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The Effect of Religiosity and Spirituality on Mental Health: Reply to Two Commentaries

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ABSTRACT

We published a meta-analysis to determine the longitudinal positive effect of religion or spirituality (R/S) on mental health. Forty-eight longitudinal studies were summarized (59 independent samples). The meta-analysis yielded a significant, but small overall effect size of $r = .08$. We concluded that there is evidence for a positive effect of R/S on mental health, but this effect is small. Our meta-analysis was recently criticized in this Journal by Koenig et al. Scientific debate is welcome, but we disagree with most of their comments. Our reply focusses on the following topics: Is the effect of R/S small? Might methodological issues underlie the small overall effect size? Randomized controlled studies, and change course and look elsewhere for more convincing results?

Introduction

The question whether religiousness or spirituality (R/S) positively influences mental health has intrigued researchers and laymen for decades. The answer to this question was often sought in cross-sectional studies. This is, however, not a satisfying approach. A negative relationship found in such studies between R/S and, for instance, depression could be explained by a protective role of R/S on depression. However, a reverse causation is not excluded: depression might prevent people from visiting a church, which also leads to a negative association. Fortunately, nowadays many longitudinal studies are available. These were recently reviewed with respect to depression (Braam & Koenig, 2019), and summarized in a meta-analysis, published in this journal, with respect to mental health (Garssen et al., 2020). Our meta-analysis elicited two very different responses: one included a rather long list of critical comments to a length extending our original article (Koenig et al., 2020), whereas we were praised in the other response for having “provided a tremendous service to the religion-health research community by carrying out an extensive meta-analysis” (VanderWeele, 2020). Because of these contradictory evaluations, we felt we needed to share our views on the comments by Koenig et al. In the end, the following two issues seemed most important: Is the effect of R/S “small”, as we noted, and – if so – would that be a reason to change course and look elsewhere for more useful results?

As indicated above, we summarized longitudinal studies on the relationship between R/S and mental health ($N = 48$; 59 independent samples). Because R/S and mental health are multidimensional concepts, we distinguished between eight different R/S categories and five mental health categories (distress-continuous, distress-dichotomous, well-being, life satisfaction or Quality of Life). The correlation coefficient was used as the effect measure. A small, but significant overall effect size of $r = .08$ was found.

Is the effect of R/S small?

An overall effect size of $r = .08$ means that 0.6% of the variance in mental health is explained by R/S. This appears – also according to Koenig et al. – rather trivial. Of course, an overall effect size gives only a global impression. More informative are the effect sizes specific for a certain aspect, such as “What is the effect of church attendance on mental health, and is it larger than those of other R/S predictors?” or “Is the association between R/S and mental health larger for women than for men?” The various questions led to a total number of 46 calculated effect sizes, times two for the analyses with and without outliers. The largest effect size among the 92 calculated was $r = .12$, explaining 1.4% of the variance, and the smallest was $r = .01$, explaining 0.01%. Imagine that between 0% and 1.4% of the variation in one’s mood is explained by the type of food ingested. Would one then conclude that the effect of food is small or even negligible, or that it is of importance?

Koenig et al. did consider it important, after converting the correlation coefficient into an Odds Ratio (OR). The effect size for Participation in public religious activities as a predictor of distress-dichotomous was $r = .11$. This corresponds with an OR = 1.49 (CI = 1.24–1.73). This rather abstract information was translated by Koenig et al. as: “There is an average 39–49% increased likelihood that R/S individuals will be in the non-distressed group. This is far from trivial – particularly in clinical populations”.

The question remains, however, whether this should be considered a strong or a weak relationship. The findings in terms of OR is: Those who do not develop serious distress symptoms are 1.49 more likely to have frequently visit a religious service (not being a “R/S individual”, as Koenig et al. said). To get a clear picture of what this means, we will use an imaginary example. From a sample of 200 people half develops serious distress symptoms, and 90 frequently attend a religious service (see Table 1). Among those who did not get serious distress symptoms the ratio for frequent church visits is 1:1, whereas it is 2:3 for those who suffer from such symptoms. The OR = $1/(2/3) = 1.49$. Does this distribution indicate a strong or a weak relationship? We might not consider the difference between the two group sizes substantial. This is in agreement with Chen et al., who interpret an OR = 1.49 as “very small”, and the confidence interval as ranging from “very small” to “small” (Chen et al., 2010).

Vanderweele suggested that a small effect can nevertheless lead to a substantial influence, provided

Table 1. An imaginary example of the association between church attendance and distress symptoms that yields an odds ratio of 1.49.

	Serious distress Symptoms	No serious Distress symptoms
Frequent church visitors	40	50
Non-frequent visitors	60	50

that the causative agent is active for a long time (VanderWeele, 2020). For example, church attendance appears to have a small effect of $r = .08$ on depression one year later. Frequency of church attendance may be constant throughout life. So, if the influence of church attendance on mental health remains stable, the level of depression lowers each year by the same small amount, ending in a significant decline. However, this argument would also imply that the effect sizes would be larger with longer follow-up. We found the opposite trend.

Might methodological issues underlie the small overall effect size?

Koenig et al. also criticized the methodological rigor of our meta-analysis, which may have contributed to us finding a small overall effect size. Koenig et al. compared a meta-analysis in this field as lumping together apples and oranges. “The approach taken in the current meta-analysis (and prior meta-analyses) is analogous to studying the effectiveness of medication on health by averaging the effects of ibuprofen, clozapine, amoxicillin, and insulin on pain, psychosis, pneumonia, and diabetes. It is impossible to learn anything unless you specify which medication, which disease, for whom, and

when”. We may hope Koenig has included the meta-analysis performed by himself into this dismissive comment (Braam & Koenig, 2019).

As we will see below there are various factors that do make it difficult to clearly separate the dimensions of R/S (“the medication”), the dimensions of mental health (“the disease”), the populations, and the circumstances. However, we believe results are not completely useless when attempts are made to account for these different categories. We have, at least, tried our best. In this section, we will discuss the main methodological critiques by Koenig et al.: the selection of studies, the categorizations of R/S and mental health, and the types of effects calculated.

Selection of studies

Because a certain amount of homogeneity and a certain level of study quality is needed to conduct a reliable meta-analysis, we have applied relatively strict inclusion criteria in our study. This led to the exclusion of 52 studies. Koenig et al. question whether this was justified, arguing that such judgments are subjective. We agree that some amount of subjectivity is involved in every judgment that we make as researchers and as people generally. However, what matters in science is that such subjective judgments are explained in such a way that they can be checked and their results reproduced. We feel this is what we have done, both in Figure 1 and in the Supplementary files 3 and 4.

The commentators also mention “problems” with the exclusion of specific studies (Good et al., 2009; Krause, 2009; Miller et al., 2012). The reason why we excluded the first two studies is explained in Supplementary File 3. It is common practice, even required, for a meta-analytic study to check for outliers. The third study was such an outlier. We presented the outcome *including and excluding* outliers.

Study quality

Study quality can play an important role in the reliability and validity of the effect sizes in a study and, thus, the findings of a meta-analysis. We did not assess overall quality, and agree with Koenig et al., that this is to some extent a shortcoming. Instead we determined separately the effect of number of control variables and whether the predictor and outcome variables were measured with reliable and valid instruments. These three quality measures were – in our view – the most important aspects of overall quality, and less subjective than overall study quality.

Categorization of R/S predictors

Taking into account the multidimensionality of R/S we used eight categories. Koenig et al. are not satisfied by our categorization: “The authors arbitrarily combine R/S predictors into nine categories without a clear explanation”. We did not combine “arbitrarily”, the strategy was mentioned, an explanation for each category was presented, and it should be eight categories, not nine.

Koenig et al. point out that there is a certain degree of arbitrariness in such categorizations. For example, they ask: “Why did the authors include “Importance of Religion” and “Subjective Religiousness” in separate categories?”. (We used the term “Intrinsic Religiousness”, not ‘Subjective Religiousness’). We described “Importance of Religion” as: “The feeling that religion is a strong guide in one’s life and Intrinsic Religiousness as “the feeling that one is a religious person”, which is weaker than the first category.

Curious about how Koenig handled this problem himself, we looked at the review of Braam and Koenig (Braam & Koenig, 2019). Five of the ten predictors were similar to ours, two categories were rather vague, namely “Other measures (beliefs/God)” and “Spirituality-other measures”, and the category of “Spiritual well-being” was included, discouraged by Koenig in an earlier publication (Koenig, 2008). No strategy or explanation for their choice of predictors was presented.

Defining mental health

We focused on mental health, which was operationalized as distress-continuous, distress-dichotomous, well-being, life satisfaction or QoL. Koenig et al. commented that there are more psychological outcome variables than mental health, which we did not consider, and mental health is a broader concept than in our opinion. Though we agree that “emotional health” may be a more appropriate term for what we included than was “mental health”, we do believe that not including other psychological factors was the right decision to take to keep the meta-analysis clear and informative (think back to the “apples and oranges” critique). We invite other researchers to perform meta-analysis regarding other factors of psychological well-being and/or mental health. We clearly stated in the Discussion to refrain from making any statement about R/S effects on other outcome measures than mental health, mentioning especially posttraumatic growth, physiological responses, unhealthy behavior, sleeping problems, medical decisions, or marital conflicts.

A problem of including many categories of variables in a meta-analysis is that sample sizes might become too small, because too few studies are available. This was the reason why we combined depression, anxiety and general distress into one category, labeled “distress”, even though this may have diluted some differences between the outcomes and their relations to R/S.

Type of samples

We agree with Koenig et al. that types of samples can have an impact on the R/S – mental health associations, and that probably the association is highest among distressed individuals. However, we had found that sample characteristics had no significant effect on mental health, but Koenig et al. suggest that “only four effect sizes were from seriously distressed populations, i.e., psychiatric samples. Thus, the effects of R/S among distressed individuals in this meta-analysis were likely diluted out”. This comment seems a bit odd to us, because when comparing the separate samples, the effect size for studies that involved “Psychiatric patients or people at risk for depression” was $r = .10$, whereas it was $r = .12$ for students and for somatically ill patients. In their own review of the longitudinal effects of R/S Braam and Koenig conclude that the greatest decline in depression occurred persons with psychiatric illness *and* in population samples (Braam & Koenig, 2019), page. 436). This implies that highly distressed persons are not characterized by a high R/S – distress relationship, and thus that there is no place for dilution.

Moderation analysis

Several demographic and design factors, such as age, sample size, type of samples and length of follow-up, may have an effect on the strength of the association between predictors and outcome. A moderator analysis can clarify which factors are important. The commentators admitted that a considerable number of moderators were investigated in our analysis, but added: “Several important ones were left out, including race, socioeconomic status, and religion, along with combinations of moderators”. As can be seen in supplementary Table 5 of our meta-analysis, these variables were rarely or inconsistently included in the studies we examined. Braam and Koenig must have experienced a similar lack of data in their sample of studies, because they also did not include these moderators in their meta-analysis (Braam & Koenig, 2019). We have also examined the effects of three combinations of moderators, but the number of available effect sizes for such an analysis is soon too small.

Mediation and non-linear effects

The commentators argue that an effect on emotional health may be mediated by emotional indicators like “guilt, shame, uncontrolled anger, suicidal ideation, marital or work satisfaction . . .” and so on, and that “only main (or direct) effects were considered in this meta-analysis, and the

findings, therefore, do not include the indirect effects that R/S may have on outcomes through mediating variables”. Such indirect or mediating effects were not at all neglected in our analysis. Some studies that also determined indirect (mediating) effects are mentioned in Supplementary Table 5, last column.

Finally, there might be a non-linear relationship between R/S and mental health, as Koenig et al. also rightly noted. To get an impression of whether this phenomenon would overthrow our final conclusion, we looked for studies that reported non-linear relationships, and found three such studies (King et al., 2007; Ronneberg et al., 2016; Schnittker, 2001). However, these studies showed inconsistent results (Garssen et al., 2020).

RCT studies

Koenig et al. suggest that we should have placed more focus on randomized controlled (RCT) studies. We also mentioned that such studies might be useful in answering questions about the effect of R/S on mental health, saying: “Only the experimental manipulation of R/S could provide more certainty on the causal relationship”. However, we also mentioned a problem with such studies, namely: “However, a host of other factors will also affect the outcome of these interventions, such as the expectations people have about the advantages they will experience from their participation, learning to relax, acquiring more coping abilities, and the quality of the therapeutic relationship”.

In addition, not all of these studies show a clear benefit of including R/S in therapy. The meta-analysis Captari et al. (2018) that Koenig et al. put forward, showed that R/S-adapted psychotherapy did outperform standard psychotherapy on spiritual outcomes, but not on psychological outcomes. This means that R/S-accommodated treatments were as effective, but not more effective, than standard psychotherapy (page 1945). In fact, this finding would underscore our conclusion that we should look elsewhere for the effect of R/S.

Change course and look elsewhere for more convincing results?

We were so bold to conclude in the section “Future directions”: “The findings of the present study raise the question whether further research into the contribution of R/S to mental health is worthwhile. The heterogeneity of studies in the field suggests that one can endlessly search for relationships between all the various aspects of R/S and all the various aspects of mental health, but what is the practical significance of such studies?”. Notice that this is not a prescription – how could we? – but a suggestion. Koenig et al. did not like this suggestion, because it would dismiss the findings of hundreds of (a) cross-sectional studies, (b) randomized controlled trials (RCT), (c) longitudinal studies, and (d) qualitative studies that do suggest that R/S affects mental health. The problem of cross-sectional studies has been shortly described above and extensively elsewhere, and the use of RCTs in this field has been discussed above. Longitudinal studies were the focus of our meta-analysis. Qualitative studies may show how religious or spiritual people describe the relationship between their attitude in life and the experience of well-being. To study the association between R/S and mental health in a population is the exclusive prerogative of quantitative studies, using an aselect and large enough sample.

After summarizing dozens of longitudinal studies and finding a weak effect of R/S on a select number of outcome variables, it might be time to examine different types of questions about the effects of R/S on our lives. As we suggest in our paper, it might be much more informative (also from a clinical perspective) to focus on other types of outcomes than indicators of mental health.

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