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Big Five Personality and Religiosity:
Bidirectional Cross-Lagged Effects and their Moderation by Culture

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Abstract

Objective. Personality has long been assumed to be a cause of religiosity, not a consequence. Yet, recent research suggests that religiosity may well cause personality change. Consequently, longitudinal research is required that examines the bi-directionality between personality and religiosity. The required research must also attend to cultural religiosity—a critical moderator in previous cross-sectional research.

Method. We conducted four-wave, cross-lagged panel models assessing the bi-directional effects between religiosity (measured as religious attendance) and the Big Five personality traits over 12 years in 14 samples (N_{total}=44,485). Each sample used population-representative data from a different German federal state—states that vary widely in religiosity.

Results. The findings were the following: (1) Agreeableness, openness, and conscientiousness were associated with changes in religiosity, with the latter two effects being culture-contingent. (2) Religiosity was associated with changes in agreeableness and openness, with the latter effect being culture-contingent. (3) The cross-lagged effects of personality on religiosity were overall stronger than the reverse effects.

Conclusions. The directionality between the Big Five and religiosity seems to go both ways and culture matters for those effects. We discuss the power of religiosity to alter personality and the role of culture for this effect and for personality change more generally.

KEYWORDS: Religiosity, Big Five, Personality change, Culture.

Religiosity has shaped human life for millennia. For example, religiosity has been considered indispensable for the evolution of culture (Norenzayan, 2013; Norenzayan et al., 2016), with culture being the reason for humans' extraordinary evolutionary success (Boyd & Richerson, 1985; Henrich, 2015). Even today, two out of three people on our planet say that religion plays an important part in their daily lives (Diener et al., 2011; Joshanloo & Gebauer, 2019). Understanding religiosity is, thus, central to psychology (McCullough et al., 2003; Sedikides, 2010).

Personality psychologists have typically sought to better understand religiosity by studying its association with personality traits (Ashton & Lee, 2021; Saroglou, 2010). In fact, this is a classic endeavor, dating back to the very beginnings of personality psychology (Allport, 1950). By now, a rather clear empirical picture has emerged regarding the association between personality and religiosity. In the terminology of the Big Five taxonomy, religious people are more agreeable, more conscientious, and less open (Gebauer et al., 2014; Saroglou, 2010) and those associations are particularly pronounced in religious cultural contexts (Entringer et al., 2021; Gebauer et al., 2014).

The associations between the Big Five and religiosity have long been taken as rather clear evidence that personality is a *cause* of religiosity. Yet, in principle, there is another possible explanation for those associations: Religiosity may—at least in part—also lead to changes in personality (Ashton & Lee, 2021; Entringer et al., 2021). This possibility has long been neglected based on the traditional premise that personality traits are endogenous basic tendencies immutable to life experiences, while religiosity is a characteristic adaption of those traits (McCrae & Costa, 2008).[1] Hence, based on this traditional reasoning, the causality could only run from personality to religiosity, not vice versa. The accumulating evidence, however, suggests strongly that life experiences do shape personality (Bleidorn et al., 2021; Wagner et al., 2020) and, thus, it is well possible that the association between the Big Five and religiosity is not solely due to an effect of the former on the latter. In fact, a religious life

entails so many powerful beliefs and practices (Norenzayan, 2013; Gebauer & Sedikides, 2021) that an effect of religiosity on changes in personality may perhaps even explain the lion's share of variance in the association between the Big Five and religiosity.

What is needed, then, is longitudinal research that (1) tests for cross-lagged effects of the Big Five on changes in religiosity, (2) tests for cross-lagged effects of religiosity on changes in the Big Five, and (3) compares the two types of cross-lagged effects in size. Such research, however, is lacking almost entirely. In fact, as detailed later, there are only two published articles on the possibly bi-directional, cross-lagged effects between the Big Five and religiosity (Huuskes et al., 2013; Wink et al., 2007).

Thus, to help close this gap in the literature, we conducted the first-ever longitudinal test of the association between the Big Five and religiosity that (1) is large in scale (N_{total} = 44,485), (2) attends simultaneously to cross-lagged effects in both directions, and (3) tests for those cross-lagged effects in cultural contexts that vary widely in religiosity.

This Introduction proceeds as follows: We, first, review the theoretical reasons for why certain Big Five traits may lead to changes in religiosity over time and for why those changes in religiosity may be pronounced in religious cultural contexts. Second, we provide theoretical reasons for why religiosity may lead to changes in certain Big Five traits over time and for why those changes, too, may be pronounced in religious cultural contexts. Finally, we conclude the Introduction with a review of the literature on the longitudinal effects underlying the associations between the Big Five and religiosity.

Theory on Possible Effects of the Big Five on Changes in Religiosity

Over 70 years ago, Gordon Allport (1950) theorized that personality traits akin to high agreeableness, low openness, and high conscientiousness should lead to increases in religiosity. Allport seemed to envision an explanation that psychologists today call "niche picking" (Penke & Jokela, 2016): people pick socio-environmental niches (here: a religious lifestyle) that allow them to behave in consistency with their personality. The religious socio-

environmental niche primarily allows people to behave in ways consistent with high agreeableness (e.g., religious prosociality commandments; Norenzayan, 2013), low openness (e.g., religious conservatism norms; Gebauer & Sedikides, 2021), and high conscientiousness (e.g., religious self-control rules; Koole et al., 2010). Consequently, people high in agreeableness, low in openness, and high in conscientiousness should pick religious niches and, thus, become increasingly religious over time (Entringer et al., 2021; Gebauer et al., 2014).

Recently, the Sociocultural Norm Perspective (SNP) on Big Five prediction offered an additional—and complementary—explanation for why high agreeableness, low openness, and high conscientiousness should lead to higher religiosity over time (Eck & Gebauer, 2022). According to the SNP, those three personality characteristics all elicit normative behavior (all for distinct reasons; see Eck & Gebauer, 2022).[2] Consequently, high agreeableness, low openness, and high conscientiousness should lead to higher religiosity over time, if religiosity is normative in the ambient cultural context. If religiosity is not normative, however, those three personality characteristics should not lead to higher religiosity over time. Cross-sectional evidence on the Big Five and religiosity is consistent with the SNP's predictions (Entringer et al., 2021; Gebauer et al., 2014). At a more general level, this evidence showcases the importance to attend to the cultural context in research on personality and religiosity (see also Ashton & Lee, 2019, 2021).

Theory on Possible Effects of Religiosity on Changes in the Big Five

By now, the literature on personality change has uncovered a large array of life experiences that seem to elicit changes in the Big Five (Bleidorn et al., 2021; Wagner et al., 2020). So far, however, that literature paid little attention to religiosity, albeit there is theoretical reason to believe that a religious way of life may exert particularly powerful effects on personality. After all, its very nature allows religion to exploit exceptionally powerful means to promote and prohibit behaviors that result in personality change (Gebauer

et al., 2018; Norenzayan, 2013). Those means include *supernatural* elements, such as promises of eternal joy (heaven), if the believer behaves in a certain way and threats of eternal agony (hell), if the believer does not behave this way (Berkessel et al., 2021; Johnson, 2016). In addition, religion relies heavily on *repetition* to engrave certain behaviors (e.g., the Lord's Prayer is part of *every* Sunday church in Christian religion) and repetition is a key driver of personality change (Hennecke et al., 2014; Wrzus & Roberts, 2017). In the previous section, we have already seen that religion typically calls for behaviors that correspond to high agreeableness (e.g., religious prosociality commandments; Norenzayan, 2013), low openness (e.g., religious conservatism norms; Gebauer & Sedikides, 2021), and high conscientiousness (e.g., religious self-control rules; Koole et al., 2010). In addition, religion typically prohibits—and fiercely so—behaviors that correspond to low agreeableness, high openness, and low conscientiousness (Gebauer & Sedikides, 2021; Norenzayan, 2013). Overall, then, if religiosity was powerful enough to elicit change in the Big Five over time, religiosity should elicit increases in agreeableness, decreases in openness, and increases in conscientiousness.

Furthermore, a religious cultural context likely serves as a *social facilitator* and *enforcer* of any possible effect of religiosity on personality change (Gebauer & Sedikides, 2021). To illustrate, religious cultural contexts may facilitate effects of religiosity on higher agreeableness, because such contexts contain many likeminded believers who make it easier for religious people to practice agreeableness (cf. Gervais et al., 2011). Similarly, religious cultural contexts may facilitate effects of religiosity on decreases in openness, because such contexts contain many people who endorse a conservative ideology that ultimately curbs openness (cf. Jost et al., 2008). More generally, then, any longitudinal effect of religiosity on personality change may be most pronounced (or even restricted to) religious cultural contexts.

Review of Previous Longitudinal Research

Cross-sectional research on the association between personality and religiosity is extensive (Ashton & Lee, 2021; Saroglou, 2010) and now includes several large-scale

investigations on cross-cultural differences (Ashton & Lee, 2019; Entringer et al., 2021). By contrast, longitudinal research on the cross-lagged effects between personality and religiosity is rather limited: Six published studies tested for cross-lagged effects of personality on changes in religiosity and only two of them tested for cross-lagged effects of religiosity on changes in personality. We, first, summarize the former six.

Six longitudinal studies examined the cross-lagged effects of personality on changes in religiosity. Importantly, the majority of those studies stem from the U.S., a rather religious cultural context (Gebauer et al. 2014—Germany; Heaven & Ciarrocchi, 2007—Australia; Huuskes et al., 2013—Australia; McCullough et al., 2003, 2005—both times U.S.; Wink et al., 2007—U.S.). With one exception only, all those studies found a cross-lagged effect of higher agreeableness on increases in religiosity (exception: Huuskes et al., 2013). Half of the studies also found a longitudinal effect of lower openness on increases in religiosity (McCullough et al., 2003; Gebauer et al. 2014; Wink et al., 2007). And a slightly different half of the studies found a longitudinal effect of higher conscientiousness on increases in religiosity (Heaven & Ciarrocchi, 2007; McCullough et al., 2003; Wink et al., 2007).

As noted earlier, only two of the six longitudinal studies from the previous paragraph examined cross-lagged effects of religiosity on changes in personality (Huuskes et al., 2013; Wink et al., 2007). Notably, those two longitudinal studies received contradictory results: Huuskes et al. (2013) found cross-lagged effects of religiosity on increases in agreeableness and decreases in psychoticism. Wink et al. (2017), by contrast, did not find any evidence for cross-lagged effects of religiosity on changes in personality.[3]

In summary, published longitudinal studies quite consistently found evidence for an effect of agreeableness on changes in religiosity; the evidence for an effect of openness and conscientiousness on changes in religiosity, however, is rather mixed. In addition, evidence for an effect of religiosity on changes in personality is inconclusive. One reason for the rather inconclusive state of the literature might be that the previous studies were comparatively

small in scale and ignored cross-cultural differences almost entirely (the only cross-cultural study is reported in Gebauer et al., 2014). Hence, there is a real need for large-scale, culture-attentive, longitudinal research on religiosity and personality—a need that has recently been described as a frontier in personality psychology (Ashton & Lee, 2021). The present research responds to this need.

Method

We carefully searched for openly accessible datasets that fulfill the following requirements: The datasets needed to be longitudinal (preferably with more than two waves), large-scale (preferably population representative), culture-attentive (preferably spanning very religious and very non-religious cultural contexts), and the datasets needed to assess the relevant constructs (religiosity and the Big Five traits). Only a single existent dataset satisfied all our criteria: The German Socio-Economic Panel (SOEP; Version 35; German Institute for Economic Research). The SOEP is a household-panel that assesses religiosity and the Big Five traits in four of its waves (interval between those waves: four years). The SOEP is also large in scale and population representative for all German federal states. The German federal states vary widely in their cultural religiosity and, thus, the SOEP is ideally suited to test for context effects in the domain of cultural religiosity. Attesting to the data's validity, the SOEP is one of the most frequently used panel studies in the social sciences (Online Supplement S1 provides additional information on the SOEP).

Open Science Disclosure

None of our hypotheses were pre-registered and, thus, the present research should be regarded as *entirely exploratory*. We consider our exploratory approach acceptable in the present case, as we sought to ensure robustness of results by using large-scale data (N_{total} = 44,485), standard analysis strategies (Orth et al., 2021), and extensive robustness checks (Online Supplement S2). The SOEP data is freely available for scientific use upon request: https://www.diw.de/en/diw_01.c.601584.en/data_access.html. All study materials can be

accessed here: https://www.diw.de/en/diw_02.c.222729.en/questionnaires.html. All of our data-analysis scripts are publicly available on the Open Science Framework: https://osf.io/gn8xa/?view_only=27038964954d4485a2d418dd3a469838.

One previous study already used the SOEP to examine longitudinally the associations between the Big Five and religiosity (Gebauer et al., 2014). Critically, however, that study relied on much less data than our present study: Back then, relevant data was available only from two waves across 4 years; at present relevant data is available from four waves across 12 years. More important, the previous study only modelled cross-lagged effects of the Big Five on changes in religiosity (i.e., half cross-lagged design), whereas the present study models cross-lagged effects of the Big Five on changes in religiosity as well as cross-lagged effects of religiosity on changes in the Big Five (i.e., full cross-lagged design).

Participants

All participants provided informed consent; ethical permission for the SOEP was granted by the Scientific Advisory Board of the DIW Berlin, Germany. We used data from a total of 44,485 participants across 14 federal states of Germany (age at wave 1: $M = 47.73$ years, $SD = 17.46$; sex ratio at wave 1: 52% female, 48% male). Notably, Germany consists of 16 federal states of which Bremen and the Saarland are the smallest. Sample sizes of these states were too small (337 and 476, respectively) for our statistical model to converge. Thus, the main text focuses exclusively on the results of the 14 larger federal states. Yet, Online Supplement S3 includes two sets of complementary analyses, both of which include the data from Bremen and the Saarland. Both sets paint a coherent picture and overall support the conclusions drawn from the main-text analyses. Table 1 includes the number of participants per federal state and their demographics. The SOEP data includes a Big Five measure in four waves with inter-wave intervals of 4 years each (2005, 2009, 2013, 2017) and, thus, we used the data from those four waves spanning a period of 12 years (all four waves also included a measure of religiosity). Our study included all SOEP participants who completed at least one

item of each of our six focal constructs (religiosity and the five Big Five traits) over the course of the study period (i.e., 2005-2017). This liberal inclusion strategy, coupled with FIML-estimation of missing data, is superior to more conservative inclusion strategies (e.g., listwise deletion if any data is missing; Schafer & Graham, 2002). In addition, we excluded a relatively small number of additional participants (2.35%), who moved from one federal state to another over the course of the study period. That way, we assured that the cultural context remained constant for all participants throughout the study.

Measures

Personal religiosity. The most frequently used approach to assess religiosity is by the frequency with which participants attend religious services or events (Schwartz & Huismans, 1995). In line with that, most large-scale panel studies assess religiosity via the frequency of religious attendance (Inglehart et al. 2014). The SOEP is one of those panel studies, asking: "How often do you attend church, religious events?" (1 = *at least once a week*, 2 = *at least once a month*, 3 = *less often*, 4 = *never*; reverse-keying assured that higher scores reflect more religiosity).

Big Five traits. The SOEP-BFI is a 15-item short form of the Big Five Inventory (Gerlitz & Schupp, 2005). It assesses each Big Five trait with three items. Example items are: "I see myself as someone who…" "is considerate and kind to almost everyone" (agreeableness), "is original, comes up with new ideas" (openness), "tends to be lazy" (conscientiousness, reverse-keyed), "is talkative" (extraversion), "is relaxed, handles stress well" (neuroticism, reverse-keyed) (1 = *strongly disagree*, 7 = *strongly agree*). The items of the SOEP-BFI were selected to maximize each scale's correlation with the original Big Five Inventory and, thus, to retain construct breadth (all *r*s > .86; Donnellan & Lucas, 2008). The SOEP-BFI scales' 6-week retest reliabilities are high (*r*s > .75; Lang, 2005). In our own analyses, all five SOEP-BFI scales evidenced metric measurement invariance (Cheung &

Table 1

Sample Characteristics of each of the 14 Samples Used in the Present Research

state		sample characteristics						Big Five traits													model fit		
		state-level religiosity		age		gender (in %)		agreeableness			openness			conscientiousness			extraversion			neuroticism					
	N	*M*	*SD*	*M*	*SD*	men	women	*M*	*SD*	alpha	*M*	*SD*	alpha	*M*	*SD*	alpha	*M*	*SD*	alpha	*M*	*SD*	alpha	*CFI*	*RMSEA*	*SRMR*
Bavaria	7,085	1.95	0.98	47.10	17.18	47.18	52.82	5.40	1.39	0.50	4.48	1.61	0.65	5.90	1.24	0.64	4.75	1.59	0.65	3.94	1.66	0.61	.898	.022	.050
Baden-Wuerttemberg	5,423	1.85	0.94	46.03	17.39	48.25	51.75	5.40	1.36	0.55	4.40	1.59	0.68	5.79	1.27	0.67	4.75	1.61	0.69	3.90	1.68	0.65	.902	.022	.055
Rhineland-Palatinate	2,088	1.80	0.89	48.40	17.57	46.70	53.30	5.45	1.43	0.62	4.34	1.67	0.65	5.95	1.23	0.63	4.83	1.66	0.70	3.95	1.80	0.65	.873	.024	.064
North Rhine-Westphalia	9,621	1.77	0.93	48.06	17.47	48.21	51.79	5.47	1.44	0.51	4.55	1.63	0.61	5.94	1.27	0.64	4.85	1.66	0.67	3.94	1.76	0.59	.888	.022	.050
Hessen	3,151	1.74	0.90	47.41	16.61	48.45	51.55	5.46	1.44	0.58	4.49	1.67	0.63	5.84	1.26	0.65	4.87	1.59	0.71	4.03	1.77	0.63	.895	.023	.057
Lower-Saxony	4,392	1.73	0.86	48.21	17.86	48.46	51.54	5.74	1.52	0.54	4.76	1.92	0.64	6.06	1.37	0.73	5.05	1.76	0.53	3.86	1.89	0.64	.874	.024	.056
Schleswig Holstein	1,551	1.56	0.78	50.25	17.70	47.12	52.88	5.48	1.37	0.57	4.30	1.65	0.65	6.04	1.25	0.70	4.77	1.57	0.59	3.64	1.69	0.66	.843	.024	.073
Hamburg	686	1.50	0.76	47.90	18.17	48.53	51.47	5.59	1.37	0.50	4.72	1.64	0.52	5.75	1.26	0.60	4.93	1.58	0.63	3.81	1.67	0.68	.811	.030	.084
Thuringia	1,742	1.44	0.76	47.82	17.73	47.98	52.02	5.38	1.38	0.56	4.42	1.54	0.54	5.89	1.22	0.64	4.89	1.54	0.69	4.15	1.68	0.50	.877	.027	.059
Berlin	1,615	1.40	0.78	46.73	17.28	47.80	52.20	5.43	1.49	0.53	4.78	1.64	0.65	5.73	1.38	0.61	4.86	1.66	0.59	3.94	1.77	0.52	.899	.023	.057
Saxony	2,816	1.39	0.77	47.76	17.65	49.46	50.54	5.48	1.39	0.57	4.36	1.59	0.66	5.96	1.22	0.65	4.80	1.59	0.67	4.05	1.63	0.51	.887	.026	.059
Brandenburg	1,703	1.31	0.65	48.85	17.50	48.80	51.20	5.48	1.41	0.54	4.41	1.60	0.64	6.01	1.20	0.67	4.81	1.60	0.65	4.10	1.75	0.51	.880	.025	.060
Saxony-Anhalt	1,628	1.30	0.67	49.07	17.68	47.19	52.81	5.48	1.40	0.48	4.48	1.64	0.63	6.00	1.21	0.65	4.93	1.61	0.66	4.03	1.74	0.62	.871	.027	.066
Mecklenburg-Vorpommern	984	1.28	0.61	48.70	17.34	46.27	53.73	5.61	1.38	0.52	4.66	1.55	0.58	6.14	1.06	0.66	4.89	1.65	0.68	3.98	1.75	0.60	.834	.030	.075
Complete	44,485	1.70	0.90	47.73	17.46	48.01	51.99	5.45	1.41	0.54	4.49	0.64	0.65	5.91	1.26	0.65	4.83	1.62	0.67	3.96	1.72	0.60	.897	.022	.047

Note. Federal states are sorted according to their state-level religiosity, starting with the most religious federal state. *N* reflects the total sample size across all four waves within each federal state. The sociodemographic characteristics and the Big Five descriptives are from wave 1 (2005). *CFI* and *RMSEA* fit indices are robust estimates. On first sight, the *CFI*s seem somewhat too low, but recall that the *CFI* unduly penalizes model complexity (Kenny & McCoach, 2003) and, thus, it is not surprising that our highly complex models (four-wave cross-lagged design with 20 latent variables per model) yielded low *CFI*s; tellingly, the *RMSEA*s and *SRMR*s indicate acceptable model fit.

Rensvold, 2002) across our 14 federal states (Online Supplement S4). Table 1 includes the descriptive statistics for all five Big Five scales per federal state.

Cultural religiosity. Cultural religiosity is conventionally assessed by averaging participants' personal religiosity scores within each cultural unit (Gebauer et al., 2012). Following that convention, we averaged the religiosity scores of our participants within each federal state. The resultant cultural religiosity index was near-perfectly correlated with external indices from the German ALLBUS, $r(12) = .95$, 95% CI [.85, .99], and the KERB Religiosity Index based on the percentage of church members (catholic or protestant) per federal state in 2018, $r(12) = .97$, 95% CI [.91, .99] (GESIS - Leibniz Institute for the Social Sciences, 2020; Bundeszentrale für politische Bildung, 2020). Table 1 includes our cultural religiosity index.

Modeling Strategy

In a first step, we specified one cross-lagged panel model (Asendorpf, 2021) for each of the 14 German federal states, using the *lavvan* 0.6-9 package (Rosseel, 2012) in *R* 4.0.5 (R Core Team, 2020). The cross-lagged panel model is the most suitable type of model to test whether individual differences (here: religiosity) predict subsequent rank-order change in other individual differences (here: Δ in Big Five traits) and vice versa (Orth et al., 2021). Figure 1 depicts one of our 14 cross-lagged panel models.

Each of those models included all Big Five traits simultaneously—a common (Soto & John, 2017) and most suitable (Costa & McCrea, 2008) practice and an established standard in research on the Big Five and religiosity (Entringer et al., 2021; Gebauer et al., 2014). All cross-lagged, synchronous, and stability effects were specified as time-invariant, imposing stationarity of associations across waves. To interpret the results, we extracted the standardized estimates based on the variances of all included variables (observed and latent).

To aid readability, the figure is a simplification of our actual model in two ways. First, the figure omits all cross-sectional inter-relations between the Big Five traits. Second, the

figure also omits the measurement model of the Big Five traits. The measurement model took the following form: Each Big Five trait was indicated by three items and the factor loading of each item was constrained to be equal across waves (thereby imposing metric measurement invariance across time; Cheung & Rensvold, 2002). Also, the residual of each item was allowed to correlate with the residuals of the same item at different waves (thereby controlling for bias due to indicator-specific variance not captured by the latent variables; Cole & Maxwell, 2003). Finally, we allowed three cross-loadings because they strongly improved the fit of the model: Cross-loadings between (a) the third extraversion item and the agreeableness factor, (b) the third neuroticism item and the openness factor, and (c) the third neuroticism item and the conscientiousness factor.

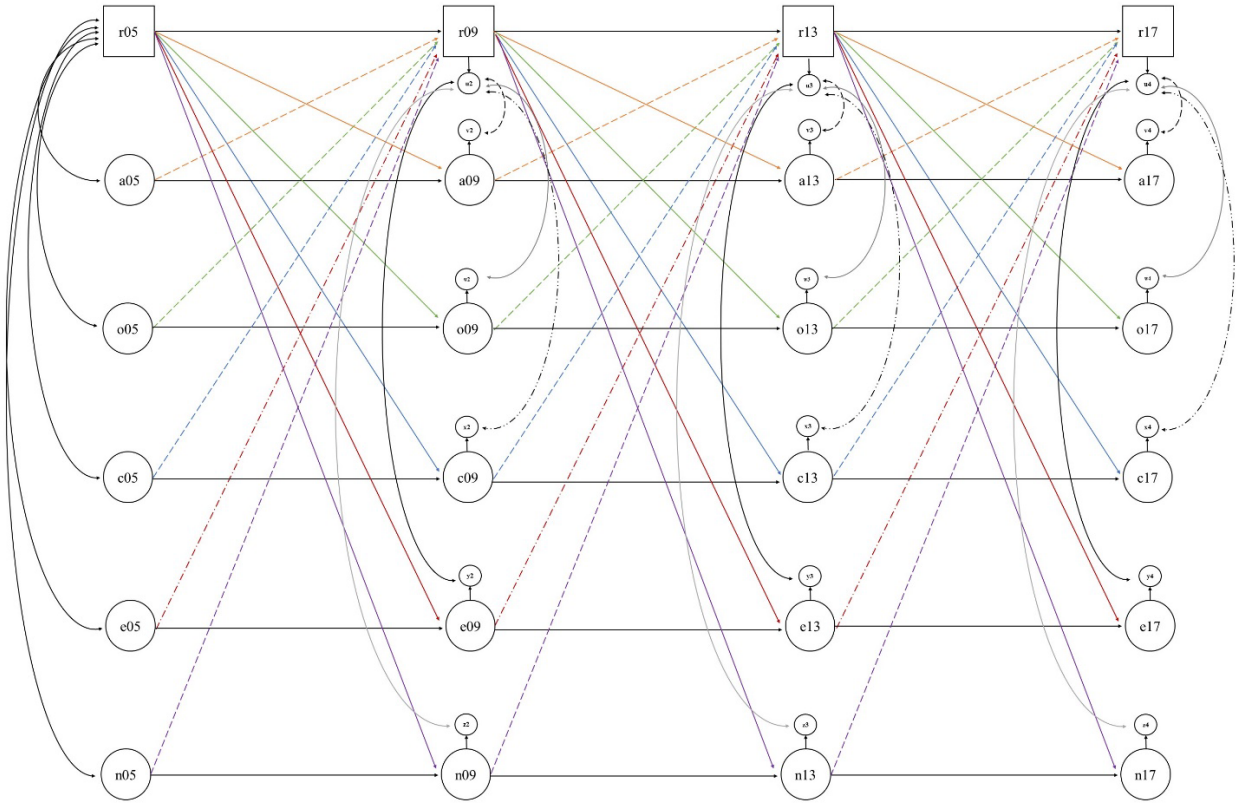
To deal with missing data, we used FIML-estimation with robust standard errors (Schafer & Graham, 2002). Table 1 shows that all 14 models fitted the data sufficiently well.

In the second step, we meta-analytically aggregated the results obtained in the first step (i.e., the results of each of our 14 cross-lagged panel models; Orth et al., 2021). To aggregate without bias, we transformed the standardized estimates of our 14 models into Fisher's z scores prior to aggregation (Rosenthal & Rubin, 1982) and weighted those z scores by their inverse variance weights (Hedges & Olkin, 1985).

Online Supplement S2 reports extensive, supplementary analyses, which show that our meta-analytically aggregated results are not due to the results of any single federal state (i.e., "outlier" results of a single state). Most relevant, our meta-analytic approach allowed us to treat cultural religiosity as a sample-level moderator (here: state-level moderator), using random-effects meta-regression with FIML-estimation (Lipsey & Wilson, 2001) in the *R* package *metafor* 3.0-2 (Viechtbauer, 2010). Conceptually, our analysis strategy is, thus, equivalent to a latent multi-level model (participants nested in federal states) with a focus on the cross-level interaction between personal and cultural religiosity on the Big Five traits.

Figure 1

Cross-Lagged Panel Model Estimated Within Each Federal State (Simplified)



Note. One of the 14 cross-lagged panel models. r = religiosity, a = agreeableness, o = openness, c = conscientiousness, e = extraversion, n = neuroticism. Numbers denote year of data assessment (e.g., 05 = year 2005). Simplifications are explained in the main text.

The advantage of our strategy is that it is much less likely to cause convergence problems than the latent multi-level strategy. In fact, we first pursued the latter strategy and had convergence problems, rendering our meta-analytic strategy the most suitable one for our purposes.

Effect Size Expectations

Cross-sectional associations in personality psychology reflect a mixture of bi-directional effects that accumulated over people's entire lifespan (plus additional influences, such as effects of third variables, genetic variance, and shared method variance). Cross-lagged effects, by contrast, only reflect uni-directional effects over a single inter-wave interval (and

the additional influences are also much better controlled for). As a result, cross-sectional associations are typically much stronger than are cross-lagged effects. To illustrate, the cross-sectional association between self-esteem and depression is among the strongest associations in personality psychology. In a meta-analysis, this cross-sectional association was close to r = -.60—translating to a shared variance of R^2 = 36% between both constructs (Sowislo & Orth, 2013). In the same meta-analysis, however, the cross-lagged effect of self-esteem on depression was "only" β = -.16—translating to a shared variance of only R^2 = 2.56% between both constructs. That is, the cross-lagged association is 3.75 times smaller than the cross-sectional association—or, stated differently, the two constructs share 14.06 times more variance in the cross-sectional than in the cross-lagged association. The cross-sectional associations between the Big Five and religiosity are much smaller than the cross-sectional associations between self-esteem and depression. Saroglou's (2010) influential meta-analysis, for example, revealed a cross-sectional association between agreeableness and religiosity of r = .19—and hence a shared variance of R^2 = 3.61% between both constructs. Thus, it is unreasonable to expect any cross-lagged effects in the present research to be much larger than r = .19 / 3.75 = .05—that is to expect a shared variance much larger than R^2 = 3.6% / 14 = 0.26%. In fact, this is an upper-bound estimate, because agreeableness is the Big Five trait most strongly related to religiosity (Saroglou, 2010). Those effect size considerations further underscore the importance of large-scale data for the present research questions and, thus, the unique suitability of the SOEP.

Results

The results of our 14 cross-lagged panel models provide rich insights into religiosity, the Big Five, and their association (e.g., temporal stability of the involved constructs, correlated change between religiosity and the Big Five). This Results section, however, focuses on the subset of results relevant for our research questions: (1a) Are there cross-lagged effects of the Big Five on changes in religiosity and (1b) are those effects contingent

on the cultural context (i.e., cultural religiosity)? (2a) Are there cross-lagged effects of religiosity on changes in the Big Five and (2b) are those effects contingent on the cultural context (i.e., cultural religiosity)? (3) Are the Big Five stronger predictors of change in religiosity or is religiosity a stronger predictor of change in the Big Five? Online Supplement S5 includes all additional results that our 14 cross-lagged panel models revealed (i.e., results on the temporal stability of the involved constructs, results on the correlated change between religiosity and personality).

Culture-Contingent Cross-Lagged Effects of the Big Five on Changes in Religiosity

Table 2 reports on the cross-lagged effects of the Big Five on changes in religiosity in each of our 14 cultural contexts. The top rows include the results for the most religious cultural contexts, the bottom rows the results for the most secular contexts. For each Big Five trait, we will next describe the meta-analytic summary of those results and whether cultural religiosity moderates them.

Agreeableness → Δreligiosity. Agreeableness yielded an omnibus cross-lagged effect on later increases in religiosity, $\beta = .039$, 95% CI [.027, .049]. Cultural religiosity did not moderate this effect, $\beta = .003$, 95% CI [-.008, .014]. Evidently, agreeableness was associated with increases in religiosity over time, irrespective of cultural context.

Openness → Δreligiosity. Openness yielded no omnibus cross-lagged effect on later changes in religiosity, $\beta = -.013$, 95% CI [-.029, .003]. Cultural religiosity, however, moderated this non-significant effect, $\beta = -.014$, 95% CI [-.027, -.002]. This means openness was associated with decreases in religiosity over time, but only in religious cultural contexts.

Conscientiousness → Δreligiosity. Conscientiousness yielded no omnibus cross-lagged effect on later changes in religiosity, $\beta = -.003$, 95% CI [-.012, .011]. Cultural religiosity, however, moderated this non-significant effect, $\beta = .014$, 95% CI [.006, .021]. In other words, the effect was stronger (more positive) in religious cultural contexts and weaker (more negative) in non-religious cultural contexts.

Table 2

Cross-lagged Effects from the Big Five Traits on Religiosity Estimated Within Each Federal State and the Associated Meta-Analytical Results

state	*N*	state-level religiosity	cross-lagged effects (β) of the Big Five on changes in religiosity				
			agreeableness → Δreligiosity	openness → Δreligiosity	conscientiousness → Δreligiosity	extraversion → Δreligiosity	neuroticism → Δreligiosity
Bavaria	7,085	1.95	.038 [.014, .063]	-.026 [-.051, -2E-04]	.012 [-.013, .037]	-.023 [-.047, .002]	.001 [-.017, .019]
Baden-Wuerttemberg	5,423	1.85	.040 [.011, .069]	-.030 [-.062, .003]	.013 [-.016, .042]	-.011 [-.040, .019]	-.002 [-.024, .020]
Rhineland-Palatinate	2,088	1.80	.038 [-.010, .086]	-.009 [-.057, .039]	.015 [-.037, .068]	-.011 [-.062, .040]	.024 [-.017, .064]
North Rhine-Westphalia	9,621	1.77	.046 [.022, .070]	-.046 [-.072, -.020]	-.003 [-.027, .020]	.027 [.003, .052]	.016 [-.003, .034]
Hesse	3,151	1.74	.030 [-.011, .071]	-.033 [-.077, .012]	-4E-04 [-.047, .047]	.005 [-.038, .048]	.021 [-.010, .051]
Lower-Saxony	4,392	1.73	.031 [-.002, .063]	.022 [-.017, .062]	.013 [-.021, .046]	-.033 [-.070, .005]	.006 [-.019, .031]
Schleswig Holstein	1,551	1.56	.109 [.051, .167]	-.039 [-.107, .029]	-.056 [-.128, .015]	.071 [.005, .138]	.092 [.032, .151]
Hamburg	686	1.50	.058 [-.013, .129]	-.080 [-.202, .042]	.016 [-.057, .089]	-.012 [-.126, .102]	.042 [-.027, .111]
Thuringia	1,742	1.44	-2E-04 [-.042, .042]	.025 [-.031, .081]	.012 [-.029, .053]	-.029 [-.079, .021]	-.009 [-.038, .019]
Berlin	1,615	1.40	.008 [-.047, .062]	-.011 [-.068, .046]	-.026 [-.083, .032]	.012 [-.037, .061]	.023 [-.014, .059]
Saxony	2,816	1.39	.037 [.010, .064]	.021 [-.012, .053]	-.039 [-.069, -.008]	-.008 [-.040, .024]	-.005 [-.028, .019]
Brandenburg	1,703	1.31	.046 [.003, .089]	-.016 [-.063, .031]	-.032 [-.083, .019]	.028 [-.021, .078]	.021 [-.014, .056]
Saxony-Anhalt	1,628	1.30	-.002 [-.040, .035]	.030 [-.017, .077]	-.010 [-.047, .027]	-.021 [-.062, .020]	-.010 [-.038, .019]
Mecklenburg-Vorpommern	984	1.28	.072 [.010, .134]	.019 [-.059, .096]	-.014 [-.075,.046]	-.006 [-.076, .063]	.006 [-.046, .057]
omnibus results	**44,485**		**.039* [.027, .049]**	**-.013 [-.029, .003]**	**-.003 [-.012, .011]**	**-.001 [-.017, .013]**	**.011 [-.006, .019]**
cultural religiosity as moderator	**44,485**		**.003 [-.008, .014]**	**-.014* [-.027, -.002]**	**.014* [.006, .021]**	**-.004 [-.019, .007]**	**-.002 [-.014, .008]**

Note. The cross-lagged effects across waves differed minimally from each other, even though we constrained them to be equal. This is common because waves typically differ in their *n*s and, thus, their *SE*s. Following Orth et al.'s (2021) recommendation, we report the average cross-lagged effects across the three inter-wave intervals. We used non-parametric bootstrapping on 5,000 bootstrapping samples to estimate the *SE*s for the meta-analytic results. * indicate significant meta-analytical results. Note, though, that we omitted all asterisks from the single models to aid readability.

Extraversion → Δreligiosity. Extraversion yielded no omnibus longitudinal effect on later changes in religiosity, β = -.001, 95% CI [-.017, .013], and cultural religiosity did not moderate this non-significant effect, β = -.004, 95% CI [-.019, .007].

Neuroticism → Δreligiosity. Neuroticism yielded no omnibus longitudinal effect on later changes in religiosity, β = .011, 95% CI [-.006, .019]. Cultural religiosity did not moderate this non-significant effect, β = -.002, 95% CI [-.014, .008].

Culture-Contingent Cross-Lagged Effects of Religiosity on Changes in the Big Five

Table 3 reports on the cross-lagged effects of religiosity on changes in the Big Five in each of our 14 cultural contexts. The top rows include the results for the most religious cultural contexts, the bottom rows the results for the most secular contexts. As before, we will next describe the meta-analytic summary of the results—that is, the omnibus cross-lagged effects. We will also describe whether cultural religiosity moderates the effects—that is, whether the cross-lagged effects of religiosity on changes in the Big Five are culture-contingent.

Religiosity → Δagreeableness. Religiosity yielded an omnibus cross-lagged effect on later increases in agreeableness, β = .011, 95% CI [.0004, .022]. Cultural religiosity did not moderate this effect, β = .003, 95% CI [-.009, .013]. Evidently, religiosity was associated with increases in religiosity over time, irrespective of cultural context.

Religiosity → Δopenness. Religiosity yielded no omnibus cross-lagged effect on later decreases in openness, β = -.007, 95% CI [-.022, .007]. Yet, cultural religiosity moderated this non-significant effect, β = -.013, 95% CI [-.024, -.0002]: The effect existed in religious cultural contexts, but was absent in secular contexts. In other words, religiosity was associated with decreases in openness over time, but only in religious cultural contexts.

Religiosity → Δconscientiousness. Religiosity did not yield an omnibus cross-lagged effect on later changes in conscientiousness, β = -.004, 95% CI [-.013, .005], and cultural-

religiosity did not moderate this non-significant effect, β = -.003, 95% CI [-.013, .004]. This absence of an effect may be surprising. We will elaborate on it in the Discussion.

Religiosity → Δextraversion. Religiosity yielded no omnibus cross-lagged effect on later increases in extraversion, β = .004, 95% CI [-.001, .009]. Cultural religiosity did not moderate this non-significant effect, β = -.001, 95% CI [-.008, .004].

Religiosity → Δneuroticism. Religiosity yielded no omnibus cross-lagged effect on later changes in neuroticism, β = .002, 95% CI [-.004, .008], and cultural religiosity did not moderate this non-significant effect, β = -.0001, 95% CI [-.007, .007].

Effect Size Comparisons

The cross-lagged effects of the Big Five on changes in religiosity (Table 2) were considerably larger than the cross-lagged effects of religiosity on changes in the Big Five (Table 3). At the same time, the uncovered bi-directionality underlying the association between the Big Five and religiosity is important, because it suggests a run-away process over the life span: Personality is associated with changes in religiosity four years later, which—in turn—is associated with changes in personality over the next four years, and so on. Thus, albeit the results met our expectations that the *cross-lagged* effect sizes will not exceed β = .05, the results also suggest that the bi-directionality of effects will accumulate to substantial associations between the Big Five and religiosity across the life span.

Discussion

In hundreds of samples, millions of people provided data on the cross-sectional associations between the Big Five and religiosity (Gebauer et al., 2014; Saroglou, 2010). For decades, this cross-sectional data seemed to satisfy the curiosity of psychologists, because they assumed that the Big Five were endogenous basic tendencies, immutable to life experiences (McCrae & Costa, 2008). Advances in the personality change literature, however, have shown that the Big Five are much more mutable to life experiences than has traditionally been assumed (Bleidorn et al., 2021; Wagner et al., 2020). Thus, we cannot take it for granted

Table 3

Cross-Lagged Effects from Religiosity on the Big Five Traits Estimated Within Each Federal State and the Associated Meta-Analytical Results

state	*N*	state-level religiosity	cross-lagged effects (β) of religiosity on changes in the Big Five				
			religiosity → Δagreeableness	religiosity → Δopenness	religiosity → Δconscientiousness	religiosity → Δextraversion	religiosity → Δneuroticism
Bavaria	7,085	1.95	-.004 [-.026, .018]	-.020 [-.041, .001]	-.013 [-.033, .008]	-.002 [-.020, .017]	.004 [-.015, .024]
Baden-Wuerttemberg	5,423	1.85	.017 [-.008, .042]	-.026 [-.050, -.002]	-.026 [-.051, -.001]	-.004 [-.025, .018]	-.0001 [-.023, .023]
Rhineland-Palatinate	2,088	1.80	-.021 [-.065, .023]	-.046 [-.085, -.008]	-.018 [-.061, .025]	.001 [-.034, .036]	.013 [-.025, .051]
North Rhine-Westphalia	9,621	1.77	.032 [.012, .051]	.021 [.003, .039]	.008 [-.010, .027]	.011 [-.005, .028]	.003 [-.014, .021]
Hesse	3,151	1.74	.028 [-.003, .059]	-.028 [-.059, .003]	.019 [-.012, .051]	.012 [-.015,.038]	-.020 [-.050, .010]
Lower-Saxony	4,392	1.73	.016 [-.011, .043]	-.018 [-.045, .009]	-.005 [-.033, .022]	.003 [-.022, .028]	.001 [-.026, .028]
Schleswig Holstein	1,551	1.56	.038 [-.011, .087]	-.022 [-.072, .027]	-.006 [-.056, .044]	-.001 [-.048, .045]	.048 [-.008, .105]
Hamburg	686	1.50	.043 [-.024, .109]	-.014 [-.081, .052]	.002 [-.067, .071]	.019 [-.034, .072]	-.003 [-.053, .047]
Thuringia	1,742	1.44	-.003 [-.035, .030]	-.016 [-.049, .017]	.013 [-.019, .044]	-.010 [-.040, .020]	-.027 [-.058, .005]
Berlin	1,615	1.40	-.008 [-.047, .031]	.019 [-.015, .053]	.010 [-.022, .041]	.010 [-.020, .041]	.007 [-.030, .044]
Saxony	2,816	1.39	-.004 [-.032, .024]	.035 [.009, .062]	.004 [-.024, .033]	.005 [-.018, .028]	-.002 [-.028, .024]
Brandenburg	1,703	1.31	-.004 [-.040, .032]	-.020 [-.061,.021]	-.017 [-.060, .026]	.018 [-.017, .054]	.022 [-.013, .057]
Saxony-Anhalt	1,628	1.30	-.002 [-.039, .034]	.030 [-.005, .065]	-.016 [-.053, .021]	-.018 [-.051, .015]	.007 [-.032, .045]
Mecklenburg-Vorpommern	984	1.28	.007 [-.047, .061]	.012 [-.045, .068]	-.012 [-.063, .039]	.015 [-.029, .059]	-.018 [-.070, .033]
omnibus results	**44,485**		**.011* [.0004, .022]**	**-.007 [-.022, .007]**	**-.004 [-.013, .005]**	**.004 [-.001, .009]**	**.002 [-.004, .008]**
cultural religiosity as moderator	**44,485**		**.003 [-.009, .013]**	**-.013* [-.024, -.0002]**	**-.003 [-.013, .004]**	**-.001 [-.008, .004]**	**-.0001 [-.007, .007]**

Note. The cross-lagged effects across waves differed minimally from each other, even though we constrained them to be equal. This is common because waves typically differ in their *n*s and, thus, their *SE*s. Following Orth et al.'s (2021) recommendation, we report the average cross-lagged effects across the three inter-wave intervals. We used non-parametric bootstrapping on 5,000 bootstrapping samples to estimate the *SE*s for the meta-analytic results. * indicate significant meta-analytical results. Note, though, that we omitted all asterisks from the single models to aid readability.

that the associations between the Big Five and religiosity reflect effects of the former on the latter rather than the reverse. In fact, religion is such a powerful social force (Gebauer & Sedikides, 2021; Norenzayan, 2013) that religiosity is a prime candidate to change personality. After all, no other social ideology motivates behavior by promises of eternal joy in heaven and threats of eternal agony in hell (Berkessel et al., 2021; Johnson, 2016). Thus, the directionality underlying the association between personality and religiosity has recently sparked psychologists' curiosity (Ashton & Lee, 2021; Entringer et al., 2021).

Here, we conducted the first large-scale, bi-directional research on the cross-lagged effects between the Big Five and religiosity. Moreover, our research is also the first that attends to cultural differences in religiosity. That attention was important, because cross-sectional research on personality and religiosity has identified cultural religiosity as a key moderator (Ashton & Lee, 2019; Entringer et al., 2021). Overall, we relied on data from a total of 44,485 participants, representatively sampled from 14 federal states of Germany—states that vary widely in their cultural religiosity. Our study included four assessment waves with four-year inter-wave intervals and, thus, spanned over a total of 12 years.

Consistent with previous research, our results revealed that higher agreeableness led to increases in religiosity over time and this agreeableness effect was equally strong in religious and non-religious cultural contexts alike. Furthermore, lower openness and higher conscientiousness did not lead to changes in religiosity overall, but cultural religiosity qualified those two (non-significant) cross-lagged effects. Consistent with the SNP (Eck & Gebauer, 2022), in both cases the effects became stronger in religious cultural contexts and weaker in non-religious cultural contexts. By demonstrating the culture-contingency of the two effects, the present research may help explain why previous research found mixed evidence for the effects of openness and conscientiousness on changes in religiosity.

Inconsistent with the SNP, however, the effect of agreeableness on changes in religiosity was similarly strong across cultural contexts, no matter whether cultural religiosity

was high or low. This inconsistency with the SNP is puzzling for three reasons. First, cross-sectional data clearly shows stronger associations between agreeableness and religiosity in religious than non-religious cultures (Ashton & Lee, 2019; Gebauer et al., 2014). Second, experimental evidence has shown quite clearly that agreeable people robustly show normative behavior in general (here: being particularly religious in religious cultures) (Eck & Gebauer, 2022). Finally, Gebauer et al. (2014, Study 3) used two waves of SOEP data (back then, only the 2005 and 2009 waves were available) and found the expected moderation effect in their cross-lagged analyses of agreeableness on changes in religiosity.

The results of Gebauer et al.'s Study 3 and the present study also differed in another respect. Both studies found that cultural religiosity moderated the effect of openness on changes in religiosity and this moderation was the same in both studies. But in Gebauer et al.'s Study 3 the main effect of openness on increases in religiosity was significant, in the present study that main effect was insignificant. For two reasons, we consider the present results more telling than the results of Gebauer et al.'s Study 3: First, the present results rely on much more data (four waves vs. two waves). Second, the present statistical model is superior (full cross-lagged design and latent measurement model vs. half cross-lagged design and manifest measurement model).

Of primary interest, our results did not only reveal cross-lagged effects of the Big Five on changes in religiosity. We also found cross-lagged effects of religiosity on changes in the Big Five—effects that were examined in only two extant studies and yielded mixed results. First and foremost, higher religiosity led to increases in agreeableness over time and those increases were equally strong in religious and non-religious cultural contexts alike. These results are consistent with evidence on the effectiveness of the religious prosociality commandments (Lang et al., 2019; Purzycki et al., 2016). Additionally, higher religiosity led to decreases in openness, but only in religious cultural contexts. This result is consistent with the proposal that a religious cultural context functions as a social facilitator, which

strengthens the effects that individual religiosity tends to exert (Gebauer & Sedikides, 2021). In this case, religiosity calls for a non-open, conservative mindset and it succeeds only, if the ambient cultural context is religious and, thus, facilitates conservative thinking. Contrary to our expectation, we did not find a cross-lagged effect of higher religiosity on increases in conscientiousness. We expected such an effect at least in religious cultural contexts, where social facilitation is high, but we found none in either context (religious and non-religious). This finding was unexpected because many religious practices seem to train and enhance conscientiousness (e.g., fasting, abstinence; Koole et al., 2010). In hindsight, however, we cannot help but suspect that practices like fasting and abstinence are not all that central to a religious life in Germany any more. Replication attempts are, thus, required in nations that still consider practices like fasting and abstinence core to religious life.

A key question of the present research was to compare the relative power of the Big Five on changes in religiosity versus the relative power of religiosity on changes in the Big Five. In line with the intuitions of many personality psychologists (Gebauer et al., 2014; Saroglou, 2010), this comparison revealed that the cross-lagged effects of the Big Five on changes in religiosity were the stronger ones. Thus, the conclusions drawn from the extensive, cross-sectional literature do not have to be revised entirely. But the present results do suggest that some re-thinking is nonetheless in order: The causal arrows between the Big Five and religiosity seem to go both ways and this bi-directionality is especially consequential because it most likely kicks-off a runaway process in which personality leads to changes in religiosity over time, which—in turn—leads to changes in personality over time, and so on. A result of such runaway processes are substantial effects when considered over the full life span.

As stated before, the results of our study are largely consistent with the results of previous longitudinal studies on personality and religiosity. This consistency is noteworthy, because previous studies differed from the present study in several important ways. For example, most previous studies started out in adolescence (Heaven & Ciarrocchi, 2007;

Huuskes et al., 2013; McCullough et al., 2003, 2005; Wink et al., 2007), used much shorter inter-wave intervals (2 years; Heaven & Ciarrocchi, 2007) or much longer ones (almost 70 years; Wink et al., 2007), and generally relied on two waves only (with the exception of McCullough et al., 2005, which used three waves).

Finally, our research also showcases the importance to attend to the cultural context in research on personality change. For quite a while now, cross-cultural personality psychologists have called for attention to culture in personality-change research (Benet-Martinéz & Oishi, 2008). To the best of our knowledge, the present research is the first one that tests for the moderating role of culture (here: cultural religiosity) on the effect of life experiences (here: religiosity) on personality change (here: Big Five change). Perhaps our research may help spark additional research along similar lines.

Limitations and Future Directions

We have carefully reviewed available datasets and only found a single suitable one, the SOEP. This dataset is valuable due to its size, time coverage, and the variables it includes. What was particularly valuable for us, however, was that the SOEP includes representative samples of all German federal states and that those federal states vary widely in cultural religiosity (Gebauer et al., 2014). That wide variation allowed us to adequately test for cross-cultural differences. Still, replication efforts in other nations are important for future research, particularly for two reasons. First, Germany as a whole (i.e., on average, across all its federal states) belongs to one of the most secularized nations in the world. Studies in more religious nations are valuable, because they would most probably find stronger overall effects of personality on religiosity and vice versa. Second, Germany has a unique history: The East German states belonged to the former German Democratic Republic (GDR), a communist nation with an antireligious attitude. This explains the low levels of religiosity in the East German states. Thus, we should not take it for granted that the present results necessarily

replicate perfectly in nations with very different histories, especially nations without a communist history in their recent past.

Future research should also attempt replication of the present findings with other measures of religiosity, including ones that focus more on private thoughts and feelings rather than actual behavior. It is, for example, well possible that private religious thoughts exert weaker longitudinal effects on personality, because it is the (repetitive) religious behavior that brings about religious change. In fact, previous studies on the longitudinal effects of religiosity on personality have often measured religiosity with items on private religious thought (Heaven & Ciarrocchi, 2007; Huuskes et al., 2013) and those studies have often failed to find longitudinal effects. And, yet, previous studies also differ from the present studies in several other ways (our study being much larger, including more waves, attending to culture much more, and using more up-to-date methodology). It is, of course, also possible that those other differences account for differences in the results. In fact, private religious thought is more intrinsically motivated than is religious behavior (which may well be extrinsically motivated; Allport & Ross, 1967) and, thus, private religious thought may exert even stronger effects on personality change. In any case, future research seems necessary to sort out those different possibilities.

Future research should also attempt replication of the present findings with other measures of the Big Five. The SOEP includes a 15-item short form of the BFI (John et al., 2008). Time and space constraints in panel studies rendered this short-form a most sensible choice. But Saroglou (2010) has shown that the associations between the Big Five and religiosity are typically larger when longer scales are used. Reliance on the 15-item SOEP-BFI may, thus, help explain why some associations in the present study were small, including a non-significant effect of religiosity on changes in conscientiousness. Thus, the use of the BFI in its full 44-item version would be welcome for future research, as would be the full 60-item BFI2 (Soto & John, 2017), and 60-item NEO-FFI (Costa & McCrae, 1992), and the 240-

item NEO-PI-R (Costa & McCrae, 2008). In a related vain, future research may also want to examine the longitudinal relations between religiosity and other personality traits, including ones that are similarly broad than the Big Five (e.g., honesty-humility—Ashton & Lee, 2019; d—Moshagen et al., 2018; negative valence—Benet & Waller, 1995) and ones that are considerably more specific (e.g., narcissism—Sedikides, 2021; prosociality—Caprara et al., 2012).

Another open question is which precise elements of religiosity elicit personality change. One plausible possibility is repetitive religious behavior, such as regular prayer (Wrzus & Roberts, 2017). Another plausible possibility is religious norms, such as the norm (or commandment) to behave prosocially (Norenzayan & Shariff, 2008). Future research should competitively test these and other possibilities against each other and be prepared to find that multiple religious elements may well elicit personality change independent of each other (cf. Koole et al., 2010).

Finally, almost all panel data is gathered via self-report questionnaires, often in personal interviews (Kühne, 2018). Whenever self-report questionnaires serve as the assessment method of choice, social desirability may play some role (Paulhus, 1984). Oftentimes, social desirability poses no threat to the validity of the results, because oftentimes social desirability is a valid process that exerts effects on real life behavior. For example, religious people may possess a general desire to please others (i.e., high social desirability), which may elicit more frequent prosocial behavior, thereby, increasing agreeableness. At times, however, social desirability may be restricted to contaminating questionnaire responses ("faking good") without any reflection of real-life behavior. For example, people with a general proclivity to fake good may fake on questionnaires higher levels of religiosity and also higher levels of agreeableness (assuming that those people consider religiosity and agreeableness as "good" qualities, which is typically the case; Sedikides & Gebauer, 2010). As a result, the association between religiosity and agreeableness may be partly spurious, due

to individual differences in the proclivity to fake good. Fortunately, cross-sectional research is far more vulnerable to such spuriousness than is longitudinal research, especially when the longitudinal data is modelled via cross-lagged panel designs (Kühne, 2018; Rindfleisch et al., 2008). Nonetheless, replications would be valuable under conditions that minimize motives to fake good (e.g., responses are provided in the absence of interviewers, experimenters, and any other—real or imagined—audience).

Conclusion

There is a large literature on the Big Five and religiosity, but that literature is largely cross-sectional by nature. For long, this seemed justified because there was little theoretical reason to believe that a religious way of life may change the Big Five. And, thus, it seemed self-evident that the cross-sectional associations are due to effects of the Big Five on religiosity, not vice versa. Advances in the personality change literature, however, told us otherwise, creating a real need for cross-lagged research that examines the potential bi-directionality between the Big Five and religiosity. The needed research also required attention to cultural religiosity, a key moderator of cross-sectional associations between the Big Five and religiosity in past research. We conducted the needed research and indeed found bi-directional cross-lagged effects between the Big Five and religiosity, especially in religious cultural contexts. More specifically, we found longitudinal effects of agreeableness, openness, and conscientiousness on changes in religiosity, with the latter two effects being culture-contingent. At the same time, we found longitudinal effects of religiosity on changes in agreeableness and openness, with the latter effect being culture-contingent. Finally, the longitudinal effects of personality on religiosity were typically stronger than the reverse effects.

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Notes

1 In fact, is has been argued that religiosity itself is a personality trait. In particular, religiosity is very stable and manifests itself in enduring patterns of behavior, thought, and feelings—the key definitional aspect of personality traits. However, religiosity is also different from personality because it possesses features that personality traits do not possess (such as believing, binding, behaving, and bonding). For those reasons, religiosity has been described as a characteristic adaption of a basic tendency in the literature (Ashton & Lee, 2014).

2 People high on extraversion, too, tend to conform to social norms (Eck & Gebauer, 2022). Thus, from the perspective of the SNP, it is possible that higher extraversion leads to increases in religiosity in religious cultural contexts. From the niche picking perspective, however, extraversion should be unrelated to religiosity (cf. Allport, 1950).

3 Three additional studies examined the effects of religious (de-)conversion on the Big Five (Hui et al., 2018; Paloutzian et al., 1999; Stronge et al., 2021). Yet, their results are also contradictory. Stronge et al. (2021) found that religious conversion was associated with an increase in traits related to agreeableness, whereas religious de-conversion was associated with a decrease in those agreeableness-related traits. Hui et al. (2018) and Paloutzian et al. (1999), by contrast, found no evidence that religious (de-)conversion is associated with any changes in the Big Five.