Does a Pandemic Increase Religiosity in a Secular Nation? A Longitudinal Examination

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Abstract: The COVID-19-pandemic offers a unique, if tragic, opportunity to assess the impact of a worldwide crisis on religion. Theories from various disciplines including the psychology of religion and cultural evolution suggest that crises cause higher levels of religiosity. However, such theories also predict that levels of religiosity should remain stable in the context of well-functioning governments, secular institutions and norms that might address social, epistemic, and material needs in a crisis. While the relationship between crisis and religion have been examined in countries with higher levels of religiosity, it has yet to be extensively empirically assessed in countries with lower levels of religiosity. Here, on the basis of explicit causal assumptions and using Bayesian multi-level modeling, we analyze quasi-representative longitudinal data from Denmark collected over the course of the pandemic from May 2020 to December 2021. Our analysis shows that self-reported religiosity did not increase during the pandemic on average, an inference that is robust to a range of model specifications, including full Bayesian imputation of missing covariates and poststratification. We discuss possible interpretations of this finding and argue for an emphasis on cultural context going forward in theorizing on crises and religion.

Keywords: Religiosity – COVID-19 – Panel data – Bayesian Multilevel Modeling – Denmark

Replication statement: The data and code that support the findings of this study are openly available at <https://github.com/tbendixen/dk-pandemic-panel>

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1. Introduction

It has often been argued that the *raison d'être* of religion is to offer meaningful explanations and coping strategies in times of crisis, a notion encapsulated in the popular phrase that “there are no atheists in foxholes”. Researchers from across disciplines argue that religion serves as a particularly potent tool when individuals or groups face a crisis and its related negative consequences. The proposed mediating mechanisms are many. In the psychology of religion, the focus is mainly on the individual level. One proposed mechanism is religious coping (K.I. Pargament 2001), defined as “efforts to understand and deal with life stressors in ways related to the sacred” (K. Pargament, Feuille, and Burdzy 2011). Another potential mediating mechanism is meaning making, which underscores how religion can help the individual negotiate and re-make meaning after crises (Park 2005; Steger and Frazier 2005; Park et al. 2017). A third mechanism is found in terror management theory, which argues that the human awareness of the finity of life combined with a strong wish for living can provoke existential crisis and death-anxiety, which must be dealt with through psychological buffers such as religious beliefs about an afterlife (Van Tongeren et al. 2017; Vail III, Soenke, and Waggoner 2019; Vail et al. 2010; Greenberg, Pyszczynski, and Solomon 1986).

In the cultural evolution literature, religion is also argued to serve a crisis-management function. Here, the explanations are primarily related to the group or societal level. Threats such as war (Henrich et al. 2019) and natural disasters (Sibley and Bulbulia 2012; Sinding Bentzen 2019) have been found to increase people's adherence to already-existing social and cultural norms such as religious norms. Responding in this way to external threats might contribute to sustaining local cooperation and coordination in the face of hardship (Roos et al. 2015; Henrich et al. 2019).

However, while such theories generally predict that levels of religion will increase during crises, they also leave room for other worldviews, such as secular ones, to fill this role. Within terror management theory, it is emphasized that while religion can function particularly well as a psychological buffer (Vail et al. 2010), the main mechanism in the face of death is increased support to one's *existing* worldviews (Wojtkowiak and Rutjens 2011), including religious, secular, scientific, and political worldviews (Solomon and Thompson 2019). From a cultural evolutionary perspective, it has also been underscored

that norms which support cooperation can be both religious or secular as long as they encourage an increase in support of sociality and local institutions (Henrich et al. 2019).

Numerous studies investigate the role of religion in times of crises during the COVID-19-pandemic. Some studies have examined how religious communities have responded to and been affected by restrictions caused by the pandemic (cf., DeFranza et al. 2020; Kühle and Larsen 2021; Du Plessis 2021; Riexinger 2021). Other studies have investigated how the health of religious individuals might potentially benefit from religiosity when handling the pandemic (Pirutinsky, Cherniak, and Rosmarin 2020; Thomas and Barbato 2020; Koenig 2020; Schnabel and Schieman 2022; Cowden et al. 2022), and large studies have examined have shown how the pandemic has led to an increase in prayer internationally (Bentzen 2021). Much of this research, however, has been undertaken in a North-American, cross-sectional context and thus we lack examinations from more secular countries as well as studies which can scrutinize the relation of religion and crises over time.

In this article, we examine longitudinal self-reported religiosity during the pandemic in a culturally and nationally secular context, namely that of Denmark. Denmark is often described as one of the most secular countries in the world (Zuckerman 2020). While 74 percent of the population are members of the Evangelical-Lutheran Church of Denmark (Church-Ministry 2021), attendance at services is generally low and Danes consistently score low on various measures of religious beliefs (Furseth 2017). Consequently, religious membership is less about beliefs and practices and more about community. That is, membership is instead based on feelings of a collective, cultural, and national affiliation with the church (Mauritsen et al. in press; Storm, Rutjens, and van Harreveld 2020). Denmark therefore offers an unique cultural context for investigating the relationship between crisis and religiosity.

The first case of COVID-19 was found on the 27th of February 2020 in Denmark and on the 11th of March 2020, an extensive lock-down was implemented, shutting down many welfare institutions: schools, daycare-institutions, and public institutions as well as non-critical private enterprises. The consequences of the pandemic were many, both in terms of physical, mental and public health (Sønderskov et al. 2020; Petersen et al. 2021), limited levels of freedom and mobility (Zhang et al. 2021) as well as financial consequences (Bess et al. 2020) and increased levels of unemployment (Pultz, Hansen, and Jepsen 2021). Taking these perspectives into account, we argue that the pandemic can be

defined as a crisis, i.e. an event “associated with urgent, high-stakes challenges in which the outcomes can vary widely (and are very negative at one end of the spectrum) and will depend on the actions taken by those involved” (Penuel, Statler, and Hagen 2013).

How, then, did Danes respond during this crisis in terms of levels of self-reported religiosity? And how can our findings contribute to the theoretical work on the relationship between religiosity and crises? Here, on the basis of explicit causal assumptions (Bulbulia et al. 2021; Pearl, Glymour, and Jewell 2016) and using Bayesian multilevel regression, we analyze quasi-representative longitudinal data from Denmark collected over the course of the pandemic from May 2020 to December 2021. Our analysis shows that self-reported religiosity did not increase during the pandemic on average, an inference that is robust to a range of model specifications, including full Bayesian imputation of missing covariates and poststratification. We discuss possible interpretations of this finding and argue for an emphasis on cultural context going forward in theorizing on crises and religion.

2. Methods and Results

The Methods and Results section is organized as follows. First, we briefly discuss the promises and pitfalls of causal inference in the context of a longitudinal survey study and lay out our causal assumptions in diagram form (Section 2.1). This section leads into details about data and analysis (Section 2.2), including data background (Section 2.2.1), operationalization of outcome and covariate variables and details on missing values (Section 2.2.2) and the overall analysis plan (Section 2.2.3). We present results in Section 2.3. In the supplements, we discuss the statistical models in more detail, including missing data assumptions, the formal structure of the main models, prior and posterior predictive checks, poststratification, imputation of missing covariates, and supplementary plots and results. All data and code to reproduce the reported results and figures are publicly available.

2.1 Causal Inference

Central to the above-outlined hypotheses is an explicit causal question, namely whether experiencing a pandemic impacts individual-level religiosity. We illustrate our assumptions about the causal structure of the data-generating process in Figure 1. The figure is a causal diagram in the form of a *directed acyclic graph* (DAG), where letters, or nodes, represent (sets of) variables (or parameters), and arrows, or edges, represent causal effects of some functional relationship (for an introduction to causal diagrams and related literatures, see e.g., Pearl et al., 2016). In Figure 1, P denotes exposure to the pandemic, R_{t1-4} denote level of religiosity across the four time points, C denotes a set of “time-varying” covariates (see below) through which we assume that the potential effects of the pandemic on religiosity is mediated, and Z denotes a set of “time-invariant” covariates (specifically, education level, age, and gender)¹.

In typical applied settings, one compares a group of exposed individuals with an otherwise comparable unexposed group to infer the average causal effect of the exposure, often conditional on some covariates. Our study differs in that every individual in our data set was exposed to the pandemic one way or the other, represented by a box around P in Figure 1, and as such we do not have an actual, individually varying exposure variable. Importantly, this prohibits us from formally quantifying an average causal effect of the pandemic on religiosity with standard approaches. Instead, we leverage the power of a longitudinal design to assess withinsubject change in religiosity during the pandemic. This is under the assumption that participants at first measurement, T_1 (approximately two months after national lockdown; see Section 2.2.1), were yet relatively unaffected by the pandemic (see Discussion for further substantiation). We encode this assumption in Figure 1 with a dotted edge from C to R_{t1} . Our main analytical model is designed on the basis of this causal diagram in order to detect whether there is a change in religiosity across four time points and relevant covariates. That is, we are interested in estimating R_{t1-4} and, secondarily, $C \rightarrow R_{t1-4}$, where C serves as a proxy of pandemic exposure.

¹ Here we treat education level and age as “time-invariant”, as we use the baseline (i.e., wave 1) values (see below).

Figure 1: **Directed acyclic graph (DAG) of the assumed causal structure of the data-generating process.** P = pandemic, C = time-varying pandemic exposure (proxy), R_{t1-4} = religiosity across four time points, Z = timeinvariant confounders. The box around P indicates that the dataset is conditional on the pandemic; all participants had experienced the pandemic to some degree at the start of measurement. The dotted line denotes relatively minor pandemic exposure at time T_1 .

Given that all participants had been exposed to the pandemic one way or another at first measurement point, how might we operationalize C ? In our main models, we operationalize C with two variables (see Section 2.2): self-reported household income and subjective health condition. This is under the assumption that low financial security and/or low subjective health would be conditions through which the pandemic could impact participants' levels of religiosity, as suggested by the above-reviewed theories. In our statistical models, we allow each of these variables to interact with time so as to capture their time-varying aspect (see Section 2.3 and supplements Section S2.1).

As a robustness check, in a second set of models, we also assess an alternative operationalization of C , namely self-reported exposure to corona virus. Specifically, participants were asked whether they themselves, someone in their household, a family member, or someone in their relations “is or has been ill with corona virus” and were asked for each target to rate the severity of the exposure. We build a model for each of these exposure items as the predictor with religiosity as the outcome and an interaction between exposure item and time. Due to very few participants reporting severe exposure (i.e., hospitalization due to the virus), including covariates is not feasible. These models support the inference from the main models and are reported in the supplements (Figures S13 – S18).

In sum, despite our inability to directly and formally quantify a causal effect of the pandemic on religiosity due to the lack of a clear baseline, if we – as is the case – fail to detect an increase in religiosity across time and covariates, we draw the causal conclusion that the pandemic did not give rise to heightened levels of religiosity, at least in our sample and study period and as measured by our survey items. We now turn to a detailed description of the dataset and statistical model.

2.2 Data & Analysis

2.2.1 Data background and sample demographics

The dataset comes from an internet-based panel survey of a quasi-representative Danish sample collected by YouGov in four waves during the COVID-19 pandemic (wave 1: May 14th – 21th 2020; wave 2: September 23th – October 9th 2020; wave 3: March 8th – 21st 2021; wave 4: December 3rd – 15th 2021) comprising a grand total of 1538 individuals. For reference, the virus was confirmed to have arrived in Denmark on February 27th and national lockdown was announced on March 13th 2020. Table 1 gives an overview of key demographic variables for each wave of the total sample, in which the outcome variable and all covariates are available for at least one wave.

	Wave 1	Wave 2	Wave 3	Wave 4
<i>N</i>	941	966	841	579
Age [SD]	48.5 [17.2]	49.9 [17.1]	51.8 [16.7]	53.3 [15.7]
Gender – women*	46.3	48.8	49.6	46.8
<i>Education level</i>				
Primary education	31.0	30.4	29.7	31.8
High school/Vocational training	15.6	16.5	16.5	16.1
Short cycle higher education	1.6	1.3	1.4	1.4
(Vocational) bachelors education*	8.5	8.5	9.0	7.1
Masters education	32.3	32.1	31.4	31.8
Doctoral degree	10.9	11.2	11.9	11.9
<i>Household income</i>				
< 100.000	10.0	9.6	10.8	10.2
100.000 – 199.999	9.7	10.6	8.3	8.1
200.000 – 299.999	8.8	8.1	10.6	9.2
300.000 – 399.999	5.6	6.0	5.9	6.7
400.000 – 499.999	4.6	4.6	5.0	4.7
500.000 – 599.999*	10.4	10.7	9.9	12.6
600.000 – 699.999	3.9	2.2	2.1	0.9
700.000 – 799.999	9.2	9.0	7.7	6.4
800.000 – 899.999	13.9	14.3	15.2	15.7
900.000 – 999.999	12.5	13.1	12.8	12.6
> 1.000.000	11.3	11.9	11.5	13.0

Table 1: **Demographic overview for each wave.** *N* is number of participants, where the outcome and all covariates are available for at least one wave. Mean age and standard deviation in parantheses. Percentages for gender, educational and household income levels. Annual household income in DKK. Asterisks indicate reference categories in the regression models.

2.2.2 Operationalization of outcome and covariates

We operationalize our outcome measure of religiosity with a single item, “How important is religion in your life?”, which included the following response options: “not at all important” (1), “not very important” (2), “somewhat important” (3), and “very important” (4)². We selected a single item, rather than a set of items, as our outcome measure for three main reasons: First, regression analysis with a single ordered categorical item as the outcome lends itself to a natural interpretation, namely the cumulative probability of the response options as a function of some predictor(s). Secondly, some alternative religiosity items (i.e., about frequency of ritual performance and church attendance, personal identification as a (non-)believer, and about belief in a personal god) were not consistently included across all four survey waves. Thirdly, in order to explore whether the “importance of religion” item indeed tap into a latent measure of religiosity, we ran Bayesian reliability analysis on the religiosity items from the first wave of data collection. We found that the “importance of religion” item simultaneously covary with the other religiosity items and that when excluded in drop-item analysis the reliability estimate of the remaining set of religiosity items drops more than when excluding any other item. Specifically, when excluding the “importance of religion” item, the reliability estimate (McDonald’s ω with 95% credible (highest posterior density) intervals) of the set dropped from 0.79 [0.77, 0.81] to 0.69 [0.67, 0.72] compared to 0.73 [0.71, 0.76] for the next-largest drop in reliability, which occurred when excluding an item about belief in a personal god. For reliability analysis, we employed the Bayesrel package (Pfadt et al., 2020) for R (R Core Team, 2021) with default priors and missing values sampled as parameters from the joint conditional distribution.

Operationalizing the covariates was more straightforward. Subjective health was measured using a single item, “How would you describe your current health?”, on a scale ranging from 1 (“very bad”) to 5 (“very good”), with 3 (“okay”) set as the reference category in our regression models. For education, there are six levels: “primary school”,

² In the original survey, the order of the response options was reversed so that 1 = “very important” and 4 = “not at all important”. The original survey also included the option “prefer not to respond” (5), which we coded as NA. In wave 1, the item was worded slightly differently, namely: “To what extent do you agree with the statement: Religion is very important to me?” and included the following response options: “highly disagree” (1), “mainly disagree” (2), “neither agree nor disagree” (3), “mainly agree” (4), “highly agree” (5), and “don’t know” (6). We regard the wordings of these two items as sufficiently similar to warrant lumping them. For the wave 1 version of the item, we coded (3) and (6) as NAs.

“high school/vocational training”³, “short cycle higher education” (below three years), “(vocational) bachelors education” (3-4 years; set as the reference category), “masters education” (5 years or more), and “doctoral degree”. For household income, in addition to “prefer not to answer” and “don’t know”, both of which were coded as NA, there were 11 options, ranging from “less than DKK 100.000” to “more than DKK 1.000.000”, with intervals of DKK 99.999, and the response “DKK 500.000-599.999” set as the reference category. For comparison, the average income in Denmark (for men and women, 2019) is around DKK 338.122⁴. Table 1 provides an overview of the distribution of demographic characteristics of the study sample. In particular, higher educated individuals are over-represented in our sample compared to the general Danish population⁵, which, in order to make inferences about this population as a whole, calls for adjustment techniques that incorporate representative demographic information, such as poststratification (see supplements Section S2.3).

In wave 1, there are no missing values for age and gender and only 18 missing values for educational level⁶. Therefore, in the regression analyses, we set participants at their wave 1 values for these variables. In doing so, we obtain estimates that are naturally interpretable, namely comparing participants at their “baseline” demographic values. Across all four waves, the outcome variable has 32.8% missing data, subjective health has 25.5 % missing data, and household income has 39.8% missing data.

³ In this category, we lump “Almen gymnasial uddannelse”, “Erhvervsgymnasial uddannelse”, and “Erhvervsfaglig uddannelse”, which are on the same educational level but were originally coded as separate options, in order for the variable to capture the number years of formal education, not the specific kinds of educational degrees attained.

⁴ <https://www.dst.dk/en/Statistik/emner/arbejde-indkomst-og-formue/indkomster>

⁵ <https://www.dst.dk/en/Statistik/emner/uddannelse-og-viden/befolkningens-uddannelsesstatus/befolkningens-hoejst-fuldfoerte-uddannelse>

⁶ In the few cases of missing values for education, we propagated observed values from previous or later waves. For instance, if a participant had missing education level in wave 1, their value was imported from wave 2 or, if unavailable, from wave 3. While this approach might not be applicable in all contexts, we deem it reasonable in the present case: In addition to avoiding discarding information unnecessarily, we do not theoretically expect the attainment of an higher level of education during the study period to substantially impact personal levels of religiosity. Four participants remained for which educational level was unobserved across all four waves (0.3%); these are excluded in the adjusted complete-case analyses and imputed in the imputation model.

2.2.3 Analysis plan

To address the challenges of missingness and causal inference in the present study, we analyze the data in a series of model specifications. We first fit a model (m1) with listwise deletion of missing values, which we adjust for relevant covariates (see Section 2.2.2 and supplements Section S2.1). To assess the impact on inference by including versus excluding covariates, we also fit a listwise deleted unadjusted model (m0) as well as one model for each of the interaction terms (household income $\times$ wave: m_hinc; subjective health $\times$ wave: m_health) but with no other covariates.

Secondly, we perform two sets of robustness checks. We fit both an unadjusted (m0_complete) and an adjusted model (m1_complete) including only those participants where full data is available for all four waves. Then, since our data are not fully representative (another kind of missing data issue; see below and supplements Section S1), we fit a multilevel regression with poststratification (MRP) using external data on gender, age, and educational level to adjust for sample over-representation of certain groups, such as higher educated individuals. Finally, we employ full Bayesian imputation for missing covariates with the adjusted model (m1_imp). Table 2 provides an overview of all main models.

Throughout model-building and inference, we informally follow a Bayesian workflow (Gelman et al. 2020) using weakly regularizing priors. In the supplements, we provide further modeling details, including prior and posterior predictive checks and supplementary models. For model fitting, we use the brms (Bürkner 2017, 2018) package for R (R Core R.C. Team 2021a), an interface to the probabilistic programming language Stan (Carpenter et al. 2017; S.D. Team 2021b, 2021c). Post-processing and plots are primarily handled via the tidyverse (Wickham et al. 2019) and ggplot2 ecosystems (Wickham 2016), particularly the bayesplot (Gabry and Mahr 2021) and tidybayes (Kay 2020) packages (see Supplements for a full list of R packages, dependencies and version numbers used).

We now turn to a summary of key results. Results are qualitatively similar across all model specifications, so where nothing else is noted, we report from m1. All models exhibited reasonable chain convergence and sampling efficiency.

Model name	Adjustment	Missing values	<i>n</i>	<i>N</i>
m0	Unadjusted	Listwise deletion	1469	4134
m1	Adjusted	Listwise deletion	1231	3327
m0_complete	Unadjusted	Complete-cases for all waves	471	1884
m1_complete	Adjusted	Complete-cases for all waves	351	1404
m_hinc	Unadjusted	Listwise deletion	1232	3344
m_health	Unadjusted	Listwise deletion	1467	4106
m_mrp	Poststratification	Listwise deletion	1468	4133
m1_imp	Adjusted	Bayesian imputation	1469	4134

Table 2: **Overview of main models.** Model names are from R script. “Adjustment” refers to adjustment (or not) for the full set of covariates discussed in Section 2.2.2, except for the poststratification model, *m_mrp*, which includes age, gender, and education binned in groups to match available external census data. *n* is model-specific number of participants, and *N* is model-specific number of observations.

2.3 Results

Here, we report key results. Table 3 reports focal coefficients from across main models (cf., Table 2). The thresholds (also known as intercepts or cutpoints) represent the points at which the continuous latent variable of the outcome is partitioned to yield the observed categorical outcome (cf., Bürkner and Vuorre 2019). The β coefficient for wave (i.e., time) represents the estimated change in religiosity over time. Note, though, that due to exclusion versus inclusion of covariates, the coefficients of the models are not directly comparable; the unadjusted models’ β coefficients represent the marginal change across time, whereas the adjusted models’ β coefficients represent the change across time, when holding covariates at their reference categories and age at 0 (i.e., the sample mean). As covariates were exclusively included as “control” variables, we did not build our models to assess the “effects” of these covariates on religiosity. Therefore, we refrain from reporting the estimates of the covariates in the adjusted models so as to avoid confusing such estimates with (causal) “effects” on the outcome (see e.g., Achen 2005; Westreich and Greenland 2013).

	1st threshold	2nd threshold	3rd threshold	Wave	Wave $\times$ H. income	Wave $\times$ Health
m0	-0.92 [-1.32, -0.51]	4.13 [3.61, 4.7]	8.22 [7.45, 9.04]	-0.21 [-0.33, -0.08]	—	—
m1	-1.39 [-2.22, -0.54]	3.77 [2.85, 4.84]	8.03 [6.93, 9.46]	-0.37 [-0.59, 0.02]	0.03 [-0.01, 0.06]	-0.02 [-0.15, 0.09]
m0_complete	-0.11 [-0.83, 0.63]	4.68 [3.86, 5.58]	9.48 [8.39, 10.75]	-0.38 [-0.52, -0.25]	—	—
m1_complete	0.23 [-1.35, 1.8]	5.15 [3.5, 6.85]	10.43 [8.51, 12.5]	-0.07 [-0.64, 0.52]	0 [-0.08, 0.07]	-0.17 [-0.32, -0.01]
m_hinc	-0.95 [-1.6, -0.21]	4.26 [3.5, 5.29]	8.6 [7.57, 9.99]	-0.38 [-0.57, 0.02]	0.03 [-0.03, 0.06]	—
m_health	-0.91 [-1.51, -0.35]	4.32 [3.51, 5.07]	8.54 [7.45, 9.57]	-0.09 [-0.35, 0.1]	—	-0.08 [-0.17, 0.05]
m_mrp	-1.18 [-3.53, 0.77]	3.82 [1.45, 5.78]	7.85 [5.43, 9.86]	-0.23 [-0.35, -0.1]	—	—
m1_imp	-1.1 [-2.08, -0.16]	4.08 [2.94, 5.14]	8.27 [6.9, 9.49]	-0.07 [-0.45, 0.16]	0.02 [-0.03, 0.05]	-0.1 [-0.18, 0.04]

Table 3: **Key coefficients across main models.** Posterior means with 95% credible intervals. Coefficients are log odds. See Table 2 for model details.

There were no reliable associations (i.e., where the bulk of the probability intervals did not include zero) between any of the covariates and the outcome, except for wave and gender (see Figure 2 for gender differences). Overall, all main models yield qualitatively similar results: there is evidence for a negative trend in religiosity across time, although except for three models (m0, m0_complete and m_mrp) the β interval includes zero, suggesting little or no overall change. As for the interaction terms, only one model (m1_complete) indicated a robust association (i.e., where the β interval does not include zero) – see supplements Section S2.2 for result plots of the interaction models. However, in all other model specifications this association is smaller and compatible with both no relationship or even a reverse relationship, suggesting no or – at most – an unreliable association overall.

Table 3 reports fixed effect estimates on the log odds scale. However, what these estimates amount to in terms of effect size is not intuitive. Instead, for ordinal regression, the size and implication of the raw coefficients are often easier to interpret by assessing model predictions on the scale of the outcome (e.g., Bürkner and Vuorre 2019). Thus, Figure 2 depicts the predicted (m1) cumulative probability of the response options for men (left panel) and women (right panel) across measurement periods with the categorical covariates held at their reference categories and age set to 0 (since it is mean-centered)⁷. While men and women differ in their estimated levels of religiosity (i.e., women are slightly more religious), Figure 2 illustrates the estimated slightly negative change in religiosity overall in the sample: the lowest response option (i.e., religion is “not at all important”) becomes more probable, and the second-lowest response option (i.e., religion is “not very important”) becomes less probable, while the probability of the higher response options remain low and relatively unchanged⁸. These inferences are similar across all main models (see Table 3 and supplements Sections S2.3, S2.4 and S3).

⁷ Simulating from the observed covariate values yield similar results (see supplements Figure S10). For a discussion of various approaches to computing and reporting predicted probabilities, see e.g. Hanmer and Ozan Kalkan (2013).

⁸ Note that Figure 2 illustrates gender differences in levels of religiosity but that our models only include simple effects for the two gender categories. Therefore, if we were directly interested in these differences and their change over time, the models would have to include gender categories with varying slopes across waves.

Figure 2: **Predicted probabilities across measurement waves for each response option.** For men (left) and women (right) with categorical covariates set at their reference values and age set at 0 (as it is mean-centered). Lines are posterior predictive medians based on 300 draws for each wave and gender category. Predictions from m1. Random effects not included in predictions.

Finally, we can zoom in on a smaller subset of participants to assess how the model predictions perform across measurement waves on the individual level. Figure 3 shows 15 pseudo-randomly sampled participants, where all covariates are available for all four measurement waves⁹, their ID numbers, their observed response values (black points), and their predicted median value (dark blue band) with 97% (light blue) and 66% (darker light blue) credible intervals¹⁰. In cases of missing outcome values for a particular participant and wave (e.g., top-left panel, wave 1), the model attempts to predict that missing response. Note that the model's predictions are within ± 1 response option of the observed values. Further, most predictions are either stable or sloping downwards. Taken together, Figure 3 confirms that, conditional on the data and priors, the model expects a slightly negative – if any – change in self-reported religiosity across time.

⁹ All covariates need to be available for each individual in order for the model to make predictions for that particular individual and their combination of covariate values.

¹⁰ While 95% intervals are conventional, the length of an uncertainty interval is ultimately arbitrary. Here, our choice of interval allows us to illustrate uncertainty at two quite different quantile-levels of uncertainty.

Figure 3: **Posterior predictions for 15 pseudo-randomly sampled individuals.** Y-axis: Response options ranging from 1 (religion is “not at all important”) to 4 (religion is “very important”). X-axis: Measurement wave number. Number strings above each panel are unique ID numbers. Black points are observed values, dark blue lines are the medians of the predicted values with 97% (light blue) and 66% (darker light blue) credible intervals.

3. Discussion

On the basis of a series of Bayesian multilevel regression models of varying complexity, including poststratification and full Bayesian imputation of missing values, we conclude that self-reported religiosity in Denmark did not appear to increase during the COVID-19 pandemic on average and that, if any change occurred, there is some evidence for a slight decrease in self-reported religiosity across measurement waves. Our interaction terms between time and, respectively, household income and subjective health failed to consistently predict any change across model specifications.

Above and beyond these main conclusions, in line with previous research (Trzebiatowska and Bruce 2012), we find that women on average are more religious than men. No other covariate was clearly associated with religiosity; however, note that we did not build our models to directly assess the relationship between demographics and religiosity, as this was not the theoretical motivation of the present study. In this Discussion section, we first

consider possible interpretations of these findings and then highlight some key theoretical implications and methodological limitations of our study.

There are various relevant perspectives and possible explanations related to our finding. As described in the introduction, Denmark is generally a highly secular country. Danes generally perceive religion – and particularly the Evangelical-Lutheran Church of Denmark - as a cultural and national marker rather than as a place to look for comfort and coping, and this could be one reason as to why Danes have not become more religious due to the pandemic; because religiosity and the church is not perceived as sources of existential aid.

Another possible explanation is related to the welfare-state of Denmark, in which secular institutions such as the health-system, the state and the public schools are highly supported. If secular institutions enjoy high levels of support and the church is simultaneously perceived as a provider of national culture rather than a source of individual comfort and coping, this could in times of crises lead to consistent support of secular institutions rather than an increase in religiosity. The secular welfare-state generally handled the health crisis well (Olagnier and Mogensen 2020; Greve et al. 2021), and seemingly provided Danes with strong feelings of comfort and safety, perhaps outmaneuvering religion as a source of security and coping. This is largely consistent with cross-cultural evidence showing that non-believers do not necessarily turn to religion for comfort and coping in times of hardship but instead may derive meaning from secular institutions and notions, including science and humanism (van Mulukom et al. 2021).

Through the workings of various proximate mechanisms, which our analysis was not designed to assess, theories of terror management, religious coping, meaning-making and cultural learning all predict that religion benefits the individual in times of crisis. While this general prediction was supported in a recent systematic review (Aten et al. 2019), the present paper underscores the need for greater cultural diversity in future empirical studies on crisis and religion which should inform revisions of theory. If the current theories of religion and crisis are to apply cross-culturally, they must take into account the workings of diverse cultural contexts. Specifically, we suggest based on this study that the

beforementioned theories should consider how secular norms and institutions can be effective in same way as religious ones in times of crisis.

A key methodological limitation of our study is the lack of a proper baseline measurement; our first wave of data collection took place around two months after national lockdown. This prohibits us from formally quantifying an average causal effect of the pandemic on religiosity. For the present analysis, we assumed that Danes were yet relatively unaffected by the pandemic at the start of the study, so that the first measurement wave serves as a baseline proxy. However, this assumption might not be warranted. Self-reported weekly and monthly trends of general concern about the pandemic among quasi-representative samples of Danes suggest that general concern was actually at its peak in the first month of lockdown¹¹. On the other hand, at our first measurement point (mid-May 2020), general concern had decreased again to one of the lowest levels measured throughout the entire observation period (concluded mid-July 2021). All in all, this tentatively supports our initial assumption that the first wave of data collection may be considered a baseline proxy.

Another limitation of the present study pertains to missing data in both the outcome and covariates. We attempted to address this issue by *i*) spelling out an explicit causal model including a missingness mechanism (supplements Section S1), *ii*) comparing inferences from models that include vs. exclude key covariates (Table 3 and supplements Sections S2 and S3), and *iii*) employing full Bayesian imputation of missing values (supplements Section S2.3). While estimates are not identical across model specifications, no model indicates an increase in religiosity during the study period.

A third limitation—directly related to the issue of missing data—is the over- and underrepresentation of certain demographic groups (e.g., educational levels) in our sample. We addressed this by obtaining external census data on key demographic, time-invariant covariates with which we could poststratify our posterior predictions (Kennedy and

¹¹ HOPE Project report: https://github.com/mariefly/HOPE/raw/master/Danskernes_Adfærd_Og_Holdning_Til_Corona-epidemien_20210806.pdf. See Figure 3.2: “To what extent do you fear the consequences of the current COVID-19 situation?”

Gelman 2021; Little 1993). This procedure is a powerful if under-utilized adjustment technique; however, it allows only for adjustment of covariates for which there is proper coverage in-sample and for which a combination of reliable external data can be obtained. Since these external data are also imperfect statistical estimates, an ideal poststratification workflow would propagate that uncertainty through to the posterior predictions. Further, poststratification is dependent on an appropriate statistical model, e.g. one that captures relevant features of the data and avoids misspecification. While this assumption is difficult to verify in practice, assessing a model's posterior predictions against observed data can go some way toward addressing it (see supplements Sections S2.2 and S2.3). Our poststratified model predictions are consistent with the overall inference, namely that levels of religiosity did not increase during the pandemic.

4. Conclusion

Through our analysis of a pseudo-representative longitudinal survey sample, we investigated how levels of religiosity in Denmark changed during the COVID-19 pandemic. We failed to find evidence that self-reported religiosity increased during the pandemic on average, and we discussed how this finding can be interpreted through the lense of Danish secularity and the successful, secular welfare-state's approach to the pandemic. On this basis, we therefore argue, that while much theorizing on the relationship between crises and religion highlight various individual- and society-level benefits of increased religiosity in times of hardship, it is important to be aware of and emphasize the flexibility in these theoretical frameworks, if these are to be cross-culturally relevant in for instance a secular country as Denmark.

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