Losing my religion: The effects of religious scandals on religious participation and charitable giving ☆

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ABSTRACT

We study how the U.S. Catholic clergy abuse scandals affected religious participation, religious beliefs, and pro-social behavior. 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, a scandal causes a significant and long-lasting decline in religious participation in the zip code where it occurs. Second, the decline in religious participation does not generate a statistically significant decline in religious beliefs, pro-social beliefs, and some commonly used measures of pro-social behavior. This evidence is consistent with the view that changes in religious participation during adulthood may have limited or no effect on deep beliefs and values. Third, the scandals cause a long-lasting decline in charitable contributions. Indeed, the decline in charitable giving is an order of magnitude larger than the direct costs of the scandals to the Catholic churches (e.g., lawsuits). If we assume that the scandals affect charitable giving only through the decline in religious participation, our estimates would suggest that the strong cross-sectional correlation between religious participation and charitable giving has the presumed direction of causality.

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1. Introduction

Individuals who participate in religious congregations tend to have stronger pro-social beliefs and behave more pro-socially than individuals who do not participate in religious congregations (Putnam and Campbell, 2010). However, there is no conclusive evidence about the direction of causality; we do not observe beliefs and behavior in a counter-factual scenario in which those same individuals do not attend church. For example, it is possible that certain beliefs and behaviors are so deeply ingrained that individuals forced to abandon their congregations would nevertheless continue to believe in God, trust others, and give money to charity.[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 (Thornton and Helms, 2013), others point to social mechanisms such as peer pressure and solicitation (Soetevent, 2005). This paper studies the U.S. Catholic clergy abuse scandals as a form of natural experiment: we examine how a negative shock to religious participation affects religious participation, religious beliefs, and pro-social behavior.

Combining several sources of data, we created a unique data set 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.

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

Indeed, the vast majority of the scandals occurred decades after the abuses were allegedly committed. We identify more than 3000 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.

We find that a scandal causes a persistent decline in the local Catholic affiliation and church attendance. Some Catholics join other religious denominations during the first three years after a scandal. But these individuals later end up with no religious affiliation. Our preferred measure of overall religious participation at the zip code level is the number of religious employees working in that location. According to this measure, we find that a scandal causes a persistent 3% decline in religious participation in the zip code in which it occurs. This would be equivalent to a 9% decline in the Catholic sub-population. We find effects of similar magnitude using survey data on religious affiliation and church attendance.

Despite the significant decline in religious participation, the effect of the scandals on two measures of religious beliefs (belief in God and in the afterlife) is statistically insignificant. Due to a lack of precision, we cannot reject the possibility of small effects, but we can reject the hypothesis of a decline in religious beliefs of the same magnitude as the decline in religious participation. The finding that religious beliefs do not fall despite the drop in religious participation is consistent with the view that religious beliefs are deeply ingrained and therefore unlikely to change during adulthood (Hamberg, 1991).

We 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 effects of the scandals on pro-social beliefs (e.g., trust in others) are statistically insignificant. Again, due to lack of precision, we cannot reject the possibility of small effects, but we can reject the hypothesis of effects of the same magnitude as religious participation. We also look at the effects on some general forms of pro-social behavior studied in the literature: political campaign contributions, response rates to census forms, and voting turnout. The effects of the scandals on these outcomes are close to zero, statistically insignificant, and more precisely estimated. For example, for the census response rate, we can reject the possibility of very small effects. This evidence suggests that changes in religious participation during adulthood may not affect deep pro-social attitudes, and it is consistent with studies reporting an insignificant correlation between religious participation and pro-social beliefs and behavior (Alesina and La Ferrara, 2002; Anderson et al., 2010). Nevertheless, our evidence applies to changes in religious participation for Catholic adults; it is possible that changes in religious participation early in life, or in other religious denominations, are more important for these beliefs and attitudes.

Last but not least, we examine the effect of the scandals on charitable giving. We use administrative data from the Internal Revenue Service (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 affected zip code. In other words, for each percentage point decrease in religious participation, charitable giving declines by 0.43 percentage points (i.e., 1.3 divided by 3). To verify that the missing contributions were directed towards charitable causes rather than to the provision of religious services, we use data on contributions reported by charities on their tax filings to the IRS. These data include contributions to charities that provide social services but exclude giving to Catholic parishes and Catholic schools. We find a large and statistically significant effect on this type of donations. As an additional robustness check, we also show that the decline in charitable giving was accompanied by a significant decline in the provision of social services.

The effects of the scandals on religious participation and charitable giving follow somewhat similar patterns. The effects increase in

magnitude during the first couple of years after the scandal, and then they remain stable at that level. The affected outcomes do not revert to pre-scandal levels, even more than ten years after the occurrence of the scandal. The effects are 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. And when an accusation comes to light, it has similar consequences 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.

The estimated 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 implies an elasticity of about 0.38. If we assume that the scandals affected charitable giving solely through the effect on religious participation, then our estimates would suggest that most of the observed correlation between religious participation and charitable giving has the presumed direction of causality (i.e., from religious participation to charitable giving). However, if the scandals had a direct negative impact on charitable giving that extended beyond the decline in religious participation, then the elasticity of 0.43 reported above would over-estimate the effect of religious participation on giving.

The literature suggests a variety of mediating factors through which the drop in religious participation may have caused the drop in charitable giving. Our evidence on the effects on religious beliefs and deep pro-social attitudes suggests that those beliefs and attitudes may not be main mediating factors. Our preferred interpretation points to the role of social interactions. With the goal of raising contributions to finance the provision of social services, religious networks can take advantage of the unique conditions for eliciting charitable contributions from members, for example, through solicitation, social pressure, and social norms. For instance, one possibility is that households leaving the congregation discovered non-Catholic charities that provide social services similar to the Catholic charities, but they did not contribute to the non-Catholic charities because of the lack of social pressure they once faced while in the congregation. Similarly, the decline in charitable giving could also be a result of the loss of access to information on opportunities to give and the quality of charitable endeavors that flows through the religious network. Indeed, these same social mechanisms are believed to play a crucial role in motivating charitable giving outside the context of religious congregations (Frey and Meier, 2004; Andreoni et al., 2011; Meer, 2011; Lazear et al., 2012; 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 and the psychological effects on the victims (McMackin et al., 2009), few studies examine the broader consequences of the scandals.[2] Exceptions include Hungerman (2013), who looks at the relationship between abuse allegations and religious adherence at the state level, and Dills and Hernández-Julian (2012), who examine the relationship between abuse allegations and Catholic enrollment at the diocese level. Our paper contributes to this research by looking at the broader consequences of the scandals and by identifying the causal effects of the scandals, using an event-study framework 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

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[2] For example, the causes and circumstances of the scandals are extensively discussed in the reports prepared by the John Jay College of Criminal Justice for the United States Conference of Catholic Bishops: "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.

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 the ratio of private to social costs are similar for scandals in different contexts, 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, while Hungerman (2005) and Gruber and Hungerman (2007) find that such expenditures crowd out religious giving. We examine whether the negative shock to 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 pro-social beliefs and other forms of pro-social behavior. Section 6 discusses the interpretation of the findings. The last section concludes.

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 and conducted by the John Jay College of Criminal Justice, 5768 priests had received at least one allegation of abuse during 1950–2009 (or 5.3% of all priests active in the United States). Our list of scandals is based on the records published by Bishop Accountability (bishopaccountability.org), a nongovernmental organization that compiles a public list of Catholic clergy in the United States who have faced a sexual abuse allegation. Individuals are only included when there is substantiated documentation such as newspaper articles from a reputable source or copies of legal documents filed in court and maintained in a public file. 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 abuse scandals.

Our scandal dataset differs substantially from the datasets used in other studies (Hungerman, 2013; Dills and Hernández-Julian, 2012), particularly in terms of the use of complementary datasets and 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 at the time of the accusation and at the time of the abuse, and we also identify the precise date of the first news article mentioning each 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 the clergy committed the abuse in that same institution or some other institution. The location of the scandal is the address of the institution where the clergy member is working 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 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).

For instance, consider a priest who allegedly comitted abuse in 1975 during his appointment in a parish in town A and later allegedly committed 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.

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.[5]

Our database covers 3024 scandal events during 1980–2010 (1125 type-A and 1899 type-B scandals). Fig. 1a shows the geographic distribution of scandals in the contiguous U.S. states, and Fig. 1b shows the distribution of scandals in the state of New Jersey. In both figures, the color of the states and counties denote the density of the Catholic population. Although scandals are more likely to occur in more Catholic areas and during 2002–2003, there is substantial variation over space and time. In the author's website[6] you can find an animated demonstration of the distribution of scandals over space and time that best illustrates the richness of our data.[6] 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 can be challenging. For example, places with more scandals may happen to be places with less pro-social individuals. Also, pro-social behavior may have already been decreasing before the scandals occurred. As a result, there could be a spurious negative correlation between the number of scandals and pro-social behavior. Our identification strategy is based on an event-study analysis that takes advantage of the rich temporal and geographic variation in the distribution of the scandals. The main identifying assumption is

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

[5] Under this definition, if an accusation involves a priest working at the same institution where the abuse allegedly took place, this accusation will simultaneously generate one type-A and one type-B scandal event. Appendix F shows that the results are robust if, instead, we categorize these simultaneous instances as a single event.

[6] URL: http://research.microsoft.com/en-US/people/rtruglia/scandals.aspx.

a. Scandals in the Contiguous U.S. States
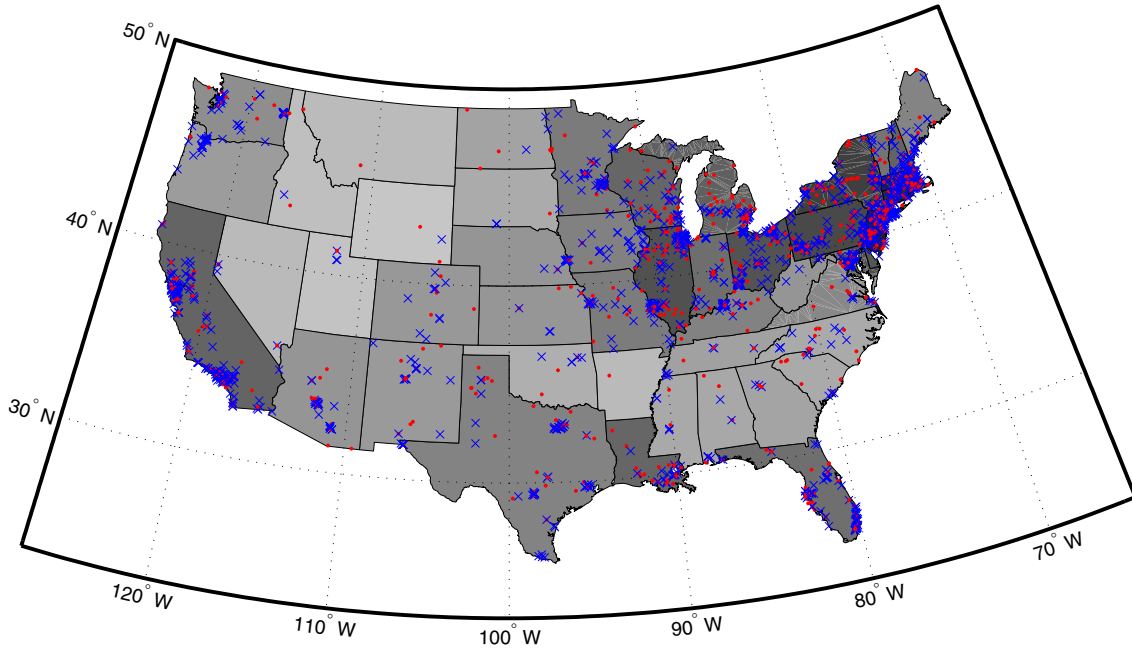
b. Scandals in the State of New Jersey
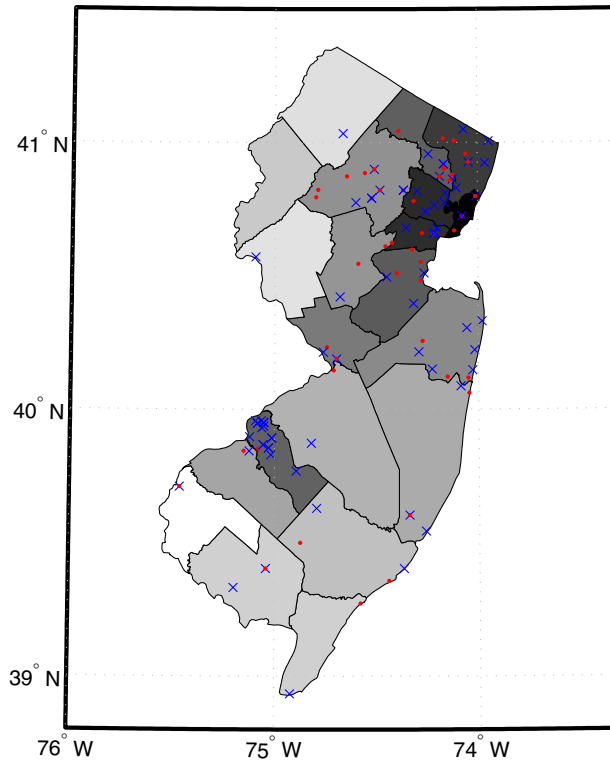
Fig. 1. Geographic distribution of Catholic clergy sexual abuse scandals. a. Scandals in the contiguous U.S. states. b. Scandals in the state of New Jersey. Notes: Each red dot corresponds to the location of a type-A scandal, and each blue X denotes the location of a type-B scandal. The location of a 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.

that the timing of the scandals is exogenous. The event-study analysis allows us to test that assumption 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).[7] 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, location 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. $S_{z,t}^{Pre-Scandal}$ 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 similarly between zip codes with and without scandals prior to a scandal. We expect the pre-scandal effect to be zero.

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, 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. 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.[8] 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 geographic aggregation.

3. Effect of the scandals on religious participation and religious beliefs

3.1. Data sources

First, we use the number of Catholic schools in a zip code as a proxy for religious participation in Catholic institutions (the results are similar when using the number of students enrolled in Catholic schools instead, as reported in Appendix F). According to the National Catholic Educational Association, the vast majority of students enrolled in Catholic schools belong to Catholic families; for example, the share of non-Catholic enrollment in Catholic schools was about 15% for the school year 2010–2011. 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 to 2010.[9] 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 working in religious establishments from the Zipcode Business Patterns during 1994–2010.[10] 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 average zip code has about eight religious establishments with a total of eighty employees.

One potential concern with the effects on religious employees is that the financial strains of lawsuits (and other abuse-related costs) could reduce the number of employees even if attendance is unaffected. As a robustness check, we use direct measures of religious affiliation and participation using individual level survey data. We employ the data from the General Social Survey, which includes multiple questions about religious affiliation and participation (because of the need for a critical sample size, we can only analyze questions that were included in several waves). Additionally, we can use this survey data to measure effects on religious and non-religious beliefs.

3.2. Results

Fig. 2a 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 are 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 after the first accusation and then stabilizes. A somewhat similar pattern of initial intensification appears repeatedly for the different outcomes analyzed in this paper. The pattern 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.[11]

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. To illustrate the severity of the scandals, the event-study estimates suggest that the scandals can explain 23% of the sharp decline of 1130 schools that occurred during 2002–2010. In contrast, Dills and Hernández-Julian (2012) report that, although statistically significant, the effects of the scandals on Catholic schools were small in magnitude.

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

[8] 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.

[9] 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 cover the totality of Catholic schools in the United States.

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

[11] For some outcomes, this pattern may also partially reflect that these institutions can survive for a few extra years while they deplete their assets. Part of the intensification pattern could also be the product of earlier scandals having a relatively stronger effect. Last, for the case of Catholic enrollment, it is possible that students already enrolled in higher grades stay in the school, but that enrollments into the lowest grades fall.

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)
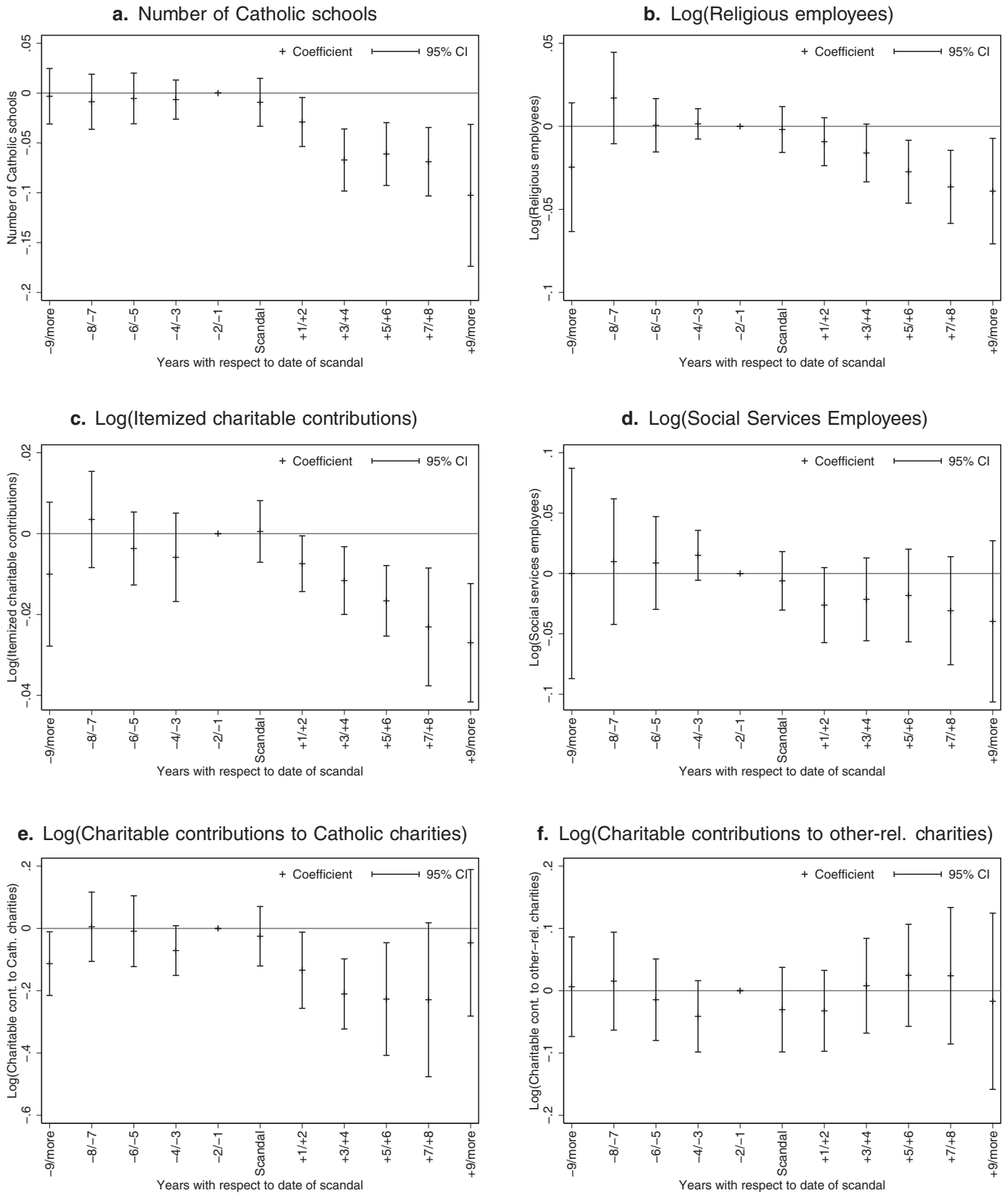
Fig. 2. Graphical event-study analysis of the effects of scandals on main outcomes. a. Number of Catholic schools. b. Log(Religious Employees). c. Log(Itemized charitable contributions). d. Log(Social Services Employees). e. Log(Charitable contributions to Catholic charities). f. Log(Charitable contributions 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 G1 and its note for descriptive statistics, data definitions and data sources.

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 zip codes	5,886	19,052	25,668	437	8,510	5,886	8,385	27,431	6,478	2,844

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 include 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-annually). *Rel. Employees* is the number of employees working in religious establishments 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.

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. Fig. 1a shows this to be an incomplete picture, because the majority of the effects of the scandals do not appear on the same year of the first allegation, but in the subsequent years.

The main falsification test is provided by the coefficient corresponding to the pre-scandal effect. This coefficient indicates whether, before a scandal occurs, the outcome variable is evolving similarly between zip codes with and without scandals. Consistent with the event-study graph, 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 Fig. 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 (Fig. 2b reports the corresponding event-study graph). The estimates suggest a long-term effect of around −3%. Given that Catholics in counties with scandals comprise about 30% of the population (Data Source: General Social Survey for year 2001), the implied effect on Catholic religious employees would be about 9% (i.e., 3% divided by 0.3).[12] As 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 only affects 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.[13] 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. Indeed, Table 2 shows that the effects of the scandals on other outcomes were also concentrated on the originating zip code.

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 for the 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.[14] As mentioned before, 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. We were granted access to county identifiers for the respondents from 1994 to 2010. Since the data is not a panel of individuals, we cannot control for individual fixed effects in the regressions. Instead, the regressions include county fixed effects, time effects, the interaction

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[12] This calculation assumes that Catholics affected by the scandals do not join other religious congregations in the long term. We explore this assumption below.

[13] We define two zip codes as adjacent (neighbors) to each other if, according to cartographic data, their boundaries touch.

[14] In columns (2) through (5) we cannot reject the hypothesis that the pre-scandal effects are the same across both types of scandals. In column (1), however, the difference between pre-scandal effects across scandal types is statistically significant at the 10% level.

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 code:					
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 zip code 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 G1 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

between time effects and the share of Catholic population, and a set of individual control variables. The results from the zip code level data indicate that each scandal primarily affects the population living in the same zip code where the scandal originated. But while the average zip code with a scandal has about 25,000 inhabitants, counties with scandals are an order of magnitude larger and more populated than zip codes, and also vary dramatically in terms of population. Because of its localized effects, a scandal in a county of 50,000 inhabitants is expected to affect half of the population in that county, while a scandal in a county with 100,000 inhabitants is expected to affect a quarter of that county's population. To account for these localized effects, in all the county level

regressions we normalize the scandal variables by dividing by 25,000 county-inhabitants. As a result, the coefficients estimated with county level data are roughly comparable to the coefficients with zip code level data (see Appendix F for a robustness test).

Table 4 examines the effect of the scandals on the subset of respondents who reported to be raised Catholic (as reported in Appendix F, we find no effects for individuals raised non-Catholic). The first two columns explore the effect of the scandals on religious affiliation. A scandal reduces the probability of declaring to be Catholic by 14.2 percentage points (p-value < 0.01) in the short term and by 10.4 percentage points (p-value < 0.01) in the long term. Consistent with Hungerman (2013), we

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

	(1) Catholic Schools	(2) Log(Rel. Employees)	(3) Log(All Cont.)	(4) Log(Catholic Cont.)	(5) Log(Social Services)
Effects of type-A scandals:					
Short-term effect	0.019	−0.013	−0.009	−0.007	−0.047
(0–3 years)	(0.025)	(0.016)	(0.007)	(0.143)	(0.035)
Long-term effect[(i)]	−0.036	−0.032	−0.017***	−0.117	−0.039
(4+ years)	(0.032)	(0.020)	(0.008)	(0.224)	(0.042)
Pre-scandal effect	0.044**	0.005	−0.003	−0.057	0.000
	(0.022)	(0.011)	(0.008)	(0.121)	(0.024)
Effects of type-B scandals:					
Short-term effect	−0.042**	−0.010	0.000	−0.092	−0.026
(0–3 years)	(0.018)	(0.010)	(0.004)	(0.070)	(0.019)
Long-term effect[(ii)]	−0.081***	−0.028**	−0.011**	−0.132	−0.036
(4+ years)	(0.023)	(0.013)	(0.005)	(0.091)	(0.023)
Pre-scandal effect	−0.010	−0.004	0.008	0.090*	−0.018
	(0.016)	(0.007)	(0.005)	(0.047)	(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 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 zip code 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 G1 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) Attends Church	(4) Prays	(5) Believes in God	(6) Believes in Afterlife	(7) Trust in Others	(8) Thinks Others are Fair	(9) Wants to Help Others
	Catholic	Other-Rel.							
Short-term effect (0–3 years)	−0.142***	0.099*	0.074	0.069	0.057	−0.015	−0.108	0.028	0.026
	(0.053)	(0.055)	(0.061)	(0.081)	(0.072)	(0.071)	(0.177)	(0.159)	(0.164)
Long-term effect (4 + years)	−0.104**	0.011	−0.122***	−0.062	0.006	0.012	0.070	0.011	0.079
	(0.051)	(0.034)	(0.047)	(0.048)	(0.067)	(0.046)	(0.198)	(0.117)	(0.114)
Pre-scandal effect	−0.024	−0.020	−0.051	0.006	0.129	−0.005	0.036	0.178	0.227
	(0.052)	(0.046)	(0.062)	(0.074)	(0.094)	(0.061)	(0.249)	(0.194)	(0.168)
Observations	7,414	7,414	7,389	4,862	3,503	5,673	4,695	4,386	4,041
No. of counties	318	318	318	310	292	316	313	313	311

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 at least once a day. *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 zip code 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.

find that most of 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, the majority of the Catholics affected by the scandals report no religious affiliation. Relative to the mean, a scandal causes a long-term decline in Catholic affiliation of 14%. The magnitude of this effect is consistent with the 9% drop in Catholic affiliation implied by the effect of the scandals on the number of religious employees (i.e., their difference is statistically insignificant).

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 religious services more than once a month, and *Prays*, which is a dummy variable indicating whether the individual prays once a day or more.[15] The averages of these outcomes are 61% and 69%, respectively. The estimates suggest that, in the long term, the scandals had a significant negative effect on church attendance of 12.2 percentage points (p-value < 0.01). Indeed, the magnitude of the effects on church attendance and Catholic affiliation are very similar. This finding confirms that the scandals did not affect only "nominal" Catholics, but also Catholics who actively participated in the congregation. There is no decline in church attendance in the short term, which again indicates that the affected Catholics were trying to attend church in other religious denominations. There is also a negative long-term effect on the probability of praying, although it is not significant at the 10% level.

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. 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). Moreover, these types of religious beliefs are significantly correlated to giving. For example, the correlation between the number of times an individual declares to have donated to charity and the belief in afterlife is 0.10 (p-value < 0.01). This magnitude of this correlation is comparable to that of the correlations between the

number of charitable donations and our measures of religious participation: 0.13 (p-value < 0.01) for *Attends Church* and 0.05 (p-value < 0.01) for *Prays*.[16]

The results indicate that, in the long term, a scandal increases the probability of believing in God by 0.6 percentage points, and increases the probability of believing in the afterlife by 1.2 percentage points. These two coefficients are positive (rather than the expected negative), small in magnitude and statistically insignificant. We can reject the hypothesis that the effect on these religious beliefs is equal to the effect on religious attendance (− 12.2 percentage points). Due to the precision of the estimates, however, we cannot reject smaller effects on these outcomes. 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) that benefit more than 70 million Americans each year (Johnson et al., 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 (Data Sources: Giving U.S.A. and Bureau of Labor Statistics for year 2010). The effect of the scandals, through a decline in religious participation, on charitable giving and the provision of social services could be ambiguous. On one hand, if religious participation is truly important for financing and providing social services, we would expect the decline in religious participation to cause a decline in charitable giving and in the provision of social services. On the other hand, former Catholics may continue to donate to Catholic charities, or they may instead substitute their donations to other non-Catholic charities. Indeed, some anecdotal accounts go as far as to claim increased support to Catholic charities as a result of the scandals.[17]

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

[16] These results are based on GSS data for 2002 and 2004, for which a question about charitable giving was included.

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

Our main measure of charitable giving consists of zip code level data on individual itemized charitable contributions; that is, the amount that taxpayers reported as charitable contributions on Schedule A from the 1040 form. These data are prepared by the Statistics of Income Division of the IRS and are available for several years during 1997–2008.[18] We define the outcome variable as the logarithm of mean itemized charitable contributions in the zip code. The mean charitable contribution is $940 (dollar amounts are always expressed in 2009 U.S. dollars), and the mean adjusted gross income is $48,900.

It is important to note that taxpayers file a Schedule A to report their total itemized deductions, which includes charitable contributions as well as medical expenses, state and local taxes, certain interest expenses, and other miscellaneous deductions. But taxpayers whose standard deduction exceeds the itemized amount generally do not file Schedule A. As a result, itemizers are different in many respects from non-itemizers (most notably, they have a higher income on average). According to data from the Panel Study of Income Dynamics for 2002, around 54% of the households that make charitable contributions are itemizers, and the total contributions by itemizers constitute over 75% of the total charitable contributions (i.e., combined contributions by itemizers and non-itemizers).

We would like to check that the scandals affected giving to organizations that provide social services, excluding donations to Catholic schools[19] and donations directed towards the provision of religious services.[20] The data on itemized contributions do not offer a breakdown by the organizations that the contributions were made to. To enable distinguishing between these types of donations, 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 they do not include contributions to churches and schools. For further details about these data, see Appendix C.

As an additional robustness check that the scandals affected giving to charitable causes and not only religious causes, we also measure the effect of the scandals on the provision of social services. We use data from the Zipcode Business Patterns from 1998 to 2010 (described previously in Section 3).[21] We focus on establishments that, according to their NAICS codes, provide social services to 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.[22] 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. The long-term effect of the scandals of about −1.3% is negative and highly statistically significant. That is, for each 1% drop in religious participation there was a 0.43% drop in religious giving (i.e., 1.3 divided by 3). The short-term effect is negative, but the magnitude of the coefficient is much smaller and the coefficient is statistically insignificant (moreover, we can reject the hypothesis that long-term and short-term effects are equal at the 1% level). One possible interpretation for the lack of short-term decline in giving is that, in the first few years after a scandal, Catholics who joined other denominations continued to contribute similar amounts to charity.

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.[23] The event-study graph reported in Fig. 2.c shows that the effects of the scandals intensify over the first few years after the first public accusation and then stabilize. Column (3) from Table 1 shows that the effects on charitable giving are concentrated in the same zip code where the scandal occurs. And Column (3) from Table 3 shows that type-A and type-B scandals have similar effects on charitable giving.

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).[24] According to data from the Panel Study of Income Dynamics, itemized charitable contributions comprise about 75% of the total charitable contributions. Assuming that the effect for non-itemizers is similar to the effect found for itemizers, the total cost in contributions would be $2.36 billion. To put these magnitudes in perspective, according to Bishop Accountability, the sum of all the lawsuits 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 measured 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.

Column (4) from Table 1 shows that there is significant long-term effect on contributions to Catholic-affiliated charities that provide social services, of about −12.9%. The magnitude of this effect is roughly consistent with the hypothesis that the effect on giving acted primarily through giving to Catholic-affiliated charities.[25] The short-term effect

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[18] 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. We focus on the subset of zip codes with positive amounts 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.

[19] If the effects on itemized giving were focused on school giving, we should find that the effects are much stronger in areas with children of school age. On the contrary, we find no such heterogeneity (results reported in Appendix F).

[20] In any case, note that only part of the money given directly to a Catholic parish is used for the provision of religious services, and the rest is used for helping people in need such as through the provision of social services. This focus 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). Thus, even if we had the ability to exclude them from itemized giving, it would be incorrect to deem the checks made to the Catholic Church as purely-religious giving. Also consistent with this view, note that the correlation between religious participation and itemized giving is very similar to the correlation between religious participation and provision of social services (see Appendix E).

[21] 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 for this type of organizations.

[22] We use the sample of zip codes that always have a positive number of social services employees 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.

[23] 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.

[24] 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 Fig. 2c is about twice the coefficient on the long-term effect reported in Table 1.

[25] According to PSID data for 2002, giving by Catholic households accounted for 23% of total itemized charitable contributions in zip codes with scandals. If we assumed the scandals only affected Catholic households, the implied long-term effect of a scandal on charitable contributions by Catholic households would be about −5.65% (i.e., 1.3% divided by 0.23). PSID data for 2002 suggests that half of contributions by Catholics may go to Catholic-affiliated charities. If we assume that the entire effect of the scandals focused on Catholic-affiliated charities, then the implied effect on this type of contributions would be 11.3% (i.e., 5.65% divided by 0.5). This estimate is close to the estimated effect of −12.9% reported in this section, suggesting that the scandals may have affected primarily giving to Catholic-affiliated charities.

is also negative and large, although statistically insignificant. Nevertheless, this short-term effect is statistically significant in many other specifications, as shown in the robustness checks reported in Section F.

Column (9) reports the effects on contributions to charities that provide similar social services but are affiliated to other religious denominations, while column (10) reproduces the analysis for charities with no religious affiliation. The long-term effects on these outcomes are positive, and smaller (in absolute value) than the effect for Catholic charities. This evidence could indicate that a partial crowding-in effect of contributions to non-Catholic charities may have occurred, and is consistent with the evidence in Hungerman (2013) that the Catholic scandals increased contributions to the Baptist Church. However, these effects are imprecisely estimated: we cannot reject the hypothesis that both effects are zero, but at the same time we cannot reject the hypothesis that the effects are (in absolute value) equal to the effects on Catholic charities. In any case, the fact that itemized contributions do not revert to pre-scandal levels (even more than ten years after the occurrence of the scandal) indicates that, if any, the crowding in was not nearly enough to offset the decline in Catholic giving.

This last source of data allows us to separate the effects on charitable contributions by the religious affiliation of the charity, but it does not allow us to 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 only affected charitable contributions by Catholic households.

Last, we examine whether the drop in charitable giving translates into a drop in the provision of social services.[26] 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 of about −3.6%.[27] This finding is consistent with the prevalent view that these social services are funded at the local level (Cnaan et al., 2002), thus making the provision of these services vulnerable to shocks to local giving.

5. Effect of the scandals on pro-social beliefs and other forms of pro-social behavior besides charitable giving

5.1. Data sources

To measure the effect 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.

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, which can be estimated with more precision. 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), a substantial portion of campaign contributions seem to be 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). The data was obtained from the public records of the Federal Election Commission from 1997 to 2012.[28] The relevant outcome variable is the logarithm of total campaign contributions to presidential candidates in the zip code. 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.

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, no private benefits, but 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; Rupasingha et al., 2006). We define the outcome variable as the logarithm of the Census mail response rate at the zip code level,[29] based on data provided by the U.S. Census Bureau for 2000 and 2010.[30]

The third measure of pro-social behavior is the rate of voting turnout. There is evidence suggesting that individuals not only vote because they want to affect the election outcome, but also because of its pro-social nature (e.g., 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.[31]

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 responses to each of these three questions were coded using the Probit-Adapted OLS transformation (Van Praag et al., 2008), but the results are robust if we instead use an arbitrary scale for coding the responses (e.g., integers from 1 to 4). The estimated long-term effects on pro-social beliefs are positive (suggesting an increase rather than the expected decline), but statistically insignificant: 0.070 for trust in others, 0.011 for the belief that others are fair, and 0.079 for the perceived importance of helping others. As a benchmark, the results from columns (1) and (3) suggest that a scandal has an effect on Catholic affiliation of −0.23 standard deviations (i.e., −0.104 divided by 0.45) and an

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[26] Some factors may augment or mitigate the reaction of social services to the drop in charitable giving. On the one hand, Catholic parishes and charities may mitigate the 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.

[27] Dills and Hernández-Julian (2014) find suggestive evidence for scandals increasing government spending on welfare. However, this increase is small compared to the decline in charitable giving.

[28] We focus on the subset of zip codes that have a positive number of contributors 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.

[29] 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.

[30] We aggregated census tract level data to zip code level data using population weights provided by the Census Bureau. Results are very similar if the analysis is conducted at the census tract level instead of the zip code level.

[31] 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.

effect on church attendance of −0.25 standard deviations (i.e., −0.122 divided by 0.49). These effect sizes (i.e., −0.23 and −0.25 standard deviations) are outside the 95% confidence intervals for the belief that others are fair and the perceived importance of helping others. That is, we can reject the hypothesis that these pro-social beliefs declined to the same extent as religious participation. Due to the precision of the estimates, however, we cannot rule out the possibility of smaller effects on these beliefs.

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), (3) and (4) show the effects of scandals on political contributions, the Census mail response rate, and voting turnout, respectively. 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. The effects on political contributions are so imprecisely estimated that we cannot reject the hypothesis that it was equal to the effect on charitable giving. In the case of the census response rate, the effects are estimated with a lot of precision. We can reject the null hypothesis that the effects on census response rate and charitable giving are the same. Furthermore, we can also rule out the possibility of even very small effects: i.e., the 95% confidence interval excludes the possibility of a long-term effect below −0.2%, which would imply an elasticity with respect to religious participation below 0.01. Last, we can reject the hypothesis that the effect on voting turnout was equal to the effect on charitable giving. We can also reject the hypothesis of effects on voting turnout in the neighborhood reported in other observational studies, and even half that magnitude.[32] However, due to the precision of the estimates, we cannot rule out the possibility of smaller effects.[33]

Even though each individual coefficient does not constitute conclusive evidence, as a group they provide a clearer pattern: the decrease in religious participation due to the scandals may have translated into small or no effects on more general pro-social attitudes.

6. Discussion

One interesting finding is that the drop in religious participation caused by the scandals is not accompanied by a significant drop in pro-social beliefs and attitudes. This evidence suggests that the correlation between religious participation and pro-social attitudes reported in some studies may be spurious. Indeed, some studies do not even find a significant correlation between religious participation and some forms of pro-social beliefs and behavior (Alesina and La Ferrara, 2002; Anderson et al., 2010). Using our own data on these forms of pro-social behavior (political contributions, census response rate, and voting turnout), we find small cross-sectional correlations with religious participation (see Appendix E). Nevertheless, there are three important caveats. First, the effects on some of these outcomes are not estimated with a lot of precision. Second, Catholicism differs in key aspects from other religious denominations, so we should be careful in extrapolating these findings to other religious denominations. Third, we are studying changes in religious participation during adulthood, but deep beliefs may be more susceptible to changes in religious participation during childhood.

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[32] For example, Gerber et al. (2015) estimate that the repeal of Blue laws reduced religious attendance by 5% of the mean and voting turnout by 3% of the mean, implying an elasticity between religious attendance and voting turnout of about 0.6 (i.e., 0.05/0.03). Given that a scandal decreases religious participation by about 3%, this 0.6 elasticity suggests that we should expect a corresponding long-term decline of 1.8% in voting turnout. But we can reject the null hypothesis of a 1.8% decline in turnout.

[33] For example, the 90% confidence interval on the effect of a scandal on turnout has a lower bound of −0.75%, which would imply an elasticity between religious participation and turnout of about 0.25 (i.e., 0.75 divided by 3).

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	−0.003	0.042*	−0.002	0.003
(0–3 years)	(0.003)	(0.021)	(0.003)	(0.004)
Long-term effect	−0.013***	−0.003	0.002	−0.001
(4 + years)	(0.004)	(0.019)	(0.002)	(0.004)
Pre-scandal effect	0.004	−0.018	0.003	−0.002
	(0.004)	(0.027)	(0.003)	(0.008)
Zip/county level data	Zip	Zip	Zip	County
Observations	175,415	43,748	45,619	15,213
No. of zip codes/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 zip code 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 include zip code (or county) fixed effects and time effects—for more details and the full list of control variables see Appendix A. See Table G1 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.

The evidence also suggests that the decline in religious participation coincides with a significant decline in charitable giving. The decline in giving somewhat follows the decline in religious participation in many respects, such as the overall 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 the number of religious employees because of the scandals, charitable giving declines by 0.43%. These estimates imply 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 E). If we assume that the effects of the scandals on charitable giving are entirely driven by the decline in religious participation, then our estimates would 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).

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 mechanism, the event-study findings imply an 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 E.

Our evidence also provides some insights regarding the causal mechanisms that may mediate the relationship between religious participation and charitable giving. Due to the apparent lack of effects on religious beliefs and more general pro-social attitudes, it is unlikely that the decline in charitable giving was due to changes in these beliefs and attitudes. Instead, our preferred explanation relies on the advantageous conditions for eliciting contributions from members, facilitated by the social interactions that take place naturally within religious congregations. Indeed, these same social mechanisms are believed to be important determinants of charitable giving outside the context of religious congregations.

Religious congregations may be successful in eliciting contributions thanks to the use of direct solicitation and social pressure (Soetevent, 2005) and even in influencing social norms about giving (Frey and Meier, 2004). For instance, Lazear et al. (2012) provide evidence from laboratory experiments suggesting that the same individuals who

choose to share income with others when that is an option, actively avoid having that option if such a thing is possible. This same phenomenon can arise as a result of social pressure. DellaVigna et al. (2012) conducted a field experiment consisting of hanging fliers on the doorknobs of houses that were going to be visited by a charity fundraiser. Consistent with the power of direct solicitation, they show that randomly adding a "Do Not Disturb" box in the flyer reduced giving by 30%. Andreoni et al. (2011) present related evidence from a field experiment during the Salvation Army' s annual campaign: randomly assigning subjects to be approached by solicitors increased donations by 75%. These mechanisms predict that the individuals who were being actively asked for donations in the congregation may not give nearly as much once they leave the congregation.

Another important advantage of religious networks lies in access to better information about charitable causes. For example, religious networks may be better at screening the beneficiaries (Dehejia et al., 2007), disseminating information about volunteer opportunities and charitable endeavors, and, more broadly, at creating links between individuals and charities. Unless they join another religious organization, individuals leaving the Catholic congregation lose access to this information network and thus giving becomes less attractive.

In any case, our interpretation of the results does not imply that religious participation cannot cause higher charitable giving through other mechanisms in different circumstances. For instance, it is possible that growing up Catholic increases pro-social behavior through higher religious beliefs. Nevertheless, we believe that the evidence is at least suggestive that social mechanisms may go a long way in explaining the cross-sectional correlation between religious participation and charitable giving.

The preceding analysis makes a critical assumption: that the effects of the scandals on charitable giving and other outcomes are entirely caused by the decline in religious participation. One possible violation of this assumption would occur if Catholics become less pro-social because of a generalized outrage effect when learning about sexual abuses. This confounding factor, however, would be at odds with the fact that the scandals did not affect pro-social beliefs and other forms of pro-social behavior. If anything, the scandals failed to have a significant effect on these outcomes in spite of any general outrage effect.

A potential confounding factor that could upwardly bias the estimates of the true causal effect of religious participation on charitable giving is that scandals could have generated a negative attitude towards everything carrying the "Catholic" brand. According to this mechanism, an individual leaving the church due to a scandal would like to contribute exactly as much as she wanted to contribute while she was an active member of the church. But since she becomes averse to the Catholic brand, her charitable giving may decline if she is unable to find other non-Catholic charities (either religious or lay) to act as substitutes for the Catholic charities. However, this outrage mechanism seems inconsistent with the fact that, during the first three years after a scandal (a period during which a majority of Catholics tried joining other religious denominations), Catholic giving declined but total giving did not decline.

A final caveat for our interpretation of the results is that those individuals leaving the congregation as a result of the scandals are probably not representative of the universe of churchgoers. Consequently, our estimates of the effect of religious participation on giving are not necessarily representative of the average effect among all churchgoers. Instead, they represent the average effect for the marginal individuals whose religious participation was affected by the scandals (Imbens and Angrist, 1994). This could lead to an over- or under-estimation of the average effect of religious participation on giving. For instance, if the marginal churchgoers affected by the scandals are less (or more) sensitive to social pressure, that would lead to an over- (or under-) estimation of the effect of religious participation on giving.

7. Conclusions

We showed evidence on the effects of the U.S. Catholic abuse scandals on religious participation, religious beliefs, and pro-social behavior. We found a significant decline in religious participation as a result of the scandals. Despite this decline, the scandals did not have a significant effect on religious beliefs or pro-social beliefs. This evidence suggests that changes in religious participation during adulthood may have a small or no effect on deep beliefs and attitudes. However, this evidence is subject to some caveats, such as the lack of sufficient statistical power to reject small effects in some cases and the possibility that the results cannot be generalized to other religious denominations. The decline in religious participation did coincide with a significant decline in charitable giving and the provision of social services. Assuming that the drop in charitable giving was entirely due to the corresponding drop in religious participation, our estimates would imply that most of the observed correlation between religious participation and charitable giving has the presumed direction of causality.

Notably, the indirect cost of the scandals through the decline in giving is an order of magnitude higher than the direct costs of the scandals to the Catholic churches, such as the cost of lawsuit settlements. Similar social costs could be equally significant in a variety of related contexts, such as charities, universities, and political organizations and for politicians in office. An avenue for future research could be to quantify these costs and to study which crisis-management methods may help minimize these costs. For example, in 1992 the CEO of United Way retired after allegations of fraud and financial mismanagement, an event that was believed to have resulted in a drop in donations to affiliates of the organization.[34] Additionally, a scandal in one charity could potentially spill over to other charities if the public becomes suspicious that those are corrupt as well. Wrong-doing by politicians may also be detrimental to political participation and involvement in civic duties more broadly. For example, after the scandal involving former Representative Mark Foley from Florida, the Republican Party was concerned the scandal would affect partisan engagement.[35] Given that the indirect costs could potentially impact a significant portion of the population, it would be valuable to identify strategies that minimize these social costs. For instance, in the case of the Catholic abuse scandals, there was variation in how each church and diocese dealt with the news of the scandals. Measuring the effects of those crisis-management strategies on charitable giving could be useful for the church and other charitable and political organizations.

Online Appendix. Further Results

The Appendix with further results for this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2015.07.008.

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[34] See for example: New York Times, April 22, 1992 http://www.nytimes.com/1992/04/22/nyregion/affiliates-feeling-pinch-of-united-way-scandal.html"Affiliates Feeling Pinch of United Way Scandal".

[35] See for example The Washington Times, October 4, 2006 http://www.washingtontimes.com/news/2006/oct/4/20061004-122013-7458r/?page=all "Conservatives worry scandal will hit 'value voter' turnout".

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