Religious Participation and Pro-Social Behavior

An Event-Study Analysis of the U.S. Catholic Clergy Scandals[*]

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Abstract

Although several studies document a strong correlation between religious participation and pro-social behavior, there is no consensus about the direction of causality. We provide novel evidence by examining variations in religious participation induced by the Catholic-clergy abuse scandals in the United States. To estimate the causal effects of the scandals on various outcomes, we conduct an event-study analysis that exploits the fine distribution of the scandals over space and time. First, we show that a scandal causes a significant and long-lasting decline in religious participation in the location where it occurs. Second, we test whether the decline in religious participation translates into a decline in pro-social beliefs and behavior. On the one hand, we find a long-lasting decline in charitable contributions. Indeed, this drop in charitable giving is an order of magnitude larger than the direct costs of the scandals to the Catholic churches (e.g., lawsuits). On the other hand, the scandals do not have a significant effect on religious beliefs, pro-social beliefs, and other forms of pro-social behavior.

JEL Classification: D64, H41, L31, Z1, Z12.

Keywords: religion, beliefs, pro-social behavior, charitable giving.

1 Introduction

Several studies document a strong positive correlation between religious participation and pro-social beliefs and behavior (Putnam and Campbell 2010), including charitable giving. However, there is no conclusive evidence about the direction of causality. For example, it is plausible that individuals with higher religious participation who are forced to abandon their religious congregations would continue to donate money to charity and to behave similarly in other pro-social dimensions.[1] Given the large, observable differences between individuals with lower and higher religious participation, spurious correlation is not only plausible, but likely. Furthermore, there is no consensus about the causal mechanisms. For example, while some studies emphasize the role of religious beliefs in eliciting higher charitable giving (Thorton and Helms 2013), others point to social mechanisms such as peer pressure and solicitation (Soetevent 2005). This paper investigates the effects of religious participation on pro-social behavior by examining variations in religious participation induced by the Catholic-clergy abuse scandals in the United States.

Combining several sources of data, we created a unique dataset containing the exact address of each parish involved in a Catholic-clergy sexual abuse scandal and the exact date when each accusation became public. Our event of interest is the scandal and not the abuse itself. Indeed, the vast majority of the scandals occurred decades after the abuses were allegedly committed. We identify more than 3,000 scandal events throughout the United States from 1980 to 2010. We combine the data on scandals with multiple sources of administrative and survey data for various outcomes, such as religious participation and charitable giving. We estimate the causal effects of the scandals on these outcomes by exploiting the fine distribution of scandals over space and time using an event-study framework.

Using administrative data on the number of religious employees, we find that a scandal causes a persistent 3% decline in religious participation in the zip code in which it occurs, which is equivalent to a 9% decline in the sub-population of Catholics. We find effects of similar magnitude when using survey data on religious affiliation and religious participation. Although some Catholics appear to join other religious denominations during the first three years after a scandal, later on these individuals abandon all religious affiliations. The scandals have a significant effect on religious participation, but they do not have a significant effect on religious beliefs, such as the belief in God and belief in the afterlife. This finding

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[1]For further discussion on this identification challenge see Gruber and Hungerman (2008).

is consistent with the view that religious beliefs are deeply ingrained during adulthood and therefore unlikely to change (Hamberg 1991).

If religious participation caused higher charitable giving, then a decline in religious participation in response to the scandals should cause a decline in charitable giving. We use administrative data from the IRS on itemized charitable contributions as reported by taxpayers on their 1040 forms. We find that a scandal causes a persistent decline in charitable giving of about 1.3% in the zip code where it occurs. In other words, for each percentage point decrease in religious participation, charitable giving declines by 0.43 percentage points. Using another administrative dataset from the IRS on contributions as reported by charities, we find evidence suggesting that the decline in charitable giving is partially ameliorated by Catholics substituting donations to non-Catholic charities. Additionally, we find that the decline in charitable giving translates into a decline in the provision of social services, which is consistent with the belief that these services are funded primarily at the local level (Cnaan et al. 2002).

The effects of the scandals on religious participation, charitable giving, and social services follow similar patterns. The effects intensify during the first five years after the scandal and stabilize after the sixth year. The affected outcomes do not revert to pre-scandal levels even more than ten years after the occurrence of the scandal. The effects are also mostly concentrated in the zip code in which the scandal occurs, with small spillovers to adjacent zip codes and no spillovers to adjacent-to-adjacent zip codes. The scandals have similar effects at the place where the accused priest is working at the time of the accusation and, if different, at the place where the accused priest allegedly perpetrated the abuse.

We also examine the effect of scandals on various measures of pro-social beliefs and behaviors that are widely used in the literature. We find that the scandals do not have a significant effect on pro-social beliefs, such as self-reported trust in others. We also find that the scandals do not have a significant effect on other forms of pro-social behavior besides charitable giving, including political campaign contributions, response rates to census forms, and voting turnout. This evidence suggests that changes in religious participation during adulthood may not affect general pro-social attitudes. This finding is consistent with studies that report an insignificant correlation between religious participation and pro-social beliefs (Alesina and La Ferrara 2002) and pro-social behavior (Anderson et al. 2010). However, our evidence does not reject the possibility that changes in participation early in life or participation in other religious denominations may have a significant effect on pro-social

attitudes.

Our findings help explain the strong correlation observed between religious participation and charitable giving. If we assume that the scandals affected charitable giving solely through the effect on religious participation, then the magnitude of the effects of the scandals imply an elasticity between religious participation and charitable giving of about 0.43. In comparison, the raw cross-sectional correlation between these two outcomes results in an elasticity of about 0.38. Thus, our estimates suggest that most of the observed correlation between religious participation and charitable giving may have the presumed direction of causality (i.e., from religious participation to charitable giving).

A potential confounding explanation is that the scandals may have a direct effect on charitable giving that extended beyond the decline in religious participation, because Catholics were outraged and thus less prone to pro-social behavior. If that were true, then the elasticity of 0.43 reported above would over-estimate the effect of religious participation on giving. We provide several pieces of evidence suggestive that this confounding factor had only a second-order effect, if any. For example, this confounding factor is at odds with the finding that scandals do not affect pro-social beliefs, such as trust in others, and other pro-social behavior besides charitable giving.

Our evidence also helps identify the mechanisms through which the drop in religious participation may have caused a drop in charitable giving. Since religious beliefs and pro-social beliefs do not change as a result of the scandals, they could not have been mediating factors. Our preferred explanation consists of social mechanisms. That is, the social interactions within religious congregations provide advantageous conditions for eliciting charitable contributions from members through direct solicitation, social pressure, social norms, and so on. Consistent with our findings, if religious participation declines and thus eliminates these social mechanisms, then charitable giving should decrease, but other forms of pro-social behavior should not be affected. Moreover, these same social mechanisms are believed to play a crucial role in motivating charitable giving even outside the context of religious congregations (Frey and Meier 2004; Andreoni and Rao 2011; Meer 2011; DellaVigna et al. 2012).

Our study relates to other literature besides that of religious participation and pro-social behavior. Although a number of studies address the causes and circumstances of the Catholic-clergy scandals[2] and the psychological effects on the victims (McMackin et al.

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[2]See for example the reports prepared by the John Jay College of Criminal Justice for the United States

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2009), few studies examine the broader consequences of the scandals. Exceptions include Hungerman (2013), who looks at the relationship between abuse allegations and religious adherence with state-level data, and Dills and Hernández-Julian (2012), who examine the relationship between abuse allegations and Catholic enrollment with diocese-level data. Our paper contributes to this research by looking at the broader consequences of the scandals and by identifying the causal effects of the scandals through an event-study analysis that exploits the fine variation of scandals over space and time.[3] We find that the indirect costs of the scandals (e.g., the drop in charitable contributions) are an order of magnitude higher than the direct costs to the Catholic Church (e.g., the legal and other abuse-related costs). This finding raises the question of whether this ratio of private to social costs are similar in other types of scandals, such as corruption scandals in the public and private sectors, and whether policymakers should take preventive and remedial measures.

Our study also relates to a literature that studies crowding out between different sources of charitable funding. For example, Andreoni and Payne (2003) find that government expenditures crowd out charitable giving, and Hungerman (2005) and Gruber and Hungerman (2007) find that government expenditures crowd out religious giving. We test whether the negative shock to the Catholic congregations produces a "crowding in" effect by increasing contributions to non-Catholic charities. Even though we find evidence suggestive of some crowding in, it is not nearly enough to offset the decrease in donations: total charitable giving does not revert to pre-scandal levels, and neither does the private provision of social services.

The paper proceeds as follows. Section 2 presents the data on Catholic-clergy scandals. Section 3 analyzes the effects of the scandals on religious participation and religious beliefs. Section 4 analyzes the effects of the scandals on charitable giving. Section 5 studies effects on other forms of pro-social behavior. Section 6 discusses the interpretation of the findings. The last section concludes.

Conference of Catholic Bishops entitled "The Nature and Scope of the Problem of Sexual Abuse of Minors by Catholic Priests and Deacons in the United States" and "The Causes and Context of Sexual Abuse of Minors by Catholic Priests in the United States, 1950-2010."

[3]Also, these previous studies estimated short-term effects of the scandals, which turn out to differ in significant ways from the long-term effects. For example, Dills and Hernández-Julian (2012) find a very small contemporaneous correlation between scandals and the number of Catholic schools, while our analysis shows that the scandals had large persistent effects.

2 Data on Scandals and Identification Strategy

2.1 Construction of the Scandals Dataset

Since the mid-1980s, the Catholic Church has repeatedly experienced revelations of sexual abuse committed by its clergy. The number of allegations increased rapidly after a story published by the Boston Globe in January 2002 about the defrocked priest John Geoghan and his long record of child sexual abuse.[4] According to the confidential reports ordered by the Catholic bishops, 5,768 priests had received at least one allegation of abuse during 1950–2009 (or 5.3% of all priests active in the United States).[5] Since these reports are confidential, we used the records published by Bishop Accountability (bishopaccountability.org), an NGO that compiles a public list of Catholic clergy in the United States who have faced a sexual abuse allegation.[6] Our empirical analysis does not rely on whether the allegations are true, but on their media repercussions. Since we are interested only in accusations that had media repercussions and since media documents are considered "sufficient" information to be included in the Bishop Accountability records, we are confident that our data give a fairly complete account of the Catholic clergy abuse scandals.

Our datasets differ substantially from other datasets that use Bishop Accountability (Hungerman 2013; Dills and Hernández-Julian 2012), particularly in terms of the use of complementary datasets and in the definition of scandal. We complement the Bishop Accountability list with several other data sources (e.g., newspaper articles, the Official Catholic Directory, the official websites of Catholic institutions, Google Maps) to identify the appointment of each accused clergy member at the time of the first public accusation and the precise date of the first news article mentioning the accusation.

Because decades often passed between the time of the abuse and the related news reports, some priests had relocated, retired, or died by the time of the accusation. We define two types of scandals, depending on the location of the accused priest at the time of the accusation. In a **type-A scandal**, a clergy member who currently works in a Catholic institution is accused of sexual abuse for the first time, no matter whether he or she committed the abuse in that same institution or some other institution. The location of the

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[4]For further details about the chain of events, see Hungerman (2013) and the references therein.

[5]The full body of Catholic bishops of the United States gathered at the General Meeting in Dallas approved the Charter for the Protection of Children and Young People. One of the steps established in the Charter was ordering a study that was conducted by the John Jay College of Criminal Justice on the nature and scope of abuse in the church (JJCCJ, 2004, 2006, 2011).

[6]This NGO includes individuals only when there is substantiated documentation such as newspaper articles from a reputable sources or copies of legal documents filed in court and maintained in a public file.

scandal is the address of the institution where the clergy member worked at the time of the public accusation. The date of the scandal is the date of the first article mentioning the abuse (as long as the newspaper's circulation covers the corresponding location). In a **type-B scandal**, a clergy member is publicly accused of committing abuse while working in the institution in the past, even if he or she did not work at that same institution at the time of the accusation. The location of the scandal is the address of the institution where the abuse allegedly occurred. The date of the scandal corresponds to the date of the first newspaper article mentioning the abuse (as long as the newspaper's circulation covers the corresponding location).[7]

Type-A and type-B scandals differ in the direct costs imposed on the local parish. For example, only type-A scandals can involve the removal of clergy, while only type-B scandals can involve abuse-related lawsuits. Nevertheless, both types of scandals are expected to reduce religious participation in the local community. To maximize statistical power, our baseline specification does not discriminate between the two types of scandals.

Our database covers 3,024 scandal events during 1980–2010 (1,125 type-A and 1,899 type-B scandals). Figure 1.a shows the geographic distribution of scandals in the contiguous United States, and Figure 1.b focuses on New Jersey. In both figures, the color of the states and counties denote the density of the Catholic population. Although scandals are more common in more Catholic areas and most of them occur during 2002–2003, there is substantial variation over space and time. The following website contains an animated demonstration of the distribution of scandals over space and time that best illustrates the richness of our data. For more details about our scandals dataset, see Appendix B.

2.2 Identification Strategy: Event-Study Analysis

Estimating the causal effect of the scandals on a given outcome variable can be challenging. For example, places with more scandals may happen to be places with less pro-social individuals or pro-social behavior may have already been decreasing before the scandals occurred, which would generate spurious negative correlations between the number of scandals and pro-social behavior. To identify the causal effect of the scandals on a given outcome

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[7]For instance, consider a priest who allegedly comitted abuse in 1975 during his appointment in a parish in town A and later allegedly comitted abuse again in 1982 during his appointment in town B. In 2003 this priest is publicly accused for his alleged abuse in town A while working in a parish located in town C. In 2006, once he was removed from priesthood, he is accused for his alleged abuse in town B. According to our definitions of scandals, this priest has one type-A scandal in town C in 2003 and two type-B scandals: one in town A in 2003 and another one in town B in 2006.

(e.g., religious participation, charitable giving), our identification strategy is based on an event-study analysis that takes advantage of the rich temporal and geographic variations in the scandals. Instead of assuming that the timing of the scandals is exogenous, the event-study analysis allows us to test that assumption directly by looking at the evolution of the outcome variable in the years before and after a scandal. Additionally, we can also test whether the effects of a scandal are localized to the area in which the scandal takes places, or whether they also affected neighboring areas.

The event-study graphs provide the best evidence by showing the evolution of a given outcome during each of the years before and after a scandal (for more details about the methodology, see Appendix A).[8] In addition to this graphical analysis, we present a compact version of the same evidence that is summarized by three parameters. Consider the outcome variable $y_{z,t}$ (e.g., mean charitable giving), where the subscript $z = 1, ..., N$ denotes location (e.g., zip code) and the subscript $t = 0, ..., T$ denotes year. The regression specification is:

$$y_{z,t} = \alpha_{ST} \cdot S_{z,t}^{Short-Term} + \alpha_{LT} \cdot S_{z,t}^{Long-Term} + \alpha_{PS} \cdot S_{z,t}^{Pre-Scandal} + X_{z,t}\beta + \epsilon_{z,t}$$

$X_{z,t}$ is a vector of control variables with, among others, zip code and time effects. $S_{z,t}^{Short-Term}$ is equal to the number of scandals that occurred between t and $t - 3$. Thus, α_{ST} measures the average effect of a scandal 0-3 years after it occurs, which we denote the Short-Term effect. $S_{z,t}^{Long-Term}$ is equal to the number of scandals that occurred in the zip code at $t \leq 4$. Thus, α_{LT} measures the average effect of a scandal 4+ years after it occurs, which we denote the Long-Term effect.[9] $S_{z,t}^{Pre-Scandal}$ is defined as the number of scandals at $t + 1$ and $t + 2$. Thus, α_{PS} measures the average effect of a scandal in the two years before it actually happens, which we denominate the Pre-Scandal effect. This is a falsification test for whether the outcome variables evolve similar between zip codes with and without scandals prior to a scandal. We expect the Pre-Scandal effect to be zero. It is important to note that even though our selection of the time period for the Pre-Scandal variable is arbitrary, it is not particularly relevant since it is possible to observe the evolution of the outcome variable for every year before the scandal in the event-study graphs.

All regressions—both for the event-study figures and the compact regression analysis—include the same set of control variables: zip code fixed effects (or county fixed effects,

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[8]For an event-study analysis that exploits geographic variation in a similar fashion to our paper, see Linden and Rockoff (2008).

[9]Results are robust to using an indicator varible for whether a scandal ever occurred instead of the number of scandals.

depending on the unit of analysis), time effects, the interaction between year effects and a set of zip code (or county) characteristics as of 1990, and state-specific time trends.[10] In practice, the inclusion of these control variables does not affect the magnitude of the estimates significantly, but it does improve their precision by reducing the variance of the error term. Also, we always present standard errors clustered at the zip code (or county) level, but the results are robust to clustering at higher levels of aggregation.

3 Effect of the Scandals on Religious Participation and Religious Beliefs

3.1 Data Sources

We first evaluate the effects of the scandals on religious participation and religious beliefs. First, we use the number of Catholic schools in a zip code as a proxy for religious participation in Catholic institutions. According to the National Catholic Educational Association, the vast majority of students enrolled in Catholic schools belong to Catholic families.[11] Data on the number of Catholic schools were obtained from the Private School Survey, a census of U.S. private schools conducted biannually from 1989-2010.[12] Descriptive statistics for these data and all the other data sources in this paper are available in Appendix G.

As a measure of the overall presence of religious institutions in a zip code (not only Catholic), we use yearly data on the number of employees[13] working in religious establishments from the Zipcode Business Patterns during 1994–2010.[14] The industry code that denotes religious institutions (NAICS code 813110) includes churches and places of worship, among others, but does not include other types of establishments maintained by religious organizations, such as educational institutions, charitable institutions, and hospitals. The

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[10]The interactive terms may account for differences in the evolution of the dependent variable across affected and unaffected zip codes that can be traced back to differences in observable zip code characteristics. Some regressions include a few additional control variables that are obtained from the same data source as the dependent variable. Because of smaller sample sizes, the regressions with survey data include fewer control variables.

[11]According to this source, the share of non-Catholic enrollment in Catholic schools for the school year 2010-2011 was about 15% and has remained about the same over the last couple of decades.

[12]This survey is conducted by the U.S. Department of Education's National Center for Education Statistics (NCES). We focus on the set of zip codes that had at least one Catholic school at some point in the sample period. By construction, these zip codes covers the totality of Catholic schools in the United States.

[13]We use the sum of the number of establishments weighted by the average number of employees in the corresponding size-category. For instance, if a zip code has 1 establishment with 1 to 4 employees and 2 establishments with 5 to 9 employees, the proxy for the number of employees is 1*2.5+2*7=16.5.

[14]This data is an annual series prepared by the U.S. Census Bureau using several sources of administrative data. We use the sample of zip codes that always have at least one religious employee over the sample period, which includes 99% of the religious employees in the country. The excluded are small zip codes, unaffected by scandals.

average zip code has about eight religious establishments with a total of eighty employees.

To explore whether the scandals affected religious beliefs as well, we employ individual-level data from the General Social Survey, which includes multiple questions about religious participation and religious beliefs.[15] We were granted access to county identifiers for the respondents from 1994-2010.[16] Counties with scandals are, on average, much larger than zip codes and vary dramatically in terms of population. Indeed, most scandals occurred in counties with several million inhabitants. Since scandals have localized effects, a scandal in a small county is expected to affect most of the population in that county, while a scandal in a very large county is expected to affect only a small fraction of that county's population. To account for these localized effects, we divide the scandal variables by 25,000 county-inhabitants, as of 1990, in all the county-level regressions. The choice of 25,000 inhabitants corresponds to the average population in the zip codes affected by scandals, so that the coefficients estimated with county-level data can be (roughly) comparable to the coefficients with zip code-level data. We found similar results when using alternative weighting methods.

3.2 Results

Figure 2.a shows the event-study graph for the effect of scandals on the number of Catholic schools in a zip code. The coefficients to the right of the date of the scandal are negative and statistically significant, suggesting that, after the occurrence of a scandal, there is a significant drop in the number of Catholic schools in zip codes affected by scandals. The estimated coefficients to the left of the date of the scandal are very close to zero and precisely estimated, suggesting that, prior to the occurrence of a scandal, the evolution of the outcome variable is the same in zip codes affected and unaffected by scandals. Some institutional factors may explain this exogenous timing of the scandals. Most important, a large time lapse occurs between the alleged abuses and the accusations, so the timing of the scandals can be exogenous even if the timing of the abuses is not. Additionally, the 2002 Boston Globe article worked as an exogenous trigger of a substantial number of scandals.

The event-study graph suggests that the effect of a scandal intensifies over the first four years and then stabilizes. This pattern of intensification over the first few years after the scandal appears repeatedly for the different outcomes analyzed in this paper. The pattern

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[15]But, because of the need for a critical sample size, we can only analyze questions that were included in several waves.

[16]Since the data is not a panel of individuals, we cannot control for individual fixed effects. Instead, we include county fixed effects in the regressions, along with time effects, the interaction between time effects and the share of Catholic population, and a set of individual control variables (e.g., gender, age).

can be at least partially attributed to the way in which a scandal typically develops over time: the date of the scandal corresponds to the appearance of the first newspaper article, followed by further news that intensifies the scandal severity in subsequent years. This news includes further proof about the first victim, allegations from additional victims, sanctions by the Catholic Church, legal sanctions, and other information.[17]

The coefficients in column (1) from Table 1 present the compact version of the findings from the event-study graph. The Long-Term effect suggests a statistically significant and permanent decline of about 0.068 schools per scandal, which is equivalent to 5.3% of the mean of this outcome.[18] To illustrate the severity of the scandals, the event-study estimates suggest that the scandal effects can explain 23% of the sharp decline of 1,130 schools that occurred during the post-2002 period.[19]

The main falsification test is provided by the coefficient on Pre-Scandal, which tests whether, before a scandal occurs, the evolution of the outcome variable is equal between zip codes with and without scandals. As expected, the Pre-Scandal effect is very close to zero, precisely estimated and statistically insignificant. As an additional falsification test, we evaluate whether scandals affected the number of private schools of other religious (non-Catholic) denominations. Column (6) of Table 1 presents the results (additionally, the event-study graph for this and other secondary outcomes are presented in Figure G.1 in the Appendix). As expected, the Catholic scandals did not affect the number of schools of other religious denominations.

Column (2) from Table 1 shows the effect of the scandals on the number of religious employees, which includes both Catholic and non-Catholic denominations (Figure 2.b reports the corresponding event-study graph). The estimates suggest a Long-Term effect on the number of religious employees of around -3%. Given that Catholics in counties with scandals comprise about 30% of the population (General Social Survey 2001), the implied effect on Catholic religious employees would be about 9% (i.e., 3% divided by 0.3).[20] As

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[17]For some outcomes, like the number of Catholic schools, this pattern may also partially reflect that these institutions can survive for a few extra years while the deplete their assets. Additionally, part of the intensification pattern could be the product of earlier scandals having a relatively stronger effect.

[18]We find a similar effect when using Catholic school enrollment instead of the number of schools as the outcome variable.

[19]On the contrary, Dills and Hernández-Julian (2012) claim that the effects of the scandals on Catholic schools were small. Nevertheless, it is straightforward to explain why they reach a difference conclusion. Their specification is based on the contemporaneous correlation between the number of scandal allegations and Catholic school enrollment. Figure 2.a shows this to be a misleading specification, because the contemporaneous effects of a scandal are very small, relative to the large effects that follow in the years after a scandal.

[20]This calculation assumes that Catholics affected by the scandals do not join other religious congregations

expected, the Pre-Scandal effect is very close to zero and statistically insignificant. As an additional falsification test, column (7) presents the results using the number of employees in a group of retail stores as the outcome variable. As expected, the scandals have no effect on this outcome.

Even if a scandal affects only Catholics living in the same zip code where it occurs, those same Catholics may use schools and religious establishments in adjacent zip codes, generating spillovers to those neighboring areas. However, we should observe that the effect of a scandal decreases with the distance from the location where the scandal originated. To test this hypothesis, Table 2 shows the effects of the scandals on the main outcomes, disaggregated by effects on the same zip code where the scandal occurs, the adjacent zip codes, and the adjacent-to-adjacent zip codes.[21] Columns (1) and (2) show the estimated effects on the number of Catholic schools and religious employees, respectively. The effects of a scandal on the adjacent zip code are statistically significant but, as expected, the magnitude of these spillovers is less than half the magnitude of the effect on the same zip code in which the scandal occurs. Moreover, the scandals do not have a significant effect on adjacent-to-adjacent zip codes. This pattern of geographic distribution of the effects of the scandals are consistent across all the outcomes studied in this paper.

Table 3 provides a comparison between the effects of the two types of scandals. Intuitively, when a priest working in town A is publicly accused of abuse that happened 20 years ago while he was working in town B, the news could have a very different effect on religious participation in town A than in town B. Columns (1) and (2) show the effects on the number of Catholic schools and religious employees, respectively. The effects are similar across both types of scandals. For each outcome, the table also reports the p-value of a test where the null hypothesis is that the Long-Term effects of type-A and type-B scandals are equal. We cannot reject the null hypothesis at standard levels of significance for any of the outcomes considered. As previously mentioned, each type of scandal has unique features. For instance, only type-A scandals can involve the removal of clergy, while only type-B scandals can involve scandal-related lawsuits. This evidence suggests that none of these unique features can fully explain the effects of the scandals.

Table 4 shows the regression results using data from the General Social Survey. This

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in the long term. We provide a test of this assumption below.

[21]We define two zip codes as adjacent (neighbors) to each other if, according to cartographic data, their boundaries touch. In this specification, we introduce three versions of each scandal variable: one counting the number of scandals in the same zip code, another counting the number of scandals in the adjacent zip code, and another counting the number of scandals in the adjacent-to-adjacent zip codes.

table examines the effect of the scandals on the subset of respondents who reported to be raised as Catholic (in unreported results, we find no effects for individuals raised in other religious denominations). The first two columns explore the effect of the scandals on religious affiliation. Consistent with Hungerman (2013), we find that most of the decline in Catholic affiliation is offset by an increase in affiliation to other religious denominations during the year of the scandal and the following three years. However, this offsetting effect is temporary: after those first three years, all the Catholics affected by the scandals report no religious affiliation. Relative to the mean outcome, a scandal causes a Long-Term decline in Catholic affiliation of 12%. The magnitude of this effect is approximately consistent with the 9% drop in Catholic affiliation implied by the effect of the scandals on the number of religious employees.

In addition to religious affiliation, columns (3) and (4) explore the effect of the scandals on two measures of religious participation: *Attends Church*, which is a dummy variable indicating whether the individual attended church more than once a month, and *Prays*, which is a dummy variable indicating whether the individual prays more than once a week.[22] The averages of these outcomes are 61% and 69%, respectively. The estimates suggest that the scandals had a significant negative effect on church attendance, very similar in magnitude to the effect on Catholic affiliation. This finding confirms that the scandals did not affect only "nominal" Catholics, but also Catholics who actively participated in the congregation. There is also a negative effect on the probability of praying, although it is not significant at the 10% level of statistical significance.

Columns (5) and (6) examine the effect of scandals on two measures of religious beliefs: *Believes in God* and *Believes in Afterlife*, which are dummy variables indicating whether the respondent believes in the existence of God and the afterlife, respectively. The averages of these outcomes are 84% and 71%, respectively. It is important to note that these are widely-studied measures of religious beliefs. For example, similar survey questions are used in the empirical studies of religion and economic outcomes (McCleary and Barro 2006) and religion and pro-social behavior (Putnam and Campbell 2010). Furthermore, using the two years for which the relevant data are available, we find a statistically significant and large correlation between each of these religious beliefs and the probability that the respondent reports giving to charity. Notably, the scandals do not have a significant effect on either of

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[22]The results are similar under alternative definitions of these variables, such as the average number of times of church attendance rather than a dummy for attending more than once a month.

these religious beliefs. The estimates are close to zero and statistically insignificant. Indeed, we can reject the hypothesis that the effect on these religious beliefs is equal to the effect on religious participation. This evidence suggests that a drop in religious participation during adulthood may have a small effect or no effect on religious beliefs.

4 Effect of the Scandals on Charitable Giving and the Provision of Social Services

4.1 Data Sources

In the United States, approximately 90% of churches are actively engaged in the provision of social services (Cnaan et al. 2002), benefit more than 70 million Americans each year (Johnson, Tompkins, and Webb 2002). These services are financed primarily through individual contributions. Indeed, more than one third of total donations of money and volunteer time in the United States goes to religious organizations (Giving USA 2010; Bureau of Labor Statistics 2010). If religious participation is truly important for financing and providing social services, we expect the decline in religious participation to cause a decline in charitable giving and in the provision of social services. However, religious participation may not be so important. For instance, former Catholics may continue to donate to Catholic charities, or they may instead donate to non-Catholic charities. Indeed, some anecdotal accounts go as far as to claim increased support to Catholic charities as a result of the scandals.[23]

Our main measure of charitable giving consists of zip code-level data on individual itemized charitable contributions (i.e., the amount that taxpayers reported as charitable contributions on Schedule A from the 1040 form).[24] These data are prepared by the Statistics of Income Division of the Internal Revenue Service and are available for several years during 1997–2008.[25] We define the outcome variable as the logarithm of mean itemized charitable contributions in the zip code.[26] The mean charitable contribution is $940 (dollar amounts

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[23]See for example: Strauss, Gary (2002), "Lay groups protest scandal with wallets." USA Today, July 31.

[24]Taxpayers file a Schedule A to report their total itemized deductions, which includes charitable contributions, medical expenses, state and local taxes, certain interest expenses, and other miscellaneous deductions. Taxpayers whose standard deduction exceeds these amounts generally do not file Schedule A. Note that itemizers are different in many respects from non-itemizers (e.g., on average they have higher income). According to data from the Panel Study of Income Dynamics for 2000–2008, around 54% of the households that make charitable contributions are itemizers, and the total contributions by non-itemizers constitute just 25% of the total charitable contributions.

[25]Data was collected for tax years 1997, 1998, 2001, 2002 and 2004-2008, but data on itemized charitable contributions was not collected for tax years 1998 and 2001.

[26]We focus on the subset of zip codes that always have at least one dollar in itemized charitable contributions during the sample period, which includes nearly all itemized contributions in the country. The excluded zip codes are very small and were not affected by scandals.

are always expressed in 2009 U.S. dollars), and the mean adjusted gross income is $48,900.

The effect on itemized charitable contributions measures the net effect on all charitable contributions, which is our main object of interest. Additionally, we can test whether the effect on Catholic contributions is partially ameliorated by a positive spillover in contributions to non-Catholic charities. To do so, we exploit data from the Core Files of the National Center for Charitable Statistics from 1989 to 2009. These files are based on administrative data reported by the charities to the IRS. We construct a measure of contributions to three groups of charities: Catholic charities, non-Catholic religious charities, and non-religious charities. These contributions correspond to charities that provide social services but do not include contributions to churches. For further details about these data, see Appendix C.

Last, we measure the effect of scandals on the provision of social services using data from the Zipcode Business Patterns from 1998–2010 (described previously in section 3).[27] We focus on establishments that, according to their NAICS codes, focus on provision of social services for low-income individuals, minorities, and other disadvantaged groups. Examples include soup kitchens, homeless shelters, housing assistance agencies, child welfare services, youth centers, teen outreach services, family welfare services, alcoholism counseling, and immigrant resettlement services. These establishments do not include religious employees as described in the previous section. We define the outcome variable to be the logarithm of the number of employees in these social-services establishments.[28] During the sample period, the average number of social-services establishments in a zip code is 6.5, and the mean number of employees is 128.

4.2   Results

Column (3) of Table 1 shows the effects of the scandals on the logarithm of itemized charitable contributions (Figure 2.c shows the corresponding event-study graph). Similar to the effects on religious participation, the Short-Term effect is negative but statistically insignificant. This finding suggests that, in the first few years after a scandal, Catholics who join other denominations continue to contribute similar amounts to charity. The Long-Term

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[27]We do not use data prior to 1998 because the ZBP classified businesses according to the Standard Industrial Classification (SIC) system, which does not have a one-to-one correspondence with the NAICS system.

[28]We use the sample of zip codes that always have at least one social-services employee over the sample period. These zip codes cover over 95% of all social-services employees in the country. The results are robust if we estimate a Poisson model with fixed effects that includes the remaining 5% of the zip codes.

effect is negative and highly statistically significant. That is, Catholics affected by a scandal decrease giving once they have no religious affiliation. As expected, the Pre-Scandal effect is statistically insignificant, very close to zero, and precisely estimated. As an additional falsification test, column (8) shows the effects of the scandals on gross income instead of charitable contributions and, as expected, there is no evidence of a causal effect on this outcome.[29]

The effects on charitable giving have many similarities with the effects on religious participation. The event-study graph reported in Figure 2.c shows that, similar to religious participation, the effects of the scandals intensify over the first few years after the first public accusation and then stabilize after the sixth year. Column (3) from Table 2 shows that, like in the case of religious participation, the effects on charitable giving are concentrated in the same zip code where the scandal occurs. Column (3) from Table 3 shows that, also like in the case of religious participation, type-A and type-B scandals have similar effects on charitable giving.

The estimated Long-Term effect on charitable contributions is about -1.3%. According to the decline of 3% in religious participation reported in the previous section, these findings suggest that for each percentage point decline in religious participation, charitable giving declines by 0.43 percentage points. Assuming that the effects are driven entirely by Catholics, the implied Long-Term effect of a scandal on Catholic charitable contributions would be about -4.3% (i.e., 1.3% divided by 0.3).

Based on the coefficients obtained from the event-study graph, we estimate a long-lasting effect of the scandals on itemized contributions of about $1.77 billion per year (equivalent to 1.2% of the total itemized contributions in the country as of 2009).[30] Assuming a similar effect on itemizers, the total cost in contributions would be $2.36 billion.[31] To put these magnitudes in perspective, according to Bishop Accountability, the sum of all the lawsuits

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[29]More precisely, the Short-Term and Long-Term effects are statistically significant at the 10% level, but they are very small (0.4%) and equal to the Pre-Scandal effect - meaning that these small differences in outcomes after the scandals occurred are most likely the product of small differences in trends from even before the scandals occurred.

[30]This long-lasting effect is defined as the sum of the effects of the scandals in same and adjacent zip codes after 9+ years. Note that the effect of a scandal after 9+ years reported in Figure 2.c is about twice the coefficient on the Long-Term effect reported in Table 1. The Long-Term effect is roughly a weighted average of the coefficients reported in the event-study that corresponds to the 4+ years after a scandal. Since this average gives more weight to more recent scandals, when the effect of the scandals are still intensifying, the Long-Term effect under-estimates the permanent effect of the scandal (e.g., those that occur nine or more years after the scandal).

[31]This calculation uses the fact that, according to data from the Panel Study of Income Dynamics, itemized charitable contributions comprise 75% of the total charitable contributions.

and other abuse-related costs over the last 40 years is estimated to be about $3 billion (not yearly, but cumulatively). Therefore, the indirect cost of the scandals caused by the decline in charitable contributions seems to be an order of magnitude higher than the direct cost of the scandals to the Catholic churches.

Additionally, we test whether the effect on Catholic contributions is partially ameliorated by a crowding in of contributions to non-Catholic charities. Column (4) from Table 1 shows that there is significant Long-Term effect on contributions to this particular set of Catholic charities of -12.9%. In turn, columns (9) and (10) report the effects on contributions to similar charities affiliated to other religious denominations and without a religious affiliation, respectively. The Long-Term effects on these outcomes are positive, and smaller (in absolute value) than the effect on Catholic charities. However, due to the lack of precision, these effects are not statistically significant at the 10% level. This evidence suggests that a partial crowding-in effect of contributions to non-Catholic charities may have occurred. This evidence is consistent with the evidence in Hungerman (2013) that the Catholic scandals increased contributions to the Baptist Church. In any case, the crowding in was not nearly enough to offset the decline in Catholic giving, as total contributions do not revert to pre-scandal levels even more than ten years after the occurrence of the scandal.

Even though these data distinguish the effects on charitable contributions by the religious affiliation of the charity, they do not distinguish the effects by the religious affiliation of the contributor. As a robustness check, Appendix D measures the effect of the scandals on charitable giving using individual-level data from the Panel Study of Income Dynamics, which contains information about the religious affiliation of the household. As expected, we find that the scandals affect charitable contributions made by Catholic households only.

Last, we examine whether the drop in charitable giving translates into a drop in the provision of social services. On the one hand, Catholic parishes and charities may avoid a decline in social services by cutting other expenditures, by seeking the help of a third party (e.g., the government), or by smoothing local shocks through a central agency (e.g., the Diocese). On the other hand, Catholic parishes and charities may have to cut back social services by more than the drop in charitable contributions, because they prefer to give priority to other types of expenditures, such as wages of religious employees and even abuse-related costs. Column (5) of Table 1 shows the effect of the scandals on the logarithm of the number of employees in social-services establishments. There is a statistically significant

Long-Term effect on this outcome of about -3.6%.[32] This finding is consistent with the prevalent view that these social services are funded at the local level (Cnaan et al. 2012), and thus local shocks to charitable giving should affect the provision of social services.

5 Effect of the Scandals on Pro-Social Beliefs and Other Pro-Social Behavior

5.1 Data Sources

In this section we examine whether the scandals affect just charitable giving or pro-social attitudes more generally. A first approach to test this hypothesis is to measure the impact of the scandals on pro-social beliefs. We use the same General Social Survey dataset described in section 3. The first measure of pro-social beliefs, *Trust*, is based on the question about whether "most people can be trusted." This survey question is the most widely-used in the social capital literature (Glaeser et al. 2000), and it also plays an important role in the literature about religion and pro-social beliefs (Putnam and Campbell 2010). The second measure, *Others Are Fair*, is a variable based on the question "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?" Finally, *Help Others* is based on a question about whether it is important to help others on a scale from most important to least important. The responses to each of these three questions are coded using the Probit-Adapted OLS transformation (van Praag and Ferrer-i-Carbonell 2008) and normalized so that they have a standard deviation of one.[33]

A second approach is to measure the effects of the scandals on other forms of pro-social behavior using zip code-level or county-level aggregates. The first of these measures is individual political campaign contributions. Even though some individuals may contribute to political campaigns because they expect something in return (e.g., influencing the election outcome, a favor from the candidate), some evidence suggests that a substantial portion of campaign contributions are motivated by a pro-social component (Perez-Truglia and Cruces 2013). Unlike charitable contributions, political contributions do not count for itemized charitable contributions (because they are not tax deductible), and religious congregations do not solicit political contributions (Putnam and Campbell 2010). We define the outcome

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[32]Dills and Hernández-Julián (2014) find suggestive evidence for scandals increasing government spending on welfare. However, this increase is relatively small in magnitude compared to the large decline in private donations and the decline of the private social service sector.

[33]The results are the same if we do not use the POLS transformation and instead use an arbitrary scale for coding the responses (e.g., integers from 1 to 4).

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variable as the logarithm of total campaign contributions to presidential candidates[34] at the zip code level, taken from the public records of the Federal Election Commission from the 1997–2000 to the 2009–2012 election cycles.[35]

The second measure of pro-social behavior is the Census mail response rate. As explained in Vigdor (2004), this measure captures pro-social attitudes because responding to the U.S. Census mail forms entails private costs and almost no private benefits, but it has significant social benefits. Essentially, failing to return the Census form increases household enumeration costs and reduces the funding assigned to the individual's community, a fact that is heavily advertised to U.S. households near the time of the Census (for more details, see Vigdor 2004). This outcome has been used as a proxy for pro-social behavior in other studies and is positively correlated with other measures of pro-social behavior (Knack 2002; Vigdor 2004). We define the outcome variable as the logarithm of the Census mail response rate at the zip code level,[36] based on data provided by the U.S. Census Bureau for 2000 and 2010.[37]

The third measure of pro-social behavior is the rate of voting turnout. Research shows that individuals vote not only because they want to affect the election outcome but also because of its pro-social nature (e.g., Gerber et al. 2014). Indeed, some studies argue that religious participation increases voting turnout because of its pro-social nature (Gerber et al. 2014). We define the outcome variable as the logarithm of the rate of turnout at the county level for presidential elections between 1992 and 2008.[38]

5.2 Results

Columns (7) through (9) of Table 4 show the impact of scandals on pro-social beliefs. The three outcomes are constructed such that higher values indicate more pro-social beliefs, and they are standardized to have a mean of zero and standard deviation of one. The estimated

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[34]We restrict our attention to presidential elections because the set of presidential candidates is the same for the entire country and thus contributions to those candidates are more directly comparable across geographic areas.

[35]We focus on the subset of zip codes that have at least one contributor per election cycle. These zip codes comprise over 95% of campaign contributions in the data. The results are identical if instead we use the number of distinct contributors as dependent variable.

[36]This rate is the ratio of the number of mail returns of occupied units and the number of occupied units in the mail back universe. Results are robust if instead we use the ratio of questionnaires returned to total number of households enumerated.

[37]We aggregated census-tract level data to zip-code level data using population weights provided by the Census Bureau.

[38]As discussed in section 3, when using county-level data, we divide the scandal variables by 25,000 county-inhabitants as of 1990 in order to account for the localized effects of the scandals.

Long-Term effects on pro-social beliefs are close to zero and statistically insignificant: 0.044 for trust in others, -0.046 for the belief that others are fair, and 0.006 for the perceived importance of helping others. As a benchmark, the results from columns (1) and (3) suggest that the scandals have an effect on Catholic affiliation and church attendance of approximately -0.2 standard deviations.

Table 5 shows the effects of the scandals on other forms of pro-social behavior. As a benchmark, column (1) shows the effect of scandals on itemized charitable contributions. Columns (2) through (4) show the effects of scandals on political contributions, the Census mail response rate, and voting turnout, respectively. There are no statistically relevant effects on these forms of pro-social behavior. The estimated Long-Term effects are close to zero and statistically insignificant: -0.3% for political contributions, 0.2% for census response rate, and -0.1% for voting turnout. These effects are substantially lower (in absolute value) than the effect of -1.3% on charitable giving. With the exception of political contributions, for which the effects are not precisely estimated, these differences are statistically significant at the 1% level. In the case of the Census response rate, the effects are so precisely estimated that we can reject the possibility of even very small effects (i.e., the 95% confidence interval excludes the possibility of a Long-Term effect below -0.2%). In the case of voting turnout, however, we do not have enough power to reject moderate effects at the 95% confidence level.[39] Taken together, the estimated effects on these various forms of pro-social beliefs and behavior suggest that the scandals do not have a significant effect on general pro-social attitudes.

6 Discussion

In this section, we discuss the implications of our findings for the causal mechanisms that mediate religious participation and pro-social behavior.

First, the evidence suggests that the scandals caused a significant decrease in religious participation and charitable giving, but not in other forms of pro-social beliefs and behavior. This suggests that a drop in religious participation during adulthood may not have a significant effect on general pro-social attitudes. Our evidence suggests that the correlation between religious participation and pro-social attitudes may be spurious (Putnam and Campbell 2010). Indeed, some studies do not even find a significant correlation be-

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[39]For example, if a scandal had a Long-Term effect of -0.6% on voting turnout, then that would imply an elasticity between religious participation and turnout of about 0.2 that would still be consistent with the effects reported in other papers (Gerber et al., 2014).

tween religious participation and pro-social beliefs and behavior (Alesina and La Ferrara 2002; Anderson et al. 2010). Nevertheless, there are two important caveats. First, since Catholicism differs in key aspects from other religious denominations, it is possible that the link between religious participation and pro-social attitudes is weak only for Catholics. Second, even among Catholics, religious participation during childhood (rather than during adulthood) may have a significant effect on general pro-social attitudes.

Second, if religious participation causes higher charitable giving, then we would expect that the decrease in religious participation caused by the scandals should cause a drop in charitable giving. Consistent with this hypothesis, the scandals caused a decline in charitable giving. This decline in charitable giving follows the decline in religious participation in many respects, such as the evolution of the effects over time, the geographic concentration in the same zip code of the scandal, and the similar effects across the two types of scandals. According to our estimates, for each 1% decrease in religious employees because of the scandals, charitable giving declines by 0.43%. If we assume that the effects of the scandals on charitable giving are entirely caused by the decline in religious participation, then that implies an elasticity between religious participation and charitable giving of about 0.43. In comparison, the raw cross-sectional correlation between these two outcomes suggests an elasticity of 0.38 (for details, see Appendix F). Therefore, our estimates suggest that most of the cross-sectional association between religious participation and charitable giving may have the presumed direction of causality (i.e., from religious participation to charitable giving).[40]

There is a potential confounding factor that can lead to an over-estimation of the effect of religious participation on charitable giving: part of the drop in charitable giving may not be the product of the drop in religious participation but rather a direct effect of the scandals and the outrage of Catholics. Several facts suggest that this mechanism had only a second-order effect, if any. First, it is not clear that outrage generates a drop in charitable giving. For example, individuals may want to help more when they find out about injustices. Second, if outrage affected charitable giving directly, it should also affect other forms of pro-social beliefs and behavior, which is not consistent with the evidence presented in section 5. This explanation is also at odds with the fact that, during the first three years after the

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[40]The effects of the scandals on the provision of social services, however, cannot be entirely attributed to the decrease in religious participation. Among other factors, the abuse-related costs may force the charities or parishes to cut back social expenditures even further. Consistent with this augmenting mechanisms, the event-study findings imply a elasticity between religious employees and social-services of about 1, which is substantially above the range 0.55 given by the cross-sectional estimates reported in Appendix F.

scandals, many Catholics joined other denominations, and charitable giving did not fall as much during that time. Last, if the effect of the scandals on charitable giving were caused by outrage about the abuses, then we would expect a decline in charitable giving after child sexual abuse scandals originating in non-religious organizations. Appendix E shows that, on the contrary, there is no significant effect of non-religious abuse scandals on charitable giving.

Our evidence also helps explain the mechanisms through which the drop in religious participation may have translated into a drop in charitable giving. Since we establish that religious beliefs and pro-social beliefs are unaffected by the scandals, they do not appear to be the main mechanism mediating the effect.[41] Our preferred interpretation is that the drop in religious participation translates into a drop in charitable giving because of the advantageous conditions for eliciting contributions from members provided by the social interactions that take place naturally within religious congregations.[42] Indeed, these same social mechanisms are believed to be important determinants of charitable giving even outside the context of religious congregations. For example, congregations can use social interactions for direct solicitation and social pressure (Soetevent 2005; Andreoni and Rao 2011; DellaVigna et al. 2012), to influence social norms about giving (Frey and Meier 2004), to better screen the beneficiaries (Dehejia et al. 2007), and to disseminate information about volunteer opportunities and charitable endeavors. Consistent with our findings, if religious participation declines and thus eliminates these social mechanisms, then charitable giving should decrease, but other forms of pro-social behavior should not be affected.

7   Conclusions

We provided evidence of the significant impact of the Catholic-clergy sexual abuse scandals on the religious and social sector in the United States. The 3,024 scandals in our database caused a long-lasting decline of 50,000 religious employees per year (about 3% of the country's total), $1.77 billion in itemized charitable contributions per year (about 1.2% of the country's total), and 37,000 social-services employees per year (about 3% of the country's

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[41]For example, if religious beliefs had decreased as a result of scandals then the drop in charitable giving could have been even deeper.

[42]These results do not imply that religious participation cannot cause higher charitable giving through other mechanisms in different circumstances. For instance, it is possible that growing up as Catholic increases pro-social behavior through higher religious beliefs. Nevertheless, the magnitude of the effects is suggestive that social mechanisms may go a long way in explaining the observed relationship between religious participation and charitable giving.

total). These indirect costs of the scandals, such as the decline in charitable giving, seem to be an order of magnitude higher than the direct costs of the scandals to the Catholic churches, such as the cost of lawsuit settlements.

We found that, even though religious participation declined, there was not a corresponding decline in religious beliefs. This suggests that changes in religious participation during adulthood may have a limited effect on religious beliefs, at least for the sub-population of Catholics. Additionally, even though the scandals caused a significant drop in religious participation, they did not have a significant effect on various measures of pro-social beliefs and other pro-social behavior besides charitable giving. This finding suggests that changes in religious participation during adulthood may have little or no effect on pro-social attitudes, again, at least for Catholics.

Our findings also help explain the strong cross-sectional correlation between religious participation and charitable giving. Assuming that the drop in charitable giving due to the scandals was entirely due to the corresponding drop in religious participation, our estimates imply that most of the observed correlation between religious participation and charitable giving has the presumed direction of causality. Our study also helps disentangle the mechanisms through which the decline in religious participation caused by the scandals translated into the decline in charitable giving. Our preferred interpretation lies on the advantageous conditions for eliciting contributions provided by the social interactions taking place in religious congregations. Indeed, these same social mechanisms are believed to play a primary role for eliciting contributions even beyond the context of religious congregations.

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Figure 1: Geographic distribution of Catholic-clergy sexual abuse scandals

a. Scandals in the Contiguous U.S. States
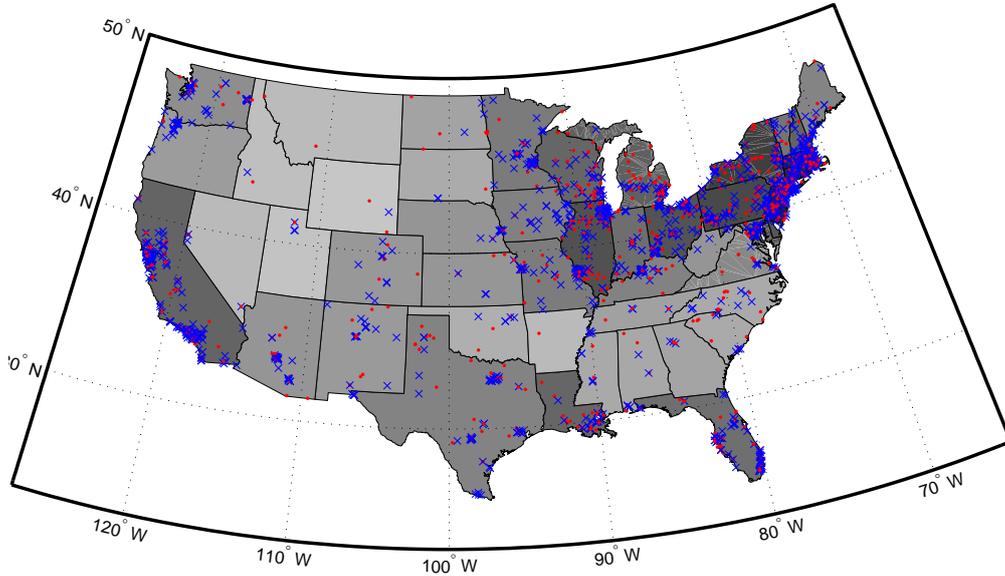
b. Scandals in the State of New Jersey
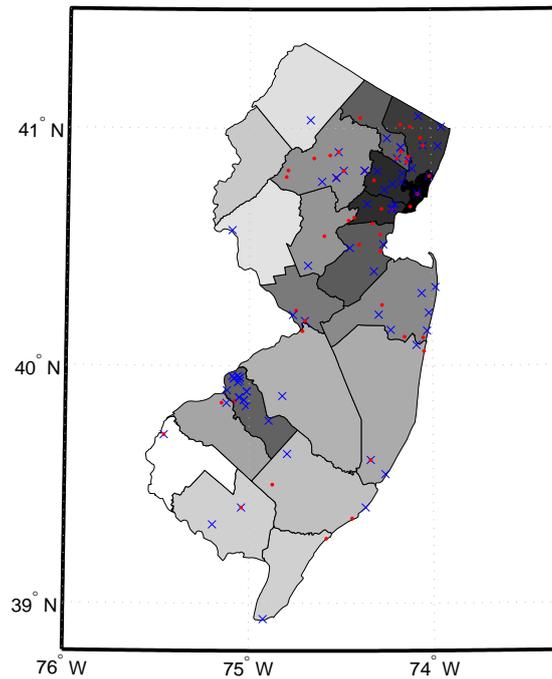
Notes: Each red dot corresponds to the location of a type-A scandal, and each blue X denotes the location of a type-B scandal. The location of a type-A scandal corresponds to the institution where the clergyman is working at the time of the accusation, whereas the location of a type-B scandal corresponds to the institution where the clergyman allegedly committed the abuse. See section 2.1 for a description of the data and more detailed definitions for type-A and type-B scandals. In panel a. (b.), the color of each state (county) corresponds to the log of the density of Catholic adherents per square mile as of 1990 (darker shading for higher density of Catholic adherents)—according to data from the 1990 U.S. Census and the 1990 Religious and Congregations Membership Study. Alaska and Hawaii are not shown on the maps, but they are included in the database.

Figure 2: Graphical event-study analysis of the effects of scandals on main outcomes

a. Number of Catholic schools
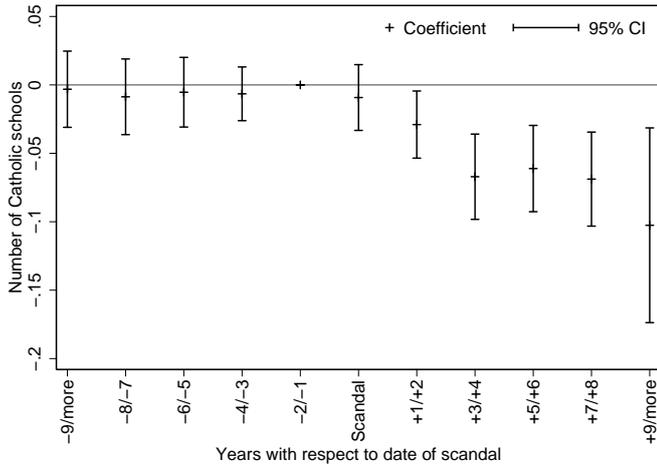
b. Log(Religious employees)
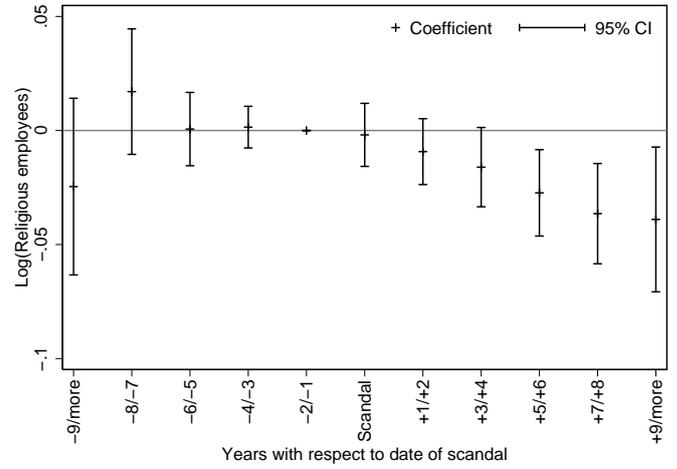
c. Log(Itemized charitable contributions)
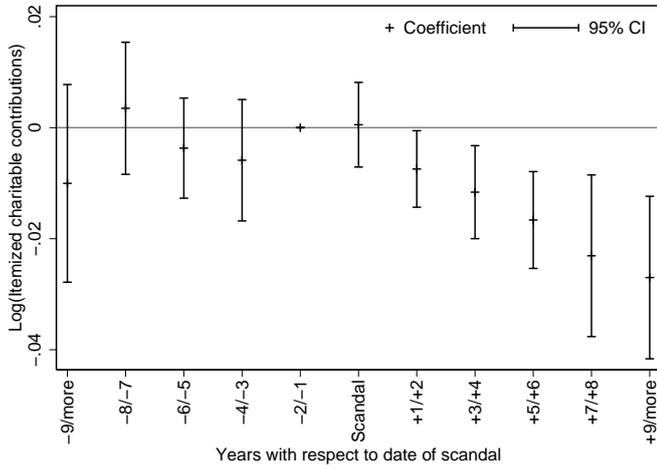
d. Log(Social Services Employees)
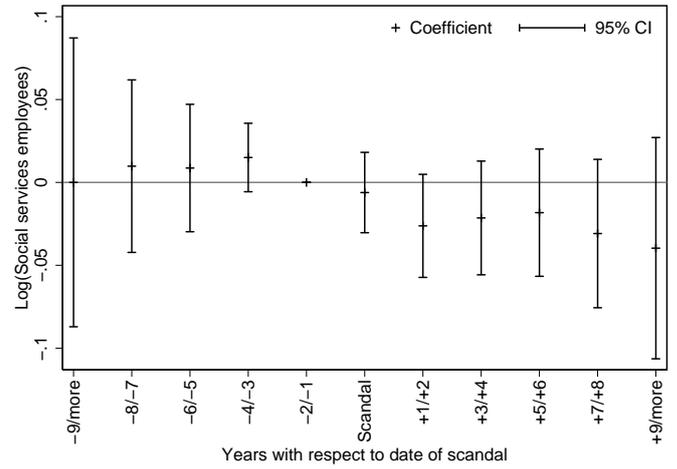
e. Log(Charitable contributions to Catholic charities)
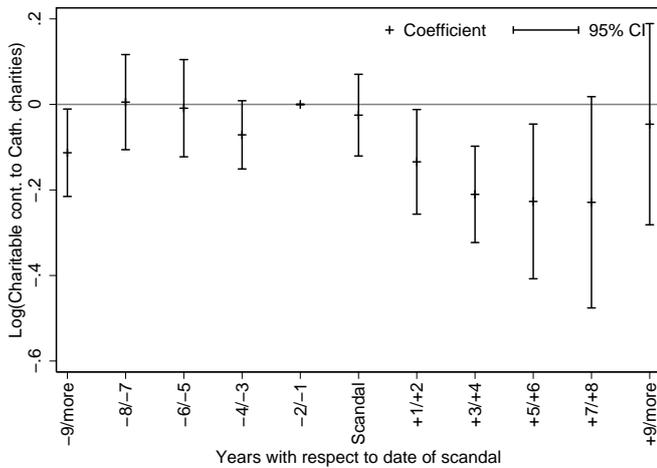
f. Log(Charitable contributions to other-rel. charities)
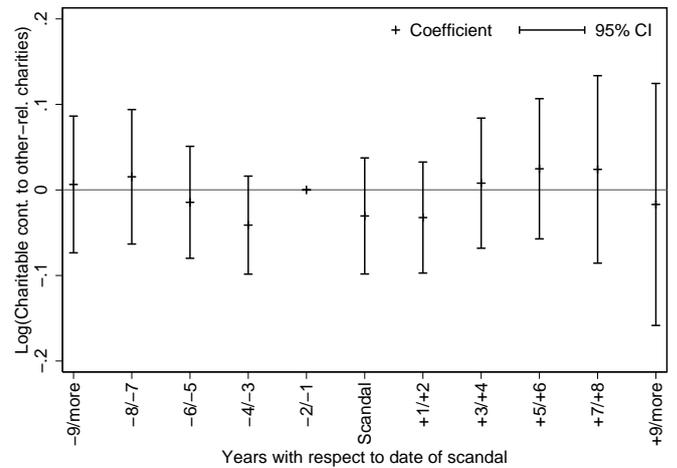
Notes: See Appendix A for a description of the event-study methodology. Each bracket represents the 95% confidence interval, and the center of the bracket represents the corresponding point estimate. Confidence intervals were constructed with heteroskedasticity-robust standard errors, clustered at the zip code level. The coefficient for the group "-2/-1" (i.e., the two years prior the scandal) was normalized to zero. The regressions include zip code fixed effects and time effects—for more details and the full list of control variables see subsection 2.2. See Table G.1 and its note for descriptive statistics, data definitions and data sources.

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Table 1: Effects of scandals on main and placebo outcomes

	Main Outcomes					Non-Catholic and Placebo Outcomes				
	(1) Catholic Schools	(2) Log(Rel. Employees)	(3) Log(All Cont.)	(4) Log(Catholic Cont.)	(5) Log(Social Services)	(6) Other-Rel. Schools	(7) Log(Retail Employees)	(8) Log(Income)	(9) Log(Other-Rel. Cont.)	(10) Log(Non-Rel. Cont.)
Short-term effect (0-3 years)	-0.023* (0.012)	-0.011 (0.008)	-0.003 (0.003)	-0.069 (0.051)	-0.032** (0.015)	0.012 (0.015)	-0.004 (0.010)	0.004** (0.002)	-0.021 (0.035)	0.008 (0.035)
Long-term effect (4+ years)	-0.068*** (0.016)	-0.030*** (0.010)	-0.013*** (0.004)	-0.129* (0.072)	-0.036** (0.018)	0.004 (0.017)	0.006 (0.011)	0.004* (0.002)	0.031 (0.042)	0.059 (0.044)
Pre-scandal effect	0.007 (0.011)	-0.001 (0.005)	0.004 (0.004)	0.061 (0.040)	-0.012 (0.013)	0.003 (0.018)	0.002 (0.007)	0.004*** (0.001)	0.008 (0.030)	-0.038 (0.054)
Observations	64,746	247,676	175,415	4,067	110,630	64,746	106,366	242,733	57,925	27,113
No. of zipcodes	5,886	19,052	25,668	437	8,510	5,886	8,385	27,431	6,478	2,844

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Notes: Each column corresponds to a different OLS regression. Short-term (long-term) measures the average effect of the scandals during the 0 to 3 (4 or more) years after it took place. The variable Pre-scandal is included as a "placebo test" measuring the effect of a scandal 1-2 years before they happen. The regressions includes zip code fixed effects and time effects—for more details and the full list of control variables see subsection 2.2. *Catholic Schools* is the number of Catholic schools in the zip code, covering the period 1990-2010 (bi-anually). *Rel. Employees* is the number of employees working in religious stablishments in the zip code, covering the period 1994-2010. *All Cont.* is the mean itemized charitable contributions in the zip code, covering the years 1997, 2002, 2004-2008. *Catholic Cont.* is the total charitable contributions to Catholic charities that provide social services in the zip code, covering the period 1989-2009. *Social Services* is the number of employees working in establishments that provide social services in the zip code, covering the period 1998-2010. *Other-Rel. Schools* is the number of schools of Non-Catholic religious denominations in the zip code, covering the period 1990-2010 (bi-annually). *Retail Employees* is the number of employees working in supermarket and car dealerships in the zip code, covering the period 1998-2010. *Income* is the mean gross income in the zip code, covering the years 1997, 1998, 2001, 2002, 2004-2008. *Other-Rel. Cont.* (*Non-Rel. Cont.*) is the charitable contributions to non-Catholic religious (non-religious) charities that provide social services in the zip code, covering the period 1989-2009. See Table G.1 for descriptive statistics, and its note for data definitions and data sources. Heteroskedasticity-Robust standard errors in parentheses, clustered at the zip code level. Stars indicate significance level: * p<0.1, ** p<0.05, *** p<0.01.

Table 2: Geographic distribution of the effects of scandals on main outcomes

	(1) Catholic Schools	(2) Log(Rel. Employees)	(3) Log(All Cont.)	(4) Log(Catholic Cont.)	(5) Log(Social Services)
Effects on same zip:					
Short-term effect	-0.022*	-0.010	-0.003	-0.083	-0.031**
(0-3 years)	(0.012)	(0.009)	(0.003)	(0.051)	(0.015)
Long-term effect	-0.059***	-0.026***	-0.012***	-0.115	-0.036*
(4+ years)	(0.016)	(0.010)	(0.004)	(0.072)	(0.018)
Pre-scandal effect	0.006	-0.002	0.004	0.057	-0.013
	(0.011)	(0.005)	(0.004)	(0.043)	(0.013)
Effects on adjacent zip code:					
Short-term effect	-0.005	-0.005*	0.001	0.020	-0.009
(0-3 years)	(0.006)	(0.003)	(0.001)	(0.027)	(0.007)
Long-term effect	-0.024***	-0.012***	-0.002	-0.008	-0.012
(4+ years)	(0.007)	(0.004)	(0.002)	(0.044)	(0.008)
Pre-scandal effect	-0.001	0.000	-0.001	0.012	-0.003
	(0.005)	(0.002)	(0.002)	(0.028)	(0.006)
Effects on adjacent-to-adjacent zip code:					
Short-term effect	-0.001	0.002	0.001	0.034	0.003
(0-3 years)	(0.003)	(0.001)	(0.001)	(0.026)	(0.004)
Long-term effect	-0.008**	0.001	-0.000	0.010	0.007
(4+ years)	(0.003)	(0.002)	(0.001)	(0.044)	(0.004)
Pre-scandal effect	-0.000	0.001	-0.000	0.007	0.005
	(0.003)	(0.001)	(0.001)	(0.026)	(0.003)
Observations	64,746	247,676	175,415	4,067	110,630
No. of zip codes	5,886	19,052	25,668	437	8,510

Notes: Each column corresponds to a different OLS regression. Short-term (long-term) is the coefficient for the variables that count the number of scandals in the corresponding zipcode within the past 0 to 3 (4 or more) years. The variable Pre-scandal is included as a "placebo test" that equals the number of scandals occurring in the future 1-2 years. The regressions includes zip code fixed effects and time effects—for more details and the full list of control variables see Appendix A. See Table G.1 for descriptive statistics, and its note for data definitions and data sources. Heteroskedasticity-Robust standard errors in parentheses, clustered at the zip code level. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 3: Effects of scandals on main outcomes, by type of scandal

	(1) Catholic Schols	(2) Log(Rel. Employees)	(3) Log(All Cont.)	(4) Log(Catholic Cont.)	(5) Log(Social Services)
Effects of Type-A scandals:					
Short-term effect (0-3 years)	0.019 (0.025)	-0.013 (0.016)	-0.009 (0.007)	-0.007 (0.143)	-0.047 (0.035)
Long-term effect[i] (4+ years)	-0.036 (0.032)	-0.032 (0.020)	-0.017** (0.008)	-0.117 (0.224)	-0.039 (0.042)
Pre-scandal effect	0.044** (0.022)	0.005 (0.011)	-0.003 (0.008)	-0.057 (0.121)	0.000 (0.024)
Effects of Type-B scandals:					
Short-term effect (0-3 years)	-0.042** (0.018)	-0.010 (0.010)	0.000 (0.004)	-0.092 (0.070)	-0.026 (0.019)
Long-term effect[ii] (4+ years)	-0.081*** (0.023)	-0.028** (0.013)	-0.011** (0.005)	-0.132 (0.091)	-0.036 (0.023)
Pre-scandal effect	-0.010 (0.016)	-0.004 (0.007)	0.008 (0.005)	0.090* (0.047)	-0.018 (0.016)
P-value of test (i)=(ii)	0.323	0.894	0.588	0.954	0.949
Observations	64,746	247,676	175,415	4,067	110,630
No. of zipcodes	5,886	19,052	25,668	437	8,510

Notes: Each column corresponds to a different OLS regression. Short-term (long-term) is the coefficient for the variables that count the number of scandals in the corresponding zipcode within the past 0 to 3 (4 or more) years. The variable Pre-scandal is included as a "placebo test" that equals the number of scandals occurring in the future 1-2 years. The regressions includes zip code fixed effects and time effects—for more details and the full list of control variables see Appendix A. See Table G.1 for descriptive statistics, and its note for data definitions and data sources. Heteroskedasticity-Robust standard errors in parentheses, clustered at the zip code level. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 4: Effects of scandals on religious participation, religious beliefs and pro-social beliefs

	Rel. Affiliation		Rel. Participation		Rel. beliefs		Non-rel. beliefs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Attends		Believes	Believes in	Trust	Thinks	Wants to
	Catholic	Other-Rel.	Church	Prays	in God	Afterlife	in Others	Others Are Fair	Help Others
Short-term effect (0-3 years)	-0.132**	0.101**	0.096	0.012	0.039	-0.006	-0.124	-0.017	-0.035
	(0.052)	(0.050)	(0.062)	(0.078)	(0.067)	(0.070)	(0.162)	(0.159)	(0.146)
Long-term effect (4+ years)	-0.090**	0.014	-0.093**	-0.086	-0.004	0.023	0.044	-0.046	0.006
	(0.043)	(0.022)	(0.039)	(0.058)	(0.062)	(0.035)	(0.158)	(0.113)	(0.111)
Pre-scandal effect	0.002	-0.006	-0.003	-0.003	0.020*	0.015	-0.023	0.000	-0.001
	(0.008)	(0.004)	(0.011)	(0.009)	(0.010)	(0.011)	(0.021)	(0.023)	(0.022)
Observations	7,469	7,469	7,444	4,913	3,549	5,724	4,734	4,422	4,071
No. of counties	319	319	319	311	293	317	314	314	312

Notes: Each column corresponds to a different OLS regression. Sample only includes individuals that responded to have been raised as Catholic. *Catholic* and *Other-Rel.* are dummy variables indicating whether the respondent is Catholic or has another religion, respectively (atheist/agnostic is the omitted category). *Attends Church* is a dummy variable indicating whether the individual reported to attend religious services more than once a month. *Prays* is a dummy variable indicating whether the respondent prays more than once a week. *Believes in God* (*Believes in Afterlife*) is a dummy variable indicating whether the respondent believes in the existence of God (the Afterlife). *Trust*, *Others Are Fair* and *Help Poor* correspond to survey questions whose responses were transformed using the POLS method and stardardized so that each has a standard deviation of one. See Table G.2 for descriptive statistics, and its note for more details about the data. All regressions include county-specific fixed effects, time effects, the interaction between time effects and the share of Catholics in the county as of 1990, plus a set of individual control variables: gender, age, age squared, dummies for black and white, three dummies about marital status, household income, number of children, education and a set of four dummies for employment status. Short-term (long-term) is the coefficient for the variables that count the number of scandals in the corresponding zipcode within the past 0 to 3 (4 or more) years. The variable Pre-scandal is included as a "placebo test" that equals the number of scandals occurring in the future 1-2 years. The three variables are normalized by dividing by 25,000 inhabitants in the county according to the 1990 U.S. Population Census. Survey data from the General Social Survey, 1993-2010. Heteroskedasticity-Robust standard errors in parentheses, clustered at the county level. Stars indicate significance level: * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Effects of scandals on charitable giving and other forms of pro-social behavior

	(1) Log(Char. Contribution)	(2) Log(Political Contribution)	(3) Log(Census Resp. Rate)	(4) Log(Voting Turnout)
Short-term effect (0-3 years)	-0.003	0.042*	-0.002	0.003
	(0.003)	(0.021)	(0.003)	(0.004)
Long-term effect (4+ years)	-0.013***	-0.003	0.002	-0.001
	(0.004)	(0.019)	(0.002)	(0.004)
Pre-scandal effect	0.004	-0.018	0.003	-0.015
	(0.004)	(0.027)	(0.003)	(0.044)
Zip/County level data	Zip	Zip	Zip	County
Observations	175,415	43,748	45,619	15,213
No. of zipcodes/counties	25,668	10,937	27,032	3,090

Notes: Each column corresponds to a different OLS regression. Columns (1)-(3) use zip code level data, while column (4) uses county level data. Short-term (long-term) is the coefficient for the variables that count the number of scandals in the corresponding zipcode or county within the past 0 to 3 (4 or more) years. The variable Pre-scandal is included as a "placebo test" that equals the number of scandals occurring in the future 1-2 years. The regressions includes zip code (or county) fixed effects and time effects—for more details and the full list of control variables see Appendix A. See Table G.1 for descriptive statistics, and its note for data definitions and data sources. Heteroskedasticity-Robust standard errors in parentheses, clustered at the zip code level. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Online Appendix: NOT FOR PUBLICATION

A Econometric Model of the Event-Study Analysis

Taking advantage of the detailed scandals data, our identification strategy is based on an event-study analysis.[43] That is, we study how a given outcome evolves during the years before and after the timing of a scandal. Consider the outcome variable $y_{z,t}$ (e.g., number of religious employees, mean charitable giving) observed in zip codes $z = 1, ..., N$ during the years $t = 0, ..., T$. Define the set of variables $d_{z,t}^s$ for all integers s from $-s_0$ to s_0. If $-s_0 < s < s_0$, then $d_{z,t}^s$ takes the value of the number of scandals in zip code z at time $t - s$. Therefore, if a zip code had its first and only scandal in year 2004, $d_{z,t}^1$ takes the value 1 iff $t = 2005$, $d_{z,t}^2$ takes the value 1 iff $t = 2006$, an so on. The variable $d_{z,t}^{s_0}$ ($d_{z,t}^{-s_0}$) takes the value of the number of scandals in zip code z from $t - s_0$ backwards (from $t + s_0$ onwards). Let the set S include all integers from $-s_0$ to s_0 except -1. The regression model is:

$$y_{z,t} = \sum_{s \in S} \gamma_s d_{z,t}^s + \mu_z + \delta_t + X_{z,t}\beta + \epsilon_{z,t}$$

The parameter μ_z represents the zip code (or county - depending on the geographic aggregation of the data) fixed effects, accounting for any fixed differences between zip codes; δ_t denotes the year effects; $X_{z,t}$ denote control variables that vary both over zip code and over time; and $\epsilon_{z,t}$ denotes the usual error term. All regressions using zip code-level data include the same set of control variables.[44] We include the interaction between the year effects and a set of zip code characteristics as of 1990. These interactive terms account for any differences in the evolution of the dependent variable across affected and unaffected zip codes that can be traced back to differences in observable zip code characteristics. For instance, analysis on the locations of scandals shows that they are more likely to take place in more populated zip codes, because of the larger pool of potential perpetrators and victims (see Appendix B). Given that Catholics are more concentrated in some states, we also include state-specific time trends. The inclusion of these control variables does not affect the magnitude of the effects significantly, but it does improve the precision of the estimates by reducing the variance of the error term (results are robust to alternative specifications;

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[43]For an introduction to event-studies, see for example: MacKinlay, A. Craig. (1997), "Event studies in economics and finance." Journal of economic literature, Vol. 35 (1), pp. 13-39.

[44]Some regressions include a few additional control variables that are obtained from the same data source as the dependent variable. Because of smaller sample sizes, the regressions with survey data include fewer control variables.

for example, restricting the sample to zip codes that are near to at least one scandal).. We always present standard errors clustered at the zip code level, but the results are robust if we use standard errors clustered at the county or state level.

Since γ_{-1} is normalized to zero, the coefficients $\{\gamma_s\}_{s \in S}$ indicate how the outcome variable changes with respect to the year prior to a scandal. We report the coefficients in graphical form, with their corresponding 95% confidence intervals (see for example Figure 2). If the coefficients $\{\gamma_s\}_{s>0}$ (i.e., the coefficients in the figure to the right of the date of the scandal) are negative, that means that the outcome variable declines in the aftermath of a scandal. If γ_s is negative only for some $0 < s \leq \bar{s} < s_0$, then that means that the decline in the outcome variable is transitory, lasting up to $\bar{s}$ years after the date of a scandal. If the coefficients in $\{\gamma_s\}_{s<0}$ (i.e., the coefficients in the figure to the left of the date of the scandal) are positive, that would mean that the outcome variable was already declining before the date of the scandal.

For each of the dependent variables we find that, during the years before a zip code is affected by a scandal, the outcome variable evolves exactly like in zip codes without scandals (i.e., $\gamma_s = 0$ for $s < 0$). An alternative interpretation for this finding is that it would not be possible to predict when or where a scandal is going to occur based on the evolution of the outcome variable. Two institutional factors may have contributed to this finding. First, the 2002 *Boston Globe* article worked as an exogenous trigger of a substantial number of the scandals. Second, there is a large time lapse between when the alleged abuses were perpetrated and when the accusations were made: even though most of the abuses took place before 1985, most of the accusations came to light in the 2000s.

In addition to the graphical representation, we report regression tables with a "compact" version of the event-study analysis. This specification replaces the $d_{z,t}^s$'s by three variables: $S_{z,t}^{Short-Term}$ is equal to the number of scandals that occurred between t and $t-3$, $S_{z,t}^{Long-Term}$ is equal to the number of scandals that occurred in the zip code at $t \leq 4$, and $S_{z,t}^{Pre-Scandal}$ is equal to the number of scandals that occur in the future 2 years. If the coefficient on $S_{z,t}^{Pre-Scandal}$ were not zero, that would indicate that the outcome variable was already increasing/decreasing before the date of the scandal. Therefore, the coefficient on $S_{z,t}^{Pre-Scandal}$ provides a test of differential trends between zip codes affected and unaffected by scandals. The fact that the definition of $S_{z,t}^{Pre-Scandal}$ is arbitrary is not particularly relevant since the event-study figures show the full picture of how the coefficients evolve before and after a scandal.

B Understanding the Geographic Distribution of the Scandals

In order to to provide some insight about the geographic distribution of scandals, this subsection explores the characteristics associated to the presence of scandals at the county and zip code level.

Figure B.1 shows the distribution of scandals over time. It illustrates the magnitude of the outbreak of scandals following the 2002 *Boston Globe* article. Almost by definition, a scandal must take place in a location with at least one Catholic institution. Figure 1 shows the geographic distribution of the scandals across the U.S. and in the state of New Jersey (for an animation visit this link). States (or counties) colored with a darker shade indicate a higher density of Catholic adherents per square mile. As expected, the scandals are more concentrated in states and cities with high concentration of Catholics. Figure B.2 illustrates the strong correlation between the average Catholic population in a county versus the number of scandals in that county.

As a thought experiment, imagine that we go back to the year 2000 and we try to predict the number of scandals that will take place in the period 2001-2010 for each county in the U.S. We estimate a regression where the dependent variable is the number of scandals that actually happened during that period and the independent variables are characteristics of those counties (e.g., population size) as of the year 2000. Since perpetrators and victims were Catholic, the main predictor of the number of scandals is the size of the Catholic congregation. For the number of Catholic adherents per county we use the 2000 Religious Congregations and Membership Study (RCMS), collected by the Association of Statisticians of American Religious bodies. For data on county-level socio-economic characteristics we use the 2000 U.S. Census. Table B.1 shows descriptive statistics and data definitions for all the variables used in the regression analysis.

Table B.2 shows the regression results. Columns (1) to (3) show the results using the number of type-A scandals as dependent variable, while columns (4) to (6) reproduce the results for the type-B scandals. The coefficients reported are incidence-rate ratios from a Negative Binomial Regression. The coefficient on the logarithm of the number of Catholics from column (1) is 2.37, which means that an increase of 0.1 logs in the number of Catholic adherents (approximately a 10% increase) would increase the expected number of scandals by about 13.7% (that coefficient is roughly similar in column (4)). Column (2) suggests that, holding the number of Catholics constant, the expected number of scandals does not

seem to be influenced by the total population (the corresponding coefficient from column (5) is also small). Last, the regressions in column (3) and (6) include a number of additional explanatory variables. These variables are standardized by dividing them by their corresponding standard deviation, so all the standardized variables have a standard deviations equal to 1. Thus, their coefficients can be directly interpreted as the incidence-rate ratios for a one-standard deviation increase in the corresponding variable. The results from columns (3) and (6) suggests that some county characteristics help predicting the number of scandals, especially for type-B scandals. However, a comparison of the pseudo-R^2 with and without the additional explanatory variables indicates that—once we control for the size of the Catholic population—other county characteristics have little power to explain the distribution of scandals across counties. Some of those county characteristics could simply be picking up variation from measurement error in our measure for Catholic population.

The majority of the analysis in this paper is done with zip code level data. Zip codes—created by the U.S. Postal Service as a tool to help deliver the mail more efficiently—are in many aspects not ideal for research purposes. Unlike census tracts, when a zip code changes its definition it does not change its name. In a few cases these changes are dramatic, but more commonly they are small and subtle. To the best of our knowledge, there is no existing geographic equivalency file relating zip codes at different points in time. We dropped zip codes from our database whenever we found evidence that their definitions changed substantially over time (about 1% of zip codes). Fortunately, the sample size with administrative data is so large that the measurement error introduced by the imperfections of the zip code geography is not a major concern. We are interested in whether the zip codes that experienced scandals are different in some dimensions from the zip codes that did not suffer any scandals, and from their adjacent zip codes. Unfortunately, we do not have zip code-level data on the size of religious congregations, so we cannot perform the same regression analysis that we did for the county-level data above. Nevertheless, we can still use Census data on several interesting socio-economic indicators. Table B.3 provides a comparison of a selection of zip code characteristics (e.g., population, land size) among those where there was a scandal, zip codes that did not suffer any scandals but are adjacent to a zip code that suffered a scandal, and zip codes that did not suffer scandals and are not adjacent to a zip code with scandals either.

The first row provides the most important fact: scandals tend to happen in zip codes that are more populous. The difference is not only statistically significant, but also econom-

ically significant. The reason is straightforward: more people translates to more Catholic churchgoers and priests, so the potential number of abusers and victims is higher. The second and third rows are an immediate consequence of the first row: since more populous zip codes are on average smaller in area and more urban, scandals tend to happen in zip codes that are smaller and more urban. The differences in other dimensions are sometimes statistically significant, but economically not very significant. Furthermore, the differences in those other variables are most likely explained by the differences in population size: e.g., median income is increasing in population density, and since zip codes with scandals have higher density it is not surprising that median income is a little higher in the zip codes that had scandals. Last, it is interesting to note that zip codes with scandals are more similar to those adjacent to the scandals than to the rest of the zip codes, because large zip codes tend to be adjacent to other large zip codes.

Figure B.1: Distribution of Catholic clergy sexual abuse scandals over time
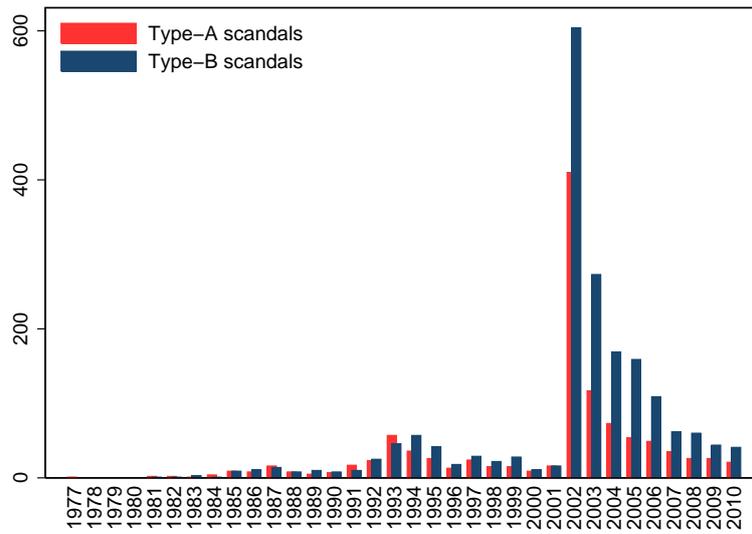
Notes: Data on Catholic-clergy abuse scandals compiled by the authors. See section 2.1 for a description of the data.

Figure B.2: Relationship between Catholic clergy sexual abuse scandals and Catholic population
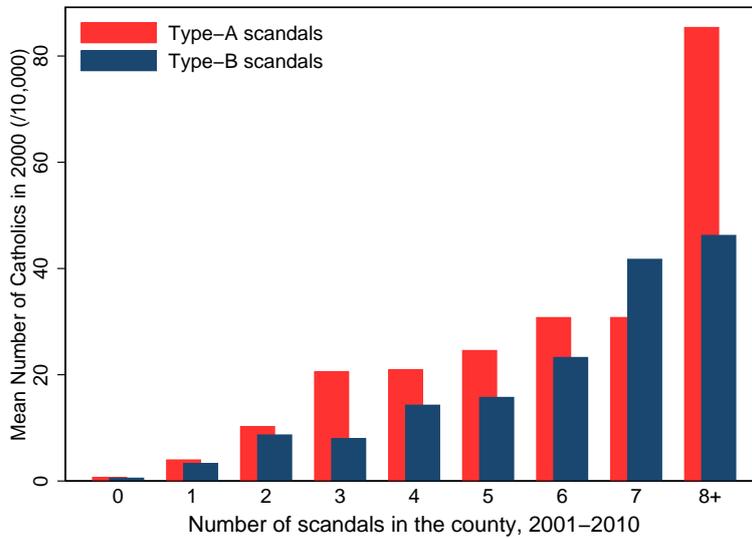
Notes: The bars indicate the average number of Catholic adherents in the counties with the same number of scandals. Data on mean number of Catholics by county as of year 2000 obtained from the Religious Congregations and Membership Study. The number of scandals correspond to the totality of scandals in the period 2001-2010. See section 2.1 for a description of the scandals data.

Table B.1: Summary statistics for county-level data on socio-economic characteristics

	Obs	Mean	Sd	Min	Max
No. of type-A Scandals	2956	0.27	1.12	0.00	16.00
No. of type-B Scandals	2956	0.51	2.47	0.00	46.00
No. of Catholic Adherents (/10,000)	2956	2.10	10.43	0.00	380.64
Population (/10,000)	2956	9.44	30.08	0.04	951.93
Share Urban Pop. (%)	2956	41.85	30.60	0.00	100.00
Share White Pop. (%)	2956	84.93	16.11	4.51	99.74
Share Hispanic Pop. (%)	2956	6.42	12.30	0.08	97.54
Share College Graduates (%)	2956	16.80	7.81	4.92	63.75
Mean HH Income (/1,000)	2956	36.80	9.23	13.12	85.72

Notes: Data on the number of Catholic adherent per county from the 2000 Religious Congregations and Membership Study (RCMS). Other county characteristics obtained from the 2000 U.S. Population Census. Share college is the share of the population 25 years old or older that completed college education, and Median Income is the median income in the county in 2000 U.S. dollars. The number of scandals is the count of scandals after the year 2000. Data on scandals was compiled by the authors.

Table B.2: Analysis of factors associated to the number of scandals in a county

	No. of Type-A scandals			No. of Type-B scandals		
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Catholic adherents)	2.373***	2.194***	2.324***	2.772***	2.266***	2.381***
	(0.066)	(0.139)	(0.155)	(0.085)	(0.139)	(0.157)
Log(Total Population)		1.111	1.236**		1.318***	1.350***
		(0.086)	(0.116)		(0.104)	(0.123)
Share Urban (standardized)			0.789**			1.100
			(0.088)			(0.130)
Share White (standardized)			1.003			0.800**
			(0.089)			(0.085)
Share Black (standardized)			0.966			0.692***
			(0.094)			(0.079)
Share Hispanic (standardized)			0.813***			0.713***
			(0.052)			(0.044)
Share College (standardized)			1.019			1.057
			(0.057)			(0.064)
Mean Income (standardized)			0.963			0.814***
			(0.053)			(0.046)
Observations	2956	2956	2956	2956	2956	2956
Pseudo-R-Squared	0.30	0.30	0.31	0.32	0.32	0.33

Notes: The coefficients are incidence-rate ratios from a Negative Binomial Regression. The dependent variable is the number of scandals in the county after the year 2000. Data on Catholic adherence rates obtained from the 2000 Religious Congregations and Membership Study. Data on county characteristics obtained from the 2000 U.S. Population Census. The county characteristics that appear in columns (3) and (6) have been standardized by dividing them by their corresponding standard deviations. See Table B.1 for descriptive statistics about the data, and its note for data definitions. Heteroskedasticity-robust standard errors in parentheses. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table B.3: Zip code characteristics by proximity to a religious scandal

	Type-I Scandal			Type-II Scandal		
	(1) Same	(2) Adjacent	(3) Neither	(4) Same	(5) Adjacent	(6) Neither
Population (/1000)	23.049 (16.107)	12.954 (14.174)	8.257 (11.293)	26.381 (17.547)	12.335 (13.215)	7.283 (10.068)
Area (miles)	57.762 (129.759)	66.829 (133.443)	95.198 (184.029)	43.895 (114.392)	67.761 (133.902)	100.663 (193.264)
Share Urban (%)	77.209 (34.002)	49.500 (45.021)	31.427 (41.198)	84.416 (28.789)	50.022 (44.573)	27.943 (39.420)
Share White (%)	83.657 (21.599)	86.984 (20.282)	88.428 (18.279)	81.299 (22.811)	86.839 (20.174)	89.147 (17.528)
Share Black (%)	9.730 (18.477)	7.963 (17.186)	7.636 (15.991)	10.564 (19.571)	7.604 (16.313)	7.364 (15.583)
Share Indian (%)	0.597 (2.152)	0.889 (4.402)	1.107 (5.187)	0.590 (1.388)	0.801 (3.215)	1.132 (5.379)
Share Asian (%)	2.512 (5.787)	1.775 (5.366)	1.008 (3.667)	3.101 (7.658)	1.899 (5.690)	0.815 (3.074)
Share Hispanic (%)	7.982 (15.908)	5.458 (12.560)	3.985 (10.424)	10.135 (18.073)	5.978 (13.505)	3.447 (9.278)
Average HH Size	2.625 (0.327)	2.673 (0.345)	2.663 (0.325)	2.627 (0.363)	2.674 (0.361)	2.663 (0.313)
Share college (%)	7.463 (5.770)	6.199 (5.418)	5.061 (4.567)	8.050 (6.510)	6.097 (5.131)	4.877 (4.488)
Share school (%)	17.319 (4.159)	18.221 (4.552)	18.927 (4.554)	17.003 (4.340)	18.277 (4.690)	19.033 (4.481)
Median Income (/1000)	32.337 (12.796)	31.209 (13.390)	27.093 (10.704)	32.784 (12.515)	31.451 (13.705)	26.636 (10.371)
Observations	851	3288	18462	1072	3219	17160

Notes: Data on zip code characteristics obtained from the 1990 U.S. Population Census. *Share college* is the share of the population 25 years old or older that completed college education, *Share School* is the share of the population enrolled in school and *Median Income* is the median income in the county in 2000 U.S. dollars. Data on scandals was compiled by the authors (see section 2.1 for a description of the data). The zip codes under the title "Same" are those that were affected by at least one scandal. The zip codes under "Adjacent" are those that were never affected by a scandal, but are adjacent to at least one zip code in the group "Same." Zip codes under "Neither" are those zip codes that do not belong to "Same" nor "Adjacent." The sample excludes zip codes that are non-standard, and zip codes that changed substantially over time.

C Measuring Contributions with Data from the NCCS Core Files

This Appendix provides details about the definition of charitable contributions using the NCCS Core Files. The National Center for Charitable Statistics (NCCS) produces this data using administrative data provided by the IRS. The data covers all 501(c)(3) organizations that complied with the requirement to file a 990 or 990-EZ Form. The IRS does not record data for organizations that had less than $25,000 in gross receipts or were religious congregations. Note that this data includes contributions from itemizers as well as non-itemizers. Since organizations must provide an address with their returns, we are able to aggregate them at the zip code level to form a panel.

In order to identify a group of religious-affiliated charities oriented towards social services, we take advantage of the National Taxonomy of Exempt Entities (NTEE) system used by the IRS and the NCCS to classify nonprofit organizations. Note that churches are not included in these organizations. Organizations under code X are religious-affiliated organizations, with the subcategories within the X code denoting the particular denomination or group of denominations with which the nonprofit is affiliated. We identify 478 organizations under code X22, denoting Catholic charities. Organizations under code X are more heterogeneous than organizations in other NTEE codes (e.g. NTEE code B25 only includes schools). Upon inspection of the websites for a random sample of organizations under NTEE code X, most of them provide at least some type of social service. This focus of faith-based organizations on delivering services to disadvantaged populations rather than facilitating worship and ritual practices is in line with the findings of comprehensive studies of the religious charitable sector (Cnaan et al., 2002). Additionally, we identified 157 additional Catholic charities by searching for Catholic keywords in the primary and secondary names of social-services charities. The results are robust if we exclude these 157 Catholic charities from the sample.

We observe the address reported by each organization in its 990 Form.[45] Note that there may be some central organizations with multiple branches that may file one form under the legal address of a central agency. The address for this central agency may not correspond to any of the establishments where the organization conducts business. Additionally, some central organizations do not even have branches (e.g., they raise contributions nationwide). We exclude all central agencies from our sample since scandals near the addresses

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[45]If an organization conducts business in a single location, the reported address corresponds to the physical location of the organization, or at least close to that location. The same principle applies for central organizations when each branch files its own report.

reported by them are not expected to affect their charitable contributions. We identify central organizations using information readily available from the IRS data, such as whether the name of the organization contains certain keywords such as "National", "America" or "International". In unreported results we constructed an alternative indicator for central organizations by manually inspecting the websites of every Catholic organization. Around 23% of the organizations were identified as central. Estimates of scandals on contributions to Catholic-affiliated organizations are robust to either definition. Constructing the alternative indicator for other (non-Catholic) religious-affiliated organizations would be unfeasible since it would require inspecting thousands of websites.

We aggregate the charity information at the zip code-level. However, since most zip codes have one Catholic charity, the results are similar if the analysis is conducted directly at the charity level instead of the zip code level. The final sample consists of 437 (6,478) zip codes that have at least one charitable organization affiliated with the Catholic (other non-Catholic) religion. The outcome variable is the logarithm of charitable contributions, as defined in line 1d from the 990 Form.[46] The median zip code with at least one Catholic-affiliated charity receives about $218,000 in contributions per year (in 2009 dollars).[47]

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[46]This is the sum of direct public support (line 1a), indirect public support (line 1b), and government contributions and grants (line 1c). Unfortunately, the data for these separate components is only available for a very limited number of organizations and years.

[47]The distribution of contributions is very skewed, with the top 25 percentile of Catholic-affiliated charities in raising over $2 million in contributions and the largest charities raising several million dollars. We report the results for the entire sample. Nevertheless, the results are robust if we exclude charities in the top 1% or 10% of mean contributions, as well as if we exclude charities in the lowest 1% or 10%.

D Measuring the Effect of Scandals on Charitable Contributions using Data from the Panel Study of Income Dynamics

In this section we study the effect of the scandals on charitable contributions using survey data from the Panel Study of Income Dynamics (PSID). One key advantage of this data with respect to the other sources of data on charitable giving is that it allows us to separate the effects of the scandals by the religious affiliation of the household head. However, the sample size is much smaller than the other sources of data, resulting in estimates that are substantially less precise.

The PSID follows a nationally representative sample of families and it is widely regarded as one of the best sources of survey data on consumption. We focus on data for 2001, 2003, 2005, 2007 and 2009, because in those years the PSID included a module on charitable giving known as the Center on Philanthropy Panel Study. During those years, households were asked numerous questions about charitable donations made during the previous fiscal year. A panel of households was constructed following household heads over time. We use this information to compute an estimate of total charitable contributions, measured in 2008 dollars.[48] The PSID asks individuals about their religious affiliation when they first enter the panel, and also to all respondents in the 2003 wave. We identify a household as Catholic if the household head reported to be Catholic at least once. We were granted access to zip code identifiers for all respondents in the survey, which allow us to match the data on scandals.

Respondents are asked whether the household made $25 or more in charitable contributions over the past year. They are required to answer detailed questions about donations only if they respond affirmatively. About a quarter of the households in our sample never report any charitable contributions. These households do not offer any within-variation in contributions and are dropped from the sample. The remaining households report to donate at least $25 about 80% of the time. Table D.2 shows basic descriptive statistics. Conditional on making a contribution, Catholic households contribute an average of nearly $2,000 per year.

Table D.1 shows the regression results. All regressions include household fixed effects, year dummies, the logarithm of household income, number of children and dummies for

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[48]For each type of charitable donation, the respondent is first asked the amount contributed, and if he or she is incapable of recalling he is asked a set of follow-up questions trying to obtain an estimate. We use the mean value of each category to impute the value of the missing donations: e.g., if the respondent reported to donate between $100 and $300, we assign a value of $200.

whether the household head is working and married. Note that for the analysis in this section we combine the variables $S_{z,t}^{Short-Term}$ and $S_{z,t}^{Long-Term}$ into a single variable: $Post-Scandal_{z,t}$. This is done to maximize power given the relatively small sample size. The dependent variable in the first two columns is a dummy variable for whether the individual reported to donate at least \$25. Coefficients in columns (1) and (2) suggest that the scandals did not affect this extensive margin. This is not surprising, since most donors do not contribute to a single organization but to multiple organizations. The dependent variable in columns (3) and (4) is the logarithm of charitable contributions. This regression is intended to measure the effects on the intensive rather than the extensive margin.[49] The coefficient on Post-Scandal from column (3) suggests a 5.6% decline in charitable contributions by Catholics after a scandal. This effect is statistically significant at the 10% level. Column (4) adds the variable Pre-Scandal to the regression. The coefficient on Post-Scandal remains unchanged and statistically significant, while the coefficient on Pre-Scandal is close to zero and statistically insignificant.

Consistent with the results from section 4, the coefficients from column (5) suggest that a scandal has a significant effect when it takes place in the same zip code where the household is located, but an insignificant effect when the scandal affects an adjacent zip code. However, because the estimates are not very precise, we cannot reject the hypothesis of small negative spillovers from scandals in adjacent zip codes. Finally, columns (6) and (7) explore the effect of the scandals on non-Catholic households, while column (8) explores the effect of the scandals on the subset of protestant households. As expected, the evidence suggests that neither group was affected significantly by the Catholic-clergy abuse scandals.

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[49]This regression includes the subset of contributions of at least \$25. Since we found no effect at the extensive margin, focusing on these observations should not introduce a significant concern for selection bias. Moreover, the data suggests that a substantial share of observations with contributions below \$25 are actually the product of misreporting. Just to mention one example, we find that, for households who itemize, a significant share of reported contributions below \$25 are not consistent with itemized charitable contributions well over \$25 reported in a different item from the questionnaire.

Table D.1: Effect of the scandals on charitable giving, by religious affiliation of the household

	Catholic HH					Non-Catholic HH		Protestant HH
	(1) Some	(2) Some	(3) Log(Amount)	(4) Log(Amount)	(5) Log(Amount)	(6) Log(Amount)	(7) Log(Amount)	(8) Log(Amount)
Post-Scandal, same zip	0.009 (0.011)	0.008 (0.011)	-0.056* (0.033)	-0.055* (0.033)	-0.051 (0.035)	-0.007 (0.026)	-0.011 (0.026)	-0.004 (0.032)
Pre-Scandal, same zip		-0.020 (0.026)		0.014 (0.082)	0.013 (0.083)		-0.041 (0.064)	-0.073 (0.076)
Post-Scandal, adjacent zip					-0.004 (0.013)			
Pre-Scandal, adjacent zip					0.000 (0.038)			
Observations	7197	7197	4722	4722	4722	17735	17735	14569
No. of households	2025	2025	1555	1555	1555	6291	6291	5048
R-Squared	0.01	0.01	0.06	0.06	0.06	0.05	0.05	0.05

Notes: Each column corresponds to a different OLS regression. *Some* is a dummy variable indicating whether the respondent reported to have donated $25 or over. *Log(Amount)* denotes the logarithm of the amount reported in charitable contributions in 2008 U.S. dollars. All regressions include household fixed effects, year dummies and a the following set of control variables: the logarithm of household income, number of children and dummies for whether the household head is working and married. Data on contributions and household characteristics obtained from the PSID. See Table D.2 for descriptive statistics, and its note for more details about the data. Post-Scandal is the stock of past scandals in the zip code, and Pre-Scandal is the number of scandals during the following two years. Heteroskedasticity-Robust standard errors in parentheses, clustered at the household level. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table D.2: Descriptive statistics for data from the Panel Study of Income Dynamics

	Obs	Mean	Sd	Min	Max
Catholic Household Head (%)	30063	20.42	40.31	0.00	100.00
Protestant Household Head (%)	30063	64.82	47.75	0.00	100.00
Charitable cont., Catholic (/100)	5986	13.56	33.24	0.00	846.46
If positive:	4730	17.16	36.55	0.02	846.46
Charitable cont., Protestant (/100)	19004	19.25	39.31	0.00	934.90
If positive:	14595	25.07	43.21	0.01	934.90
Post-Scandal, same zip	31013	0.18	0.57	0.00	13.00
Pre-Scandal, same zip	31013	0.01	0.14	0.00	3.00

Notes: Household-level data from the PSID, rounds 2001, 2003, 2005, 2007 and 2009. Charitable contributions, which are expressed in 2008 U.S. dollars, correspond to the previous year of survey (i.e., the 2001 wave asks about charitable contributions in year 2000). The amount contributed is constructed using several detailed questions about charitable contributions from the corresponding waves of the Center on Philanthropy Panel Study, a module within the PSID. The PSID asks household heads about their religious affiliation when they first enter the panel survey, and also to all households in the 2003 wave. Catholic (Protestant) households are defined as those where the household head reported to be Catholic (Protestant)—at least once, if asked more than once. Post-Scandal is the stock of past scandals in the zip code and Pre-Scandal is the number of scandals during the following two years (including type-A and type-B scandals).

E   Comparison between the Effects of Religious and Non-Religious Scandals

This section provides additional evidence about the possibility of a direct effect of the scandals on charitable giving. If the effect of the scandals on charitable giving was due to outrage about the abuses but unrelated to the decline in religious participation, then we could also expect a decline in charitable giving after child sexual abuse scandals originating in non-religious organizations. We created a dataset similar to that of the Catholic-clergy child sexual-abuse scandals, but covering child sexual-abuse scandals in non-religious schools. The data was compiled using a variety of sources (e.g., databases of historical newspapers) and using the same criteria used for the religious scandals. One difference between the two databases is that lay teacher abuse scandals become public soon after the abuses take place, while the abusers are still working at the same schools. In that sense, all the teacher scandals are simultaneously type-A and type-B. We identified 653 non-religious scandals between 1997 and 2010. Approximately 90% of scandals take place in public schools, and the remaining scandals take place in non-religious private schools.

Table E.1 shows the regression results. Just like in the previous section, we combined the variables $S_{z,t}^{Short-Term}$ and $S_{z,t}^{Long-Term}$ into a single variable ($Post-Scandal_{z,t}$) to maximize power given the relatively smaller sample size of non-religious scandals. The dependent variable in the first two columns is the logarithm of mean itemized charitable contributions. The dependent variable in the last two columns is the logarithm of the number of social-services employees. Consistent with the findings from section 4, columns (1) and (3) suggest negative and significant effects of religious scandals on charitable giving and social services. On the contrary, columns (2) and (4) suggest that the effects of non-religious scandals on charitable giving and social services are close to zero and statistically insignificant. However, given that the number of non-religious scandals is much lower, those coefficients are estimated with much less precision. In sum, this evidence goes against the hypothesis that the abuse scandals had a direct effect on charitable giving.

Table E.1: Comparison between the effects of religious and non-religious scandals

	Log(Charitable Cont.)		Log(Social Services)	
	(1)	(2)	(3)	(4)
Post-Scandal, Religious	-0.016***		-0.041***	
	(0.004)		(0.016)	
Pre-Scandal, Religious	-0.002		-0.013	
	(0.004)		(0.013)	
Post-Scandal, Non-religious		-0.001		-0.008
		(0.012)		(0.043)
Pre-Scandal, Non-religious		0.005		-0.049
		(0.009)		(0.030)
Observations	62561	21385	58357	19942
No. of zipcodes	8964	3064	4489	1534
R-Squared	0.42	0.47	0.02	0.03

Notes: Each column corresponds to a different OLS regression. All regressions include zip code fixed effects, time effects, and the interaction between the time effects and the logarithm of the population in the zip code (from the 1990 U.S. Population Census). In addition, columns (1) and (2) include the same set of additional control variables taken from the IRS SOI dataset used in Table 1. Post-Scandal is the stock of past scandals, and Pre-Scandal is the number of scandals during the following two years. The non-religious scandals correspond to the teacher abuse scandals in non-religious schools. The regressions in columns (1) and (3) include zip codes with religious scandals or adjacent to religious scandals. Equivalently, the regressions in columns (2) and (4) include zip codes with non-religious scandals or adjacent to non-religious scandals. The dependent variable in the first two columns is the log of mean charitable contributions in the zip code, from the IRS SOI dataset. The dependent variable in the last two columns is the number of employees in charitable establishments in the zip code, from the ZBP dataset. Heteroskedasticity-Robust standard errors in parentheses. Stars indicate significance level: * p<0.1, ** p<0.05, *** p<0.01.

F   Cross-sectional Correlation Between Religious Congregations, Charitable Giving and the Provision of Social Services

In this section we analyze the relationship between religious organizations, charitable giving and the provision of social services across counties in the U.S. We employ the same measures of religious congregations, charitable giving and social services used in the event-study analysis of religious scandals. The presence of religious congregations is measured by the (logarithm of) per capita number of employees working in religious establishments in the county. Charitable giving is measured as the (logarithm of) mean itemized charitable contributions. The provision of social services is measured as the (logarithm of) per capita number of employees working in social-services establishments. See Table G.1 for descriptive statistics, and its note for more detailed data definitions.

Table F.1 shows the estimation results. The main independent variable is the size of religious congregations and the dependent variables are charitable giving and social services in column (1) and column (2), respectively. For each dependent variable, Table F.1 provides elasticities estimated with three different regression specifications. The first row reports the coefficient from a regression that includes only a basic set of control variables: the logarithm of county population, mean income and a set of state dummies. The second row includes additional county characteristics as control variables, such as education, racial composition, etc. The third row has the same control variables than the second row, but uses the share of religious adherents as an instrumental variable for the size of religious congregations.

The first column from Table F.1 shows the elasticity between religious congregations and charitable giving. The three estimates of that elasticity suggest an economically and statistically significant relationship between those two variables: moving to a county with a 10% higher presence of religious congregations is associated with approximately 1.7% to 5.2% higher mean charitable giving. The second column shows the elasticity between religious congregations and the provision of social services. The three estimates of that elasticity are also economically and statistically significant: a 10% increase in religious congregations is associated with approximately 2.7% to 5.5% higher provision of social services.

One possible interpretation for these correlations is that religious congregations cause higher charitable contributions, which in turn finance a higher provision of social services. However, these correlations are not informative about the direction of causality. More religious areas may elicit higher charitable giving because of unobservable characteristics that

are correlated to religious congregations. For instance, more altruistic people may be selected into counties with higher religious participation. According to this hypothesis, people in counties with high religious participation would still donate as much and provide as much social services even in absence of religious congregations. The comparison between the first two rows of coefficients from Table F.1 show that controlling for basic county-level socio-economic characteristics substantially reduces the magnitude of the estimated elasticities: by 55% and 51% in the regressions with charitable giving and social services as dependent variable, respectively. This substantial correlation between religious congregations and observable determinants of charitable giving and social services is suggestive of an equally-substantial correlation of religious congregations and unobservable determinants of charitable giving and social services. Based on this evidence, it would not be unreasonable to consider the possibility that the correlations between religious congregations and charitable giving and social services are entirely spurious.

On the other extreme, it is possible that the coefficients reported in the first two rows of Table F.1 under-estimate the true causal effects of religious congregations on charitable giving and social services. For example, the number of religious employees is just a proxy for how important religious congregations are in a county, and therefore the coefficients in the first two rows can suffer from considerable attenuation bias. The third row from Table F.1 uses the share of religious adherents in the county as an instrumental variable for the number of religious employees, in addition to the extended set of controls. This should eliminate the attenuation bias, although at the same time can substantially aggravate the selection bias. Consistent with the hypothesis of attenuation bias, the estimated elasticities reported in the third row of Table F.1 are more than twice as large as the elasticities reported in the second row.

Table F.1: Cross-county relationship between religious congregations, charitable giving and social services

	(1) Log(Char. Cont.)	(2) Log(Social Serv.)
Basic controls:		
Log(Religious congregations)	0.3801***	0.5528***
	(0.0615)	(0.0574)
Extended controls:		
Log(Religious congregations)	0.1724***	0.2697***
	(0.0531)	(0.0377)
Extended controls + IV:		
Log(Religious congregations)	0.5270***	0.5461***
	(0.1185)	(0.1220)
No. of counties	3038	2877
R^2 basic controls	0.49	0.43
R^2 extended controls	0.57	0.56
R^2 extended controls + IV	0.55	0.55

Notes: Each coefficient corresponds to a different linear regression. See Table G.1 and its note for data definitions and descriptive statistics. The coefficients in the first row (*basic controls*) correspond to regressions with state dummies and two county characteristics taken from the 2010 U.S. Census: the logarithm of population and the logarithm of median income. The second row (*extended controls*) includes additional county characteristics: share of urban population, share of white, black and Hispanic, unemployment rate, share of married households, share of individuals in 7 education categories, mean household size, share of the population below the poverty line, and the share of votes to Obama relative to McCain in the 2008 presidential election. The third row (*extended controls + IV*) uses the share of religious population in the county (from the 2010 Religious Congregations and Membership Study) and its square as instrumental variables for the logarithm of religious employees. Heteroskedasticity-Robust standard errors in parentheses. Stars indicate significance level: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

G   Additional Tables and Figures

Figure G.1: Event-study analysis of the effects of scandals on other outcomes

a. Log(Gross income)

b. Log(Retail Employees)

c. Number of Other-Religion Schools

d. Log(Political Contributions)

e. Log(Census mail response rate)

f. Log(Voting Turnout Rate)
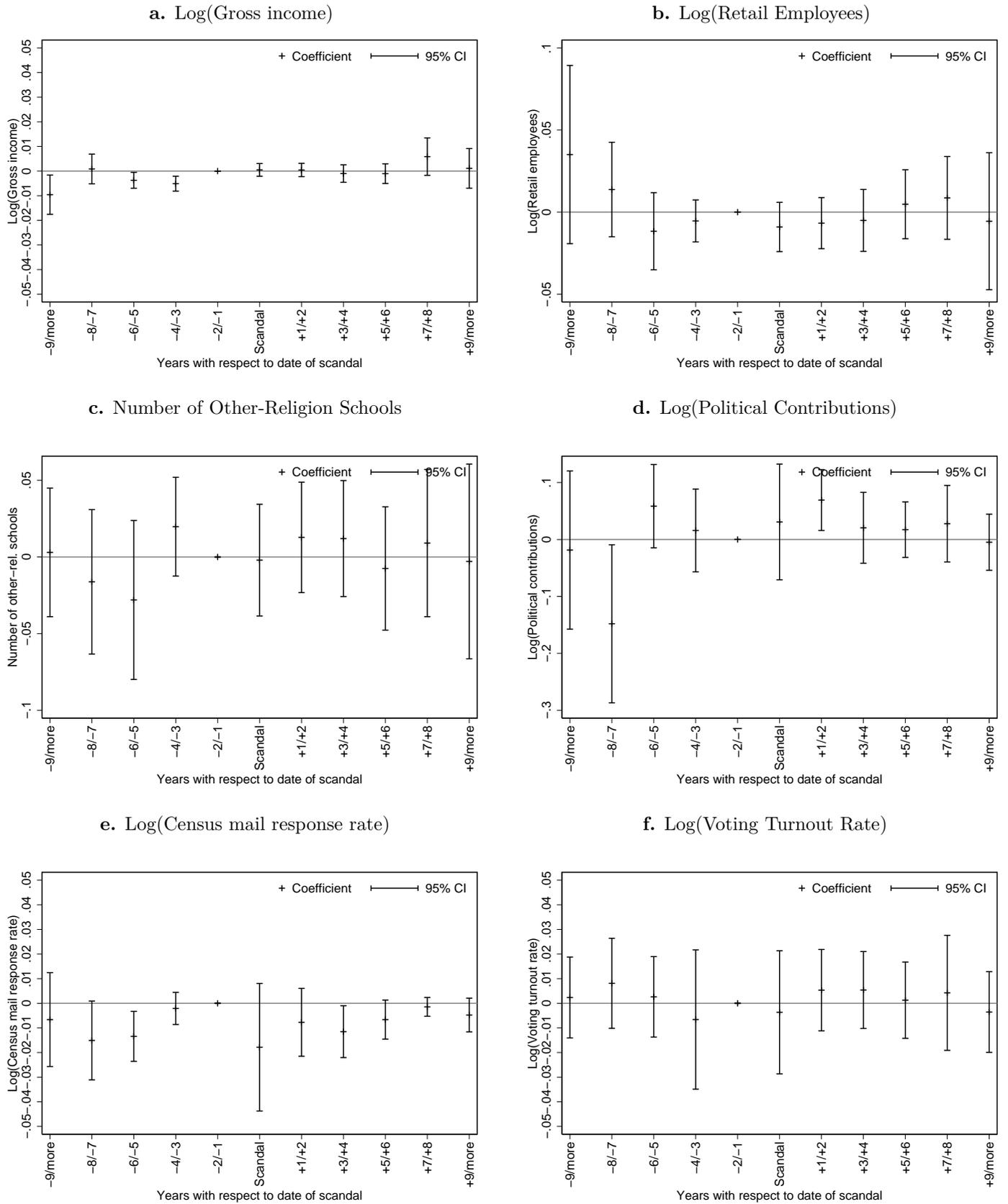
<u>Notes</u>: Each bracket represents the 95% confidence interval, and the center of the bracket represents the corresponding point estimate. Confidence intervals were constructed with heteroskedasticity-robust standard errors, clustered at the zip code/county level. The coefficient for the group "-2/-1" was normalized to zero. For more details see Appendix A. See Table G.1 for descriptive statistics, and its note for data definitions and data sources.

Table G.1: Descriptive statistics for all variables from zip code and county level data

	Obs	Mean	Sd	Min	Max
Panel A: Zipcode-level outcomes	ref.				
Catholic Schools[a]	64,746	1.29	1.09	0.00	14.00
Rel. Organizations[b]	323,884	7.79	7.49	1.00	76.00
Rel. Employees[b]	323,884	79.96	127.31	2.50	2,233.50
Avg. Contributions (in thousands)[c]	175,436	0.94	1.64	0.00	119.42
Cont. Catholic Charities (in thousands)[d]	9,177	1,189.60	4,213.23	0.00	72,106.38
Social Services Organizations[b]	110,630	6.49	7.51	1.00	197.00
Social Services Employees[b]	110,630	127.64	216.15	2.50	5,646.50
Other-Rel. Schools[a]	64,746	1.13	1.65	0.00	49.00
Retail Stores[b]	110,630	9.70	8.70	0.00	156.00
Retail Employees[b]	110,630	362.73	374.72	0.00	3,744.00
Avg. Income (in thousands)[c]	242,756	48.97	35.94	0.25	3,019.34
Cont. Oth.-Rel. Charities (in thousands)[d]	163,191	148.98	1,463.81	-10.31	271,746.28
Cont. Non-Rel. Charities (in thousands)[d]	59,724	1,528.86	5,762.16	0.00	172,395.88
Cont. Political Campaign (in thousands)[e]	43,748	32.60	122.39	0.19	7,459.09
Census Mail Return Rate[f]	45,619	78.77	7.48	10.23	100.00
Panel B: County-level outcomes					
Voting Turnout[g]	15,213	0.56	0.11	0.01	0.99

Notes: *Catholic Schools* is the number of Catholic schools in the zip code, covering the period 1990-2010 (bi-anually). *Rel. Employees* is the number of employees working in religious stablishments in the zip code, covering the period 1994-2010. *All Cont.* is the mean itemized charitable contributions in the zip code, covering the years 1997, 2002, 2004-2008. *Catholic Cont.* is the total charitable contributions to Catholic charities that provide social services in the zip code, covering the period 1989-2009. *Social Services* is the number of employees working in establishments that provide social services in the zip code, covering the period 1998-2010. *Other-Rel. Schools* is the number of schools of Non-Catholic religious denominations in the zip code, covering the period 1990-2010 (bi-anually). *Retail Employees* is the number of employees working in supermarket and car dealerships in the zip code, covering the period 1998-2010. *Income* is the mean gross income in the zip code, covering the years 1997, 1998, 2001, 2002, 2004-2008. *Other-Rel. Cont.* (*Non-Rel. Cont.*) is the charitable contributions to non-Catholic religious (non-religious) charities that provide social services in the zip code, covering the period 1989-2009. *Cont. Political Campaign* is the charitable contributions to political campaigns on presidential election cycles in the zip code, covering presidential election years 2000 through 2012. *Census Mail Return Rates* is the zip code mail response rates for the 2000 and 2010 US census. *Elections Turnout* is the voting turnout at the county level covering presidential election years from 1992 to 2008.

Data Sources: [a] Private School Survey; [b] Zipcode Business Patterns; [c] Statistics of Income, IRS; [d] National Center for Charitable Statistics Core Files, IRS; [e] Federal Election Commission; [f] US Census Bureau; [g] Voting records combined with U.S. Census data on number of voting-age U.S. citizens.

Table G.2: Descriptive statistics for data from the General Social Survey

	Obs	Mean	Sd	Min	Max
Catholic Aff.[h]	7,469	0.73	0.45	0.00	1.00
Other-Rel. Aff.[h]	7,469	0.16	0.37	0.00	1.00
Attends Church[h]	7,444	0.61	0.49	0.00	1.00
Prays[h]	4,913	0.69	0.46	0.00	1.00
Believes in God[h]	3,549	0.84	0.37	0.00	1.00
Believes in Afterlife[h]	5,724	0.71	0.45	0.00	1.00
Trust in Others[h]	4,734	0.71	0.57	0.00	2.00
Others are Fair[h]	4,438	0.87	0.94	0.00	2.00
Wants to Help Others[h]	4,071	2.59	1.01	1.00	5.00

Notes: Survey data from the General Social Survey, 1994-2010. Sample includes only individuals that responded to be raised as Catholic. *Catholic* and *Other religion* are dummy variables indicating whether the respondent is Catholic or has another religion, respectively (atheist/agnostic is the omitted category). *Attend religious services* is a dummy variable indicating whether the individual reported to attend religious services more than once a month. *Prays* is a dummy variable indicating whether the respondent prays more than once a week. *Believes in God* is a dummy variable indicating whether the respondent, when asked about the existence of god, answered "While I have doubts, I feel that I do believe in God" or "I know God really exists and I have no doubts about it." *Believes in Afterlife* is a dummy variable indicating whether the respondent reported to believe in afterlife ("No" and "Undecided" were the other possible responses). *Trust in others* is the answer to the question "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people? Can't be too careful (0); Depends (1); Most people can be trusted (2)." *Others Are Fair* is the answer to the question "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? Would try to take advantage (0); Depends (1); Most people are fair (2)." *Help Others* is the answer to the question: "If you had to choose, which thing on this list would you pick as the most important for a child to learn to prepare him or her for life? To help others when they need help: Least important (0); (...); Most important (4)."