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The impact of religious involvement on trust, volunteering, and perceived cooperativeness: evidence from two British panels

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Does religious involvement make people more trusting and prosocial? Considering conflicting theories and mixed prior evidence, we subject this question to a stringent test using large-scale, representative data from the British Household Panel Survey (1991–2009, $N \approx 26{,}000$) and the UK Household Longitudinal Study (2009–2021, $N \approx 80{,}000$). We employ cross-lagged panel models with individual fixed effects to account for time-invariant confounders and reverse causality—two issues that have haunted earlier research. We find that frequency of religious service attendance on average has a positive impact on generalized trust, volunteering, and perceived cooperativeness. Other indicators of religious involvement have weaker effects. We also find variation across religious traditions: the effects of religious attendance are mostly positive for Anglicans and other Protestants, but weaker and mostly statistically insignificant for Catholics, Hindus, and the unaffiliated, and even negative for Muslims when the outcome is perceived cooperativeness. Our findings are robust to alternative model set-ups and hold up after accounting for neighbourhood religious composition, respondent and interviewer ethnicity, and other potential moderators and confounders. Altogether, our study shows that religious involvement can foster prosocial behaviours and attitudes, although in our study this effect is largely restricted to religious service attendance and majority religions.

Introduction

The question of how religious involvement affects social cohesion has a long history. In *Democracy in America*, De Tocqueville ([1835] 2003) argues that religion is a vital source of trust and civic engagement. In *The Elementary Forms of Religious Life*, Durkheim ([1912] 1995) portrays religion as an institution that nurtures solidarity by enforcing collective norms and offering believers a sense of purpose. A prediction emerging from this work is that religious individuals, compared to their non-religious counterparts, will hold more cooperative attitudes and behave more prosocially.

However, the empirical evidence on this topic remains mixed. Several studies conclude that religious involvement *fosters* prosocial attitudes and behaviours. Stavrova and Siegers (2014), for example, show that individual religiosity is positively associated with charity work and negatively with fraud. Bennett and Einolf (2017) demonstrate that frequent churchgoers are more likely to help strangers. Similarly, there is a well-documented positive relationship between religious involvement and volunteering, especially for religious service attendance (Wilson and Musick, 1997; Ruiter and De Graaf, 2006; Putnam and Campbell, 2010) but also for more private expressions of religiosity (Paxton, Reith and Glanville, 2014; Storm 2015). Others show that religious believers are often perceived as more trustworthy and cooperative than non-believers (Edgell, Gerteis and Hartmann, 2006) and that frequent religious attendance is associated with a more inclusive societal outlook (McAndrew 2020). Laboratory experiments indicate that religious participants are more likely to reciprocate trust, while they are also more likely to be trusted, including by adherents of different religions and the nonreligious (Tan and Vogel, 2008; Hall et al., 2015). Moreover, priming participants by reminding them of God increases altruism towards strangers (Shariff and Norenzayan, 2007).

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Nevertheless, other scholars argue that religious involvement *reduces* trust and prosocial behaviours. Berggren and Bjørnskov (2011), for example, conclude that trust is lower in places where people value religion more. Hempel, Matthews and Bartkowski (2012) show that beliefs about the authoritativeness of the Bible, the existence of hell, and the need for a born-again experience undermine trust towards strangers in the United States. Valente and Okulicz-Kozaryn (2021: p. 344) find that stronger religious beliefs and more frequent praying are associated with higher levels of misanthropy, bringing them to the conclusion that 'connecting with God disrupts connection with humans'. These findings resonate with portrayals of religious associations as bonding social capital (Putnam 2000), bolstering social ties between fellow believers, yet impeding wider social cohesion and solidarity with outgroup members. Likewise, multiple experimental studies suggest that believers display ingroup favouritism by being only more helpful and trusting towards co-religionists (Karlan 2005; Galen 2012; Preston and Ritter, 2013; Chuah et al., 2016). Furthermore, Jackson and Gray (2019) find that people who believe in God's intervention in daily events more often commit passively immoral behaviours, like parking across multiple spaces or keeping library books overdue.

Aside from variation in the outcome of interest, the measure of religious involvement, and the study context, these conflicting findings may reflect methodological issues, as most studies rely on either cross-sectional surveys or small-scale experiments, both of which can have serious shortcomings. With cross-sectional surveys, it is usually impossible to control for all relevant confounders or to rule out reverse causality. Both issues are relevant here: many factors may affect religious involvement as well as prosocial attitudes and behaviours (e.g. personality, social background), and causality may well run both ways (e.g. trusting people might be more likely to join religious communities). Experimental studies, in turn, usually offer high internal validity, yet questionable external validity. Moreover, it is well-nigh impossible and ethically contentious to randomly manipulate religious involvement (cf. Bryan, Choi and Karlan, 2021).

Our study offers new evidence on the impact of religious involvement on cooperative behaviours and trust, making three contributions. First, we draw on high-quality, nationally representative longitudinal data from the British Household Panel Survey (BHPS) and the UK Household Longitudinal Study (UKHLS). In doing so, we add to a growing body of longitudinal studies on the wider social impacts of religious involvement (Johnston 2013; Kim and Jang, 2017; Son and Wilson, 2021). More specifically, we apply cross-lagged panel data models with individual fixed effects (Allison, Williams and Moral-Benito, 2017). These models enable us to simultaneously address time-invariant confounding and reverse causality; see also Leszczensky and Wolbring (2022).

Our other two contributions are substantive. Firstly, we probe how religious involvement affects three distinct but related outcomes: generalized trust (a self-reported belief-based measure), volunteering (a self-reported behavioural measure), and perceived cooperativeness (measured externally by the interviewer, based on their interactions with the respondent). While previous studies usually consider one indicator of prosociality in isolation, our focus on these three complementary measures within the same analysis helps to establish the breadth of any effects of religious involvement.

Additionally, we investigate the effects of religious involvement separately for the religiously unaffiliated, Anglicans, other Protestants, and Catholics, as the largest religious groups in the UK, and Hindus and Muslims, as the largest non-Christian groups. Notwithstanding notable exceptions (e.g. Wilson and Janoski, 1995; Beyerlein and Hipp, 2006; Welch, Sikkink and Loveland, 2007; Daniels and Von der Ruhr, 2010; Traunmüller 2011), most prior research implicitly assumes that the effects of religious involvement are the same across religious traditions, yet this is far from evident, given variations in theological doctrines and how religious groups are embedded into society.

Theory and predictions

Our main interest is in *generalized* forms of prosociality and trust, whereby the target of the action or attitude is a generic alter such as a stranger. For *particularized* forms, by contrast, the target is known by the focal actor or an ingroup member (e.g. co-religionists). It is generalized trust and prosociality that is thought to contribute to social cohesion at large (Putnam 2000; Delhey, Newton and Welzel, 2011). Increases in particularized bonds could even be harmful, for they might crowd out generalized cooperation, and hence reduce aggregate social cohesion (Ermisch and Gambetta, 2010). That said, the evidence for a strict trade-off is weak: for example, various studies show that particularized and generalized trust are positively related (Newton and Zmerli, 2011; Welzel and Delhey, 2015).

There are conflicting theories about how religious involvement affects generalized forms of prosocial behaviours and trust. A first set, building on De Tocqueville ([1835] 2003) and Durkheim ([1912] 1995), predict that religious involvement has a *positive* effect, by altering people's preferences, their

opportunities and incentives, and/or their interpersonal and civic skills. Regarding the preferences channel, almost all religions have moral teachings promoting cooperation, solidarity, and altruism (Batson, Schoenrade and Ventis, 1993), with most religions having some version of the 'golden rule' (e.g. 'do unto others as you would have them do unto you'). These teachings often focus on 'generalized others', stressing the importance of treating strangers or outgroups well. Furthermore, many faiths encourage selflessness, sympathy for the needs of others, and care for one's wider community (Daniels and Von der Ruhr, 2010; Lewis, MacGregor and Putnam, 2013). Such values are solidified during shared rituals and communal worship.

Religious involvement additionally provides social incentives and opportunities for prosocial behaviour. Networks developed through communal religious practices can monitor participants' behaviours and enforce religiously inspired norms. In addition, religious communities coordinate many civic initiatives, which offer adherents opportunities to engage in prosocial behaviours, both within their congregation and the wider community. Indeed, Lewis et al. (2013) show that people are more likely to do volunteer work when they have more religious friends, likely because people with more religious contacts are more often asked to volunteer (Merino 2013).

Finally, active involvement in religious communities may help develop civic skills, as collective religious activities are almost always socially mediated (Djupe and Gilbert, 2006). Participants thus develop civic and social skills through planning meetings, giving speeches, and discussing social issues during religious events. Although such skills can also be honed by non-religious activities, Djupe and Gilbert (2006) argue that religious communities provide safe spaces that are especially conducive to nurturing such skills. As the argument goes, the skills thus developed likely spill over to interactions with the wider community, thereby fostering generalized cooperation and trust.

By contrast, a second set of theories predict that religious involvement has nil or *negative* effects on generalized forms of prosociality and trust. These theories focus on the possible role of religious practices and communities in shifting preferences and opportunities for prosociality. Take moral self-licensing: the notion that actions which improve someone's self-image make them less concerned about the consequences of any immoral behaviours (Merritt, Effron and Monin, 2010). Accordingly, if one feels obedient to a divine authority, for example by attending religious services regularly, one may feel less constrained by norms of the mundane, which may weaken prosociality (Jackson and Gray, 2019), especially towards strangers.

Another theory posits that certain religious doctrines may undermine generalized trust and people's commitment to the wider community (Daniels and Von der Ruhr, 2010). This especially applies to conservative groups who interpret religious scriptures narrowly. Many conservative Christians in the United States, for example, believe that people have an inherent disposition to choose evil over good and that 'trust is something rightfully reserved for God and … one's born-again co-religionists' (Hempel et al., 2012: p. 526). Greater involvement in such groups may hence cultivate distrust and a detachment from wider society; see also Valente and Okulicz-Kozaryn (2021).

The coexistence of these contrasting theories calls for a rigorous empirical test, which we provide here. Our test is rigorous in three ways: it is longitudinal, it considers multiple measures of religious involvement and prosociality, and it differentiates between religious traditions. Longitudinal data offer opportunities to address methodological issues that often plague cross-sectional research, such as reverse causality and omitted variable bias. As we explain in the next section, both issues are relevant to our setting. In recent years, other studies have already harnessed longitudinal data to study the impact of religious involvement on volunteering (Johnston 2013; Kim and Jan, 2017; Son and Wilson, 2021). Although most of these studies are based on the same data source (Americans' Changing Lives 1986–2001), they illustrate the value of a longitudinal approach and add greater credibility to the idea that religious involvement boosts volunteering, both for religious and non-religious organizations. We extend this work to the British context, incorporate additional outcomes, and distinguish between religious traditions.

As a measure of religious involvement, we focus on frequency of religious service attendance, as this 'behaving' dimension of religiosity has been most consistently linked to prosocial and cooperative outcomes (e.g. Wilson and Musick, 1997; Ruiter and De Graaf, 2006; Anderson, Mellor and Milyo, 2010; Putnam and Campbell, 2010; Bennett and Einolf, 2017). The networks fostered through regular service attendance appear key in instilling and enforcing religious norms, offering social support, developing social skills, and providing opportunities for prosocial activities (Lim and MacGregor, 2012; Lewis et al., 2013; Merino 2013). By contrast, merely 'belonging' or 'believing' in a religion may not be enough to durably enhance trust and prosociality. Indeed, most studies that report negative effects of religious involvement focus on affiliation- or belief-based measures (e.g. Berggren and Bjørnskov, 2011; Galen 2012; Hempel et al., 2012; Valente and Okulicz-Kozaryn, 2021). Nevertheless, we also conduct analyses using non-behavioural measures.

Considering multiple outcomes is important. Firstly, our key question is how religious involvement affects *generalized* forms of prosociality and trust. Reliance on a single outcome poses the risk that the selected outcome variable inadvertently primarily measures the particularized form. Looking at multiple indicators of prosociality and trust reduces the risk of such misattribution errors. Secondly, the impact of religious attendance may depend on the outcome considered. For example, while reverence for the golden rule may induce frequent churchgoers to report high trust levels, this may not translate into behaviour. More generally, because most religions value 'doing good', religious adherents may overreport socially desirable attitudes or behaviours. This could result in larger effects of religious attendance on self-reported measures of trust and prosociality. It may also explain why survey research often finds more positive effects than experimental research (Galen 2012). Relatedly, religious attendance may have a larger impact on planned and publicly observable behaviours (e.g. volunteering) than on more spontaneous and private expressions of prosociality (e.g. showing compassion to strangers). It is thus informative to study multiple outcomes, from attitudes to self-reported and externally evaluated behaviours.

We finally expect heterogenous effects of religious attendance across religious traditions. More specifically, involvement in more hierarchical religions, such as Catholicism, may be less conducive to the development of generalized trust and cooperativeness than involvement in religions that rely more on horizontal bonds of fellowship, such as most Protestant denominations (e.g. Putnam 1993). In this context, Traunmüller (2011) finds in Germany more positive effects of religious attendance on generalized trust among Protestants than among Catholics. This difference may also reflect how different religions relate to co-religionists vis-à-vis outsiders. American research shows, for example, that conservative Protestants are more inward-looking and less inclined to trust strangers or to join secular volunteering activities than liberal Protestants, with Catholics situated in between (Wilson and Janoski, 1995; Beyerlein and Hipp, 2006; Welch et al., 2007; Daniels and Von der Ruhr, 2010).

Most Protestants in the United Kingdom (the majority Anglican or Presbyterian) are relatively liberal and reputed for taking an active role in wider community life, possibly reflecting their majority status. The Church of England, for example, is traditionally involved in many civic projects that cross social boundaries, such as food banks and community cafés, and engages with politics by publishing pamphlets (e.g. Church of England 2015, 2018). As such, more regular church attendance among Protestants plausibly promotes trust, cooperativeness, and societal engagement—and not just towards co-religionists but also towards generalized others.

The second-largest religious group in the United Kingdom are Catholics. Given the more hierarchical nature of their church, we expect weaker effects of religious attendance on generalized trust and prosociality for this group. Their stronger internal hierarchy may inhibit the development of civic skills and contacts with fellow churchgoers, which are critical in reducing intergroup prejudice and fostering generalized trust. Furthermore, the norms and practices preached in Catholic churches are, relatively speaking, more inward-oriented and less inclusive. Supporting this argument, data from the Pew Research Center (2017) indicate that British Catholics are less likely than British Protestants to view Catholics and Protestants as religiously similar, and that they are less willing to accept Protestants as relatives or neighbours than vice versa. Moreover, our data indicate that Catholics are slightly more likely than Protestants to participate in *religious* groups but considerably less likely to participate in other civic groups.

There are also fast-growing non-Christian minorities in the United Kingdom (Bruce 2016). The largest are Muslims and Hindus. Because being Muslim or Hindu in the United Kingdom is associated with other types of minority status or disadvantage (e.g. migration experiences), it is difficult to isolate the influence of these religious traditions. Nevertheless, we expect especially for Muslims relatively weaker effects of religious attendance on generalized trust and prosociality. One reason is that Muslims have a strong ingroup orientation. For example, in the UKHLS, 15 per cent of Muslims say religion affects their friendship choices 'a lot', whereas only around 5 per cent of Protestants, Catholics, and Hindus say so. Experiences of religious discrimination or acculturation stress (Aidenberger and Doehne, 2021; Aksoy et al., 2022)—both plausibly more common among adherents who regularly attend religious services (Helbling 2014)—may also hinder generalized trust and cooperativeness among non-Christian minorities. This is particularly so as 'generalized others' may have a stronger outgroup connotation among these minorities. Storm, Sobolewska and Ford (2017) show that especially British Muslims face intense hostility, although they do not seem to react by becoming more hostile to other groups themselves.

Another fast-growing group is the religiously unaffiliated. One may be tempted to disregard this group, assuming they do not attend religious services anyway. Yet, this assumption is unwarranted, as religious beliefs, behaviours, and affiliations often exhibit some incongruence (Chaves 2010). In our data, we do indeed observe that 8 per cent of the unaffiliated attend religious services at least once a year. Although religious attendance may also for this group influence trust and prosociality, it is questionable whether such effects will materialize. After

Table 1 Summary of key variables

Variable	Scale	Mean	Standard deviation	
			Between	Within
BHPS				
Religious attendance	0 = practically never, …, 1 = at least once a week	0.24	0.34	0.13
Generalized trust	0 = you can't be too careful/ it depends, 1 = most people can be trusted	0.37	0.40	0.30
Volunteering	0 = (almost) never, …, 1 = at least once a week	0.13	0.23	0.18
Perceived cooperativeness	0 = (very) poor, 0.33 = fair, 0.67 = good, 1 = very good	0.94	0.13	0.10
UKHLS				
Religious attendance	0 = practically never, …, 1 = at least once a week	0.24	0.36	0.13
Volunteering	0 = (almost) never, …, 1 = at least once a week	0.15	0.26	0.19
Perceived cooperativeness	0 = (very) poor, 0.33 = fair, 0.67 = good, 1 = very good	0.91	0.17	0.10

Notes: Statistics are based on the BHPS and UKHLS waves included in our main analyses (see Figures 1 and 2). We decompose the standard deviation of each variable into its between- and within-respondent component.

all, one plausibly needs to properly subscribe to a religion for religious attendance to affect attitudes and behaviours. Moreover, the unaffiliated generally attend religious services sporadically, which may not be a large enough 'dose' to elicit strong effects. We thus expect small if any effects of religious attendance for the unaffiliated.

Data and methods

Data source

We first analyse the BHPS, an annual household panel study that ran from 1991 to 2009. The initial sampling was done using a two-stage stratified probabilistic method, resulting in broadly nationally representative samples. The same people were re-interviewed in successive waves. The household-level response rate in wave 1 was 74 per cent, with a within-household individual-level response rate of 92 per cent. The average individual-level re-interview rate over the BHPS window was 93 per cent. These high rates are important, as our key variables may be linked to panel attrition (Sherkat 2007; Abraham, Helms and Presser, 2009). We then analyse the successor of the BHPS, the UKHLS, another nationally representative annual panel. The UKHLS has a larger sample size, starting with 40,000 households in 2009, and includes an ethnic minority boost sample. This enables us to separately analyse non-Christian groups. However, the UKHLS measures religious attendance only four times. For details on the BHPS and UKHLS and access to the publicly available data, see University of Essex (2022).

Key variables

Our independent variable concerns religious involvement. We focus on *frequency of religious service attendance*, measured as interval with the responses: at least once a week = 1, at least once a month = 0.67, at least once a year = 0.33, and practically never = 0. It is measured in waves 1, 3, 4, 5, 7, 9, 11, 14, 16, and 18 of the BHPS and waves 1, 4, 8, and 12 of the UKHLS. Table 1 summarizes this and other key variables. Online Supplement A contains additional information on changes in religious attendance, showing that 'never' and 'weekly' are the most stable patterns of attendance. However, there are also many changes between waves: e.g., of those who attend services at least once a month, 15 per cent increase their attendance in the next wave and 40 per cent decrease it. We also analyse two alternative indicators of religious involvement, namely *subjective importance of religion* ('how much difference would you say religious beliefs make to your life?') and *religious affiliation* ('do you regard yourself as belonging to any particular religion?').

We consider three outcome variables. The first is *generalized trust*, derived from the question 'Generally speaking, would you say that most people can be trusted, or that you can't you be too careful in dealing with people?', with 'most people can be trusted' coded as 1 and all other responses as 0.[1,2] Trust is measured in waves 8, 10, 13, 15, 17, and 18 of the BHPS and wave 1 of the UKHLS.

The second outcome is *volunteering*, based on a question that asks respondents how frequently they do unpaid volunteer work. In the BHPS, this question has five response categories, which we treat as interval: (almost) never = 0, once a year = 0.25, several times a year = 0.5, at least once a month = 0.75, at least once a week = 1. The UKHLS question has nine response categories, which we recode to match the BHPS categorization. Volunteering is measured in waves 6, 8, 10, 12, 14, 16, and 18 of the BHPS and waves 2, 4, 6, 8, 10, and 12 of the UKHLS.[3]

The last outcome is *perceived cooperativeness*, capturing the interviewer's assessment of how cooperative the respondent was during the interview. We also treat this outcome as interval: (very) poor = 0, fair = 0.33, good = 0.66, very good = 1. It is measured in all survey waves (except UKHLS wave 12) and provides an external measure of generalized cooperativeness, being assessed by trained interviewers, based on interactions between strangers. See online Supplement B for a validation check of this measure, which shows that perceived cooperativeness predicts whether one is willing to work with others to improve one's neighbourhood. Considering that the latter information is collected through self-completion questionnaires that are not monitored by the interviewer, our interviewer-assessed measure of cooperativeness thus likely captures actual cooperativeness.

Together, our outcome variables offer complementary insights: trust is a self-reported attitudinal measure, volunteering a self-reported behavioural measure, and perceived cooperativeness concerns a behavioural assessment by an external observer.

Since we expect the influence of religious involvement to vary across religious traditions, we conduct analyses pooled and separated by religious tradition. We distinguish the *never affiliated* (who do not belong to a religion in any survey wave), *Anglicans* (affiliated with the Church of England at least once), *Protestants* (any other Protestant affiliation at least once—e.g. Church of Scotland, Methodist, Baptist), *Catholics* (Catholic at least once), and in the UKHLS also *Muslims* and *Hindus*.[4] Online Supplement A shows how religious attendance and the outcome variables vary across these traditions.

Analytical strategy

We fit cross-lagged panel models with individual fixed effects (Allison et al., 2017), as also applied by Son and Wilson (2021).[5] Unlike conventional panel models, which force researchers to choose between controlling for time-invariant confounders (via fixed effects) or reverse causality (via lagged dependent variables), our models address both issues simultaneously. First, by including individual fixed effects that freely correlate with all time-varying independent variables, they control for all time-invariant confounders. Second, by allowing for correlations between time-varying independent variables and past residuals of the dependent variable, they address reverse causality.

Leszczensky and Wolbring (2022) demonstrate that these models are highly effective when one anticipates both time-invariant confounding and reverse causality. Our case fits this scenario, as there are various unobserved factors (e.g., personality, social origins) that may influence both religious involvement and our outcome variables, while our outcome variables may also affect religious involvement (e.g., higher trust facilitating religious involvement). Indeed, the correlations between religious involvement and past residuals of our outcome variables are statistically significant in several of our models, indicating the presence of reverse causality (see online Supplement C).

Another strength of these models is that they can be fitted within the Structural Equation Modelling (SEM) framework, which offers flexibility for model specification and estimation. We exploit this flexibility by applying Full Information Maximum Likelihood (FIML) estimation, which helps to address missing data and panel attrition. FIML produces unbiased estimates under the assumptions of data being missing at random (MAR) and multivariate normality. Although these are demanding assumptions, FIML is robust to violations of the latter assumption (Enders and Bandalos, 2001), and the inclusion of individual fixed effects and earlier measures of the outcome variables should absorb many causes of panel attrition and item nonresponse, lending more credibility to the MAR assumption.

Figures 1 and 2 display path diagrams that illustrate our models for, respectively, the BHPS and UKHLS. In the upper panel of Figure 1, the dependent variable, generalized trust (*tr*), is measured in six waves (subscripts indicate the waves). Trust is regressed on the most recent measure of religious attendance (*at*) and on its own previous measure, capturing state dependence. The first measurement of the outcome (i.e., trust in wave 8) is treated as an exogenous variable. Crucially, religious attendance freely correlates with past values of (residual) trust, reflecting possible reverse causality. Individual fixed effects (*FE*) appear as a latent variable with a unit effect on all measurements of trust, barring the first, capturing the influence of any time-invariant determinants of trust. The fixed effects freely correlate with the independent variables (and the first trust measurement), as in conventional fixed-effects models. Finally, all measurements of religious attendance freely correlate with one another (and the first trust measurement), capturing any state dependence in religious involvement. The remaining panels of Figures 1 and 2 summarize our other models, which are set up similarly (we cannot fit trust models to the UKHLS, which measured trust only once). To facilitate comparisons, we start our BHPS analyses from wave 6, as trust is not measured in the early BHPS waves. Nevertheless, our results for volunteering and cooperativeness remain virtually identical if we include the earlier waves.

Notice that the measurement lags in Figures 1 and 2 occasionally vary. For example, in Figure 1, tr_{10} is regressed on at_9(one-wave gap), while $tr_{13}$is regressed

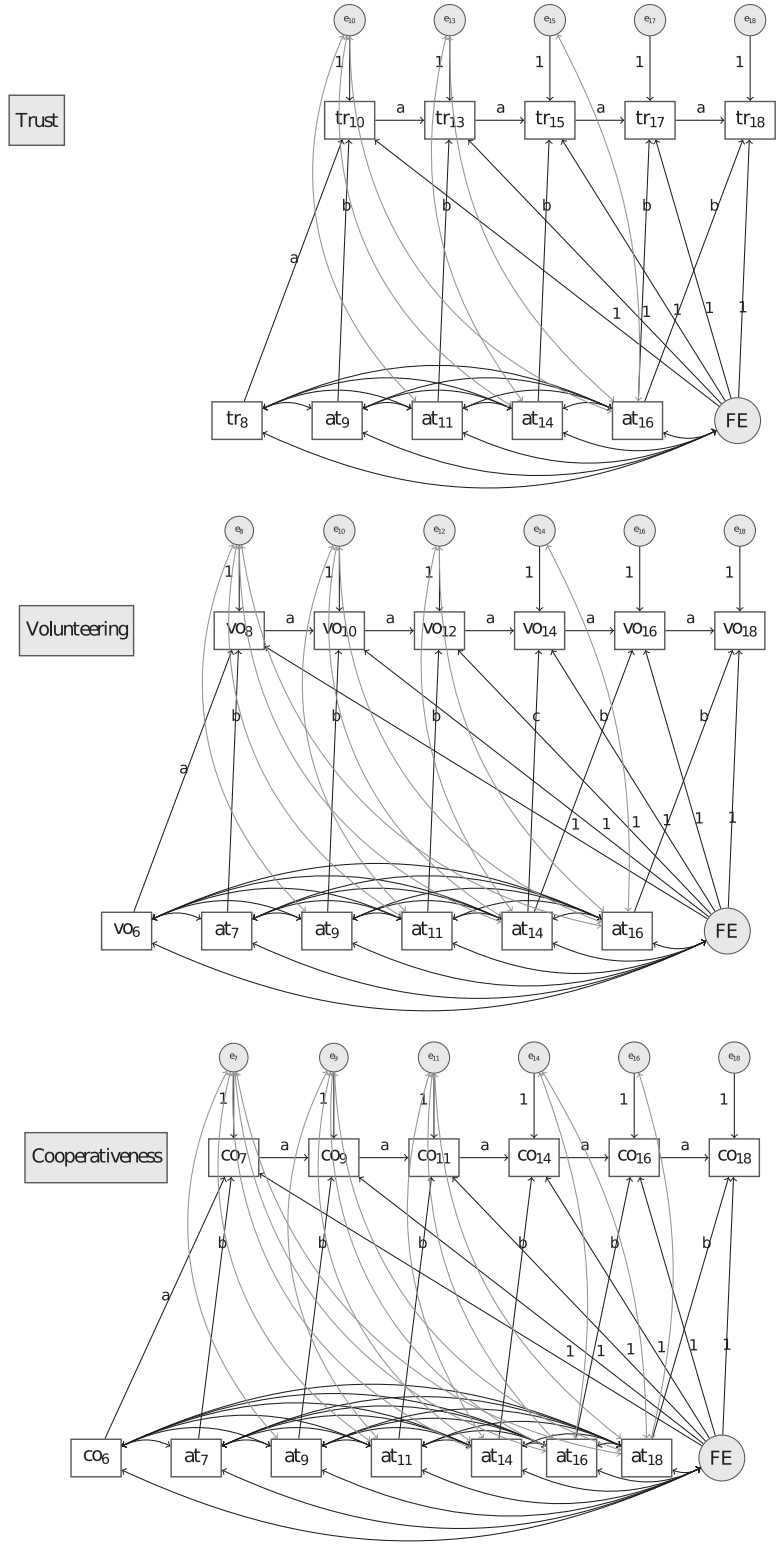

Figure 1 Path diagrams of cross-lagged panel models with individual fixed effects (BHPS)

Note: Models are based on the BHPS data. Dependent variables: generalized trust (upper), volunteering (middle), and perceived cooperativeness (lower). Independent variable: religious attendance. Subscripts denote survey waves. FE stands for fixed effects.

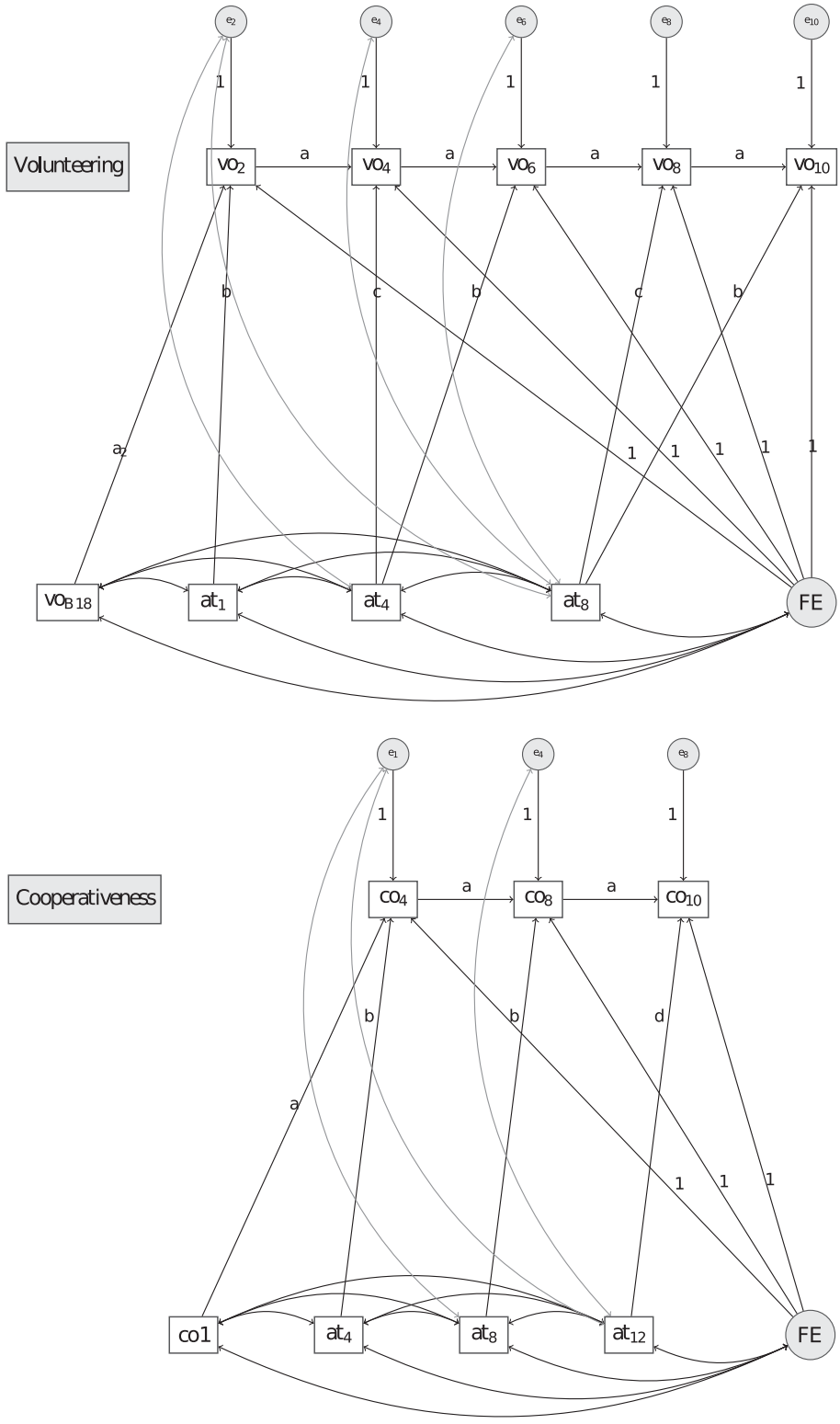

Figure 2 Path diagrams of cross-lagged panel models with individual fixed effects (UKHLS)

Note: Models are based on the UKHLS data. Dependent variables: volunteering (upper) and perceived cooperativeness (lower). Independent variable: religious attendance. Subscripts denote survey waves. FE stands for fixed effects.

on at_{11}(two-wave gap), because religious attendance was not measured in wave 12. Such irregularities pose no problem in SEM, as coefficients can be allowed to differ by the measurement lag. We have tried such specifications, yet they did not alter the key results. Hence, for simplicity, we present models where most coefficients are constrained to be invariant to the measurement lag. One exception is the regression of vo_{14} on at_{14}. Because this represents a contemporaneous effect, we allow it to be different from the lagged effects. Likewise, we estimate two coefficients for the effect of religious attendance on volunteering in the UKHLS, distinguishing contemporaneous and lagged effects. We further note that we regress perceived cooperativeness on religious attendance in the same wave. This is because cooperativeness is measured *after* the interview, while the attendance variable refers to behaviour *before* the interview, creating a natural time gap.[6]

We must also discuss other implicit assumptions of our models. Most of those apply to any model using unit fixed effects. Firstly, our models rely on *within*-person changes in religious involvement and the outcomes. This is how all time-invariant covariates with constant effects are controlled for, but it means that between-person variation in religious involvement, which likely exceeds the within-person variation, is not exploited (Engzell and Hällsten, 2023). Nevertheless, because time-invariant confounding is a major threat in our setting, this is a worthwhile sacrifice. Secondly, our models assume there are no unobserved time-variant confounders. While it is impossible to prove the validity of this assumption, it becomes more plausible the more time-variant confounders we can incorporate. Below we will, therefore, report results obtained after adjusting for several potential confounders—self-reported health, marital status, labour market status, having school-age children, contacts with neighbours and friends, home ownership and house moves—finding that our results remain largely the same. Thirdly, it is assumed that our models specify the correct temporal lag structures. This implies that earlier patterns of religious attendance have no direct effect on prosociality beyond people's most recent attendance (no 'legacy effects'). This seems a defendable assumption considering recent research showing that religious exposure earlier in life has no *direct* effects on volunteering later in life (Son and Wilson, 2021).

Results

BHPS

Table 2 presents our BHPS results; see online Supplement D for the full set of estimates. Most fit measures (i.e. RMSEA, CFI, TLI) indicate a satisfactory fit for all models. Figure 3 visually displays the estimated coefficients for lagged religious attendance.

The pooled analyses show that religious attendance has a significantly positive effect on trust, volunteering, and perceived cooperativeness. Moving from never attending religious services to attending every week is associated with (i) a 9 percentage points increase in the probability of reporting that most people can be trusted, (ii) a 3 percentage points increase in volunteering, and (iii) a 2 percentage points increase in cooperativeness. For reference, the within-individual standard deviations of the outcomes are, respectively, 30, 18, and 10 percentage points. While the estimated effects of religious attendance are thus modest, they are not negligible. This holds especially given that our models control for time-invariant unobservables and reverse causality.

Our analyses by religious tradition generally show the strongest positive effects of religious attendance among Anglicans and other Protestants. For both groups, increases in attendance are associated with significant increases in volunteering and cooperativeness, and for Protestants also in trust. By contrast, most effects of religious attendance among the never affiliated and Catholics are smaller and statistically insignificant.

UKHLS

Table 3 and Figure 4 summarize our UKHLS results, which also include Muslims and Hindus. See again online Supplement D for all estimates. Although we were unable to calculate fit statistics for the volunteering models and although the TLI values in the cooperativeness models suggest room for improvements in model fit, plausibly reflecting the large sample size relative to the degrees of freedom, most fit statistics in Table 3 are reasonable.

The UKHLS results broadly support those based on the BHPS: religious attendance mostly has a positive effect on volunteering and perceived cooperativeness among Christians, but the strength of this effect varies by religious tradition. One difference is that the estimated effect of attendance on volunteering among Catholics is now also significantly positive. In addition, the effect of religious attendance on perceived cooperativeness in the pooled UKHLS sample is close to zero and statistically insignificant. This may reflect that the UKHLS includes more 'religious nones' (43.5 versus 39.4 per cent) and non-Christian minorities (13.9 versus 2.4 per cent) than the BHPS. Generally, however, the effect sizes in the UKHLS analyses are remarkably similar to those obtained from the BHPS. We further observe that the estimated effect of religious attendance on volunteering is close to zero and statistically insignificant for Muslims and Hindus. For perceived

Table 2 Regression results for cross-lagged panel models with individual fixed effects (BHPS)

Outcome: Trust	Pooled	Never affiliated	Anglican	Protestant	Catholic
Trust on					
Trust (lagged)	0.087** (0.006)	0.093** (0.013)	0.088** (0.010)	0.085** (0.014)	0.059** (0.019)
Religious attendance (lagged)	0.079** (0.025)	0.101 (0.053)	0.046 (0.036)	0.141** (0.050)	0.058 (0.064)
Variance (fixed effects)	0.08** (0.002)	0.079** (0.004)	0.085** (0.004)	0.085** (0.004)	0.085** (0.007)
Fit measures					
Chi-sq (df)	193.9 (21)	76.0 (21)	94.7 (21)	59.7 (21)	41.2 (21)
RMSEA (90% CI)	0.016–0.020	0.015–0.024	0.018–0.028	0.013–0.023	0.009–0.023
CFI	0.991	0.990	0.991	0.991	0.992
TLI	0.986	0.983	0.985	0.985	0.986
Number of respondents	25,921	7,193	6,711	5,697	3,718
Outcome: Volunteering	**Pooled**	**Never affiliated**	**Anglican**	**Protestant**	**Catholic**
Volunteering on					
Volunteering (lagged)	0.274** (0.006)	0.243** (0.011)	0.297** (0.009)	0.262** (0.012)	0.269** (0.016)
Religious attendance (lagged)	0.030** (0.009)	0.029 (0.021)	0.061** (0.015)	0.043* (0.018)	0.002 (0.018)
Variance (fixed effects)	0.016** (0.001)	0.009** (0.001)	0.016** (0.001)	0.025** (0.002)	0.015** (0.001)
Fit measures					
Chi-sq (df)	442.4 (31)	83.9 (31)	259.3 (31)	145.5 (31)	120.7 (31)
RMSEA (90% CI)	0.020–0.024	0.011–0.019	0.029–0.036	0.021–0.029	0.022–0.033
CFI	0.987	0.989	0.982	0.987	0.977
TLI	0.978	0.981	0.971	0.978	0.963
Number of respondents	27,038	7,624	7,043	5,811	3,821
Outcome: Cooperativeness	**Pooled**	**Never affiliated**	**Anglican**	**Protestant**	**Catholic**
Cooperativeness on					
Cooperativeness (lagged)	0.279** (0.006)	0.278** (0.013)	0.310** (0.010)	0.276** (0.014)	0.279** (0.018)
Religious attendance (lagged)	0.022** (0.005)	−0.018 (0.013)	0.022** (0.008)	0.035** (0.010)	0.015 (0.011)
Variance (fixed effects)	0.003** (0.000)	0.002** (0.000)	0.003** (0.000)	0.003** (0.000)	0.003** (0.000)
Fit measures					
Chi-sq (df)	926.7 (32)	323.1 (32)	376.1 (32)	227.2 (32)	159.6 (32)
RMSEA (90% CI)	0.030–0.034	0.031–0.038	0.035–0.043	0.028–0.036	0.027–0.037
CFI	0.947	0.919	0.953	0.946	0.942
TLI	0.905	0.855	0.917	0.905	0.896
Number of respondents	26,939	7,705	7,708	5,830	3,839

Notes: See Figure 1 for model set-ups. Data source: BHPS. Non-structural parameters (e.g. correlations between exogenous variables and variances of error terms) are suppressed for brevity. See online Supplement D for the full set of estimated parameters. $^{**}P < 0.01$, $^{*}P < 0.05$.

cooperativeness, both minorities have a *negative* coefficient for religious attendance, which is statistically significant for Muslims, with an effect size comparable to the positive effect of attendance among Protestants.

Overall, most results suggest a positive impact of religious attendance on trust, volunteering, and cooperativeness. Yet, the importance of religious traditions is also underscored: while more frequent attendance is associated with increased trust and prosociality among some religious groups, the association is much weaker, non-existent, or even negative for others.

Robustness checks and additional analyses

We have conducted numerous robustness checks, with results broadly in line with those reported above. Most importantly, we have repeated our analyses while

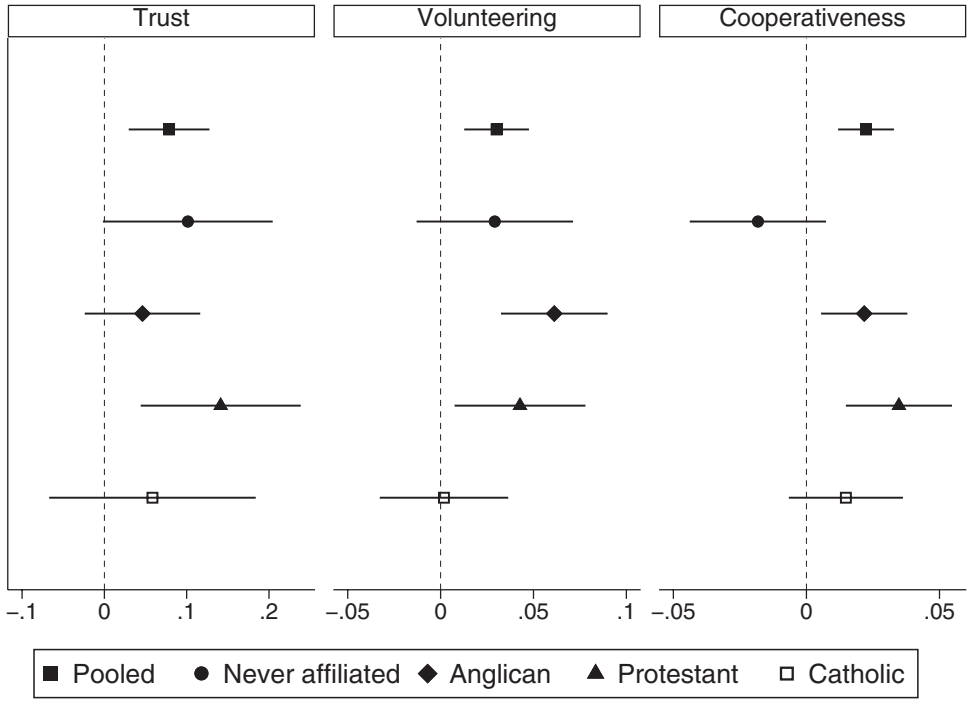

Figure 3 Estimated effects of religious attendance on trust, volunteering, and cooperativeness (BHPS)

Note: Estimates are based on cross-lagged panel models with individual fixed effects applied to the BHPS data (see Figure 1 and Table 2). Error bars represent 95 per cent confidence intervals. All variables have been normalized to the [0,1] range.

controlling for potential time-varying confounders: self-reported health, labour market status, marital status, having children aged 3–15, home ownership, having moved since the last wave, and frequency of contact with friends and neighbours (online Supplement E). Moreover, to verify that our measure of cooperativeness does not capture language problems or disruptive behaviour of other household members, we have added controls for interviewers' perceptions of respondents' understanding of the survey questions and for whether there were other people present who (negatively) influenced the interview (online Supplement F).

We have also extended our analyses in several ways. Firstly, our theoretical arguments concern the effect of religious involvement on *generalized* cooperation and trust. If our outcome variables rather capture trust and prosociality towards co-religionists or other ingroups, our results would be less relevant as a test of the theory. We have therefore done the following (details in online Supplement G): (i) Taking into account the religious composition of respondents' neighbourhoods (if religious attendance primarily affects particularized prosociality, we would see weaker effects in neighbourhoods with fewer co-religionists); (ii) Distinguishing civic activities for religious versus non-religious organizations (if we only find effects of religious attendance on involvement in religious organizations, this may reflect ingroup bias); (iii) Accounting for the ethnicity of the interviewer (if the effects of religious attendance on perceived cooperativeness reflect ingroup biases of the interviewer or religiously active respondents, then the strength of the effect of religious attendance should vary by the ethnicity of the interviewer).

Regarding (i), we have used Census information to classify Lower-Layer Super Output Areas (LSOAs; each comprising 1,000–3,000 residents) into Christian-denominated neighbourhoods, neighbourhoods with strong representations of non-Christian religions, and relatively non-religious areas. We find that the effects of religious attendance do not vary much across these neighbourhood types. Regarding (ii), we find that religious attendance is primarily linked to involvement in religious organizations, but that it also boosts participation in non-religious organizations among Anglicans and other Protestants. Regarding (iii), we have firstly controlled for the ethnicity of the interviewer, using information available in the UKHLS. This makes no difference for the effect of religious attendance on cooperativeness. Secondly, we have conducted analyses that only include respondents who are exclusively interviewed by ethnic minority interviewers. Compared to the results reported above, these analyses reveal more *positive* effects of attendance on perceived cooperativeness among Anglicans and other Protestants

Table 3 Regression results for cross-lagged panel models with individual fixed effects (UKHLS)

Outcome: Volunteering	Pooled	Never affiliated	Anglican	Protestant	Catholic	Muslim	Hindu
Volunteering on							
Volunteering (lagged)	0.252** (0.004)	0.243** (0.007)	0.302** (0.007)	0.245** (0.011)	0.244** (0.012)	0.204** (0.017)	0.203** (0.027)
Religious attendance (lagged)	0.071** (0.013)	0.039 (0.030)	0.056** (0.021)	0.080** (0.035)	0.070* (0.034)	−0.002 (0.025)	−0.001 (0.057)
Variance (fixed effects)	0.018** (0.000)	0.015** (0.001)	0.020** (0.001)	0.026** (0.002)	0.016** (0.001)	0.010** (0.001)	0.014** (0.002)
Number of respondents	80,486	28,605	18,921	8,502	9,086	7,381	2,618
Outcome: Cooperativeness	**Pooled**	**Never affiliated**	**Anglican**	**Protestant**	**Catholic**	**Muslim**	**Hindu**
Cooperativeness on							
Cooperativeness (lagged)	0.153** (0.009)	0.181** (0.016)	0.151** (0.014)	0.215** (0.024)	0.225** (0.023)	0.086** (0.031)	0.108+ (0.058)
Religious attendance (lagged)	0.014 (0.014)	−0.036 (0.033)	0.022 (0.023)	0.054+ (0.032)	−0.009 (0.031)	−0.064* (0.031)	−0.015 (0.058)
Variance (fixed effects)	0.002** (0.000)	0.000 (0.000)	0.001 (0.001)	0.002* (0.001)	0.001 (0.001)	0.008** (0.002)	0.006* (0.003)
Fit measures							
Chi-sq (df)	276.9 (4)	52.0 (4)	36.3 (4)	75.6 (4)	69.2 (4)	28.5 (4)	16.4 (4)
RMSEA (90% CI)	0.027–0.033	0.016–0.026	0.015–0.028	0.040–0.049	0.035–0.053	0.020–0.040	0.018–0.053
CFI	0.938	0.949	0.964	0.883	0.885	0.933	0.901
TLI	0.766	0.810	0.865	0.561	0.568	0.747	0.627
Number of respondents	74,064	27,653	17,613	7,470	8,446	7,154	2,552

Notes: See Figure 2 for the set-up of the models. Data source: UKHLS. Non-structural parameters (e.g. correlations between exogenous variables and variances of error terms) are suppressed for brevity. See online Supplement D for the full set of estimated parameters. $^{**}P < 0.01$, $^{*}P < 0.05$, $^{+}P < 0.1$. Fit measures for volunteering are not reported by the software, for the software was unable to empirically identify the reference model to which our model is compared (the reference model is a saturated model with zero degrees of freedom, whereby all variables are correlated with each other).

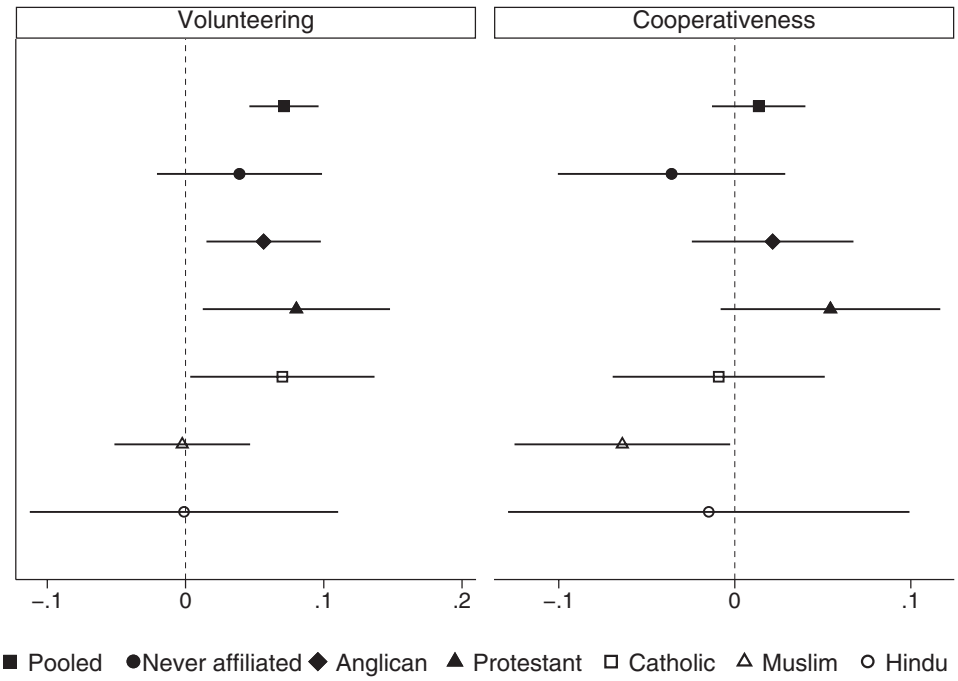

Figure 4 Estimated effects of religious attendance on volunteering and cooperativeness (UKHLS)

Note: Estimates are based on cross-lagged panel models with individual fixed effects applied to the UKHLS data (see Figure 2 and Table 3). Error bars represent 95 per cent confidence intervals. All variables have been normalized to the [0,1] range.

and more *negative* effects among Catholics, Muslims, and Hindus. These patterns contradict the argument that religious attendance only affects cooperativeness towards co-religionists. Overall, our main results thus do not seem driven by religious involvement merely promoting prosociality towards co-religionists.

Secondly, religious attendance may have different meanings for different groups. For example, while weekly attendance is obligatory for Muslim men, it is not for Muslim women. More generally, the link between service attendance and 'inner' religiosity may vary across groups (Aksoy and Gambetta, 2021). We have therefore studied the correlations between people's subjective importance of religion, their frequency of prayer outside religious services, and religious attendance (see online Supplement H). Large differences across religious groups in these correlations would suggest a lack of measurement invariance. However, despite finding some differences, these could not explain our main results. Indeed, since the correlations are very similar among Anglicans, other Protestants, and Catholics, measurement invariance cannot explain the smaller effects of religious attendance for Catholics. Moreover, although the correlations between subjective importance of religion, frequency of private prayer, and service attendance are weaker among Muslim women, among Muslim men they are similar as for Anglicans, other Protestants, and Catholics. Yet, the negative effect of attendance on cooperativeness is most pronounced among Muslim men. So, measurement variance is not a likely explanation for the negative effect of attendance on cooperativeness among Muslims. We have also explored the influence of *subjective importance of religion* and *religious affiliation* on our outcomes (see again online Supplement H). We find that both indicators generally have positive effects, although their effects are smaller than for religious attendance. Moreover, they attenuate when religious attendance is controlled for, whereas the estimated effects of religious attendance remain similar as before.

Thirdly, to rule out that the differences between religious groups reported above reflect differences in their ethnic makeup, we have repeated our analyses for various ethnic subsamples (online Supplement I). For the BHPS, we have re-run our analyses using White British respondents only. For the UKHLS, we have focused on Catholics and Muslims (ethnically the most diverse groups) and distinguished between White British, Other White, and ethnic minority Catholics, and between Pakistani, Other Asian, and Black Muslims. These analyses provide little indication that our findings are driven by the ethnic makeup of religious groups. Similarly, we find little evidence that

differences in migration experiences cause the reported differences across religious traditions.

Discussion and conclusion

This study advances our understanding of how religious involvement affects cooperation. Given the conflicting predictions and findings in past research, our contribution is threefold. First, while most earlier studies rely on cross-sectional surveys or small-scale experiments, our analysis is based on large-scale, representative and recently collected panel data and a novel method that addresses time-invariant confounding and reverse causality. Second, we examine three complementary outcomes, which helps to assess the scope of the effects of religious involvement. Third, we investigate how the effects of religious involvement vary across religious traditions.

We find that, on average, frequent service attendance increases generalized trust, volunteering, and perceived cooperativeness. The impact of religious attendance thus seems reasonably consistent across different outcomes. This is a noteworthy finding, as few studies to date have considered multiple prosocial or cooperative outcomes simultaneously. Our positive estimates for the effects of religious attendance on trust and cooperativeness are particularly important, because earlier work looking at these outcomes offered conflicting evidence (e.g. Anderson et al., 2010; Berggren and Bjørnskov, 2011; Traunmüller 2011). Our study thus suggests that the positive effects of religious attendance may extend to a broader range of outcomes than previously thought.

Our findings are based on a stringent panel design and hold up in various robustness checks. Hence, for most groups, the positive effects of religious involvement—through the norms fostered within religious communities and the networks built through joint worship—seem to outweigh any negative effects that might arise from moral licensing, from religious adherents focusing on God at the expense of worldly matters, or from the religious reserving their kindness exclusively for co-religionists. Indeed, our additional analyses concerning the religious composition of respondents' neighbourhoods, the distinction between religious and non-religious volunteering, and interviewers' ethnicity suggest that religious attendance fosters not only particularized but also generalized trust and prosociality. Moreover, by showing stronger effects of religious attendance than of subjective importance of religion or being religiously affiliated, our analyses support earlier studies arguing that the effects of religious involvement on prosociality and cooperation mainly operate through social channels, and that religious beliefs and belonging *per se* may not be as consequential (e.g. Putnam and Campbell, 2010; Lim and MacGregor, 2012).

That said, the reported effects of religious attendance are modest. This probably reflects our study design, which only considers within-individual variation in religious attendance, whereas the corresponding between-individual effects are likely larger and more consequential for aggregate social cohesion. Furthermore, it is important to stress that the positive effects of religious attendance do not apply equally strongly across all religious traditions. As we expected, we find clear evidence that religious attendance promotes generalized trust and prosociality among Anglicans and other Protestants, but there are weaker effects among Catholics, non-Christian minorities, and the religiously unaffiliated. Accordingly, it is not a matter of 'any involvement goes': the community one is involved in matters, too.

Here, our findings for minority faiths are noteworthy. Especially the patterns for Muslims stand out, with no effect of religious attendance on volunteering and a significantly negative effect on perceived cooperativeness. There could be several explanations for these effects. The null effect on volunteering might, for example, reflect that Muslims express their prosociality differently, with religious attendance boosting informal care for needy relatives instead of volunteering for welfare organizations. The negative effect on cooperativeness, in turn, might be interpreted as support for moral licensing theory or the notion that religious involvement increases particularized cooperativeness at the expense of generalized cooperativeness. Indeed, this finding is in line with a study of friendship choices among German adolescents (Leszczensky and Pink, 2017), which documents stronger ingroup preferences among Muslims the more religious they are. Alternatively, the negative effect of religious attendance on perceived cooperativeness could signify mechanisms linked to Muslims' minority status, such as discrimination or acculturation stress, which may be linked to more frequent religious attendance. Future research could test such mechanisms by including measures of exposure to discrimination or acculturation stress. In any case, our additional analyses indicate that the differences we observe between religious groups are not solely the result of variation in their social makeup (e.g., ethnicity, migration history) or differential attendance norms.

Overall, while our study reveals important differences in the effects of religious attendance between religious traditions, more work remains to be done to ascertain what is driving these differences. One way forward would be to consider even more outcome measures. Future research could contrast the effects

of religious attendance on generalized versus particularized outcomes (e.g., ingroup cooperativeness), the latter of which were beyond the scope of this study, while also considering behavioural measures of trust (Ermisch et al., 2009).

It further remains an open question whether our findings generalize to other places. As some argue, the links between religious involvement and prosocial outcomes may depend on national religious contexts (Stavrova and Siegers, 2014; Bennett and Einolf, 2017), with an influential theory stating that individual religiosity will matter less in more devout environments (Ruiter and De Graaf, 2006). Because the United Kingdom is nowadays a relatively secular country, one might thus expect weaker effects of religious involvement on generalized trust, volunteering, and cooperativeness in more religious countries. Research on non-Western countries with Muslim or Hindu majorities would be particularly interesting. One may well find different effects of religious attendance there, if only because discrimination towards Muslims or Hindus would be weaker or absent.

It is finally relevant to relate our findings to ongoing religious trends in many Western societies. Most notably, the religiously unaffiliated population has grown substantially, alongside a drop in religious attendance and other religious practices, and a weakening of religious beliefs (Bruce 2016). Our findings provide insights into the potential wider consequences of these trends. For example, given the strong links we find between religious attendance and volunteering, declining religiosity may well threaten the voluntary sector. Moreover, the links between religious attendance and prosocial outcomes may evolve as religious changes unfold. One possibility is that these links become stronger, as only the most committed believers stay religiously involved. Yet, increased polarization between the unaffiliated and the religiously committed may also imply that religiosity becomes increasingly associated with particularized rather than generalized forms of trust and prosociality. Furthermore, secular alternatives to religious attendance (e.g. the Sunday Assembly) may develop that compensate for declining religiosity and weaken the links between religious attendance and prosocial outcomes. Examining how such dynamics play out represents another worthwhile avenue for future research.

Notes

1. Delhey, Newton and Welzel (2011) show that responses to this question correlate more strongly with outgroup trust (people one meets for the first time or with another religion or nationality) than ingroup trust (trust in family, neighbourhood, people one knows personally).
2. The BHPS allows 'it depends' responses to this question, but only in wave 18 are respondents offered this as an explicit option. Our results are robust to excluding wave 18 from our analyses. 'Don't know' responses, accounting for less than 1 per cent of all responses, are treated as missing.
3. Whereas this variable may measure both generalized and particularized forms of volunteering, the next section will present additional analyses which attempt to filter out volunteering for religious organizations as the most obvious form of ingroup volunteering.
4. Because people sometimes move between religions, a small minority appears in multiple of our subsamples. Our pooled analyses comprise people from all traditions, including smaller ones that we do not single out.
5. Stata code to replicate our analyses is available at https://github.com/dingemanwiertz/BHPS_UKHLS_religion.
6. Two issues need clarification for the UKHLS models. First, in the volunteering model, the first outcome measure (vo_{B18}) corresponds to the last BHPS wave, for the UKHLS did not measure volunteering before its first measurement of religious attendance. Removing vo_{B18} from the model does not change the results qualitatively but affects the coefficient sizes. Second, the UKHLS did not measure cooperativeness in wave 12, which was conducted online during the pandemic. To identify the model for all subgroups for cooperativeness, we however need three endogenous outcomes. Hence, we included a path from att_{12} to co_{10}, which is the last cooperativeness measurement pre-pandemic. Because this represents a retrospective path, we estimate it separately, yet we remain mainly interested in the earlier paths.

Supplementary data

Supplementary data are available at *ESR* online.

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