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Compensatory control and religious beliefs: a registered replication report across two countries

Suzanne Hoogeveen[a], Eric-Jan Wagenmakers[b], Aaron C. Kay[c] and Michiel Van Elk[a]

[a]Department of Social Psychology, University of Amsterdam, Amsterdam, The Netherlands; [b]Department of Psychological Methods, University of Amsterdam, Amsterdam, The Netherlands; [c]The Fuqua School of Business, Duke University, Durham, USA

ABSTRACT

Compensatory Control Theory (CCT) suggests that religious belief systems provide an external source of control that can substitute a perceived lack of personal control. In a seminal paper, it was experimentally demonstrated that a threat to personal control increases endorsement of the existence of a controlling God. In the current registered report, we conducted a high-powered (N = 829) direct replication of this effect, using samples from the Netherlands and the United States (US). Our results show moderate to strong evidence for the absence of an experimental effect across both countries: belief in a controlling God did not increase after a threat compared to an affirmation of personal control. In a complementary preregistered analysis, an inverse relation between general feelings of personal control and belief in a controlling God was found in the US, but not in the Netherlands. We discuss potential reasons for the replication failure of the experimental effect and cultural mechanisms explaining the cross-country difference in the correlational effect. Together, our findings suggest that experimental manipulations of control may be ineffective in shifting belief in God, but that individual differences in the experience of control may be related to religious beliefs in a way that is consistent with CCT.

Why do so many people across the world believe in a supernatural being that can exert a causal influence on human affairs? Why do they engage in time-consuming rituals to ask an invisible entity for a favor or blessing? Take for instance a devout Catholic who prays to God for healing her sick son. Or consider a Hindu who offers valuable goods to his deities in order to obtain their blessing. According to Compensatory Control Theory (CCT), in all these cases, people try to gain a sense of control over their environment through religious beliefs and practices.

The basic rationale of CCT holds that believing in the power of God or other super-natural agents can compensate for the feeling that one lacks personal control over important life outcomes, and hence may partially alleviate the uncomfortable feeling elicited by uncertainty and randomness (Kay, Gaucher, McGregor, & Nash, 2010; Kay, Gaucher, Napier, Callan, & Laurin, 2008; Landau, Kay, & Whitson, 2015). Indeed, humans

have a deep-rooted desire for personal control; we are reluctant to accept randomness and inclined to believe that we can at least to some extent predict, influence, and control the world around us (Lerner, 1980; Maier & Seligman, 1976). Yet situational constraints and the complex reality of our environments often substantially reduce the degree to which we can perceive ourselves as being in control. To alleviate this discomfort, individuals can attempt to reaffirm personal control directly through their own actions, e.g. by performing superstitious rituals (Whitson & Galinsky, 2008). Alternatively, when exerting personal control is impossible, individuals might resort to external sources of control, for instance by affiliating with a societal institution, a governmental system, or a religious ideology (Rothbaum, Weisz, & Snyder, 1982).

More specifically, CCT posits that in the face of low or reduced personal control, people will restore their feeling of control by more strongly endorsing belief in the existence and influence of external controlling powers, such as an intervening God. In the classic demonstration of this effect, introduced by Kay et al. (2008) in Study 1, participants were assigned to either a control affirmation or control threat condition in which perceived personal control was strengthened or reduced, respectively, by means of an autobiographic recall task. Subsequently, participants indicated their belief in a controlling God. As predicted, the results of the original study revealed that participants whose perception of personal control was threatened showed a significantly stronger belief in the existence of a controlling God, compared to participants whose personal control was affirmed. Notably, when the controlling nature of God was deemphasized, i.e. God was presented as a creator, the control threat effect was absent. This dissociation underlines the relevance of religious beliefs providing a source of compensatory control, rather than being comforting in general. As noted by the authors, given that profound beliefs such as those associated with religion and supernatural beings are highly stable and difficult to manipulate experimentally (e.g. Yonker, Edman, Cresswell, & Barrett, 2016), the fact that this simple control manipulation is capable of "shift[ing] these beliefs is rather striking" (Kay et al., 2008, p. 23).

CCT has been supported by many empirical findings and is accepted as an important psychological and motivational account with respect to religious beliefs (Sedikides, 2010). According to Google Scholar, as of September 2018, the paper by Kay et al. (2008) has been cited 602 times. More importantly, the original article inspired a large body of research on compensatory control mechanisms related to a wide variety of structure-restoring tendencies (reviewed in Landau et al., 2015). The breadth of the phenomenon can be illustrated by the variety of research approaches (e.g. temporal fluctuations on a national level, individual differences, experimental manipulations) as well as the range of examined compensation strategies: in correlational designs, lack of personal control has been associated with stronger attraction to astrology (Lillqvist & Lindeman, 1998), stronger endorsement of conspiracy beliefs (Newheiser, Farias, & Tausch, 2011), higher levels of superstition (Padgett & Jorgenson, 1982), and higher conversion rates to authoritarian relative to non-authoritarian churches (Sales, 1972). In an experimental context, personal control manipulations have been shown to affect illusory pattern perception and conspiracy beliefs (Whitson & Galinsky, 2008), endorsement of horoscope descriptions (Wang, Whitson, & Menon, 2012), belief in precognition (Greenaway, Louis, & Hornsey, 2013), support for meritocratic systems (Goode, Keefer, & Molina, 2014), preference for structured consumption items (Cutright, 2011), belief in the efficacy of rituals (Legare & Souza, 2014) and belief in order-providing

theories such as a predictable, non-random version of evolution theory (Rutjens, van der Pligt, & van Harreveld, 2010). In a recent meta-analysis by Landau et al. (2015), including 55 studies, it was established that control-threat manipulations exerted a moderate ($r = .24$, $\delta = .494$) though robust effect on different "epistemic structuring tendencies".

The primary finding by Kay et al. (2008) with belief in a controlling God as the dependent variable has indeed been replicated (either successfully or unsuccessfully) in seven studies – however always as part of more elaborate designs or additional research questions. Figure 1 summarizes the replications of the crucial effect of personal control threat on belief in a controlling God, including a model-averaged Bayesian meta-analysis. The top row of the figure refers to the original study, and the subsequent rows list existing replications. Across all studies, the outcome variable was "belief in a controlling God", measured by the items specified in the Methods section of this paper. Note that the figure displays results of the main experimental effect of the control manipulation on belief in a controlling God, although the listed studies' primary interest in some cases focused on different aspects. The studies investigated for instance the role of specific mediators (defensive reactions towards randomness; Kay et al., 2008, Study 2), moderators (anxiety; Laurin, Kay, & Moscovitch, 2008[1]; Kay, Moscovitch, & Laurin, 2010, a personality trait related to independence and desire for autonomy; Alper & Sümer, 2017), included additional conditions (a neutral condition; Verburg et al., 2016)[2] or a different source of control was manipulated (governmental control; Kay, Shepherd, Blatz, Chua, & Galinsky, 2010).
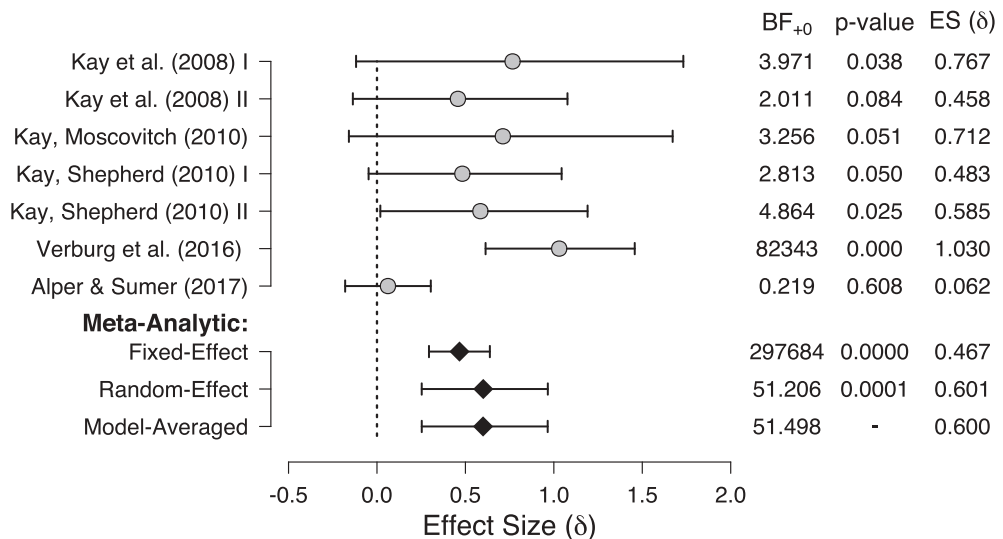
Figure 1. Summary of previous studies plus meta-analysis on the effects of control threat on belief in a controlling God. The Bayes factor BF_{+0} quantifies the evidence that the data provide for $\mathcal{H}_+$ (i.e. the presence of the compensatory control effect) relative to $\mathcal{H}_0$ (i.e. the absence of the compensatory control effect). Created using the *metaBMA* R package (Gronau et al., 2017; Heck & Gronau, 2017). Exact *p*-values were not always given in the articles, but were recalculated based on the reported statistics, converting the one-way ANOVA *F*-values to *t*-values. For Kay et al. (2010), the misattribution (i.e. no anxiety) condition is excluded.

We conducted a Bayesian reanalysis and meta-analysis (Gronau et al., 2017; Scheibehenne, Gronau, Jamil, & Wagenmakers, 2017) of the previous findings to assess the strength of the evidence provided by the replications of the primary effect.[3,4] As can be seen in Figure 1, the data from most studies provide only weak evidence for the effect of personal control threat on belief in a controlling God. Specifically, based on the commonly used interpretation categories of Bayes factors (e.g. Lee & Wagenmakers, 2013, p. 105; Jeffreys, 1939, the studies by Kay et al. (2008, 2010, 2010) all yielded evidence that is considered anecdotal to moderate. Indeed, only the findings by Verburg et al. (2016) yielded compelling evidence for the control threat effect on religious belief. The study by Alper and Sümer (2017), on the other hand, appears to provide moderate evidence *against* the presence of the effect.

Overall, our Bayesian meta-analysis indicates strong evidence in favor of the presence of a control threat effect on belief in a controlling God. However, our meta-analysis also suggests that there is substantial heterogeneity; the random effects model has far more predictive adequacy than the fixed effects model, hence, the averaged model is primarily determined by the random effects model. Furthermore, the credible interval of the average meta-analytic effect size is rather large; CI ranges from 0.250 to 0.962, with a median of $\delta = .600$. This further supports the motivation to conduct the high-powered proposed replication study. In conclusion, in spite of the theoretical and empirical support for the CCT as an overarching framework, the evidence for the primary effect regarding belief in God is not as unequivocal as one might have assumed.

Motivation

Given the impact of the original study, it is quite surprising that, to our knowledge, there have not been any high-powered direct replications of the effect of the personal control manipulation on belief in God. Our primary motivation for the current replication attempt thus naturally arises from the influential status of the study, reflected in the large body of research and theoretical reviews that it inspired. Secondly, all but the last two of the replication experiments used sample sizes smaller than $n = 50$, which translates into a maximum of 25 participants per group. In fact, the original effect was established based on nine participants per group. We used the meta-analytic effect size of $\delta = .539$ ($r = .24$) reported by Landau et al. (2015), as well as a corrected estimate of $\delta = .379$ ($r = .186$) reported by van Elk and Lodder (2018) to calculate the achieved power of the original study. That is, van Elk and Lodder (2018) report that the standard errors of the studies included in the meta-analysis by Landau et al. (2015) have been overestimated, possibly due to a coding error. As a consequence, the funnel plot asymmetry and hence the amount of missing studies and the extent of publication bias are underestimated. Crucially, whereas the original meta-analysis found no indication for publication bias and reported a final overall effect size of $r = .24$, the reanalysis by van Elk and Lodder yielded an initial effect size of $r = .26$ that should be adjusted to $r = .186, p < .0001$, indeed still reflecting a small to medium but robust effect.[5] The post-hoc power analyses based on the effect size of the meta-analysis, as well as on its corrected version, indicate that the original study was indeed highly underpowered (achieved power = 0.17 or 0.12, respectively).[6]

Moreover, the previous studies all used frequentist significance tests. Although in many cases, both frequentist and Bayesian analyses yield the same conclusions, we have some

arguments for why we believe Bayes factors are favorable over *p*-values (see Wagenmakers et al., 2018 for an elaborate argumentation): First, whereas frequentist statistics solely allows one to either reject or fail to reject the null hypothesis, Bayesian analyses can additionally distinguish between "absence of evidence" and "evidence of absence" (Dienes, 2014). This seems highly relevant in social psychological research, where effects of interest are generally of a small-to-medium size (Wagenmakers et al., 2016), and perhaps even more so for replication studies (Wagenmakers et al., 2018). Based on the meta-analysis (Landau et al., 2015), we indeed expect a small to medium effect for the current control-threat effect ($\delta = .38$). Second, we believe Bayes factors are intuitive; they arguably do what we desire (and assume) statistical tests to do. That is, they allow us to quantify the evidence that the data provide for $\mathcal{H}_0$ versus $\mathcal{H}_1$. As such, Bayes factors provide a direct comparison between the two hypotheses, conditional on the observed data (e.g. Jeffreys, 1939). Frequentist *p*-values, on the other hand, are calculated conditional on the null hypothesis being true; predictions of the alternative hypothesis are irrelevant and not taken into account in the evaluation. Third, and relatedly, Bayes factors only rely on data that were actually observed, rather than hypothetical data. In contrast, *p*-values are defined as the probability of obtaining the obtained results – *or more extreme results* – given that the null hypothesis is true, thus basically conditioning on the data plus hypothetical data that have not been observed at all (Berger & Wolpert, 1988).

Nevertheless, we are aware of the arguments against the use of Bayesian frameworks, and Bayes factors specifically (e.g. Gelman & Shalizi, 2013). For instance, Bayesian inference does not solve some of the issues associated with null hypothesis significance testing; in large samples, even small and practically meaningless effects will also generate "strong evidence". However, meaningfulness can never be resolved by statistical analysis; it is always a context-dependent concept that deserves a scholarly discussions by experts in the field. From the other end of the spectrum, it has been argued that Bayes factors are biased *against* small effects (Simonsohn, 2015). However, this only applies under the combination of (1) a small sample size; (2) a small true effect size; and (3) a prior distribution that represents the expectation that effect size is large. Indeed, in the present study, we precluded (1) and (3), so we are confident that our analysis is not prejudiced against finding an effect. Therefore, given the listed advantages, we will analyze the data of the present replication study in a Bayesian framework.[7]

Furthermore, it is important to determine the effectiveness and validity of the experimental manipulation (i.e. control threat vs. control affirmation). Particularly, we included manipulation check items (e.g. "To what extent do you feel like you are the one who is in control in your life?") to test whether the control threat manipulation indeed affected feelings of personal control in one's life. In other words, we considered the manipulation effective if the affirmation of control results in higher ratings of feelings of general personal control relative to threats to control, at the group level. Importantly, the inclusion of these manipulation check items additionally allowed us to adopt an individual differences approach in case the experimental manipulation turned out to be ineffective. That is, we hypothesized that a lower feeling of general control in one's life would be related to a stronger belief in a controlling God – irrespective of the experimental manipulation. In this way, any ambiguity regarding the interpretation of an eventual null result, i.e. the inadequacy of the manipulation or the absence of a compensatory control effect, could be eliminated.

Whereas self-reported religiosity was included as a covariate in the original study, we additionally assessed religiosity as a moderator of the experimental effect. That is, while Kay et al. (2008, p. 23) mention that "the manipulation of personal control did not significantly affect this covariate [i.e., religiosity]", we argue that it may nevertheless moderate the effect of control threat on belief in a controlling God. More specifically, belief in a controlling God may be an especially appealing substitute for personal control for those who are (at least somewhat) religious, whereas God's control might not be considered an alternative among atheists, similar to effects of religious priming only affecting religious individuals (Shariff, Willard, Andersen, & Norenzayan, 2016). However, as we did not find any studies examining the potential moderating role of religiosity on the effect of control threat on belief in God, we left this possibility open and investigated it only exploratorily.[8]

Finally, with one exception (i.e. Alper & Sümer, 2017), all previously studied populations are from North America, mostly the United States (US). Besides the moderating effect of religiosity at the individual level, we expected additional cross-country differences between the Netherlands and the US, for two intertwined reasons. That is, the tendency to resort to belief in God as a source of control may vary between countries due to (1) differences in the cultural prevalence of religious beliefs and (2) the availability of alternative secular sources of security and control.

The Netherlands can be defined as a highly secularized country; national statistics indicate that as of 2015, only 12% of the Dutch population regularly attended church, and 32% believed in a personal God or a higher power (Bernts & Berghuijs, 2016; Kregting, Scheepers, Vermeer, & Hermans, 2018). Thus, although some people can still be considered religious, the majority of the Dutch population do not endorse traditional religious beliefs in God. This stands in contrast with the US, which can be considered a highly religious country, where the majority of the population endorses traditional religious belief in a powerful, intervening, and controlling God (i.e. as of 2016, 79% of the US citizens indicated to believe in God; Gallup, 2016; Stavrova, Fetchenhauer, & Schlösser, 2013).

The development of social security has proven to be a relevant factor in explaining secularity over time in Western countries (Kregting et al., 2018; Reitsma, Pelzer, Scheepers, & Schilderman, 2012). For instance, predictors of existential securities (i.e. political, material, and financial security) and religious socialization and control (i.e. being raised in a religious family or environment) have been shown to partly explain the difference in religious attendance across 60 countries, including the Netherlands and the US (Ruiter & van Tubergen, 2009). Interestingly, it appears that these socioeconomic security factors may also partly explain the "exceptional pattern" of religiosity found in the US. The US have been reported to occupy an outlier position, as a highly modern yet highly religious society, with religion deeply ingrained in culture and social identity (Kelley & de Graaf, 1997; Tiryakian, 1991; Warner, 1993). Taking into account the importance of social security, Ruiter and van Tubergen (2009) argued that the US was no longer exceptional; the persistent strong socioeconomic inequalities and strong religious history explain the high prevalence of religiosity in the US.

These country-level differences suggest that religion and belief in God may have an important function for providing a sense of control in people's lives in the US. In the Netherlands, however, the social safety net may be so prevalent that it leaves far less room for religious beliefs to compensate for loss of personal control. In addition, based on

the differential cultural prevalence of religion in the US and the Netherlands, we expected US participants to resort more easily to belief in a controlling God when lacking control – which is accepted as a socially desirable option in US culture. In contrast, in the Netherlands, strengthening one's belief in a controlling God as a consequence of control threat does not fit with general cultural expectations. Accordingly, we expected the effect of control threat on belief in a controlling God to be stronger in the US than in the Netherlands.

Notably, the original experiment comprised of a 2 × 2 design, with the emphasized aspect of the nature of God as an additional between-subjects factor. That is, half of the participants in the study by Kay et al. (2008) rated their belief in the existence of God as a creator and half rated their belief in the existence of God as a controller. As predicted by CCT, personal control manipulations only affected belief in God when the controlling nature of God is emphasized – only then God serves as a compensation for a lack of personal control. Therefore, in the light of efficiency and relevance, in the present replication, we chose to focus solely on the crucial effect with regard to belief in a controlling God.

The decision to omit the control condition with "God as a creator" as a dependent variable comes at an informational cost. Admittedly, we cannot completely preclude the possibility that any effect of personal control threat causes increased belief in God due to some other characteristics of religious beliefs (e.g. the nature of God as loving, compassionate, all-knowing, or as a designer, etc.) rather than belief in a controlling God per se. Interestingly though, later versions of CCT have also included more abstract epistemic structuring tendencies as compensatory strategies (e.g. an ordered, non-random version of evolution theory, stage theories of moral development and Alzheimer's disease, and aesthetically bounded vs. unbounded products; Cutright, 2011; Rutjens et al., 2010; Rutjens, van Harreveld, van der Pligt, Kreemers, & Noordewier, 2013). Therefore, even when presented as a creator, belief in God may still serve as a compensatory strategy, by offering an epistemically structured conception of the world. Indeed, as mentioned in Landau et al. (2015), religious beliefs may present an especially well-suited opportunity for restoring feelings of control, exactly because they provide multiple means to this end. "For example, adhering zealously to religious beliefs may bolster external agency (through faith in beneficent intervention), affirm specific epistemic structure (by specifying consequences of moral conduct), and affirm nonspecific epistemic structure (portraying the universe as obeying a few well-observed and immutable laws)" (Landau et al., 2015).

This debate is beyond the scope of the present manuscript, however. Instead we currently aimed to focus on investigating the primary effect that has been documented for CCT (i.e. control threat manipulations increase belief in a controlling God), which provides the strongest test of the theory. When evidence for this specific effect has been convincingly reported, this then paves the way for further research on the boundary conditions of the effect or potential extensions.

We thus aimed to conduct a direct replication of crucial effect of the original Study 1 by Kay et al. (2008), including exactly the same manipulations and measures (excluding the extra control condition). At the same time, we extended the original study in five ways, one related to the design, two related to the sampling (power and included population), and two related to the analysis (model and statistical framework). First, we include a measure of generalized feelings of personal control, allowing for a manipulation check and individual differences approach. Second, we increased the sample size ($n = 800$ in total) to ensure

sufficient power for detecting a small to medium effect. Third, we conducted the study in a relatively religious as well as a relatively secular country. Fourth, we included religiosity as a potential moderator, rather than solely as a covariate. Fifth, we used a Bayesian hypothesis testing framework, allowing quantification of the evidence for or against the null hypothesis. Importantly, we note that only the first extension changed the experiment itself, yet in no way does it impede the validity of our direct replication attempt, as additional measures were included only *after* the original study had been conducted. Moreover, we believe the outlined extensions of the design allow us to better interpret any obtained results and provide a more sensitive test of the underlying theory.

Hypotheses

The predictions of the current replication attempt were straightforward: we expected that participants would "endorse the existence of [a controlling] God more strongly following the no-control memory task [compared to the control memory task]" (Kay et al., 2008, p. 22). More specifically, the main hypothesis, i.e. the replication hypothesis of primary interest, can be specified as follows:

$\mathcal{H}_{\textbf{exp}}$: Primary experimental effect: recall of a positively valenced situation in which one had *no personal control* (e.g. "Describe a pleasant event or situation over which you had absolutely no control") will result in more fervent belief in the existence of a controlling God, compared to recall of a positively valenced situation in which one did have personal control (e.g. "Describe a pleasant event or situation over which you had total control").

Auxillary hypotheses that were tested are:

$\mathcal{H}_{\textbf{cov}}$: Covariate: levels of self-reported religiosity are positively related to belief in the existence of a controlling God.

$\mathcal{H}_{\textbf{man}}$: Manipulation check: recall of a positively valenced situation in which one had *no personal control* will result in lower levels of general feelings of personal control in one's life ("To what extent do you feel like you are the one who is in control in your life?"), compared to recall of a positively valenced situation in which one did have personal control.

$\mathcal{H}_{\textbf{cor}}$: Correlational effect: levels of general feelings of personal control in one's life are negatively related to belief in the existence of a controlling God.

$\mathcal{H}_{\textbf{cul}}$: Cross-cultural effect: the primary experimental effect of personal control threat vs. affirmation on belief in a controlling God is moderated by cultural and socioeconomic factors reflected at the country level; the experimental effect is stronger in the US than in the Netherlands.

The exact sequence of hypothesis testing, as well as the drawn inferences are depicted in Figure 2. These hypotheses, as well as the planned analyses were agreed on by all involved parties and reviewers prior to the start of data collection. All materials, the full preregistered analysis plan, the anonymized raw and processed data, and the analysis scripts to conduct all confirmatory and exploratory analyses (including all figures) are available on the Open Science Framework (OSF; see https://osf.io/49xz3/).
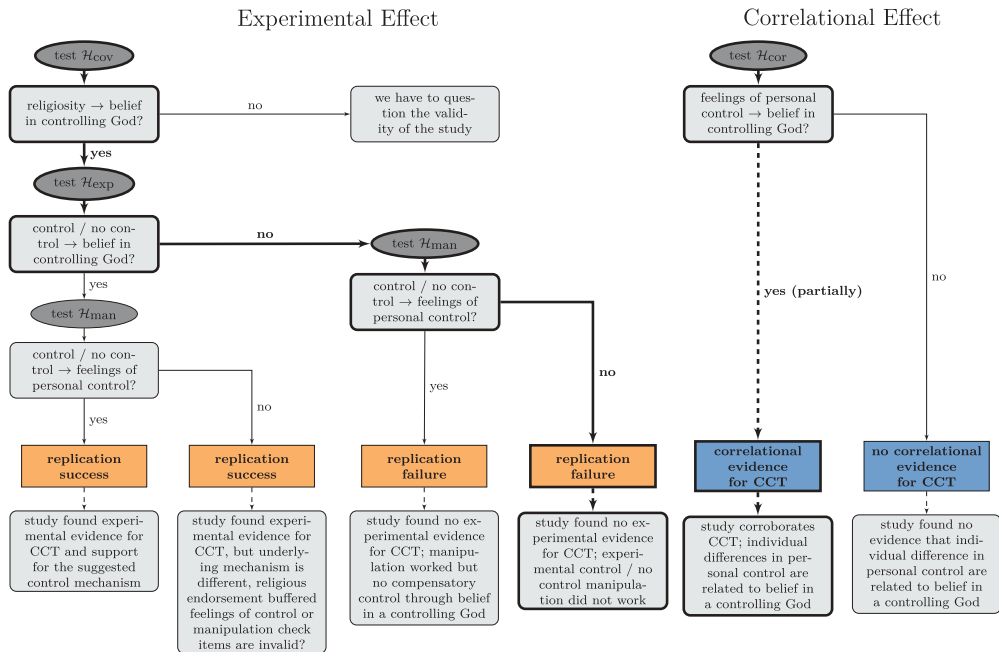
Figure 2. The preregistered analysis pipeline, displaying tested hypotheses and interpretation of possible results for both the experimental and correlational approach of CCT for religious beliefs. The results of the study suggested to follow the two paths indicated with the thick lines.

Methods

Participants

The study was conducted in collaboration with the independent research agency Kieskompas (Amsterdam, the Netherlands; www.kieskompas.nl). Kieskompas specializes in online tools for assisting a general public in voting choices (e.g. for elections), but also offers panels for scientific research. They are affiliated with the Free University of Amsterdam, and have access to a (largely) representative sample of the 45 countries in which they operate.

Individuals older than 18 were eligible for participation. We specified no a priori exclusion criteria, which is in line with original study and converges with the meta-analysis by Landau et al. (2015) indicating no significant moderating effects of gender, college vs. non-college participants, form of compensation (credits vs. money), or region of data collection (US vs. outside of the US). However, we specified a criterion for the minimum time interval for completing the study. That is, we excluded participants who spent less than a particular number of minutes on the task (also known as "speeders") and whose data are therefore assumed to be invalid. As specified a priori, the criterion was set to 40% of the median of the total duration of the experiment, i.e. participants who spent less than 40% of the median time of the task, were excluded (Greszki, Meyer, & Schoen, 2015). In practice, this resulted in a cutoff of 224 seconds for the Dutch version of the task and 159.4 seconds for the English version of the task. Additionally, as preregistered, we excluded the data from participants

who wrote nonsensical stories in the recall task. This led to the exclusion of 34 participants in total; 22 and 9 participants were excluded for speeding in the Netherlands and the US, respectively, and 1 and 2 participants for writing nonsensical stories.[9]

After exclusions, the final samples consisted of 438 (51.6% female) participants in the Netherlands, and 391 (43.0% female) in the US. The average age of the Dutch participants was 58.4 ($SD = 15.3$; range $= 20 - 91$) and 50.2 ($SD = 16.1$; range $= 18 - 89$) for the American participants. We declare that all preregistered methodology was followed exactly unless explicitly stated otherwise.

Sampling plan

Our sampling plan was based on Bayes factor design analysis (BFDA; Schönbrodt & Wagenmakers, 2018; Stefan, Gronau, Schönbrodt, & Wagenmakers, 2019), a recently developed method to help balance informativeness and efficiency of planned experiments within a Bayesian framework. We used the BFDA R package to compute the required sample size given the corrected effect size of the meta-analysis (i.e. $\delta = .379$; Schönbrodt, 2017). The analysis indicated that we would need 185 observations per group in order to obtain a Bayes factor in favor of $\mathcal{H}_{exp}$ larger than 10 with a probability of $p = 0.8$.[10] Following this indication, we decided to aim for a final sample of 200 participants per group per country; $n = 400$ per experiment (see online supplementary materials for the distributions of expected Bayes factors generated based on the power analysis; https://osf.io/49xz3/).

Materials

Participants received all materials in their respective native language, i.e. English in the US and Dutch in the Netherlands.[11] Dutch materials were translated and back-translated by two different parties.

Recall task

As in the original study, participants were first presented with one of the two memory tasks probing them to recall a recent positive event over which they did or did not have control. The Dutch and English items can be accessed on the OSF. The task instruction was taken from Kay et al. (2008) and read as follows: "Please try and think of something positive that happened to you in the past few months that you had [total/absolutely no] control over. Can you remember such a situation or event? Try to briefly describe this [un] controllable event in no more than 100 words. What happened and how did you feel?"

Belief in a controlling God

The dependent variable was equal to the one used in the original study; belief in the existence of God was assessed based on two items:

(1) To what extent do you think it is feasible that God, or some type of nonhuman entity, is in control, at least in part, of the events within our universe?

(2) To what extent do you think that the events that occur in this world unfold according to God's, or some type of nonhuman entity's, plan?

Following the original study, the items were evaluated on a 7-point Likert scale with descriptive labels at the extremes, hence ranging from *tremendously doubtful* to *very likely*. Ratings for the two items were averaged to reflect the level of belief in a controlling God.

Manipulation check items

In order to mask the dependent variable and reduce the chances of participants readily discovering the purpose of the study, the items on belief in God were immediately followed by six general questions and four questions on the situation described by the participants in the recall task. The general questions included a manipulation check on general feelings of control in one's life; the items on the recalled situation served as a check on instruction compliance (i.e. the situation in the control affirmation condition did indeed involve high levels of personal control; the situation in the control threat condition involved low levels of control) and as a reinforcement of the idea that the study supposedly investigated memory. Similar to the items on belief in God, all questions were evaluated on a 7-point Likert scale with descriptive labels at the extremes. The crucial manipulation check items assessing general feelings of control in one's life were:

(1) To what extent do you feel like you are the one who is in control of your life?
(2) To what extent do you consider yourself the actor in, or the director of, your life?

The ratings on these two items were averaged to reflect general feelings of personal control. The additional personality questions assessed self-esteem (1 item) and mood (2 items; following Kay et al., 2008), and extroversion (1 item). The 4 items on the recalled situation assessed perceived control, affect, vividness, and significance.

Although inclusion of these additional questions and manipulation check items deviates from the original study, we believe that it did not meaningfully change the crucial effect of the experimental control manipulation on belief in a controlling God. Specifically, because all added questions were presented *after* measurement of the dependent variable, the main study remained a direct replication of the experiment by Kay et al. (2008). Moreover, we believe this deviation was justifiable as it reduces the probability of participants correctly identifying the tested hypothesis, a risk we considered fairly high.

Religiosity and demographics

Finally, participants' age, gender, and level of religiosity were assessed at the end of the experiment. Level of self-reported religiosity was expected to be highly correlated with the dependent variable and was included in the analysis. Again, a 7-point Likert scale was used to measure religiosity ("How religious do you consider yourself?"), ranging from *not at all religious* to *extremely religious*.

Procedure

Although the original study administered materials on paper, the current replication used a computerized version presented using the survey software Qualtrics. We believe this

adjustment of the original experiment was reasonable in light of the advantages of an online experiment in terms of efficiency and potential for recruiting a large and more representative sample, as well as the fact that we saw no reason to assume that the application of an online version might change the experiment in any meaningful way. Importantly, in the meta-analysis by Landau et al. (2015), 36% of the studies were conducted online. The authors found no effect of method of presentation (called "region" in the article), corroborating research demonstrating cross-method consistency between lab and online studies in various social-psychological domains (e.g. Buchanan & Smith, 1999; Gosling, Vazire, Srivastava, & John, 2004; Robins, Trzesniewski, Tracy, Gosling, & Potter, 2002; Srivastava, John, Gosling, & Potter, 2003).

The experiment was conducted in the order as presented under *Materials*. That is, after a short introduction, participants were presented with the recall task for which they were randomly assigned to either the control affirmation condition or the control threat condition. Subsequently, participants rated their belief in the existence of a controlling God (2 items) and filled out the additional questions on general personality and on the recalled situation (10 items), including the two manipulation check items. Finally, participants provided demographics, including the religiosity item, and completed an awareness check.[12]

Data analysis

All analyses were conducted in a Bayesian framework. The *BayesFactor* R package was used to calculate Bayes factors in order to quantify the evidence for or against the main experimental and the covariate hypothesis (Morey & Rouder, 2015). Specifically, we used the *lmBF* function which allows for the inclusion of categorical (i.e. control condition) and continuous (i.e. religiosity) predictors. Moreover, the statistics software JASP (JASP Team, 2018) was used to calculate the Bayes factors for the manipulation check hypothesis (i.e. a directed independent samples *t*-test) and the correlational hypothesis (i.e. a Kendall's tau negative correlation test). The full R code as well as the JASP files are published on the OSF (https://osf.io/49xz3). The online supplement additionally contains the detailed description of all anticipated analysis paths as preregistered, plus the application of these analyses on a simulated data set. As the description included potential outcomes that were not observed and analysis steps that were therefore irrelevant, in the main text we confine ourselves to the relevant analysis paths (see Figure 2). We declare that the proposed confirmatory analyses were followed exactly.

Prior specification

A default Jeffreys-Zellner-Siow (JZS) prior for ANOVA/general linear models was used, with an r-scale of fixed effects of 0.5 (for the control condition variable), and r-scale of covariates of .354 (for religiosity; Rouder, Morey, Speckman, & Province, 2012; Wetzels, Grasman, & Wagenmakers, 2012). For the Kentall's tau correlation, the default uniform prior proposed by Jeffreys (1961) was used (van Doorn, Ly, Marsman, & Wagenmakers, 2018). That is, a stretched beta prior with width 1.

Calculation of Bayes factor

For all our specified hypotheses, we expected a directed effect, i.e. a one-sided test. Therefore, Bayes factors BF_{+0} or BF_{-0} were calculated in order to evaluate the extent to

which the data were likely under the alternative hypothesis $\mathcal{H}_+$ or $\mathcal{H}-$ versus the null hypothesis $\mathcal{H}_0$. Note that the subscripts on Bayes factor to refer to the hypotheses being compared, with the first and second subscripts referring to the one-sided hypothesis of interest and the null hypothesis, respectively. BF_{+0} is used in case of a hypothesized positive effect for the reference group or a positive relation between variables; BF_{-0} is used for a negative effect for the reference group or a negative relation between variables.

The Bayes factor reflects the change from prior model probabilities to posterior model probabilities and as such quantifies the evidence that the data provide for $\mathcal{H}_+$ versus $\mathcal{H}_0$. For the experimental effect, this can be specified as $\mathcal{M}_{exp}$ versus $\mathcal{M}_{cov}$, reflected by:

$$\underbrace{\frac{p(\mathcal{M}_{exp}|\text{data})}{p(\mathcal{M}_{cov}|\text{data})}}_{\text{posterior odds}} = \underbrace{\frac{p(\mathcal{M}_{exp})}{p(\mathcal{M}_{cov})}}_{\text{prior odds}} \times \underbrace{\frac{p(\text{data}|\mathcal{M}_{exp})}{p(\text{data}|\mathcal{M}_{cov})}}_{\text{Bayes factor}} \tag{1}$$

Indeed, the Bayes factor BF_{+0} then represents the ratio of the marginal likelihoods of the observed data under $\mathcal{M}_{exp}$ and $\mathcal{M}_{cov}$:

$$BF_{10} = \frac{p(\text{data}|\mathcal{M}_{exp})}{p(\text{data}|\mathcal{M}_{cov})} \tag{2}$$

By default, prior model odds were assumed to be equal for both models. As the evidence is quantified on a continuous scale, we also present the results as such. Nevertheless, we included a verbal summary of the results by means of the interpretation categories for Bayes factors proposed by Lee and Wagenmakers (2013, p. 105), based on the original labels specified by Jeffreys (1939).

Results – preregistered

For the confirmatory analyses, we followed the analysis pipeline as specified in the preregistration. Figure 2 represents the pipeline and highlights the route and subsequent conclusions that the results indicated. Below, the results of the individual analysis steps are outlined.

Experimental effect

In the analysis of the original study, a two-way univariate ANOVA was conducted, including the factors *control* (threat vs. affirmation), *nature of God* (controlling vs. creating) and religiosity as a covariate (i.e. an ANCOVA). However, since the replication focused solely on the crucial control threat effect on belief in a controlling God, we preregistered and conducted a one-way ANCOVA instead. Specifically, we calculated the Bayes factor for the hypothesis that the personal control threat induced a higher rating for belief in a controlling God, compared to the personal control affirmation (BF_{+0}), in addition to the effect of religiosity. The descriptive statistics for the experimental hypothesis are given in Table 1 and the data are plotted in Figure 3.

Table 1. Descriptive statistics of belief in a controlling God by country and control condition.

Country	Condition	n	Mean	Median	SD
Netherlands	Control affirmation	214	2.76	2.00	1.97
Netherlands	Control threat	224	2.55	1.50	1.94
United States	Control affirmation	197	3.20	2.50	2.20
United States	Control threat	194	3.37	2.75	2.41

Note. Belief in a controlling God as measured on a 7-point Likert scale and averaged over the two items.

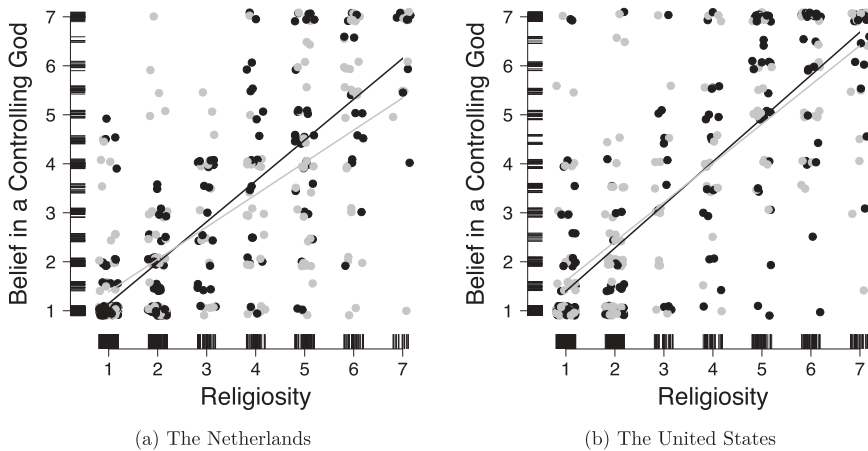(a) The Netherlands   (b) The United States

Figure 3. Scatter plots with the relation between religiosity and belief in a controlling God: (a) in the Netherlands and (b) in the US. Light dots represent individuals in the control threat condition and dark dots individuals in the control affirmation condition. Note that the data points are jittered to enhance visibility of overlapping observations.

Outcome neutral criterion

First, we tested the covariate hypothesis to assess whether the outcome neutral criterion was met. That is, we compared the null model ($\mathcal{M}_0$) to the model including religiosity (covariate; $\mathcal{M}_{cov}$) to validate the positive relation between religiosity and belief in a controlling God. Results revealed a Bayes factor of 2.20×10^{71} in favor of $\mathcal{M}_{cov}$ relative to $\mathcal{M}_0$; indicating that – given the data – a positive correlation between religiosity and belief in a controlling God is about 2.20×10^{71} times more likely than no relation. In the US, a similar relation was observed; here we found a Bayes factor of 8.39×10^{74} in favor of $\mathcal{M}_{cov}$ relative to $\mathcal{M}_0$. In order words, for both countries, the data provide overwhelming evidence for the covariate hypothesis.

Experimental effect

In order to quantify the evidence for the control threat effect on belief in a controlling God, we compared the model including only religiosity ($\mathcal{M}_{cov}$) to the model including religiosity and control condition ($\mathcal{M}_{exp}$). In the Netherlands, we found a Bayes factor of 0.18 in favor of $\mathcal{M}_{exp}$ over $\mathcal{M}_{cov}$; $BF_{+0} = 0.18$ (i.e. the evidence for the null hypothesis was: $BF_{0+} = 5.41$). This means that the data are about 5.41 times more likely under the null model including only religiosity, compared to the alternative model that also includes the control-threat manipulation.

This is constitutes moderate evidence against an effect of control threat on belief in a controlling God. In the US, a similar pattern was observed; $BF_{+0} = 0.09$ (i.e. $BF_{0+} = 11.16$) indicates strong evidence for the null hypothesis over the experimental hypothesis that the control threat manipulation resulted in heightened belief in a controlling God. Following the analysis plan, these findings are taken as "replication failure" for the experimental control threat effect on belief in a controlling God. The raw data for both countries are displayed in Figure 3 (posterior distributions of the model parameters are plotted in the online supplement).

Interaction effect

Although we did not find a main effect of control condition on belief in a controlling God, there may have been an interaction between religiosity and control condition, e.g. the control-threat effect could be present only for those who are already strongly religious. In order to investigate this possibility, we compared $\mathcal{M}_{exp}$ to the model including religiosity, control condition, and the interaction between religiosity and control condition ($\mathcal{M}_{full}$). This yielded no evidence for an interaction effect: $BF_{10} = 1.72$ for $\mathcal{M}_{full}$ relative to $\mathcal{M}_{exp}$ in the Netherlands. In the US, we found a $BF_{10} = 0.10$ for the $\mathcal{M}_{full}$ relative to $\mathcal{M}_{exp}$ (i.e. $BF_{01} = 10.16$), indicating strong evidence in favor of the no-interaction hypothesis.

Posterior model probabilities

Assuming equal prior probabilities for all three models and using Bayes' rule (see Equation 1), the posterior model probabilities are 0.744 and 0.889 for $\mathcal{M}_{cov}$, 0.094 and 0.101 for $\mathcal{M}_{exp}$, and 0.162 and 0.010 for $\mathcal{M}_{full}$, for the Netherlands and the US, respectively (see Table 2). These results demonstrate again that the religiosity-only model predicted the observed data better than the control threat model and the full model.

Manipulation check

In order to assess the effectiveness of the experimental manipulation, we tested whether the personal control threat condition indeed elicited lower general feelings of personal control, relative to the personal control affirmation. In the Dutch sample, we found no evidence that the control threat manipulation lowers feelings of general control, relative to the affirmation condition, indicating that the manipulation was not successful. The effect size was $\delta = 0.008$, 95% CI [−0.176, 0.193], $BF_{-0} = 0.11$ (i.e. $BF_{0-} = 8.79$), which qualifies as moderate evidence for the null hypothesis.[13] Similarly, in the American sample, there was no evidence for the effectiveness of the manipulation: $\delta = 0.081$, 95% CI [−0.116, 0.277], $BF_{-0} = 0.25$ (i.e. $BF_{0-} = 4.06$), which qualifies as moderate evidence for the null hypothesis. As specified in the analysis plan, these results indicate that the manipulation was unsuccessful.

Table 2. Posterior model probabilities.

	Netherlands	United States
Religiosity Only	0.744	0.889
Religiosity + Control	0.094	0.101
Religiosity + Control + Religiosity*Control	0.162	0.010

Note. All three models were assumed to be equally likely a priori.

Correlational effect

In addition to the experimental hypothesis, we assessed the relationship between feelings of personal control and belief in a controlling God. As we expected a monotonic, but not necessarily linear relation, a one-sided (negative) Bayesian Kendall's tau correlation test was used. In the Netherlands, we found $\tau = -0.010$, 95% CI [−0.074, 0.052], $BF_{-0} = 0.08$ (i.e. $BF_{0-} = 12.24$). This qualifies as strong evidence for the null hypothesis. In the US, on the other hand, we found $\tau = -0.144$, 95% CI [−0.210, −0.078], $BF_{-0} = 1185$. This qualifies as extreme evidence for the presence of an inverse relation between general feelings of personal control and belief in a controlling God.

Cross-cultural effect

The results of the cross-cultural analysis with combined data from the Dutch and American sample corroborate the findings from the separate analyses; we find $BF_{10} = 0.08$ (i.e. $BF_{01} = 12.02$), indicating that the data are 12.02 times more likely under the Religiosity + Country model compared to the Religiosity + Country + Control-Threat Condition model. This indicates strong evidence for the null hypothesis that the control threat manipulation did not have an effect on belief in a controlling God. As seen in Table 3, adding an interaction between country and condition also did not increase the posterior model probability. The model including only Religiosity and Country outperforms the alternative models. Note that the added predictive adequacy of the Country parameter reflects the main effect of country, i.e. belief in a controlling God is higher in the US compared to the Netherlands.

Additional analyses

Positive controls

The relationship between belief in a controlling God and gender was included as a "positive control test" to establish the validity of the dependent variable. The relation between gender and religiosity appears one of the most robust effects with regard to religious beliefs; women consistently report being more religious than men (Bradshaw & Ellison, 2009; Collett & Lizardo, 2009; Francis, 1997; Miller & Hoffmann, 1995; Roth & Kroll, 2007). Indeed, in both samples, we found evidence for the hypothesis that women more strongly believe in a controlling God than men: $BF_{+0} = 6.07$ (i.e. moderate evidence) in the Netherlands and $BF_{+0} = 24.42$ (i.e. strong evidence) in the US.

Control for effort

In order to investigate whether there were any differences in the amount of time or number of words participants spent on writing for the experimental manipulation, we conducted two Bayesian default two-sided t-tests. The amount of time spent on the memory recall item did not differ between conditions in the Netherlands: $BF_{10} = 0.19$ (i.e. $BF_{01} = 5.40$; moderate evidence for the null hypothesis), or in the US: $BF_{10} = 0.17$ (i.e. $BF_{01} = 6.02$; moderate evidence for the null hypothesis). Furthermore, the number of words used to describe the memory likewise did not differ between conditions in the Netherlands: $BF_{10} = 0.12$ (i.e. $BF_{01} = 8.69$; moderate evidence for the null hypothesis), or

Table 3. Posterior model probabilities for the cross-cultural effect.

Model	Posterior Probability
Religiosity + Control	0.000
Religiosity + Country	0.912
Religiosity + Control + Country	0.077
Religiosity + Control + Country + Control*Country	0.010
Religiosity + Control + Country + Control*Country*Religiosity	0.001
Religiosity Only	0.000

Note. All six models were assumed to be equally likely a priori.

in the US: $BF_{10} = 0.13$ (i.e. $BF_{01} = 7.53$; moderate evidence for the null hypothesis). See Table 3 in the Online Supplement for descriptives.

Results – exploratory

Instruction compliance

The data showed that the memory recall manipulation did not substantially affect generalized feelings of personal control. Accordingly, it could be that the manipulation was either insufficiently strong to change feelings of control, or that participants simply did not understand or comply with the instructions to report a personal memory in which they did or did not have control over a situation. To explore this issue, we investigated the item in which participants indicated how much control they had experienced in the described situation. Here, we found extreme evidence for the hypothesis that experienced control was higher in the control affirmation ($M = 5.62; M = 5.89$) compared to the control threat condition ($M = 2.85; M = 2.32$) in both countries: $BF_{10} = 8.4 \times 10^{47}$ and $BF_{10} = 1.1 \times 10^{71}$, in the Netherlands and the US, respectively.

Finally, although in both conditions the valence of the described situation was above the midpoint of the scale (i.e. participants rated the situation as positive), we did observe a difference between conditions; the control affirmation situation was experienced as more pleasant ($M = 6.06; M = 6.15$) than the control threat condition ($M = 5.41; M = 5.66$). In the Dutch sample, there was extreme evidence for a difference in valence: $BF_{10} = 568.4$; in the US, the evidence was very strong: $BF_{10} = 23.58$.

Experimental effect excluding unsuccessful recalls

In the analyses reported above, we followed our preregistration by excluding only those participants who wrote nonsensical stories in the recall task. Whereas nonsensical descriptions were rare ($n = 3$ in total), there were a number of participants who indicated that they could not recall a situation that met the requested characteristics, i.e. being recent and positive and over which they had total control/no control. There were 55 and 15 individuals in the Dutch and in the US sample, respectively, who indicated not being able to access an episode as specified.

We re-ran the models including only the participants who succeeded to recall a specific event, in order to investigate whether the experimental effect would be present in this sub-sample. Again, we collected moderate evidence against the experimental control hypothesis in the Netherlands: $BF_{+0} = 0.26$, i.e. $BF_{0+} = 3.89$. Similarly, in the US, the

evidence pointed against the experimental effect: $BF_{+0} = 0.13$, i.e. $BF_{0+} = 7.97$. In other words, the results as reported for the confirmatory analysis did not change when we additionally excluded participants who attempted but could not describe a situation in line with the experimental control manipulation.

Discussion

In the current replication study, we revisited the initial finding suggesting a causal effect of the loss of experienced personal control in one's life on belief in a controlling God (Kay et al., 2008). Our results indicate moderate to strong evidence for the absence of this effect: belief in (a controlling) God is not modulated by a threat compared to an affirmation of personal control. Using large samples ($N \approx 400$) we did not replicate the original experiment by Kay et al. (2008) in the Netherlands, nor in the US. In a complementary analysis, we assessed the correlational relationship between feelings of personal control and belief in a controlling God. In the Dutch sample, no relationship was found. In the American sample, people who experienced lower levels of personal control in their lives, reported a stronger endorsement of belief in a controlling God – although the effect size of this relationship was small.

The data also showed no effect of the personal control manipulation on feelings of personal control in one's life. This manipulation failure is remarkable for several reasons: (1) affecting feelings of personal control is the very purpose of the experimental manipulation (Kay et al., 2008); (2) an effect of the manipulation on generalized personal control has been validated in a separate pilot study reported by Kay et al. (2008); (3) feelings of personal control are the core construct of CCT (Kay, Whitson, Gaucher, & Galinsky, 2009; Landau et al., 2015); and (4) these manipulation check items have been successfully used in previous studies (e.g. Cutright, 2011; Goode et al., 2014; Rutjens et al., 2010, 2013). It should be noted that Kay et al. (2008) verified the effect of the control threat manipulation on general feelings of personal control in an independent study, rather than adding the manipulation check to the main study. The reason for separating the two effects was that the intervening opportunity to affirm control – via endorsing the existence of a controlling God – should eliminate any effect of the control manipulation on the manipulation check; people who have already restored their sense of structure or order will not report a residual lack of control. Nevertheless, in our study, the control threat manipulation did not influence belief in a controlling God. Therefore, if participants' feelings of control were threatened, the lack of control was not yet buffered and should have been reflected in the manipulation check items.

The lack of an experimental effect may be related to the framing of the autobiographical recall task and/or to the potential inefficacy of experimental control threat manipulations. First, it could be that the specific instruction to recall a recent *positive* memory might be related to the absence of an effect. A positive situation is typically not experienced as *threatening*; prototypical examples of positive situations in which one lacks personal control are the experience of "luck", "happy coincidence", or "fate". Quite a few participants in our study had a hard time recalling a recent positive situation in which they had or lacked control; 70 individuals (i.e. 8.4% of the total sample) reported not being able to recall such a situation. Although exploratory analyses found that participants evaluated the described episodes less positive in the control threat condition than in the control

Table 4. Examples of recalled situations in the control affirmation and control threat condition as reported by participants.

Condition	Description
Control Affirmation	"I completed my first 5K run. While I did not place first, I was nonetheless pleased with my performance. The run itself was exciting and I felt a sense of satisfaction when I was done"
Control Affirmation	"I lost 15 lbs by cutting sugar from my diet and controlling my eating for 30 days"
Control Affirmation	"I entered several pieces of art into a juried show; they were accepted. And while I didn't have total control over their acceptance, I did over the production of the art pieces. Which is enough"
Control Threat	"Insurance Visa card payed up for an item not received or ordered"
Control Threat	"Potential for promotion/title change with change in admin and bureaucracy. Happy, to an extent, but not totally consistent with my future goals"
Control Threat	"My wife and I went out for dinner with a neighbor. The neighbor paid the bill without us noticing. It was a very thoughtful gesture and I felt appreciated"

Note. These examples are randomly drawn from all responses in the autobiographical recall task (excluding unsuccessful recalls) in the US sample, since these were written in English and did not require translation.

affirmation condition, many of the situations that participants reported would not qualify as "threatening". Some of the situations that our manipulation elicited are exemplified by a collection of responses. These were randomly drawn from both the control affirmation and the control threat condition and are displayed in Table 4.

Our manipulation was similar to the original study by Kay et al. (2008). The rationale for asking participants to recall a positive situation was to control for the possible confound that any effect might be simply related to the valence of the memory. Research on divine responsibility additionally alludes to the notion that positive episodes may be associated with God. Early studies already suggested that people often tend to make supernatural attributions, also in the case of positive experiences (Gorsuch & Smith, 1983; Ritzema & Young, 1983). For example, Gorsuch and Smith (1983) found that positive outcomes of good fortune were frequently regarded as acts from God's hand. Similarly, Norenzayan and Lee (2010) found that scenarios about winning a lottery or meeting the love of one's life were often attributed to fate, and mostly so for religious individuals, suggesting these individuals inferred divine responsibility to be at play. Following this line of argumentation, it could well be that uncontrollable positive situations as induced in the present autobiographical recall task foster belief in a controlling God as a compensatory source of control and as a causal agent (indirectly) explaining the occurrence of these uncontrollable events.

At the same time, however, the literature indicates that divine attributions tend to occur more frequently for extraordinary and improbable events that lack alternative explanations (Gorsuch & Smith, 1983; Ritzema & Young, 1983), whereas participants in the current study mostly reported mundane events. According to CCT, people have a fundamental drive to obviate the experience of randomness in the world (Kay et al., 2009). Compensatory strategies such as endorsing belief in an intervening God are triggered when personal control is low, in order to satisfy the basic need to maintain a sense of non-randomness. This assumes that the lack of personal control is experienced as an aversive state. However, the uncontrollable yet positive and mundane situations described by participants in our study likely did not sufficiently activate the need to restore a sense of control through compensatory efforts.

A second reason for our replication failure could be related to the possibility that an experimental recall manipulation may be ineffective in instilling a sufficiently powerful sense of (un)controllability. Autobiographical recall tasks have been used extensively in

research on mood induction (e.g. Strack, Schwarz, & Gschneidinger, 1985). Although many studies provide supportive evidence for the efficacy of autobiographical recall in inducing basic emotions and mood (e.g. Jallais & Gilet, 2010; Siedlecka & Denson, 2019), other studies have failed to find these effects (Göritz & Moser, 2006). Recalling a particular episode in a lab or behind a computer is probably too subtle to produce an experience that is comparable to that in the original situation and hence may fail to exert causal impact on any outcome of interest (see, for instance, Schjoedt, 2009 for a similar argument in the context of religious and mystical experiences). This may be a particular concern for manipulations aiming to induce a relatively complex cognitive state (e.g. experience of power or control), rather than an arguably stronger emotional state.

Our findings cohere with those of van Elk and Lodder (2018). Across seven experiments they found no support for the effectiveness of various personal control manipulations, including the autobiographical recall task used in the present study. Our suggestion that autobiographical recall manipulations may be ineffective echoes recent discussions in the priming literature, where the effectiveness of behavioral priming generally (Cesario, 2014; Doyen, Klein, Pichon, & Cleeremans, 2012; Pashler, Rohrer, & Harris, 2013; Shanks et al., 2013; Stroebe & Strack, 2014), and religious priming specifically (Gomes & McCullough, 2015; van Elk et al., 2015; van Elk, Rutjens, van der Pligt, & van Harreveld, 2016) was called into question. Some contested effects also included autobiographical recall manipulations, for instance with respect to experimental effects of feelings of power (e.g. Galinsky, Magee, Gruenfeld, Whitson, & Liljenquist, 2008) and morality (Fayard, Bassi, Bernstein, & Roberts, 2009; Zhong & Liljenquist, 2006).

In response to these replication failures, Lammers, Dubois, Rucker, and Galinsky (2017) argued that ease of retrieval can moderate the effectiveness of recall manipulations of cognitive constructs such as power and control. The authors showed that recall manipulations are ineffective or even counter-effective when the instructed situation or experience is highly inaccessible. Although we did not directly address this possibility, we consider this explanation of our null results unsatisfactory for two interrelated reasons: First, the exploratory analysis which only included participants who managed to successfully recall a situation likewise provided evidence in favor of the null hypothesis. Second, when we added the interaction between time spent on the recall task (as a proxy for ease of retrieval) and control condition to the model, this resulted in very strong evidence against the moderation model: $BF_{10} = 0.008$ and $BF_{10} = 0.030$ for the Netherlands and the US, respectively.

We found supportive evidence for a correlational effect consistent with predictions derived from CCT, namely: in the US sample overall feelings of control were related to belief in a controlling God. This finding is in line with previous observations by van Elk and Lodder (2018), who exploratorily found that general subjective feelings of control were associated with different dependent variables related to epistemic structuring tendencies across four of the seven experiments. This again suggests that it is difficult to manipulate feelings of control experimentally, but that relatively stable individual differences in the experience of control are associated with compensatory strategies in a way that is compatible with CCT.

In our study, the correlation between feeling of control and belief in a controlling God was only found in the US and not in the Netherlands. This cross-national difference may be related to country-level differences in the cultural prevalence of religiosity and existential security (Barber, 2011). Religion is deeply rooted in US cultural identity; Christianity

presently continues to shape American lives and guide politics (Wald & Calhoun-Brown, 2014). As such, the notion of a controlling God may – unconsciously – be seen as an especially appealing and comforting belief – especially for individuals who experience little personal control. At the same time, strong faith in a controlling God logically implies reduced personal control – as exemplified for instance in the Protestant notion of "Predestination" (Weber, 1930). These mechanisms but may be mutually reinforcing, together contributing to the negative relation between perceived personal control and belief in divine control as found for the US sample.

In the Netherlands, in contrast, the role of religion in socialization and education has rapidly declined over the last 50 years, curtailing religion's pervasiveness in society (Kregting et al., 2018). Combined with the relatively strong welfare system in the Netherlands, the marginal role of religion makes God a far less likely source for offering a sense of order and control in the world than in the US (Norenzayan & Gervais, 2013). It may well be that in the Netherlands, faith in the government or science constitutes a stronger source for offering compensatory control.

In conclusion, one important general lesson from this work is that caution is warranted in generalizing the effectiveness of experimental manipulations of control across samples and contexts (e.g. Cesario, 2014). Psychological researchers should be sensitive to and explicit about contextual boundaries of the phenomena of interest. In the current study, we anticipated that the cultural religious context would be a boundary condition for the compensatory control effect with respect to religious beliefs. Indeed, we showed that cultural setting affected the relation between feelings of control and belief in God – but only when using an individual differences approach. For the experimental effect, the cultural background appeared to be irrelevant as the manipulation was ineffective across the board; we did not find the experimental effect in a secular country (i.e. the Netherlands), nor in a highly religious country (i.e. the US).

It seems plausible that in periods and places characterized by little personal control some people are drawn to religion to reduce uncertainty and unpredictability in their lives; churches and temples may thrive during times of war or natural disaster, but it remains difficult to investigate this theory by means of experimental and autobiographical priming manipulations.

Notes

1. The authors only reported statistics for the main effect for anxiety and the anxiety–personal control interaction, but omitted results for the main effect of control on belief in God. Therefore, the result of this replication cannot be quantified. The figure on page 1561, however, suggests that the main effect is not significant.
2. Importantly, this study was presented on poster that reported only the F-values and p-values ($F(2, 151) = 30.11, p < .001$), illustrated with a graph of the descriptives. Notably, although belief in God is reportedly measured on a 7-point Likert scale, based on visual inspection of the graph, the mean of the control threat condition appears to be approximately 7.8. We approached the authors to validate these results and request descriptive statistics per group, but we did not receive a reply. Therefore, some caution is warranted in evaluating this finding.
3. As the paper by Laurin et al. (2008) did not include any statistics on the main effect of the personal control manipulation on belief in God, we were not able to calculate the Bayes factor for this study.

4. Bayes factors were calculated based on the *F*-value converted to *t*-value and sample size reported in the original studies, using the meta.ttestBF function of the package BayesFactor (one-sided) in R with default priors (Morey & Rouder, 2015). The exact number of participants per group was not reported in any of the studies, and we therefore assumed that participants were uniformly distributed over conditions.
5. See van Elk and Lodder (2018, pp. 29–31) for a detailed description on the error in the original meta-analysis and their reanalysis.
6. Achieved power was calculated with G*Power 3.1, using the sample size ($n = 18$) and *F*-value ($F = 5.12$) of the experiment by Kay et al. (2008) and the converted meta-analytic effect size of $f = 0.247$ (original) and $f = 0.189$ (corrected; Faul, Erdfelder, Lang, & Buchner, 2007).
7. For more details and extended discussion on Bayesian inference, we recommend the recent special issue of Psychonomic Bulletin & Review (Etz & Vandekerckhove, 2018).
8. In an opposite but complementary fashion, Cutright (2011) showed in Study 6 that religiosity moderated the effect of control threat on the tendency to prefer bounded relative to unbounded products. As they interpreted the preference for boundaries as a epistemic structuring tendency, they argued that highly religious individuals do not respond to control threats by compensating though choosing boundaries, as they already have a better alternative for restoring structure, i.e. belief in a controlling or structuring God. Analogously, atheists may use their trust in the government or a societal institution, rather than belief in God to buffer against the feeling of discomfort elicited by the control threat.
9. Note that we only excluded senseless stories, as preregistered. There were, however, also participants who wrote that they could not recall a situation that fit the particular characteristics that were requested. These participants were retained in the sample for the main analyses, but in the exploratory results section, we additionally report analyses excluding these participants.
10. We chose the corrected effect size of the meta-analysis, rather than the effect size of the original study ($\delta = .769$) as this provides the most conservative estimate. We realize that BFDA is developed for planning designs in the context of a directed independent-groups *t*-test. Although we will use a one-way ANCOVA instead of a *t*-test, we believe BFDA can still provide a valuable indication of the desired sample size. That is, since our analysis will also focus on a directed hypothesis comparing two independent groups, we consider BFDA more suitable for the current study than a traditional power analysis.
11. Although this inevitably creates a language confound – as in any cross-national study – we believe the use of the different, i.e. native languages has higher ecological validity. Moreover, conducting the study in participants' second language induces a probably even larger confound (i.e. it will be more difficult to describe a situation in their second rather than their first language).
12. In our preregistration, we specified that we would investigate whether there was a difference between participants who correctly identified the relation of interest vs. participants who did not. However, analysis of the awareness check (i.e. "What do you think this research was about?") indicated that only one person in the Dutch sample and two people in the US sample correctly derived that the study investigated whether people tend to more strongly believe in God after recalling a situation in which they did not have control. Therefore, we decided not to run separate analyses.
13. Note that the parameter estimation for the effect size and the confidence interval are based on the unrestricted model, whereas the Bayes factor is derived from the order-restricted model. This applies to all directed tests.

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