unweighted random-effects model or the fixed-effects model, our conclusions would remain the same. Of course, one comparison is clearly not enough to form an opinion about the comparability of the three models. Clearly, the next step should be simulation studies that can pinpoint the types of data that produce similar results across analytic models and those that do not.

Summary of Main Results

As stated in an earlier section, in the present work we adopted Webster and Duffy's (2016, Study 1) analytic model to ensure that the points we make here cannot be attributed simply to the use of a different analytic procedure. Importantly, using this model and adding more studies produced estimates of the IRR that are highly similar to the corresponding estimates in Zuckerman et al. (2013), which used different statistical models. In the updated final data set, the overall correlations between intelligence and religious beliefs in college and noncollege samples ranged from $-.20$ to $-.23$, very similar to the $-.20$ to $-.25$ range that was found by Zuckerman et al. (2013). In precollege samples and when using religious behavior rather than beliefs as a measure of religiosity, the IRR was much weaker. That religious beliefs were more negatively related to intelligence (as well as to education and analytic style) than were religious behaviors reinforces the conclusion that the IRR is real. People may engage in religious behavior for reasons that are extrinsic to religion and, therefore, unrelated to intelligence. Consistent with this view, explanations for the IRR, which we turn to shortly, mostly focus on religious beliefs and not on religious behavior. The present analyses also showed that the IRR holds for both men and women equally, and that there is no evidence that it is growing weaker in more recent years.

Mediation Results

Education. We concluded that education did not mediate the IRR, mostly because of a weak relation between education and religiosity. Is it possible, however, that we analyzed the wrong educational variable? Ganzach and Gotlibovski (2013), for example, proposed that years of education is a poor measure of this construct as it does not consider the quality or the type (religious vs. secular) of educational experience. The subject matter of one's education (e.g., humanities vs. sciences) might also make a difference. Unfortunately, studies typically inquire about length of education but not about its content.

Most analyses of the education–religiosity association are cross-sectional. However, two studies (Ganzach & Gotlibovski, 2013; Schwadel, 2016) reported negative longitudinal (within-person) effects of education on religiosity, even while cross-sectional (between-person) analyses of the same data showed no relation. Schwadel (2016) suggested (and presented supporting data) that the negative within-person effect of education became null at the between-person level because religious youth are more likely to attend college than nonreligious youth. Schwadel (2016) also presented data relevant to Ganzach and Gotlibovski's (2013) inquiry about the possible relation between quality of education and religiosity. He reported that graduating from the non–top 100 colleges and universities had more negative within-person effect on religiosity than graduating from the top 50 schools. This effect also did not translate to a between-person relation—graduating from non–top 100 schools was actually positively related to religiosity, whereas graduating from the top 50 schools was negatively related to religiosity. Self-selection might again explain the contradiction as more religious youth self-select into the non–top 100 schools and less religious youth self-select into the top 50 schools.

In conclusion, there is no evidence that education mediates the IRR. It still remains to be seen whether the content of one's education plays a role in this relation.

Cognitive style: The quest for rationality and beyond. We found that analytic style (as measured by various CRTs) partially mediates the IRR. According to Stanovich's (2009, 2018; see also Stanovich, West, & Toplak, 2016) dual-process model, performance on the CRT depends on two different components of System 2. One is the reflective mind, which is the ability to detect the need to use cognitive ability to override the intuitive (and wrong) answer. The other is the algorithmic mind (similar to fluid intelligence), which is the ability to sustain the override and identify the correct solution. Successful detection that the intuitive response is problematic depends on the presence of certain mindware, that is, acquired knowledge of procedure, strategies, and so on, including knowledge of causal and scientific reasoning. If this knowledge is automatized, it becomes part of the autonomous mind (System 1 processing), allowing a correct CRT solution without involving System 2 processing. As most people do not have such automatized capabilities, it is likely that their scores reflect whether they successfully completed the CRT via System 2 processing, that is, detection, sustained override, and computation of the correct solution. Intelligence tests assess abilities associaed with both the algorithmic mind (fluid intelligence or the *g* factor) and the reflective mind. It follows that their relation with the CRT and the mediation link to religiosity also involve both the algorithmic and reflective minds.

Other theoretical models of the CRT do not distinguish between the detection and the override stages, but instead inject a motivational element into the process. For example, Pennycook, Cheyne, Barr, Koehler, and Fugelsang (2014a) proposed that performance on both intelligence tests and the CRT requires cognitive ability as well as the willingness to use it. Intelligence tests mostly measure ability as test takers know that they need to maximize performance. The CRT assesses more of the preference to be analytic as it does not require the person to maximize performance and because it offers an appealing (but wrong) intuitive solution. As people tend to use those faculties at which they excel, it is likely that intelligent people prefer to be analytic. (In Stanovich's (2009) model, the preference to use cognitive ability is related more strongly to self-reported measures that inquire about respondents' typical thinking style.)

If we accept the premise that better performance on the CRT reflects greater rationality, our findings support the view that intelligent people are less religious because they are more rational (Dutton, 2014; Dutton & Van der Linden, 2017; Nyborg, 2009). However, the models we briefly reviewed above offer a somewhat different perspective on what we mean by rationality. According to Stanovich (2016), rationality encompasses both the reflective and algorithmic mind. According to Pennycook (2014), rationality presumably (as these authors did not discuss this) would include both cognitive ability and the preference to use it (such preference, according to Stanovich, 2009, represents means to rationality rather than rationality itself).

Interestingly, the finding that the CRT mediates the relation between intelligence and religiosity provides some support to another attempt to explain the IRR—Dutton and Van der Linden's (2017) evolutionary mismatch model. In this model, religiosity was evolutionarily selected because it was advantageous at both the individual and the group level. However, religiosity acts like an instinct and is more active in times of stress or danger. Intelligence is also selected by evolution, but its function is to address problems that our instincts cannot deal with. The finding that intelligent people are more inclined to be analytic implies they are less likely to act instinctually and, hence, less likely to be religious.

As the mediation intelligence → analytic style → religious beliefs was partial, the IRR that is independent of analytic style still needs to be explained. One possibility relies on the notion that people use their religious beliefs as a means to obtain benefits associated with greater religiosity (Zuckerman, Li, & Diener, 2018). Such benefits might include greater self-enhancement, a sense of empowerment, and better self-regulation (for a review, see special issue of *Personality and Social Psychology Review*, 2010). Zuckerman et al. (2013) presented extensive research evidence that more intelligent people already benefit from higher self-evaluation, a higher sense of control, and more efficient self-control. As such, more intelligent people have less of a need for religiosity as they already possess the benefits that religion bestows upon its followers.

Limitations

Two major limitations should be noted, one pertaining to our results and one pertaining to our interpretation. The first limitation is that similar to the findings of all other meta-analyses, the present results are based only on the studies that were reviewed. Most of these studies were conducted in the West, primarily in the United States. Our findings, therefore, do not apply to Eastern religions such as Buddhism and Hinduism.

One primary characteristic of Eastern religions is that they tend to be polytheistic (e.g., Hinduism) or lack the concept of God altogether (e.g., Buddhism). We suspect that a primary reason why intelligent people find religion irrational has to do with the attributes and powers assigned to God. Taking God away from religion might abolish or weaken the negative IRR. However, a religion without God may not be considered a religion. Having multiple Gods may either augment or weaken the IRR—we can see arguments for both directions. What can be said for certain is that our findings are limited to Western religions.

The second limitation is twofold. First, we cannot provide a single conclusive interpretation of why the CRT mediates the IRR. We presented two models, a formal and fairly detailed theory by Stanovich (2018) and a more descriptive or interpretive approach by Pennycook (2014). According to Stanovich (2018), performance on the CRT is determined by the algorithmic and reflective minds, and both minds define rationality. According to Pennycook (2014), performance on the CRT is determined by cognitive ability and the motivation to use it; presumably (as Pennycook, 2014 did not mention it), both elements define rationality. Given the existence of two models and the possibility of additional interpretations of the CRT in the future, we cannot specify with certainty the exact constructs that underlie the mediation role of the CRT. For example, it is possible (although not that interesting) that the CRT mediates the IRR because, like intelligence tests, it measures some aspects of cognitive abilities.

Second, the mediation model we tested was completely correlational, that is, the relevant variables were not manipulated and all were measured at the same time. This undermines any causal interpretation. Taking both reservations into account, the results support the notion of rationality as an explanation of the IRR but they are not definitive.

Conclusion

Our evidence showed that correlations in an updated data set between intelligence and religious beliefs range from −.20 to −.23, similar to the −.20 to −.25 range obtained by Zuckerman et al. (2013). It is possible that more intelligent people are less religious because they are more rational, an inference that is consistent with the finding that the IRR was partially mediated by analytic style (CRT scores). That intelligent people tend to be more analytic might also imply that they shift away from an instinctual reliance on religion in time of stress to greater reliance on a more deliberative process aided by their cognitive ability. However, the lack of certainty about what process underlies performance on the CRT and the correlational nature of the results make this inference tentative. We conclude that although the existence and size of a negative IRR in Western countries are firmly established, the questions of how to test existing explanations (and whether these are the only possible explanations) of the IRR remain to be addressed by future investigators.

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Supplemental Material

Supplemental material is available online with this article.

Notes

1. As N of participants was not available for one of the 63 studies, it was removed from Zuckerman, Silberman, and Hall's (2013) fixed-effect analyses and from Webster and Duffy's (2016, Study 1) analyses as well as from the current analysis.
2. The formula for correcting r for range restriction is (Sackett & Yang, 2000)

$$\hat{\rho}_{xy} = \frac{(S_x / s_x) r_{xy}}{\left[1 + r_{xy}^2 \left(S_x^2 / s_x^2 - 1\right)\right]^{1/2}},$$

   where S_x and s_x are standard deviations of the unrestricted and restricted x distributions, respectively; r_{xy} is the correlation between x and y for the restricted x distribution.
3. A study by Bertsch and Pesta (2009), which is included in Zuckerman et al. (2013) as well as in the current meta-analyses, used a ratio of 1/.71 to correct the correlation between college students' intelligence and religiosity measures. The similarity between their ratio and the one computed by Sackett, Kuncel, Arneson, Cooper, and Waters (2009) is reassuring. In our meta-analysis, we used the raw "uncorrected" correlation that Bertsch and Pesta (2009) reported.
4. Webster and Duffy's (2016, Study 1) claim that education mediates the IRR was based on analysis that included Blanchard-Fields, Hertzog, Stein, and Pak's (2001) Study 1 and used religiosity rather than religious beliefs as the dependent variable. That is, in studies that included measures of both religious beliefs and behaviors, the data employed in the mediation analysis were the average of correlations of intelligence and education with religious beliefs and with religious behaviors. Using religiosity rather than religious belief in the mediation analysis of all 15 studies provided no support for the notion that education mediates the IRR (b = .00 [−.01, .02]).

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