

Water the Flowers You Want to Grow? Evidence on Private Recognition and Donor Loyalty

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Abstract

We study donor loyalty in the context of church membership in Germany. Church members have to make substantial payments to their church but can opt out at any time. In a large-scale field experiment, we examine how private recognition for past payments affects church members' loyalty. We find that recognizing past payments reduces opt-outs, but the response is temporary. The effect of recognition on retention is most pronounced among members in the bottom quartile of baseline payments to the church. Consistent with optimization frictions prior to the experiment, we observe a spike in opt-outs after treatment for particularly costly memberships.

JEL codes: D64; C93

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1 Introduction

It is widely documented that the loyalty of recurring donors is of major importance for charitable organizations, such as religious and educational organizations, hospitals, and blood donor organizations (Bagot *et al.*, 2016; Chuan *et al.*, 2018; Council for Advancement and Support of Education, 2019; Notarantonio and Quigley, 2009). While retaining the loyalty of recurring donors is empirically important, it is a widely understudied objective of charitable organizations. Most existing papers on charitable giving (reviewed in Andreoni and Payne, 2013) focus on one-time donations. Such one-time donations are arguably very important to charities, and the literature has led to significant progress in understanding the optimal design of donation asks. However, it is unclear whether one-time donations and recurring donations respond similarly to charities' fundraising efforts (Sargeant, 2008), and studying one-time donations necessarily misses out many important aspects related to time (Chuan *et al.*, 2018; Rooney *et al.*, 2019). In particular, the literature on one-time donations cannot address the question of how to maintain donor loyalty.

This paper focuses on recurring donors. It provides field-experimental evidence on how private recognition increases the loyalty of recurring donors towards a charity. Conceptually, in our setting, private recognition takes the form of sending out thank-you letters in order to boost the warm glow recurring donors experience when donating to the charity.¹ Studies by psychologists suggest that such private recognition reinforces benevolent behavior (McCullough *et al.*, 2001) by making individuals feel socially valued (Grant and Gino, 2010).

The context of our study is charitable giving to religious organizations, which in the US and many other countries are the largest recipients of charitable donations (Andreoni and Payne, 2013). In this context, the warm glow experienced by recurring donors can take many forms, including the notion that one “is doing the right thing.” Specifically, we study Protestant Church membership in Germany as a form of giving to a religious charity. Three key features of this context are relevant for our empirical analysis. First, church membership in Germany involves regular payments by the

¹In a study on new donors, Samek and Longfield (2019) find no effect of additional thank-you calls on the probability of recurring, possibly because of the co-existence of thank-you letters.

member to the church and is therefore akin to a recurring donor scheme. Church members pay an income-dependent contribution to the church, which is labeled “church tax.” In 2018, the Protestant Church in Germany raised an average church tax of € 270 per member ([Statistisches Bundesamt, 2019](#)). Second, as in other recurring donor schemes, church members can terminate their involvement with the church at any time. After opting out, individuals no longer have to pay, but can still benefit from many church services (see Section 2 for details). Third, for decades, church membership in Germany was very stable, with very low opt-out rates. In recent years, the annual rate of opt-outs from the Protestant Church (as from other churches) has steadily increased, reaching one percent in 2018 ([Statistisches Bundesamt, 2019](#)). As church finances heavily rely on church tax revenues, the loss in tax-paying members poses a serious long-term threat to the church – as the loss of recurring donors would for many non-profit organizations.

To implement our randomized field experiment, we teamed up with the Protestant Church in the federal state of Bavaria. In collaboration with the church, we varied the recognition that church members experience for their payment by a letter treatment. In February 2015, roughly 200,000 church members were randomly assigned to a control group or a private recognition-letter group. In the private recognition letter, the head of the church tax office thanked church members for their past payments and acknowledged these payments as “an important contribution to our community.” Church members in the control group did not receive a letter. About two weeks after the mailing of treatment letters, the church contacted a subsample of 4,000 church members from both the treatment and control groups with a postal survey. The survey questionnaire was designed to measure how church members perceived the recognition of their payments by the church.

Our analysis of retention rates benefits from rich administrative data on Protestant church members. We combine individual data on church membership with church tax records. This allows us to link the church members’ individual opt-out decisions to a number of individual characteristics, including past payments to the church.

Our results are as follows. First, based on the survey data we demonstrate that the treatment letter successfully communicated recognition of payments made. Treated

survey respondents feel more recognized and hold more positive views on making payments to the church. Second, in the field experiment, we find that a one-time private recognition intervention leads to a significant (but temporary) reduction in the opt-out rate among church members. The cumulative effect on the opt-out rate builds up over time and becomes statistically significant seven months after treatment, when opt-outs are reduced by 9.5 percent relative to the control group (p -value 0.050). The highest impact on opt-outs is reached nine months after treatment (minus 9.7 percent, p -value 0.022). Beginning from month ten after treatment, the treatment effect tapers off over time. Third, a heterogeneity analysis shows a monotonic relation between the cost of membership and the reduction in opt-outs, with low-paying church members responding most strongly. Fourth, we document a sharp positive spike in opt-outs in the first month after treatment among church members whose individual cost of membership is in the top quartile. Our preferred interpretation is that the monotonic relation between the cost of membership and the reduction in opt outs arises from the fact that the extent of recognition expressed in the treatment letter is identical for high- and low-income earners, while the cost of the church membership increases with income. Thus, the extent of recognition received relative to the cost of membership decreases with income. In addition, the letter might also work as a reminder that the membership is costly, and that the cost can be avoided by opting out. While such a reminder effect could be of general importance, it seems natural that it is most pronounced among church members with particularly high incomes who, therefore, make high church tax payments.²

Our paper contributes to several important strands of the literature. It advances the established literature on charitable giving (reviewed in [Andreoni and Payne, 2013](#)). Although there are some papers on repeated donations (e.g., [Meier, 2007](#)) or field experiments based on samples from previous donors (e.g., [Karlan and List, 2007](#)), the charitable giving literature has rarely focused on recurring donors, with few exceptions. [Anik et al. \(2014\)](#) observe that recurring donors are beneficial for non-profit

²The main heterogeneity analyses in this paper are based on differences in the individual cost of church membership. In our context, this cost is a (non-linear) function of income. For couples, the church tax liability of one spouse can even depend on the other spouse's membership in a (different) church. We therefore prefer the interpretation of individual church tax liabilities indicating the cost of membership rather than referring to the underlying heterogeneity in terms of taxable income.

organizations and explore the effectiveness of contingent matching incentives in turning one-time donors into recurring donors. They find that individuals are indeed more likely to upgrade to a recurring donation when the non-profit organization offers to match the donation if – and only if – 75 percent of donors agree to the same upgrade. Our paper is distinct in focusing on loyalty of (existing) recurring donors. In a series of experiments, [Gilad and Levontin \(2017\)](#) asked individuals to hypothetically donate to a specific non-profit organization. In an impact condition, participants had to imagine that the nonprofit organization informed them on the impact of their previous donation. These participants report higher satisfaction from their hypothetical donation and in turn larger subsequent hypothetical donations, suggesting that recurring donation decisions are fundamentally different than one-time donation decisions. The individuals surveyed by [Gilad and Levontin \(2017\)](#) (hypothetically) donated only once prior to deciding whether to become a recurring donor, and they take fictitious donation decisions. The individuals in our experiment have been recurring donors for several years, and their decision whether to remain loyal or not entails significant economic costs. [Bremen \(2011\)](#) carries out two natural field experiments to test inter-temporal decisions in charitable giving. She shows that charities can increase donations by allowing monthly donors to commit to future giving. In particular, monthly donors are asked to raise their donation in one month's time or in two months' time and this commitment mechanism raises the average increase in monthly donations. This two-month lag generates an economically significant treatment effect; 32 percent in the first field experiment and 11 percent in the second field experiment. A follow-up study reveals that the treatment effect is persistent and that in the long run contributions do not converge. Our study focuses on improving the loyalty of recurring donors, complementing research on contribution schemes.

By exploring the loyalty of donors, we add to the literature on the temporal nature of altruistic decisions (e.g., [Rand *et al.*, 2012](#)), on the sources of time inconsistencies in charitable giving ([Andreoni and Serra-Garcia, 2019](#)) and on the impact of pledges on donations (with mixed evidence, see [Lacetera *et al.*, 2016](#) and [Fosgaard and Soetevent, 2018](#) as well as [Andreoni and Serra-Garcia, 2019b](#)). While future demands for payment were shown to decrease initial giving ([Adena and Huck, 2019](#)),

previous donors are more likely to give (Levin *et al.*, 2016), in particular if they were initially attracted by an economic mechanism (Landry *et al.*, 2010). Also related is work on how fundraising activities shift donations over time (Scharf *et al.*, 2017).

The paper also contributes to a broader literature on the effects of recognition. This literature has studied public recognition in the context of work (Kosfeld and Necker-mann, 2011), tax compliance (Dwenger *et al.*, 2016; Slemrod *et al.*, forthcoming), political donations (Perez-Truglia and Cruces, 2017), and pro-social behavior (Ashraf *et al.*, 2014; Chetty *et al.*, 2014). In contrast, there is little causal evidence on private recognition. An exception is Samek and Longfield (2019), who find that thank-you calls do not affect the probability of new donors to make further donations to a charity. In contrast to Samek and Longfield (2019), we focus on long-term donors.

The remainder of our study proceeds as follows. The next section provides the institutional background. Section 3 describes our field experimental design and data, followed by a summary of our findings in Section 4, and the conclusion in Section 5.

2 Institutional Background

Germany has a state church tax.³ Anyone who was ever baptized and has not opted out of her church membership is considered a church member. In Bavaria, all church members are liable to pay an additional 8 percent of their annual income tax to the church. The church collects the tax using income tax records provided by the state tax authorities. For employees, the church tax is automatically deducted on a monthly basis just like payroll taxes or social insurance. The self-employed make advance payments on the church tax, either on a monthly or quarterly basis, depending on their income. These advance payments are treated as a prepayment of the tax and credited against the actual tax liability assessed when the tax year has ended.⁴ The church tax is the single most important source of revenue for the churches in Germany. In 2018, it raised € 5.8B (€ 6.6B) for the Protestant (Catholic) church (Statistisches Bundesamt, 2019).

³Similar taxes exist in Austria, Denmark, Finland, Iceland, and Sweden.

⁴In Bavaria, church members are also liable for a much lower local tax, see Dwenger *et al.* (2016).

Individuals can avoid paying the church tax by leaving the church: No further payments are owed when church members formally renounce their membership with an official declaration made in person at a district court. Non-members can still benefit from many church services: They can attend Sunday services, send their children to a church kindergarten or church school, and have family members cared for in a church nursing home.⁵ In recent years, opting out of church membership has become much more common in Germany. In 2014, the year prior to our experiment, more than 270.000 (217.000) individuals opted out of the Protestant (Catholic) church. As a result, in the decade prior to our experiment, the share of the population who were members of the Protestant church declined by about one-quarter, from 34.6 percent in 1994 to 27.9 percent in 2014 (including children and other non-income-tax-paying individuals). The trend is even more pronounced among church tax payers, posing a serious long-term threat to church finances.⁶

While the institutional setting of the Protestant Church in Germany allows us to run a large-scale field experiment on charity loyalty, some features of church membership distinguish our setting from other recurring donor systems. First, the church offers a few private goods (church weddings, becoming godparents) that are only available for church members.⁷ This could raise loyalty with the church relative to other contexts. However, it is not uncommon for charities to offer private goods to recurring donors. For instance, in the United States, many parents are asked to donate regularly to their child's school, to benefit their child's education. Many non-profit organizations maintain "circles of friends" to whom they offer special treatment such as participating in special events etc. Second, conditional on membership, payments to the church take the form of non-voluntary tax payments. While this feature and the labeling of the payments as a "tax" may seem unusual, we would like to reiterate that church

⁵Non-members may act as witnesses at baptisms, a role that may, to observers, appear undistinguishable from that of the godparents. Couples have access to marriage service or blessing if a least one partner is a member.

⁶While the church tax is certainly the main reason for disaffiliation from the church, other reasons for the increasing number of opt-outs in recent years in the German context include ideological grounds (Berghammer *et al.*, 2017) or rising opportunity costs of religious behavior due to secular competition as found in Gruber and Hungerman (2008).

⁷The social services offered by the church are available to everyone, irrespective of whether they are church members or not. Insurance motives as documented in Auriol *et al.* (2020) thus do not play a role in our context.

members can terminate their membership at any time, making the payments similar to pre-specified payment plans in a recurring donor scheme. In this sense, church membership works like a donation default, where excessive default amounts can lead to a negative extensive margin response (Altmann *et al.*, 2019). Third, payments to the church are income dependent, leading to a rising individual cost of membership for members whose income increases over time. Again, similar features are not uncommon in other contexts involving recurring payments to charitable organizations. For instance, many non-profit organizations, including learned societies and university alumni associations, offer junior members a scheme with a step-wise increase in recurring payments over time. Finally, our institutional setting may differ from other settings in terms of the transaction costs associated with opting out. High transaction costs of leaving the church may result from the fact that most church members become members by being baptized as newborns. Such an automatic enrollment (which does not include a conscious opt-in) may cause inertia and generally reduce the incidence of opt-out decisions. In addition, the joint collection of the church tax together with the income tax might increase the legitimacy of the payment, resulting in higher opt-out transaction costs than the simple termination of a permanent donation standing order. Likewise, social pressure can lead to rising transaction costs of an opt-out. While these points distinguish the church setting from other charitable giving contexts, it is important to note that high transaction costs in general are common in other charitable giving contexts as well (Huck and Rasul, 2010), as are more specific costs such as social pressure (DellaVigna *et al.*, 2012). All in all, while the specific features of the church context limit the transferability of the quantitative effect size, we still think that our paper offers important general insights into private recognition and donor loyalty that are relevant beyond the specific context.

3 Experimental Design, Data and Postal Survey

3.1 Experimental Design

In collaboration with the Protestant church in Bavaria, we designed a randomized field experiment to study how private recognition affects church members' loyalty towards the church. In the experiment, we implemented a *private recognition treatment*. In a letter sent to a random subsample of church members, the head of the church tax office thanked the letter recipients for their church tax payments and acknowledged the payments as “an important contribution to our community” (see Figure ?? in the Online Appendix for a display of the treatment letter). Church members in the control group did not receive a letter.

According to surveys conducted by the Protestant church, the majority of people who terminated their church membership did so to avoid paying the church tax. Our experiment therefore focuses on church members at working age (aged 18–65) and earning income liable to the church tax at baseline. Germany has a system allowing for the (optional) joint income tax filing of couples.⁸ Therefore, the unit of treatment in our experiment is the Protestant church tax unit, consisting of either a Protestant single filer, or a Protestant spouse in a jointly filing couple where the partner is not a member of the Protestant church, or a jointly filing couple where both spouses are members of the Protestant church. Couples where both spouses were Protestants received only one letter.

The sample for the field experiment consists of 198,036 tax units with 239,442 individual church members. The church asked us to exclude tax payers with taxable income above 250,000 euro from the experiment. The overall sample size of the field experiment was derived from power calculations with a minimum detectable effect of 10 percent, an opt-out rate of 1.5 percent (over a 12-month period) in the control group, a 5 percent level of statistical significance, and power of 80 percent. Half of the tax units in the experiment were assigned to the treatment group ($N = 119,613$), and

⁸For couples with two Protestant spouses, the Protestant church tax equals an additional 8 percent of the couple's personal income tax. In couples with one Protestant spouse only, the Protestant church tax corresponds to 8 percent of the couple's personal income tax times the Protestant's share of taxable household income.

the remaining half to the control group ($N = 119,829$). Treatment assignment was stratified, where the strata were defined by taxable income (below/above median), church members' age (below/above 35 years), and urbanization at place of living (rural, semi-urban, urban).⁹ The letters were sent out end of February 2015.

The trial covered in this paper was part of a bigger initiative by the Protestant church in Bavaria to improve the retention of tax-paying church members. As part of this initiative, the church also designed and sent out a longer letter that expressed private recognition for church tax payments and informed church members on how the church tax is spent. The random sample of church members to receive this letter was determined according to our sampling scheme for the private recognition letter. In Section 4.4, we comment on the effects of the longer letter. A letter identical to our private recognition letter was sent out a year later (February 2016).

3.2 Data

We link data from two administrative data sources: records documenting all decisions by church members to opt out of their membership, and state income tax records. We consider opt-outs in the 12 months following the mailing of the treatment letters (March 2015 to February 2016) and link those records to the income tax records for the years 2013 and 2014.

After the mailing of the treatment letters, the church invited some of the individuals in the experiment to participate in a survey (see the following subsection for details). We exclude from the evaluation of the field experiment all church members who were invited to take part in the survey ($N = 3,965$ tax units with 4,767 church members). We also exclude recipients who changed from joint to single filing (or vice versa) within twelve months of mailing the treatment letters ($N = 1,025$ tax units with 1136 church members), as changes in the filing type are often associated with events like marriages, divorces or the death of a spouse that are known to trigger church opt-outs (or opt-ins). These exclusions leave us with 233,539 sampled individuals.

⁹Based on the three stratification variables, we defined $2 \times 2 \times 3 = 12$ bins. All bins that featured annual opt-out rates of 1.3 or larger prior to the experiment were fully sampled; the sampling rate for the remaining bins was 56.2 percent.

The sampling for the field experiment was done shortly before the mailing of the letters. It was based on the church tax records for 2013 because in Germany, personal income tax filing and assessment usually happens with a time lag of between 15 and 24 months. As a result, at the time of sampling for the experiment, income records for 2014 were not yet available and we were unable to condition the sampling on actual church tax payments for 2014. In the fall of 2016, we went back to the church tax records and added the tax data for 2014 that had become available in the meantime to our database. For two reasons, tax data for 2014 was available only for a subset of 200,784 church members: Some members of the sample fell below the tax exemption threshold, others did not file a tax return for 2014 until the fall of 2016. As many low- to moderate income earners (including most retired persons) do not have to file a tax return, we are more likely to obtain tax records for individuals with higher incomes.¹⁰ A possible concern might be that the sample of the experiment comprises some church members who were not liable for the church tax in the year before the experiment and, as a consequence, did not make any payment to the church. If assigned to the treatment group, those members may consider the private recognition letter inappropriate. Therefore, this paper focuses on the sample of 200,784 individual church members for whom we observe taxable income and tax payments in 2013 and 2014. We refer to this sample as the estimation sample and discuss the robustness of our findings regarding the sample definition in Section 4.

Online Appendix Table ??, Panel A presents evidence on sample characteristics and balance across treatment and control groups for the estimation sample. In our sample, the average annual taxable income in baseline year 2013 was about € 48,900, resulting in an average annual payment for church membership of € 478. The average age of individuals in the experiment was 45 years. As noted above, the likelihood of tax information 2014 being available increases with the level of income, producing a larger share of individuals in the third and fourth quartile of the income distribution in our estimation sample. The table shows that the treatment and control groups are well balanced in observable characteristics.¹¹

¹⁰For church members who were in the bottom quartile of taxable income in 2013, we obtained tax records for 2014 in 81.8 percent of the cases. In the top quartile, this share is 89.4 percent.

¹¹The differences between treatment and control in terms of the respective sample shares belonging

3.3 Postal Survey

About two weeks after mailing the treatment letters, the church contacted 3,965 randomly drawn church members (one half from the control group, the other from the treatment group) with a postal mailing containing a survey questionnaire.¹² The mailing also included a return envelope that survey recipients were able to use to send back the questionnaire anonymously and for free. The questionnaire asked recipients to evaluate a number of statements on the church tax and on state taxes, using a five-point Likert scale (from “fully agree” to “fully disagree”). A total of 1,022 church members (527 from the treatment group, and 495 from the control group) sent back the questionnaire (response rate: 25.8 percent). The survey questionnaires contained a pre-printed code that allowed us to recover several key characteristics of the sender from incoming questionnaires (the anonymity of survey respondents was maintained).

It did not come as a surprise that the survey response was selective with respect to respondents’ observable characteristics. Panel B in Online Appendix Table ?? shows that relative to the overall sample of the experiment, survey respondents were more likely to belong to the top income quartile and were older. However, the survey respondents’ observable characteristics were balanced across treatment and control groups. This allows for causal inference on how the private recognition treatment has affected perceptions of the church tax and of state taxes in the sample of survey respondents.¹³

to the first and the fourth income quartile differ statistically from zero at the 10 percent level. However, it would be misleading to conclude that the income distributions across treatment and control exhibit any meaningful imbalance. In fact, the maximum difference in sample shares documented in Table ??, Panel A, is just half a percentage point.

¹²The sampling of the survey recipients followed the same stratification procedure as the treatment assignment. Couples with two Protestant spouses received only one questionnaire. The analysis is therefore done at the level of the tax unit.

¹³We also wanted to compare perceptions in the recognition group to perceptions in a *no-recognition* letter group. Fearing that the mailing would be perceived as a wasteful form of spending, the church was reluctant to send such a letter. In the end, the church agreed to mail 993 no-recognition letters, which pointed recipients to an existing webpage with information on the church tax. The no-recognition letter recipients were then also invited to take part in the survey. We received only 211 responses from this group. These survey respondents differed from the respondents in the treatment and control groups in terms of their taxable income. In particular, fewer church members from the top income quartile sent back this questionnaire. Due to the small sample size and the unbalanced observable characteristics, we refrain from comparing survey responses in the treatment group to those in the no-recognition letter group.

4 Empirical Results

This section reports and discusses the results of the field experiment. As stated in Section 1, our main assumption is that private recognition in the form of the “thank-you” letter boosts the warm glow of giving. We would expect that the private recognition letter fosters the loyalty of church members, in particular of those members at the margin of opting out. Specifically, we would expect that, first, private recognition leads to a more positive perception of church tax payments and an increased willingness to pay by the church members. Second, we would expect that the changed perception decreases the likelihood (at least temporarily) that church members at the margin of opting out actually terminate their church membership. These hypotheses will be tested in Sections 4.1 and 4.2, respectively.

Alternatively, the private recognition treatment could also be understood as a reminder of the cost of church membership. This seems most likely for members with a high income who accordingly pay higher church taxes. In such a case, the warm glow effect would coincide with a reminder effect. Assuming a fixed private recognition effect, which is identical for high- and low-income earners, the boost in warm glow would decrease with income, while the strength of the reminder increases with income. Such a pattern would lead to decreased opt-out rates for lower-income members and increased opt-out rates for higher-income members. This hypothesis is tested in Section 4.3.

4.1 Effect of Treatment Letter on Perceived Recognition

We first exploit the survey data to show that our treatment successfully shifted the church members’ perception of being recognized by the church. Table 1 reports the evidence from OLS regressions of the type

$$y_i = c + \beta T_i + X_i \gamma + u_i, \tag{1}$$

where T_i is a treatment indicator and X_i is a vector of controls including indicators for income quartiles, single vs. joint filing, respondent age above 35, and place of living

in (semi-)urban areas. Given that the focus of our paper is on opt-out decisions, the most direct manipulation check is to test if the treatment has reduced the likelihood of church members holding *negative* views on the church and the church tax. Therefore, in all of the regressions, the dependent variable, y_i , indicates that the respondent *disagrees* with a given *positive* statement on church payments or on her relation to the church.¹⁴

Column (1) in Table 1 shows that church members in the treatment group are indeed less likely to hold negative views regarding the recognition they receive for their church tax payments. Fewer subjects indicate disagreement with the statement “My church tax payments are appropriately acknowledged by the church”. The treatment effect is -0.170 (p -value < 0.001), which corresponds to a reduction by 35.5 percent relative to the control group mean of 0.479. Next, columns (2) and (3) evaluate the effect of the recognition treatment on members’ willingness to pay, and thus on church loyalty. Column (2) evaluates the statement “I am willing to pay the church tax because the church provides important services.” The estimate shows that respondents from the treatment group are 4.5 percentage points, or 25.7 percent, less likely (p -value 0.046) to disagree with this statement than those in the control group (mean of 0.175). These effects are corroborated in column (3), showing that respondents from the treatment group are 8.8 percentage points, or 17.3 percent, less likely (p -value < 0.01) to disagree with the statement “I am willing to pay the church tax because I benefit from church services.” Column (4) evaluates responses to the statement “Overall, I consider my personal church tax burden appropriate.” The treatment has not significantly shifted survey responses to this statement. While column (5) shows that survey respondents from the treatment group are 5.4 percentage points, or 16.6 percent, less likely (p -value 0.058) to disagree with the statement “My relation to the Protestant Church is close”, we find no significant difference between the groups for the statement “My relation to the Protestant Church has recently improved” (column (6)). All in all, the results show that survey respondents feel more recognized because of the treatment letter, tend to report better relations to the church, and hold more

¹⁴Survey respondents were able to choose between “fully agree”, “rather agree”, “undetermined”, “rather disagree”, and “fully disagree”. y_i is coded as one for all respondents stating that they “disagree” or “fully disagree” with a statement, and zero otherwise.

positive views on making payments to the church. Column (7) reports the average standardized effect (Kling *et al.*, 2004) of the treatment for columns (1) to (6), which is highly significant (p -value < 0.001).

We compare this evidence to the impact of the treatment on perceptions of tax payments to the state. As the treatment expresses recognition for past payments to the church (and not for state taxes), we expect perceptions of state taxes to remain unchanged. This is exactly what we find in columns (8) and (9) of Table 1. Among survey respondents, the treatment neither affects their perception of how appropriately state taxes are acknowledged (column (8)), nor their stated willingness to pay state taxes (column (9)). Accordingly, the average standardized effect in column (10) is far from being significant (p -value 0.69).

Our results were not sensitive to coding of the dependent variables as indicators of disagreement and estimation of the treatment effects by OLS. A more flexible estimation by ordered probit (i.e., defining y_i according to the five Likert scale items, from 1: “fully agree” to 5: “fully disagree”) produces very similar findings (see Online Appendix Table ?? for details).

Taken together, the results of the manipulation checks imply that the treatment has successfully shifted the church members’ perceived recognition for their regular payments to the church in the desired direction. Next, we analyze how the private recognition treatment has affected decisions to opt out of church membership.

4.2 Effect of Recognition on Cumulative Opt-Outs

We estimate the impact of private recognition on church opt-outs following the logic of an event-study design. The event is the randomized treatment. We focus on how this event has affected the cumulative probability of opting out over the following 12 months relative to the counterfactual of not receiving the private recognition letter.

In the following, the outcome of interest is a month t -specific indicator for opting out, y_{it} , for individual church member i .¹⁵ We define y_{it} such that it captures opt-outs in a cumulative manner: It is zero for all church members at $t = 0$ (month of

¹⁵We ignore the extremely rare case (92 observations) of opting in conditional on a previous post-treatment opt-out.

treatment), switches to one if an opt-out occurs in a given month after treatment, and continues to take value one for all remaining months up to $t = 12$. To identify the causal effect of private recognition on opt-outs, we use the OLS regression

$$y_{it} = \sum_{t=1}^{12} \delta_t m_t + \sum_{t=1}^{12} \beta_t T_i \times m_t + X_{i0} \gamma + u_{it}, \quad (2)$$

where m_t is an indicator for month t after treatment T . Note that we estimate a full set of 12 month effects and an interaction term $T_i \times m_t$ for all months after treatment (no constant included). Following standard procedures in the literature, the estimates account for strata variables X_{i0} . If no further baseline characteristics X_{i0} are included, for any given month, δ_t thus indicates the cumulative probability of an opt-out between the month of treatment ($t = 0$) and month $t = 1, \dots, 12$ in the control group, while β_t shows the month-specific difference in the cumulative opt-out probabilities of the treatment and control groups. To account for the fact that some individuals belong to the same tax unit (couples where both spouses are Protestants), we cluster standard errors at the level of the tax unit.

Note that if the treatment reduces opt-outs, this causes a different selection of individuals between treatment and control group from $t = 2$ onwards. Unlike a survival analysis, our approach does *not* aim at correcting for this type of treatment-induced selection effect because we are primarily interested in the impact of the treatment on the number of retained church member at various points in time (and not in the number of church members quitting in a certain month), i.e., in the *cumulative* impact of the treatment. In our power calculations we also sought to identify cumulative treatment effects. We would have needed to treat a much larger group, if we aimed at detecting treatment effects in each month.

Figure 1 displays our key results for the estimation sample by reporting the month-specific differences in the cumulative opt-out probabilities between the treatment and control group. For ease of interpretation, the graph shows relative effects, constructed by dividing the estimated β_t values by the cumulative opt-out probability in the control group in the respective month.¹⁶

¹⁶As documented in the first part of Online Appendix Table ??, the cumulative opt-out probability in the control group shows an almost perfectly linear trend. One year after treatment, 1.6 percent of

Figure 1 shows that the treatment effects are negative for all months. It should be noted that the figures for the first months after treatment are imprecise estimates, given that the average monthly opt-out rate is only about 0.13 percent (see Figure ?? in the Online Appendix for details on the church membership rate in treatment and control by month and the absolute number of opt-outs over time). This implies that, in the first months after treatment, even the cumulative effects reported in Panel B rely on a relatively small number of opt-outs.¹⁷ With an increasing number of opt-outs over time, the estimates become more precise. For months seven to ten after treatment, the treatment effects diverge significantly from zero at conventional levels (p -values of 0.050, 0.038, 0.022, and 0.098, respectively), indicating that the private recognition letter has diminished the cumulative opt-out rate by as much as 9.7 percent.

Starting from month ten after treatment, we observe a diminishing difference in cumulative opt-outs between the treatment and control groups. This is consistent with the notion that the treatment has helped delay opt-outs by church members who were at the margin of opting out significantly. Given that we consider a one-time recognition letter in a context where church members make significant payments to the church on an ongoing basis, it may not be surprising that the impact of the letter diminishes over time. One possible interpretation of the pattern in Figure 1 is that reducing opt-outs permanently requires renewed efforts from the charity (Sargeant, 2001, 2008).

A rough cost-benefit analysis suggests that it is likely that the increased retention due to the intervention had a small positive effect on church finances: A back-of-the-envelope calculation for the twelve months after treatment suggests that church members in the private recognition treatment paid about €33,220 of church taxes more than they would have paid in the absence of the treatment. This exceeds the total cost of the mailing of €20,250 euro. However, we would like to caution that this calculation is based on the average payment to the church in the estimation sample and the estimated coefficients of the cumulative monthly treatment effects. The calculation may overstate the true net benefit to the extent that the positive effect of

church members in the control group have opted out. For the point estimates of the β_t values and corresponding standard errors, see Online Appendix Table ??.

¹⁷As a back-of-the-envelope calculation, the average monthly number of opt-outs in the control group is about $100,000 \times 0.0013 \approx 130$.

the treatment on retention is most pronounced among church members whose cost of membership is rather low. We refrain from more nuanced cost-benefit considerations, since the underlying calculations would be loaded with assumptions we cannot test. Most importantly, our analysis makes clear that the fiscal net impact of the private recognition intervention was rather small. This stems from the fact that the letter had only a temporary effect on retention.

Online Appendix Tables ?? and ?? document that we obtain almost identical results when excluding strata controls. As mentioned before, using the estimation sample ensures that we study a sample of church members who actually made payments to the church in the year before the experiment. This is important, as church members in the treatment group who did not make any payment may perceive of the private recognition letter as inappropriate. Yet, the aforementioned Online Appendix tables also show that we obtain similar (though somewhat weaker) results when focusing on all of the church members originally sampled.

4.3 Treatment Effect Heterogeneity in the Cost of Membership

We next compare responses between different groups of church members. For two reasons, we focus on the cost of membership in the year prior to the experiment as the dimension of heterogeneity. First, the recognition expressed is identical for all church members in the treatment group. Yet, the cost of church membership varies widely between members. Therefore, high- and low-paying church members might perceive the letter differently. Second, although it is meant to express private recognition, the letter may also have served as a reminder for individuals who previously thought about opting out but then did not terminate their membership. The literature shows that, generally, reminders can significantly shift behavior (Huck and Rasul, 2010; Apesteguia *et al.*, 2013; Altmann and Traxler, 2014). In the charitable giving context, unintended consequences of reminder messages, such as subscribers removing themselves from mailing lists, are not uncommon (Damgaard and Gravert, 2018).¹⁸ By reminding (some of the) recipients of the costliness of their membership

¹⁸Unintended effects of donation solicitations are also documented in the context of university giving, albeit on an aggregated (university) level and not for individuals (Leslie and Ramey, 1988; Cunningham

the latter may encourage church members on the verge of opting out actually to go ahead and terminate their membership. This possible reminder effect may play out differently, depending on the cost of membership. In the data, a reminder effect would show up as a temporary increase in opt-outs immediately after treatment.

As discussed before, in our sample the average payment in baseline year 2013 was € 478 euro per year. Table ?? in the Online Appendix reports descriptives and balancing tests for church members in the bottom and top quartile of membership cost. The average church member in the lowest quartile pays € 76, the average church member in the top quartile € 1,147 euro per year. In the top percentile, the membership cost rises to € 10,000 or more. In the following, we exploit this stark heterogeneity to study how the private recognition treatment affects opt-outs for more or less costly memberships.

Figure 2 depicts the treatment effects on cumulative opt-outs for church members in the different cost quartiles. Several observations emerge. First, Panel A documents that in the bottom cost quartile, the private recognition letter has triggered a reduction in opt-outs immediately after treatment by 55.3 percent. The effect slowly tapers off in subsequent months but remains significantly different from zero throughout the first ten months after treatment. Second, by contrast, the treatment has increased opt-outs in the top cost quartile (Panel D). The figure shows a strong positive spike in opt-outs immediately after treatment. The point estimate for the first month after treatment indicates that the letter increased opt-outs by 54.5 percent relative to the control group (p -value 0.06). We caution, however, that the confidence interval of the estimate also includes much smaller values. After one month, the spike in opt-outs tapers off and the cumulative treatment effects quickly converge to zero. This implies that the short-term spike in opt-outs for high-cost memberships did not lead to permanent differences in opt-outs between the treatment and control groups. The resulting negative revenue effects for the church were thus small. Third, the findings for the middle of the distribution are consistent with those for the bottom and the top, although considerably weaker. The pattern in Panel B (second cost quartile) resembles that in Panel A, with negative (but in this case insignificant) cumulative point estimates and Cochi-Ficano, 2002), and of mass fundraising (Adena and Huck, 2019b).

mates. Panel C covering the second-highest quartile exhibits a (this time statistically insignificant) increase in opt-outs immediately after treatment.

Overall, Figure 2 delivers two insights. First, there is a negative monotonic relation between the cost of membership and the effectiveness of our treatment: the lower the cost of membership, the stronger is the reduction in cumulative opt-outs caused by the recognition letter. This finding resonates with the assumption that private recognition becomes less powerful for recurring donors if their payment is large, or that, as suggested by related evidence on reciprocity as a motive for giving (Falk, 2007), the relative magnitude of recognition matters. Second, for very high-paying church members, the evidence suggests that the letter reminded members of the high cost of their membership and the possibility of opting out. While such a reminder effect could be of general importance, it seems natural that it is most pronounced among church members with particularly high incomes and, therefore, high church tax payments. The interpretation that the spike in opt-outs among high-cost memberships results from a reminder effect is consistent with the notion of optimization frictions prior to the experiment. The assumption is that some church members incline towards opting out but fail to follow through with this plan, leading to a delay in planned opt-outs and the unintentional continuation of giving to the church. Although we did not design the experiment to identify an optimization friction in charitable giving, the short-term spike in opt-outs in Panel D offers interesting evidence regarding the behavior of recurring donors.

Simulation results in Young (2019) show that in the presence of heterogeneous treatment effects t -statistic-based randomization tests are much more accurate than clustered or robust standard errors. As Young points out, this can lead experimental papers to overreject the null hypothesis of no effect in heterogeneity analyses. To alleviate such concerns, we probe the robustness of our results, using randomization inference (RI). Instead of estimating the variation from repeated sampling from an underlying population (sampling-based uncertainty) as in the conventional approach, RI focuses on the uncertainty arising from the treatment assignment (design-based uncertainty). That is, it asks whether the results are robust under all possible random treatment assignments. We proceed in four steps: First, we compute and store the

t -statistics for the coefficients of interest in eq. (2) from our original data set. Second, we randomly permute the treatment indicator and re-run our estimation, storing the resulting t -statistics for the coefficients of interest. Third, we repeat the second step for 10,000 times as suggested in Young (2019). Finally, we compare the distribution of permutation-based t -statistics with the original t -value. The RI p -value is given by the share of permuted t -statistics weakly greater than the original t -statistic in absolute value.¹⁹ Online Appendix Tables ?? and ?? show that the resulting p -values are very similar to those derived from cluster robust standard errors both for the full sample and for the subsamples by cost of membership.

4.4 Robustness

We have already commented on the robustness of our main findings along several dimensions. In this section, we report on additional robustness tests. A first test addresses the wording of the treatment letter. In order to identify the effect of recognition, we wanted the letter to be short and focused around a simple and straightforward expression of private recognition. However, this lent the private recognition letter a rather matter-of-fact tone. It is an interesting question if a longer letter written in a warmer tone might be more effective in retaining church members. To shed light on this question, we analyze the impact of a longer letter, that was designed by the coordinator of the initiative undertaken by the Bavarian Protestant church to improve the retention of tax-paying church members of which our research was one element. The letter was identical to the short private recognition letter in terms of its layout, but the text of the letter was longer and not exclusively focused on private recognition. The longer letter read: “As a member of the Lutheran Church, you have supported our work again last year. On behalf of the Lutheran Church in Bavaria, I would like to thank you cordially for your church tax payment. With your help, we are able to perform our manifold tasks. In all of our Bavarian parishes, we offer pastoral care and advice, we help families by providing daycare facilities for children and nursing services for the elderly, we maintain our churches, baptize children, and celebrate

¹⁹Following Kennedy and Cade (1996) and Ferman and Pinto (2019) we base our RI procedure on cluster robust t -statistics instead of the coefficient of interest in order to rely on a pivotal statistic.

weddings. Your contribution is important and we do not take it for granted. Thank you for your willingness to support our church in solidarity with others. The ongoing support of our members provides the financial foundation of our church. The bulk of the church tax revenues goes to the parishes to fund services provided locally. In the Lutheran Church, decisions on how to spend the church tax funds are made by elected church boards and synods consisting of church members who volunteer for this service. The budget plans are made public, and the orderly disbursement of the funds is audited regularly. Your contribution benefits the people and the common weal.”

Figures ?? and ?? in the Online Appendix report the effects of the longer letter. Both figures show patterns similar to those for the pure private recognition letter, but the effects are much weaker. It appears that, in our context, a longer letter that, besides expressing private recognition, comments on how the funds are spent and is written in a warmer tone does not perform better in terms of retaining recurring donors than a simple letter with a short message of private recognition.

The evidence discussed in Sections 4.2 and 4.3 is further corroborated by the treatment effect heterogeneity in all strata dimensions. As explained before, the sample was stratified according to taxable income (below/above median), church members’ age (below/above 35 years), and urbanization at place of living (rural, semi-urban, urban). The strata dimensions were determined in terms of baseline correlations between the opt-out probability and individual characteristics.²⁰ For completeness, Figures ??, ??, and ?? in the Online Appendix report the treatment effect heterogeneity along all strata dimensions. As expected (given the heterogeneity reported in Section 4.3), Figure ?? shows that there is an effect of private recognition on retention among church members with a below-median cost of membership for up to nine months after treatment. Above the median, we again see a spike in opt-outs in the first month after treatment. Other than that, there is no effect on retention for church members with an above-median cost of membership. The figures on the heterogeneity by age and urbanization do not reveal any striking patterns.

²⁰Among church members below the age of 35, opt-outs are about twice as frequent as compared to church members above the age of 35. The same holds true for the comparison between church members living in urban vs. rural regions, with semi-urban regions taking a middle position in terms of average opt-out rates.

Finally, we test for the impact of a repeated intervention. A letter identical to our private recognition letter was sent out one year later (February 2016). This repeated intervention did not increase church members' loyalty towards the church (see Figure ?? in the Online Appendix): The coefficients for the months after the repeated intervention are all statistically insignificant. The point estimates suggest that the repeated intervention may even have led to slightly more opt-outs. We conclude that interventions that previously worked as intended may lose their bite when they are mechanically duplicated. Further research will have to determine whether a different variant of a recognition treatment would again be able to strengthen individuals' loyalty, preventing members from opting out.

5 Conclusion

This paper contributes to the literature on charitable giving, and specifically to an emerging literature on recurring donations. As half of donors worldwide are enrolled in a recurring giving program, recurring donors are of great importance for charitable organizations. Yet, little research has focused on them. Our study helps fill the void by providing causal evidence on how private recognition affects donor loyalty.

Church members in Germany make significant payments to their church on an ongoing basis in the form of a compulsory church tax. However, members are able to avoid paying the tax by terminating their membership of the church at any time. Church membership in Germany is therefore akin to participation in a recurring donor scheme. In a randomized field experiment, we manipulate the recognition that members of the Protestant church receive by sending half of them a letter which expresses private recognition for past payments.

Our main finding is that the private recognition letter increases the retention rate among church members for a period of up to ten months after treatment. The treatment temporarily reduces the cumulative opt-out rate in the treatment group by almost 10 percent relative to the control group. A heterogeneity analysis delivers more nuanced insights. First, there is a negative monotonic relation between the cost of membership and the effectiveness of the treatment, with low-paying church members

responding most strongly. Second, among church members in the top cost quartile, we observe a sharp (but temporary) increase in opt-outs immediately after treatment. This spike in opt-outs is consistent with the notion that the recognition letter has reminded high-paying members of the option of terminating their membership.

While the one-time intervention in our experiment was successful in reducing opt-outs for several months, it did not affect the long-run dynamics of opting out of church membership. As a result, the intervention had only a minor positive effect on church finances. More work is needed to explore how charities can use recognition (and other forms of interaction with recurring donors) to induce permanent improvements in donor loyalty.

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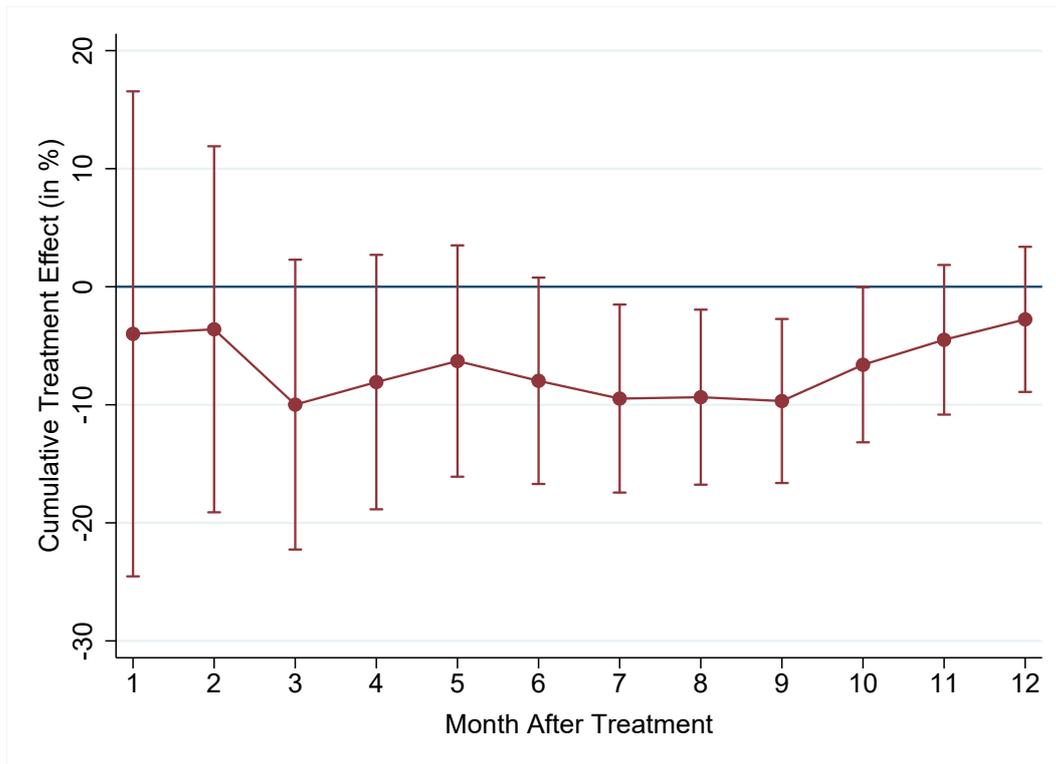
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Table 1: Manipulation Checks: Perceived Recognition and Loyalty Towards the Church

	Church Tax: Respondent Disagrees With					State Taxes: Respondent Disagrees With				
	Payments Appropriately Acknowledged (1)	Willing to Pay for Church Services (2)	Willing to Pay for Own Benefits (3)	Church Tax Burden is Appropriate (4)	Relation to Church is Close (5)	Relation to Church Improved (6)	Average Standard. Effect (7)	Payments Appropriately Acknowledged (8)	Willing to Pay for Public Services (9)	Average Standard. Effect (10)
Treatment Effect	-0.170*** (0.030)	-0.045** (0.022)	-0.088*** (0.031)	-0.020 (0.027)	-0.054* (0.029)	-0.028 (0.022)	-0.156*** (0.041)	-0.025 (0.031)	0.004 (0.018)	-0.019 (0.050)
N	1002	1016	1011	1017	1015	1018	988	1002	1013	998
Mean in Control	0.479	0.175	0.510	0.266	0.326	0.163	0.594	0.090		

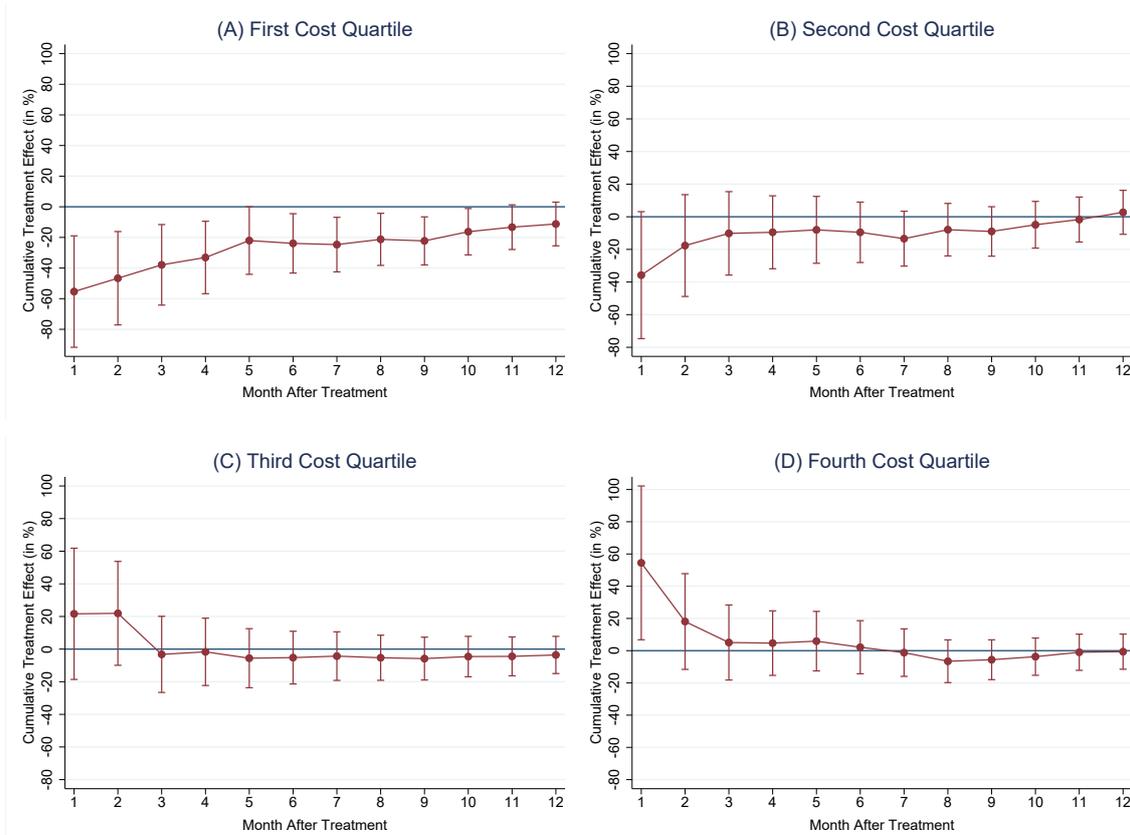
Notes: This table is based on survey responses from a 5-point Likert scale and presents evidence that the recognition treatment indeed increased perceived recognition and loyalty towards the church. All results come from OLS regressions of eq. (1). The dependent variable is equal to one for all “disagree” and “fully disagree” responses and zero otherwise. We evaluate the probability of disagreement with the following statements: Column (1): “My church tax payments are appropriately acknowledged by the church”. Column (2): “I am willing to pay church tax because the church provides important services”. Column (3): “I am willing to pay church tax because I benefit from church services”. Column (4): “Overall, I consider my personal church tax burden to be appropriate”. Column (5): “My relation to the Protestant Church is close”. Column (6): “My relation to the Protestant Church has recently improved”. Column (8): “My state tax payments are appropriately acknowledged by the state”. Column (9): “I am willing to pay the state taxes because I thereby contribute to the funding of important public services”. Columns (7) and (10) report average standardized effects according to [Kling et al. \(2004\)](#). All regressions include indicators for single vs. joint filing, respondent age above 35, income quartiles, and place of living in (semi-)urban areas. Robust standard errors in parentheses. ***, ** and * denote significance level at 1, 5, and 10 percent levels, respectively.

Figure 1: Effect of Private Recognition on Cumulative Opt-Outs



Notes: This figure shows cumulative monthly effects of the private recognition treatment on the opt-out rate relative to the month-specific cumulative opt-out rate in the control group. The whiskers indicate 90% confidence intervals accounting for clusters at the level of the tax unit (individual or married couple). The sample consists of $N \times T = 200,784 \times 12 = 2,409,408$ observations. The point estimates are reported in Online Appendix Table ??, column (1).

Figure 2: Heterogeneity by Cost of Membership



Notes: The figure depicts the heterogeneity of treatment effects by the cost of membership. All four panels depict relative cumulative treatment effects by month. Panel A shows the lowest cost quartile. Panel B reports the effects for the second, Panel C for the third, and Panel D for the top cost quartile. The cost of membership is equal to the annual church tax payment and measured in baseline year 2013. The whiskers indicate 90% confidence intervals accounting for clusters at the level of the tax unit (individual or married couple). Details on the underlying estimation (including sample sizes) are reported in Online Appendix Table ??, columns (2) to (5).