Trust in Cultural Institutions and Interfirm Cooperation: Evidence from Religious Scandals

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Abstract

We study how trust in cultural institutions affects cooperation between firms, employing local religious scandals for identification. We focus on trade credit as a trust-intensive aspect of supply chain relationships. In a triple-difference estimation, we find that firms located in scandal areas where the affected religion is prominent reduce trade credit to customers (relative to sales) by 5 percentage points. Consistent with the scandal damaging local norms of cooperation, results are stronger in relationships with limited history, greater geographic barriers, or transaction complexity. Our findings offer novel causal evidence of the influence of local culture on interfirm relationships.

Keywords: Culture, Trust, Religion, Interfirm Cooperation, Supply Chain Relationships, Trade Credit

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1 Introduction

The impact of the local culture on firms' decisions is of growing interest in finance.[1] Culture's effect on individuals' trust is particularly relevant to understanding financial interactions between counterparties, since trust is central to these relationships and financial transactions (Arrow, 1972; Guiso et al., 2004, 2008). Cultural institutions, such as religions, shape individuals' trust, which in turn affects local economic activity, e.g., growth and investment (Knack and Keefer, 1997). Despite this recognized importance, it is inherently difficult to identify plausibly causal links between culture, trust, and economic decisions, because culture and the institutions that embody it are endogenous to the environment and generally very slow-changing. Drawing on the large literature on the role of religion in shaping cultural norms (North, 1990; Putnam, 1993; Guiso et al., 2003; Enke, 2019), we address this empirical challenge by focusing on scandals affecting the Catholic Church. This identification strategy allows us to analyze the impact of cultural institutions on interfirm financial cooperation.

In January 2002, the *Boston Globe*'s Spotlight article series revealed systematic organizational misconduct by Boston's Catholic Church leader (bishop). The articles detailed how the bishop of Boston reassigned clergy whom he knew to have a history of child abuse to different churches – where they were likely to be in proximity with children and, indeed, committed more abuse. The Spotlight news articles set off a wave of disclosures of past abuse by Catholic clergy in various parts of the U.S. These events damaged trust in local Catholic Church organizations and their leadership that knowingly relocated accused clergy

[1] While "culture" can refer to several different contexts, in our setting we refer to culture as the common beliefs and values shared by the members of a group such as an ethnicity, a country, a religion, etc. (Guiso et al., 2006).

(Bruce, 2011; Esparza, 2020). The scandal also reduced Catholics' religious participation and charitable donations to Catholic organizations (Hungerman, 2013; Bottan and Perez-Truglia, 2015). That is, the scandal presented a consequential negative shock to an important religious and cultural institution for affected communities.

We expect this negative shock to translate into changes in cooperative behavior, drawing from prior work: Putnam (1993, 2000) sees participation in cultural activities as a means of building cooperation and trust. Fukuyama (1995) underscores cultural institutions' role in fostering cooperation and building trust as an important determinant of firms' decisions and success, while LaPorta et al. (1997) illustrate the role of religion in this regard. Previous literature also highlights how religion affects individuals' work decisions (Buren et al., 2020; Weaver and Agle, 2002), due both to its role in shaping social norms and to the impact of religious moral authority (McGuire et al., 2012; Peifer, 2015). The scandal, laying bare moral breaches on the part of some of the Catholic Church's local leaders, weakens the Church's moral authority and its standing in the community. Accordingly, we predict that the Catholic Church scandal will have significant spillover effects on the individuals working in firms in affected communities, negatively impacting those firms' cooperative behavior toward counterparties. To support this intuition, we document in survey data that trust in religious institutions correlates with trust in businesses, and that individuals in communities most affected by the scandals report lower levels of confidence in both the clergy and in business. These findings demonstrate how negative religious shocks can transmit trust spillovers into individuals' economic dealings, motivating our analysis of interfirm cooperative impacts.

We examine the effects of religious shocks on interfirm relationships in the context of supply chain interactions. As our outcome of interest, we focus on firms' provision of trade

credit, the informal short-term lending that takes place when firms sell products or services to customers on credit. Extending trade credit explicitly creates a financial vulnerability for the seller, exposing the firm to repayment risk and magnifying the potential for adverse supply chain spillover (e.g., Jacobson and Von Schedvin, 2015; Hertzel et al., 2008; Boissay and Gropp, 2013; Murfin and Njoroge, 2015).[2] Thus, the provision of trade credit requires the supplier's willingness to accept this risk, engage in cooperative behavior, and trust its customers (Guiso et al., 2004).[3] Indeed, previous work has used a firm's extension of trade credit to a customer to measure trust (see McMillan and Woodruff, 1999).

Consistent with our predictions, we find that firms in communities most affected by the religious scandal subsequently reduced their provision of trade credit to major customers. These effects are most pronounced in relationships in which trust is more crucial.

We are able to precisely measure trade credit provision with a granular database of pair-level trade credit between U.S. publicly traded firms (Billett et al., 2025; Freeman, 2025; Ersahin et al., 2024). In contrast to most studies examining firm-level trade credit balances (i.e., aggregated across supply chain partners), the advantage of this dataset is its pairwise nature. This feature allows us to make detailed inferences about changes in trade credit provision to individual customers following cultural shocks, while controlling for demand-side factors and firm characteristics.[4]

To identify areas impacted by this scandal, we use news revelations of a bishop's

[2]We focus on the supplier's vantage point, as it is the provision of credit (as opposed to the receipt of credit) that creates a financial vulnerability.

[3]Observed trade credit amounts reflect a bargaining outcome between the firm and its customer arising from both supply and demand factors. By exploiting a shock to the supplying firm, we can focus on equilibrium shifts arising from shocks outside the relationship.

[4]A few other papers use unique samples of supply chain contracts and trade credit. See, e.g., Klapper et al. (2012); Fabbri and Klapper (2016); Costello (2019).

disciplinary measures against clergy accused of abuse: their removal from active ministry. These removals of clergy from active ministry are the belated acknowledgment of a breach in governance by local Catholic Church leaders, signifying that members of the local clergy who should not be ministering to local Catholics were allowed to do so for some time. We read through news articles mentioning removals of Catholic clergy based on claims of abuse, creating an indicator for regions where newspapers report clergy removals from ministry. In the wave of such revelations between 2002 and 2004, we identify 113 Catholic regions ("dioceses") impacted by the scandal, out of 176 dioceses in the U.S. These treated areas cover 52% of U.S. counties, a geographically diverse span of the country as evidenced by Figure 2. Besides its cross-sectional variation across well-defined areas, the Catholic clergy abuse scandal offers several empirical advantages as a shock to local cultural institutions. First, the decision to remove a clergy member stems from allegations whose timing is not controlled by the firm or by the Catholic church, generally based on decades-old events (John Jay College of Criminal Studies, 2004). Second, Catholicism is a large religion in the U.S., making it a culturally relevant institution. Third, because Catholic dioceses are a collection of non-overlapping counties, we can differentiate between areas where the scandal is likely to inflict greater damage to the local culture (counties where Catholicism's influence is stronger) and areas where the scandal is less likely to have a pervasive local impact (counties where Catholic influence is more diffuse).[5]

Because we expect the Catholic clergy abuse scandal to affect communities most strongly where Catholicism is more prevalent and culturally formative, we use a triple-

[5]This identification based on the local prevalence of Catholicism at the county level within the U.S. is akin to Kumar et al. (2011)'s methodology in identifying the local influence of Catholic culture.

difference approach using a stacked sample around the staggered timing of the religious scandals, with the triple-difference estimator focusing on firms located in highly Catholic counties subject to the shock after the scandal revelations. This empirical design offers another strength: Any confounding event during the post-abuse crisis period would have to specifically affect firms located in Catholic-intensive counties in order to bias these estimates.[6] This greatly reduces the probability that our triple interaction term of interest systematically captures an effect not associated with the Catholic abuse crisis itself. We find that firms in areas most heavily affected by the scandal decrease their provision of trade credit by 5.3% to 5.9% of sales. Dynamic triple-difference estimation shows treated firms reduce their extension of trade credit starting in the year of the scandal, with levels remaining low in subsequent years.

Following previous studies on the relationship between cultural institutions and trust (Putnam, 1993; Fukuyama, 1995), we hypothesize that the most likely mechanism between the cultural shock and the decrease in trade credit is social capital, which Woolcock (1998) defines as the norms of cooperation shared by members of a community. To test the social capital channel, we investigate whether results vary with the importance of trust between supply chain partners. Giannetti and Wang (2016) argue that negative shocks to trust are more likely to affect counterparties that are harder to monitor, i.e., those for whom social capital plays a greater role in ensuring cooperation. We find evidence consistent with the social capital channel: Firms lower trade credit to customers who are lesser known and farther away, or when the relationship is more operationally complex.

Our trade credit results are robust to a battery of alternative specifications and tests:

[6] We utilize the technique recommended by Baker et al. (2022), who show that using a sample with stacks of treated and non-treated observation cohorts takes care of the econometric issues inherent in staggered differences designs.

We find that the magnitude of the effect is similar between earlier and later treated cohorts. Results hold for single-segment firms, alleviating concerns about geographic dispersion within firms. Moreover, our results are robust to the inclusion of entropy-balanced controls and to an alternative definition of strongly Catholic-influenced areas. We find consistent results in difference-in-differences estimations between treated and untreated firms within the subsample of heavily Catholic counties, as well as between firms in Catholic counties (subsequently referred to as "Catholic firms" for brevity) and non-Catholic counties within the subsample of firms in scandal-affected areas. One concern could be that the scandals adversely affect firms' financial ability to extend credit, but we find no evidence of any adverse effects on affected firms' financial standing. Finally, our estimation design greatly mitigates concerns about alternative explanations: First, since we focus on a 7-year window (3 years pre- and post-event) around the initial news of clergy removals, we avoid the effect of the 2008 Financial Crisis. In addition, our results are unlikely to be driven by industry-specific shocks, as we include specifications with industry-year fixed effects throughout our analysis.

This paper offers new, plausibly causal evidence on the influence of the local culture on interfirm relationships, contributing to the literature in several ways. First, we exploit quasi-exogenous shocks to show causal evidence of the role of local culture on firm behavior. Shocks to local culture are rare, but the Catholic clergy scandal allows us to observe effects of a shock to the firm's environment exogenous to the firm. Two related papers are Hilary and Huang (2023), who study the effect of the Catholic scandals on sporadic contracting decisions *within* the firm, and D'Acunto et al. (2022), who find that corporate scandals affect trust in corporate consulting contracts. Distinct from these prior works, we offer evidence that shocks to local *cultural* institutions also affect local firms' recurring interactions with supply

chain *counterparties* (external to the firm). Further, our results point to a specific social capital channel through which the shock to trust in the Catholic Church affects local firms' supply chain relationships.[7]

Second, we contribute to the literature on local culture and trust. The traditional view on culture is that the values and beliefs it embodies are very slow-moving (Putnam, 1993; Fukuyama, 1995). By contrast, a strand of literature finds that social connections (Duflo and Saez, 2002, 2003; Hong et al., 2004) and shocks to religious identity (Benjamin et al., 2016; Bryan et al., 2020) can impact individuals' behavior very swiftly. Accordingly, new theory highlights the possibility of rapidly changing cultural attitudes (Acemoglu and Robinson, 2021). Consistent with these latter papers, we find evidence that changes in local cultural institutions quickly alter firm behavior toward their counterparties. Moreover, with our focus on ongoing relationships between supply chain partners, this study is distinct from papers that focus on the impact of cultural norms on rare firm events, such as mergers and acquisitions (e.g., Ahern et al., 2015).

Third, we contribute to a stream of papers examining the role of non-pecuniary factors in trade credit decisions, such as bargaining power (e.g., Klapper et al., 2012; Murfin and Njoroge, 2015; Fabbri and Klapper, 2016; Billett et al., 2025) and competition (e.g., Giannetti et al., 2021; Chod et al., 2019). Particularly relevant to our setting, Giannetti et al. (2011) and Dass et al. (2015) show that trade credit use is higher in relationships requiring more collaboration. McMillan and Woodruff (1999) use the decision to extend trade credit as a measure of customer trust, and Antras and Foley (2015) show repeated interactions foster

[7] Hilary and Huang (2023), who also use the Catholic Church scandal as a shock, do not take an explicit stance on the channel through which the local trust shock operates in their setting. We exploit cross-sectional variation across firms' customers to show evidence for the social capital channel, but not for the channels mentioned by Hilary and Huang (2023) that would apply in our setting (risk aversion and profitability).

cooperation in import-export relationships. We contribute to this area of the trade credit literature by showing plausibly causal evidence of how the cultural environment in which the firm operates affects the provision of credit.

Two particularly related papers are Wu et al. (2014) and Levine et al. (2018). Both papers show greater use of trade credit by constrained firms in high-trust regions, emphasizing how trade credit can smooth financial frictions in these locations. We differ by abstracting away from financial constraints and studying how shocks to local cultural institutions shape trade credit decisions (outside of a crisis setting). Since societal patterns like norms of trust are long-standing and likely correlated with other social and economic patterns, our work shows a plausibly causal relationship via an adverse shock to trust in cultural institutions. Our results also differ by showing a *complementary* relationship between trust in institutions and trade credit extension: While Wu et al. (2014) and Levine et al. (2018) show that lower trust in financial and political institutions *increases* trade credit usage, we find that a decrease in trust in *cultural* institutions *reduces* trade credit. Our setting also allows us to focus on a shock affecting a set of firms with a more homogeneous legal environment (all U.S. firms). Thus, our study highlights the role of trust in trade credit decisions even in an environment with relatively stronger legal protections and financial access.

2 Background: Religious Scandals, Trust, and Trade Credit

This section provides motivation from the literature to show (1) how a negative shock to cultural institutions would affect interfirm decisions involving trust, and (2) why trade credit extension is a good laboratory for examining such interfirm decisions.

2.1 Religious Scandals and Firm Decision-Making

Cultural institutions instill norms and beliefs that affect individuals' willingness to trust (e.g., Fukuyama, 1995). Trust, in turn, impacts business judgment in day-to-day operations: For example, Pursiainen (2022) finds evidence that equity analysts' opinions are affected by their country's cultural biases toward the country of the firm they evaluate. Rothstein and Stolle (2008) underscore the importance of ethical behavior by leaders of local institutions to foster trust: Individuals infer that "if those in positions of authority cannot be trusted, then most other people can surely not be trusted" (p.446).

Following a large literature, we focus on religion as an important cultural institution (e.g., Putnam, 1993, 2000; Hong et al., 2004). Religion has a positive impact on individuals' trust (Guiso et al., 2003), which decreases contract enforcement costs (North, 1990; Knack, 2001). Employees' religion plays an important role in shaping their ethical behavior in the workplace (Buren et al., 2020; Weaver and Agle, 2002). This influence is ascribed to the weight that religious values bear on individuals' decisions, through the importance of religious tenets in local social norms and the moral authority of religious institutions (McGuire et al., 2012; Peifer, 2015). Conversely, ethical breaches by religious leaders constitute a negative shock to trust in religious authority for individuals in the local community, which we expect to negatively spill over into their business decisions. For example, Hilary and Huang (2023) find that religious scandals lead to more stringent contracts for CEOs of local firms. Our hypothesis is related but distinct: First, we study the role of trust in counterparty relationships where contracts are part of the recurrent business of the firm, while Hilary and Huang (2023) focus on the product of sporadic negotiations between the firm's principals

and their primary agent, the CEO. We look at relationships between the firm and outside parties (supply chain partners), while Hilary and Huang (2023) study relationships internal to the firm, between the board and their top employee. Moreover, our results provide evidence of a social capital mechanism through which the shock to trust impacts local firms' decisions. Finally, we study contracts for which enforcement provisions are generally implicit (and therefore highly dependent on the trade credit provider's trust), whereas Hilary and Huang (2023) focus on the characteristics of explicit provisions in compensation contracts (which are then subject to careful legal enforcement).

Since firms tend to hire locally, we expect religious scandals to impact local workers in communities where the religion is locally prevalent and, thus, has greater social weight (Kumar et al., 2011; LaPorta et al., 1997; Stulz and Williamson, 2003).[8] Prior literature shows that individual-level contextual factors affect firm performance. In addition to the cultural biases in analyst recommendations discussed above (Pursiainen, 2022), employees' cultural backgrounds influence workplace behaviors such as shirking and absenteeism (Ichino and Maggi, 2000). Also, personal life shocks affect both innovative productivity (Bernstein et al., 2021) and investment decisions (Pool et al., 2019). Since individuals' decisions are influenced by external factors, we anticipate that a negative shock to trust in religious institutions would affect firm outcomes in the community via their employees.

While previous work considers the role of direct social ties between firms and the effects of managerial actions (e.g., Dasgupta et al., 2021; Cline et al., 2018; Cavusoglu et al.,

[8]The effect of religious shocks could occur due to the social influence of a dominant religion or through firm managers' religious identity. Yin (2021) offers evidence that managers' prosocial behavior takes cues from their employees, giving credence to the former perspective. Alternatively, some papers identify firm effects based on managers' disclosed religious affiliations (Adhikari and Agrawal, 2016; Baxamusa and Jalal, 2016; Cai et al., 2019). This approach usually yields small samples as most managers do not disclose their religion. As such, we remain agnostic as to which of the two channels might be at play here.

2023), we examine the role of local cultural institutions in interfirm cooperation. Our empirical design addresses the issue of endogenous cultural factors by focusing on a local shock to a large religious denomination, Catholicism, in areas where this religious denomination is prevalent. Our key hypothesis is that the adverse shock to trust in Catholic leadership affects decision-making in firm actions involving trust.

2.2 Trade Credit and Trust

In this section, we discuss the institutional background of trade credit in our empirical setting. We focus on the incomplete contracting and implicit nature of its use that mean relational factors (such as trust) can and do play an important role in trade credit decisions. Trade credit usage is widespread globally (e.g., Rajan and Zingales, 1995) and in the U.S., where its volume is greater than 20% of gross domestic product (Garcia-Marin et al., 2019). Simultaneously, trade credit is governed under incomplete, largely informal contracts (Macaulay, 2018; Malcomson, 2012; Federal Reserve Board, 2007; Levine et al., 2018).[9] For example, Macaulay (2018) notes the prevalence of ambiguously defined terms in vertical transactions, implicitly accompanied by an acknowledgment that the parties would negotiate if needed.[10] Indeed, many papers model trade credit as a way to help alleviate the problems of incomplete contracting implicit in supply chain transactions. Dass et al. (2015) and Kim and Shin (2012) model trade credit as a way of integrating the fortunes of firms along the supply chain to help align firm goals. Knack and Keefer (1997) cite "[transactions]

[9] As an illustration of the widespread informality of trade credit, in its small business survey form, the Federal Reserve (Federal Reserve Board, 2007) notes that trade credit is not typically governed by a formal contract but rather simply an arrangement of "purchases on account."

[10] Macaulay (2018) describes a "battle of the forms" in which the buyer's purchasing order and the seller's supply order frequently include conflicting terms. Transactions continue regardless.

in which goods and services are provided in exchange for future payment" (p.1252) as an example of trust-intensive contracts. Related, Cai et al. (2023) find that corruption, which weakens trust between counterparties, is negatively correlated with the use of trade credit, illustrating the role that trust plays in the provision of trade credit.

Why does trade credit extension involve supplier trust? Trade credit in the U.S. is regulated under various state laws following the Uniform Commercial Code (UCC). The UCC specifies that trade credit must be agreed upon and ensures the seller's rights to assurance of performance, reclamation of goods, and legal recourse to sue for receivables enforcement. Thus, from a strictly legal standpoint, suppliers seemingly have at least some degree of recourse for defaulted payment. Indeed, several early papers examining the rationale for the existence of trade credit postulated that suppliers may have a lending advantage over banks due to superior ability to redeploy collateral in the case of default (e.g., Fabbri and Menichini, 2010; Frank and Maksimovic, 1998). In reality, however, practical and market factors limit the extent to which these provisions protect sellers from default or late payment. Confiscating unpaid-for products is challenging (at best) once the goods have been transformed or sold, and by definition impossible for firms supplying services rather than physical products (Costello, 2019). Even in cases where seizing collateral is technically possible, the window in which it can be done is limited. Prior to 2006, the reclamation period for goods sold on credit was 10 days under the UCC. Costello (2019) shows that after a U.S. legal change increased the reclamation period for reclaiming goods sold to 45 days,[11] firms extended more credit and to riskier customers. However, even a 45-day window typically closes before the end of terms typically observed for firms in Compustat (e.g., the mean number of days outstanding

[11]The Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA).

in our sample is 63 days of sales).[12] Thus, given logistical and legal challenges, the ability to seize collateral in the event of default is tenuous.

Further, while default is a major risk in extending credit, so is late payment. Cowton and San-Jose (2017) discuss payment length as an ethical dimension of trade credit, emphasizing the importance of the informal promise of payment between counterparties. Late payment practices are common: Ng et al. (1999) find in survey results that important customers often delay payment past terms without penalty, and Klapper et al. (2012) find that large, financially secure firms receive the most favorable terms from suppliers.[13] Murfin and Njoroge (2015) show that suppliers can become financially constrained after being forced to accommodate unilateral changes in payment policy of downstream buyers with significant bargaining power. Thus, given that large customers frequently abuse terms due to bargaining power, suppliers face risks regarding timely payment, even from customers of high credit quality.[14]

In summary, given the informality frequently implicit in trade credit agreements, large potential costs of default or late payment, and limitations in legal recourse, relational factors will influence decisions to extend trade credit. Extending trade credit to a customer explicitly creates a financial vulnerability, exposing the supplying firm to repayment risk and magnifying the potential for adverse supply chain spillover (Jacobson and Von Schedvin,

[12] Even so, in untabulated results, we find weaker effects after passage of the BAPCPA reform studied in Costello (2019).

[13] Indeed, Lieberman et al. (2025) provide asset pricing evidence that late payment often corresponds to significant vertical bargaining power and strong future returns.

[14] As another channel, counterparty *reputation* could promote greater trade credit extension as well. Our setting focuses on a supply-side negative shock to trust (for the supplier). From extant literature, it is not clear how reputation and culture interact with each other regarding trust (e.g., Dupont and Karpoff, 2025). For example, ex ante, customers with a positive market reputation could be insulated from the negative trust shock or the shock could simply adversely shift the suppliers' priors (i.e., regardless of customer reputation). In untabulated results, we find that suppliers reduced trade credit similarly to customers recognized as "most admired" firms, evidence more suggestive of the latter.

2015; Hertzel et al., 2008; Boissay and Gropp, 2013). By focusing on religious scandals in the firm's headquarter location, we exploit cultural shocks likely to affect trade credit decision makers, since receivables policy and credit decisions are typically set by middle management who are likely to work locally.[15] Particularly, a negative shock to supplier trust should adversely impact the extension of credit to a customer.

3 Shock to the Local Culture

3.1 Institutional Details

As a shock to trust in cultural institutions, we employ the Catholic clergy sexual abuse scandal triggered by the news revelations from the *Boston Globe*'s Spotlight team. In January 2002, this group of journalists documented that the head of the Catholic church for the Boston region, Cardinal Bernard Law, had reassigned priests whom he knew to have abused children in past assignments. These abuses took place decades earlier (most of the U.S. Catholic clergy abuses publicly disclosed as a result of this crisis occurred in the late 1960s and 1970s; see John Jay College of Criminal Studies (2004)). While the serial abuse by some of these priests was already known before 2002, the direct involvement of the local bishop in knowingly assigning priests accused of abuse was a thundering revelation to local Catholics.[16]

Following these revelations, Catholic bishops in many parts of the U.S. were faced with

[15] See, e.g., Masson and Clarke (2015) for a discussion of how working capital management hierarchies vary across businesses.

[16] In the Catholic church, bishops directly assign clergy; the local church does not have a say about who their local priests are. Importantly, the revelations of the Spotlight team and the subsequent crisis in the U.S. Catholic church are based on local bishops' perceived inaction in the face of the news of their abusive clergy (see Bruce (2011))–rather than about the existence of abuse by local clergy. Instances of (even serial) abuse by Catholic clergy was already well-known prior to 2002 (e.g. John Geoghan in Boston).

the call to review the files of the priests and deacons in their regions in light of resurfaced past claims of clergy abuse of minors. These reviews led to the removal of some of these ministers from their current assignment. Clergy removals implicitly acknowledge previous mishandling of abuse claims by the then-bishop: A priest who should not have been reassigned after claims of abuse decades prior had been allowed to keep working. Also, the removals present other advantages as a measure of the scandal. First, the removals are announced concurrently with the public disclosures of allegations against the clergy member. Therefore, the removals reveal the presence of a potentially hurtful clergy member in a church community (and in the communities where he worked previously).[17] Second, they only occur if allegations of abuse by a given clergy are credible, corresponding to a high "signal-to-noise" ratio relative to allegations themselves. Third, because removals require that a clergy member be actively working, they are much more salient to the local Catholic population than allegations against a long-ago retired, defrocked, or deceased priest. Removals make it clear to the local Catholic population that, though most instances of abuse occurred decades prior, bishops' mishandling of those claims has lingering consequences to date.

Bruce (2011) details how this scandal eroded Catholics' trust in their local Church leaders. Hungerman (2013) offers evidence that Catholics in regions affected by these revelations left their Church, while Bottan and Perez-Truglia (2015) find that local households reduced religious participation and charitable giving. The scandal also lowered attendance in Catholic schools (Esparza, 2020). Given that religious leaders are often seen as models of

[17]While the removal is a remedial action that signals the resolve to tackle the issue of abusive priests, evidence from the news articles discussing the removals shows that the public reaction to these revelations is overwhelmingly negative. As a corporate parallel, Palmrose et al. (2004) show negative announcement returns following accounting restatements: While revising incorrect statements is a positive action (as is removing an abusive clergy member), news of the revision (removal) also reveals the past misconduct. In both cases, the latter (negative) revelation overwhelms the former (positive) measure.

moral behavior (Weber, 1978; Rothstein and Stolle, 2008), we posit that the Catholic clergy abuse scandal, which damaged trust in the Catholic Church and its leadership, is likely to have spillover effects to trust in interfirm cooperation. We hypothesize that this shock will be most salient in areas where the Catholic culture is more prevalent.

Even within their Church, Catholics' behavioral shifts appeared to be specifically directed toward bishops – the leaders who violated their trust by reassigning accused priests. For example, Esparza (2020)'s finding of lower school attendance is limited to Catholic schools under the purview of the bishop. By contrast, Esparza does not find a significant effect of the scandal on attendance in schools supervised by other Catholic entities (viz. religious orders). Moreover, within dioceses, Catholics treat their bishop and local priests differently: They pulled back from their relationship with the former but not with the latter.[18] Following these findings, we view shocked Catholics' behavior as a redeployment of cooperative relationships away from the higher echelons of the Catholic Church, where trust is more costly, and toward closer, easier-to-monitor, representatives. This pattern is consistent with local Catholics investing less in relationships where trust is costlier.

The Catholic abuse scandal damaged trust in an influential local institution. Similar to work finding that workers' culture influences their views of other firms (as discussed in Section 2.1), our hypothesis is that this scandal affected individuals' cooperative behavior toward their firms' counterparties. This hypothesis is consistent with (while distinct from) prior work showing the scandal revelations affected financial transactions, including CEO

[18]Mary Gautier, a researcher at CARA, a center specialized in the Catholic Church: "There was a small trend in backing off from [fundraisers from the bishop], but it was offset by people giving more to their local parish," Gautier said. "We interpreted it as people saying, 'I'm disappointed in my bishop and angry about what's going on in the church, but I still love my parish and the pastor still needs money to keep the lights on'" (Albrecht, 2018).

compensation (Hilary and Huang, 2023) and stock market participation, a financial activity heavily dependent on trust and cooperation (Guiso et al., 2004, 2008; Dupont, 2025).

3.2 Motivating Evidence

To validate the conjecture of a spillover effect from trust in religious institutions to trust in a business context, we analyze the General Social Survey's questions on confidence in various institutions over the years of our sample period (GSS survey years 1998 to 2006). In particular, we focus on two questions: (i) confidence in churches and religious organizations; and (ii) confidence in large businesses. For each of these questions, respondents can answer whether they have a "great deal," "only some," or "hardly any" confidence in the organization. We create variables based on these answers: $Confidence\_Rel$ ($Confidence\_Bus$) is set to 1 if a respondent answers that they have "a great deal" of confidence in churches and religious institutions (large businesses), 0 if they answer the "only some," and -1 if they answer "hardly any." We then estimate the equation:

$$Confidence\_Bus_{i,t} = \alpha_c + \alpha_t + \beta \times Confidence\_Rel_{i,t} + \gamma * X_{i,t} + \varepsilon_{i.t} \tag{1}$$

for respondent i in year t. We include year and county (c) fixed effects based on the GSS restricted localization data. $X_{i,t}$ is a vector of controls that include the respondent's age, demographic characteristics (gender, education, race, as well as marital, employment and health statuses), political preference, and (quintiles of) income and socio-economic position. In our strictest specification, we also control for the respondent's propensity to trust strangers. This variable helps isolate the correlation between confidence in business and

religion by controlling for individuals' general willingness to trust.

Figure 1 presents the point estimates and confidence intervals for the coefficient of interest β from equation 1.[19] The tests show a positive and significant correlation between confidence in religious institutions and confidence in businesses, without controls (left), with the baseline controls in vector $X_{i,t}$ (middle), and with the baseline controls augmented by the respondent's propensity to trust strangers (right). In all cases, the correlation between trust in religious organizations and in businesses is positive and statistically significant. In untabulated tests, we verify that these results hold across subsamples regardless of Catholic affiliation, in both treated and non-treated areas, and both before and after the scandals. Admittedly, this evidence is correlative rather than causal. In this regard, our triple-difference estimation method is particularly helpful. Since the Catholic Church scandal is a negative shock to the religious institution itself, our triple-difference setting captures the effect of this shock to trust in the Catholic Church on local individuals' decisions. In Section 4.1.1, we use this triple-difference identification strategy to provide evidence that the Catholic scandal negatively impacts both confidence in religious organizations and confidence in business in areas affected by the Catholic Church abuse scandal.

As discussed in the evidence from Section 3.1, the Catholic abuse scandals are a negative shock to trust in a prominent local religious institution. The results from Figure 1 show that trust in religious institutions and trust in businesses are highly correlated. This pattern suggests that a (negative) shock to trust in the Catholic Church could (negatively) impact Catholics' trust in businesses, which would affect trade credit interactions between

[19]The full regression results can be found in Appendix D.

counterparties.[20]

With this motivating evidence on the correlation between trust in religion and trust in businesses, and given previous findings on the effect of the Catholic clergy abuse scandal, we hypothesize that the scandal will reduce the willingness of local firms to cooperate with counterparties, especially for firms located in communities where Catholicism is prominent and in vulnerable settings where trust is important. Therefore, we expect the negative shock to the local culture to lead to more restrictive trade credit provision between the firm and its customers.

The most natural channel through which damaged trust in institutions would affect interfirm cooperation is via a reduction in social capital, that is, the norms of cooperation between parties (Knack and Keefer, 1997; Guiso et al., 2004). Alternative channels might also be at play, among which the leading candidates are risk aversion and financial deterioration. We test these alternative channels in Section 6 and find no supporting evidence for either.[21] We do find evidence supporting the social capital channel: Firms particularly contract trade credit provision in customer relationships where trust is more important.

[20] The GSS question about trust in businesses is particularly helpful evidence in the context of trade credit, since it asks for confidence in large businesses, and customers in our trade credit data tend to be very large businesses.

[21] Another plausible way a religious scandal could impact firm behavior is through a personal ethics channel, since religions' moral teachings impact personal ethics. However, Bottan and Perez-Truglia (2015) find no effect of the abuse scandals on religious beliefs. Therefore we do not expect this channel to be at play.

4 Empirical Framework

4.1 Data

To analyze how a negative cultural shock affects the provision of trade credit, we first collect information from Factiva on the clergy removals discussed in Section 3. We then merge the location and date of the removals with historical firm headquarter locations, using a panel of pair-level trade credit between U.S. publicly traded firms and their customers. The following sections describe our data composition in more detail.

4.1.1 Catholic Clergy Scandal and Local Catholic Population Data

We collect information about clergy removals through a search of news articles in Factiva based on a broad set of keywords between 2001 and 2007 (see Appendix B for more details). One diocese (Greensburg, PA) had a clergy removal in 2001 which was unrelated to the aftermath of the Spotlight news series; we exclude this diocese from the sample. The dioceses with removals in 2005 (e.g., Anchorage, AK; Honolulu, HI) include hardly any industrial activity. The news search yields no new dioceses with removals in 2006. Moreover, we exclude the dioceses impacted in 2007 from the treatment cohorts, since the post-impact analysis is potentially confounded by the effects of the financial crisis of 2008. As a result, we focus on the dioceses faced with removals between 2002 and 2004. In that span, 113 out of the 176 U.S. (Roman) Catholic regions ("dioceses") were faced with clergy removals related to abuse claims, covering 52% of U.S. counties.[22]

[22] See Appendix B for a detailed explanation of the data collection method for removals and Appendix C for the list of impacted dioceses and the year of their initial year of impact.

We mark these dioceses directly affected by the Catholic Church scandal with a "Removal" indicator. The "Post" indicator is set to 1 in the year of the removal. Figure 2 shows the U.S. regions impacted by Catholic clergy removals due to claims of abuse and the initial time of the announcement of the removals. The removals cover a diverse and significant portion of the country. We link diocese boundaries to U.S. counties using a diocese-county mapping file provided by the Center for Applied Research in the Apostolate (CARA) in Washington, D.C.

To measure the prevalence of the local Catholic culture, we use an indicator set to 1 if the Catholic share of the population in a given county is above the population-weighted median among U.S. counties.[23] We obtain information on the Catholic local population from the 2000 survey of the Association of Religious Data Archive (ARDA Association of Religion Data Archives, 2000). We refer to these areas where the Catholic Church is prevalent as "Catholic counties."

Before turning to our primary focus on interfirm trade credit, we revisit the GSS data to provide evidence of how the scandal affected trust in religious institutions and large businesses. Following Hilary and Hui (2009)'s and Hilary and Huang (2023)'s work on the impact of religion on firms using the GSS, we collapse the data at the county-year level and interpolate the values of confidence in religious organizations, confidence in large businesses, and a similar set of controls to what we used in equation 1. With these interpolated data, we create a stacked sample for the event cohorts that we use in our main sample (that is, 2002-2005), with the dioceses not treated during the event window as the control group.[24]

[23] Our main results are qualitatively similar if we use the sample median (see Table 9).

[24] We use a stacked difference-in-difference framework with not-yet-treated observations as the control group, following Baker et al. (2022)'s advice on how to deal with staggered effects.

To test the impact of the scandal on local confidence in religious and business institutions, we use the following triple difference estimation:

$$Confidence\_I_{c,t} = a_{ce} + a_{te} + b_1 Post_{c,t} \times Catholic_c \times Removal_{c,e} + b_2 Post_{c,t} \times Catholic_c + b_3 Post_{c,t} \times Removal_{c,e} + c * X_{c,t} + \epsilon_{c,t,e} \quad (2)$$

for county c, year t and event cohort e, where I refers to either religious institution or businesses. $Removals_{c,t}$ is set to 1 if the county is located in a diocese with clergy removal, and $Post_{c,t}$ is set to 0 prior to the initial removal in the diocese and 1 from that date. We ignore observations when the year of the survey is the year of the initial removal, and estimate the effect for three years around the event year. We interact the county and year fixed effects with event cohort fixed effects and cluster errors at the diocese-event level.

The blue bars in Figure 3 represent the coefficients of the triple interaction term of interest(b_1). The two left-most bars show the estimates for the confidence in churches and religious organizations outcomes, without and with controls. In both specifications, respondents in Catholic counties affected by the scandal display a significantly lower trust in churches and religious institutions. The two right-most bars show a similar effect for confidence in large businesses, again both with and without controls.[25] The results emphasize the negative impact of the scandals on confidence in religious and business organizations in Catholic counties impacted by the scandal.

[25] Appendix Table D2 presents these results in regression form.

4.1.2 Firms' Trade Credit Provision

Our empirical setting focuses on the effects of the Catholic Church scandal (as a shock to trust in cultural institutions) on interfirm relationships, using trade credit between supply chain partners as our outcome of interest. To compile a dataset of trade credit between publicly traded U.S. firms, we start with the Compustat Segment database, which provides firm-customer links based on disclosures of major customers as required by the SEC's Statement of Financial Accounting Standards (SFAS) No.14 and No.131.[26] Compustat (including firms in its Segment database) provides only aggregate balance sheet information on trade credit (i.e., accounts receivable outstanding due from all customers and accounts payable outstanding due to all suppliers). In order to study granular trade credit decisions between supply chain partners, we exploit FASB No.105, which requires firms to disclose credit concentrations in their financial statements.[27] Many firms disclose receivable balances with major customers under this regulation. We manually search firm financial statements for disclosures of trade credit balances outstanding with their customers, following procedures in Freeman (2025). Specifically, starting with firm-years with disclosure of at least one customer in the Segment database, we read the firms' 10-Ks for disclosures of customer-specific receivable balances. Recording these balances results in a firm-customer-year panel with trade credit balances and annual sales at the pair level.[28] In later tests with dependent

[26] Under these regulations, firms must report the names of customers comprising 10% or more of their sales. Most of these disclosures include annual sales to these customers, and frequently include major customers under the mandated 10% reporting threshold.

[27] FASB 105 concerned concentrations of credit risk for all instruments as well as the disclosure of off-balance-sheet financial risks. FASB 105 has since been subsumed by subsequent pronouncements and amendments, but its paragraphs regarding concentrations of credit risk have been preserved by shifts to other statements, including FASB 107 then 161, with disclosure guidance virtually unchanged.

[28] As in other papers using similar data and collection procedures (Billett et al., 2025; Ersahin et al., 2024), the use of a quasi-exogenous shock alleviates potential concerns about selection issues in the sample of

variables other than *Trade Credit*, we use the expanded set of customer-supplier relationships in the Segment database ("Segment sample").

4.2 Empirical Strategy

We merge the data on Catholic population share and local clergy removals to firms' historical headquarters locations from Compustat.[29] We then construct a stacked sample following Gormley and Matsa (2011) and Baker et al. (2022) based on each "cohort" of removals occurring in years 2002-2004. In each cohort, we define treated firms as suppliers headquartered in dioceses experiencing a clergy removal (*Removal*) in that cohort-year.[30] Controls are restricted to firms not treated within the seven-year cohort event-window (i.e., the "not-yet-treated" group).[31]

To study interfirm cooperation through the lens of trade credit, we compare the extension of trade credit by treated firms (those headquartered in *Catholic* communities experiencing a *Removal*) to control firms before and after the *Removal* event in a triple difference-in-difference approach:

$$Trade\ Credit_{i,j,t} = \mu_{i,j,e} + \tau_{k,t,e} + \beta_1 Post_{i,t} \times Catholic_i \times Removal_{i,e} + \beta_2 Post_{i,t} \times Catholic_i + \beta_3 Post_{i,t} \times Removal_{i,e} + \gamma Controls_{i,j,t} + \epsilon_{i,j,t,e} \quad (3)$$

firms reporting credit balances.

[29] We use historical headquarters location sourced from Notre Dame University's 10-X Header Data. In cases where header data was not available, we use current headquarters location from Compustat. Results are robust to excluding observations without historical headquarter data (see Table 4).

[30] We follow the literature in using the headquarters location to define treatment location for Compustat firms (e.g., Chaney et al., 2012; Ersahin et al., 2024; Atanassov et al., 2024). We discuss this convention in Section 5.2.

[31] The hand-collection for our clergy removal data stops in 2007, focusing on the period after the Boston Globe Spotlight revelation in 2002. We use a "not-yet-treated" control group as opposed to a "never-treated" control group because we do not have data on removals occurring after 2007.

where i denotes a firm, j denotes a customer, k denotes the firm's industry, t denotes a year, and e denotes a cohort-event. *Trade Credit* is the ratio of trade credit to sales from firm i to customer j in year t. *Catholic* indicates a firm headquartered in a community with a high proportion of Catholic residents, *Removal* indicates a firm headquartered in a community experiencing a Catholic church clergy removal in the cohort-event-year, and *Post* indicates an observation after the *Removal*.[32] The individual effects of *Catholic*, *Removal*, *Catholic* $\times$ *Removal*, and *Post* are subsumed by fixed effects. We use a seven year event-window starting three years prior to the *Removal* event and ending three years after.

We control for pair-event (firm-customer-event) fixed effects ($\mu_{i,j,t}$) and firm industry-event-year fixed effects ($\tau_{k,t,e}$). (For brevity, while fixed effects are always interacted with the event fixed effects, we will often refer to "pair fixed effects," "year fixed effects," etc.) The pair fixed effects absorb unobservable relationship characteristics that may affect supply chain matching, allowing us to isolate variation in *Trade Credit* over time around the *Removal* events. Industry-year fixed effects help control for any time-varying industry effects, particularly useful for purging any effects from potential geographic industry clustering of treated firms (discussed in Section 4.4). *Controls* include firm and customer characteristics, described in the next section.

[32] We define *Post* on the date of the first news about the diocese scandal, setting the variable equal to 1 if the firm's fiscal-year-end is at least 30 days after this news date, based on the term of receivables typically being 30 days or more. For control firms, we define a pseudo "news date" using the median date within the cohort. Results are virtually unchanged if we instead exclude firms with fiscal year-ends within 30 or 60 days of the news dates (untabulated).

4.3 Control Variables

Control variables include firm characteristics documented to affect the supply and demand of trade credit (e.g., Petersen and Rajan, 1997; Giannetti et al., 2011; Klapper et al., 2012): *HHI*, industry concentration; *Size*, the logarithm of firm assets; *Age*, the logarithm of the number of years since the firm first appeared in Compustat; *Leverage*, the firm's book leverage ratio; *Profitability*, operating income scaled by total assets; and *R&D*, the firm's R&D expense (set equal to zero when missing) scaled by total assets. We include these controls for both the firm and its customer. All continuous variables are winsorized at the 1^{st} and 99^{th} percentiles, while *Leverage* is bounded by 0 and 1. Appendix A details variable definitions.

4.4 Summary Statistics

Panel A of Table 1 reports summary statistics for characteristics of treated and control observations in the pre-event period. Characteristics are averaged across the three pre-event years, so the number of observations indicates the number of unique firm-customer-cohort pairs. Comparing firm and customer characteristics, customers tend to be larger, more profitable, and more highly levered than their suppliers, consistent with other studies using supply chain linkages from the Segment database (e.g., Fee et al., 2006; Banerjee et al., 2008; Freeman, 2023).

Table 1 About Here

Comparison between the treated and control samples shows similar *Size, Age,* and *R&D* for both firms and customers, and similar *Leverage* and *Profitability* for supplying firms. Customer *Leverage* is marginally lower and *Profitability* marginally higher in the treated sample. The difference in *HHI* indicates that treated firms operate in less concentrated industries. Treated firms also extend more *Trade Credit* to their customers in the pre-event period. While a triple difference-in-difference analysis does not require treated and control groups to be perfectly similar across observable characteristics, the differences in *Trade Credit* could raise concerns about unobservable differences that might contaminate interpretation of the triple-difference estimates. However, the variation in industry concentration between treated and control areas suggests the difference in *Trade Credit* arises from heterogeneity in industry composition, since the nature of goods sold affects trade credit policy (e.g., Giannetti et al., 2011).[33] We confirm this by reporting an industry-adjusted measure of trade credit (*Trade Credit, Ind. Adj.*), which demeans *Trade Credit* by the average within the observation's industry-year. After demeaning, *Trade Credit, Ind. Adj.* is statistically indistinguishable between the treated and control samples. To rule out the potential for any time-varying industry patterns to affect our results, we include industry-event-year fixed effects in all of our tests. Additionally, we report a robustness test to our baseline findings using an entropy-balanced sample (Table 9).

Panel B reports summary statistics measured at the observation-level (i.e., firm-customer-event-year) for the full sample. Firm characteristics in the full panel mirror the pre-event observation-level characteristics of Panel A, with *Age* and *Size* increasing due to

[33] We find, for example, that a greater proportion of treated firms are in manufacturing industries, while a smaller proportion are in transportation or wholesale industries.

the inclusion of later years. *Trade Credit* averages 17% of annual pair-level sales, slightly lower than the pre-event averages.

5 Main Findings

5.1 Baseline Results

Our baseline tests examine whether firms in *Catholic* communities affected by a *Removal* contract their extension of trade credit after the shock. Table 2 reports results from estimating Equation 3. Columns report results in stages, based on fixed effects and sample restriction.

TABLE 2 ABOUT HERE

Columns (1) and (2) use the full sample, including pair and year fixed effects in Column (1) and adding an industry-year fixed effect in Column (2) (each interacted with the event fixed effect). Columns (3) and (4) are identical, but exclude customer-supplier pairs whose relationship was first reported after the cohort event year. Across all specifications, the triple-difference estimator, *Post*×*Catholic*×*Removal* is negative and statistically significant, and very stable throughout the various estimations. From the strictest specification in Column 4, results indicate that treated firms reduce trade credit by around 5% of sales, translating to the customer receivable balance being reduced by $8.6 million (representing 7% of the supplying firm's overall receivables and 1.1% of its total assets).[34] Said differently,

[34]The average annual pair-level sales in the sample is $175 million, so 5% × $175 = $8.75 million. Average total supplier receivables (assets) in the sample are $117.9 (775.39) million, so $8.75/$117.9 = 7% of receivables ($8.75/775.39 = 1.1% of total assets).

this corresponds to an 18-day reduction in days sales outstanding relative to the pre-event average of 69 days for treated firms.[35] To put this magnitude into perspective, Billett et al. (2025) find that stronger bargaining power facilitated by better credit access reduced trade credit to major customers by 4-5% of sales, similar to our 5% estimate. Costello (2019) shows that firms extend more and longer credit when their ability to reclaim goods in the case of customer default is enhanced. She finds firms extend terms by up to 11 days, comparable in magnitude to the negative results we find after an adverse trust shock.

Our results in Table 2 show a significant decrease in trade credit extended by *Catholic* firms in areas affected by the scandals. These findings underscore that the shock impacts firms where the local culture is heavily shaped by Catholicism – where the breach of trust from the shock is most salient and likely to impact human decision-making.[36]

Crucial to identification from difference-in-difference estimations is the assumption of parallel trends for treated and control groups absent the shocks. We test the assumption of parallel trends prior to the shock by reestimating Equation 3 in a dynamic framework:

$$Trade\ Credit_{i,j,t} = \mu_{i,j,e} + \tau_{k,t,e} + \Sigma_{t=-3}^{t=3}\beta_{1,t}Catholic_i \times Removal_{i,e} + \Sigma_{t=-3}^{t=3}\beta_{2,t}Catholic_i + \Sigma_{t=-3}^{t=3}\beta_{3,t}Removal_{i,e} + \gamma Controls_{i,j,t} + +\epsilon_{i,j,t,e} \quad (4)$$

We use year t=-1 as the benchmark, so the coefficient estimate for $\beta_{1,t}$ indicates the change in trade credit relative to the year prior to the shocks. Panel A of Figure 4 plots

[35] Trade credit terms rise and fall in a step-wise, not a continuous manner. For example, contracting credit terms previously set at 90 (60) days might result in new terms of 60 (45 or 30) days. Days sales outstanding is defined as the ratio of receivables outstanding to sales, multiplied by days in the sales period: $0.05 \times 365 = 18$ days.

[36] In untabulated results, we find an aggregate decrease in the receivables-to-sales ratio at the firm level, but the impact is weaker. We interpret this as suggesting that large credit concentrations are particularly trust-sensitive.

the results from this analysis. No observable trends are visible for the treated group prior to the event-year, but trade credit reduces sharply afterwards, remaining low in the three years after the shocks. We also examine dynamic patterns across four groups of firms, split along our triple-difference estimators: The first split is between Catholic/Non-Catholic counties, and the second is between Removal/Non-Removal dioceses. For each group, we estimate the following regression:

$$Trade\ Credit_{i,j,t} = \mu_{i,j,e} + \Sigma_{t=-3}^{t=3}\beta_t I(EventYear_t) + \gamma Controls_{i,j,t} + +\epsilon_{i,j,t,e} \tag{5}$$

Panel B of Figure 4 reports coefficient estimates for the event-year indicators (β_t) for each group, showing a significant reduction in trade credit only for the *Catholic, Removal* quadrant.

FIGURE 4 ABOUT HERE

5.2 Identification Assumptions

The baseline results in Table 2 show a significant reduction in trade credit extension from firms in *Catholic* communities after a *Removal*. Before examining the channels driving this reduction, we discuss potential concerns around two critical assumptions behind the baseline results and provide tests suggesting these concerns do not confound our findings.

We first address the possibility that omitted variables, such as unobserved political or economic changes, could drive both the clergy removals and reductions in trade credit. While completely ruling out this concern is (as in all difference-in-difference specifications) not

entirely possible, the triple-difference estimation helps alleviate these concerns considerably: Any unobserved factor driving our results would have to differentially impact specifically *Catholic* counties experiencing a clergy *Removal*. Panel B of Figure 4 shows graphically that the reduction in trade credit appears only for this specific group of firms. Nonetheless, in Table 3 we bolster our baseline findings with three sets of results to further alleviate omitted variable concerns. First, Panel A reports results collapsing our triple-difference empirical specification to two (separate) difference-in-difference analyses: In Columns (1) and (2) we include only firms headquartered in *Catholic* communities and examine the effect of *Post* × *Removal*. Treatment effects are similar to our baseline findings, showing that the effect of a *Removal* impacts firms in the community even in comparison to firms in other *Catholic* communities. Similarly, in Columns (3) and (4) we consider only firms headquartered in dioceses experiencing a clergy *Removal* and examine the effect of *Post* × *Catholic*. Treatment effects here are also similar to our baseline findings (Table 2), showing that even among a set of firms in scandal-affected communities, firms in *Catholic* communities are differentially affected.

Second, to address the possibility of legal or regulatory changes that could plausibly confound our interpretation, we replicate the baseline results including a state-year-event fixed effect (Panel B). This specification narrows our inference to comparison of firms in *Catholic* communities experiencing a *Removal* in the *Post*-event period relative to other event cohort firms in the same state and year. Results remain statistically strong, confirming that any state-level time-varying changes do not drive our findings.

Third, the most prominent regulatory change occurring around our sample period is the passage of the Sarbanes-Oxley Act (SOX), signed into law in July 2002. As the three

cohort years in our study are 2002-2004, the timing of the news shocks used in our sample overlap closely with SOX implementation. To alleviate any concerns about conflating effects, we focus a test around the SOX provision most pertinent to trade credit-related matters, Section 404, which relates to the establishment, reporting, and review of internal controls and reporting methods. We use data from Audit Analytics to link results of internal controls audits to the firms in our sample. Particularly, we construct an indicator, *RecIssue*, equal to 1 if the firm is ever in our sample period flagged as "Not effective" regarding internal controls for a reason key containing the word "receivable."[37] Firms we mark with a *RecIssue* typically have a reason phrase listing "Accounts/loans Receivable" among a list of multiple other possible reasons (e.g., "Liabilities, payables, reserves," "PPE, Intangible, or Fixed Asset (Value/Diminution) Issues," "Financial Derivatives/Hedging," etc.). Given this broad measure of *RecIssue*, our controls are likely overly conservative (i.e., many of these issues are not trade credit-related), but robust results when removing firms with a subsequent *RecIssue* should provide assurance against SOX compliance issues driving our findings. Panel C of Table 3 reports that findings are robust to either removing *RecIssue* firms from our sample (Columns (1)-(2)) or controlling for $Post \times RecIssue$ (Columns (3)-(4)).

Next, in Table 4, we consider our implicit assumption that firm headquarters location is an appropriate way to identify treated firms (*Removal*). We use historical headquarters location for the vast majority of the sample, but use the Compustat current location in the cases of missing historical data. We show in Columns (1) and (2) of Panel A of Table 4 that our baseline results are virtually unchanged if we include only observations with historical headquarters information. Even with accurate headquarters information, firms

[37] 2004-2007 represents the intersection of our sample period and SOX 404 implementation years.

may be geographically dispersed with multiple operating segments. Geographic dispersion likely does not threaten our inferences: Using a similar set of suppliers as in our sample, Barrot and Sauvagnat (2016) report that for the median firm, 67% of employees are at the headquarters location. Further, even for firms with dispersed plants, trade credit decisions are likely to be centralized: Surveys report that over 80% of firms centralize finance and accounting policies (Gitman et al., 1979; CFO.com, 2024). Regardless, we repeat our baseline results on subsamples of single-segment firms using Compustat Segment data. Columns (3) and (4) of Table 4 consider firms reporting only one operating segment, while Columns (5) and (6) consider firms reporting only one geographic segment. Results are robust to either restriction. The economic magnitude of the triple-difference estimator is notably larger in Columns (5) and (6) than in our baseline results. This is consistent with a local shock having a stronger impact on firms with more geographically centered operations. We discuss additional robustness tests in Section 7.

6 Economic Channels

Baseline results indicate that firms in Catholic areas affected by the Church scandals significantly reduce their extension of trade credit to customers in following years. In this section we empirically test the economic channels through which this credit tightening occurs. Evidence is most consistent with a social capital channel, with the religious scandals presenting a localized shock which reduces willingness to cooperate. We do not find evidence for a risk aversion channel, or any indication that firms reduced trade credit generosity due to a deterioration in financial standing.

6.1 Evidence of the Social Capital Channel

As discussed in Section 3, existing evidence of behavioral changes following the Catholic clergy abuse revelations indicates that damaged trust in an important local cultural institution weakened local social capital. We find evidence most consistent with this social capital channel in our empirical setting as well.

The importance of cooperation between a supplier and customer varies based on supply chain characteristics. If the scandals affect firm behavior towards customers via a social capital channel, then their effects should appear strongly in relationships where cooperation is more important or more difficult to cultivate – relationships where social capital is most valuable. By contrast, a shock to social capital should have minimal or no effect on interfirm cooperation in cases where vertical frictions are limited. The literature documents that trust and collaboration are particularly meaningful in supply chains characterized by relationship-specific investments, which amplify the potential for supply chain holdup problems (e.g., Williamson, 1979, 1987). In the face of incomplete contracting (Grossman and Hart, 1986), holdup costs arise from the problem of how to share relationship rents created through joint collaboration. We expect the effect of an adverse social capital shock to present only in the face of these holdup problems. Accordingly, in the following tests, we subsample relationships based on whether holdup costs are expected to be prominent. Particularly, we consider three cross-sectional tests:

First, we expect that long transaction history would insulate supply chain relationships from a negative social capital shock. Firms have better information about customers with which they have a long relationship, likely engendering confidence that may shield the

relationship from an adverse shock (e.g., Holmström and Roberts, 1998). Accordingly, when the firm has a long-established relationship with the customer, we expect a weaker or no reduction in trade credit. To examine this, we distinguish between pairs with the longest-lived relationships and others with a shorter transaction history, defining "Long" relationships as relationship tenure above the 75th percentile (seven years or more) as of the cohort event-year and "Short" relationships as those below this cutoff as of the event-year.[38] Comparisons are reported in Panel A of Table 5, where differences across subsamples are stark: While we continue to see a large reduction in trade credit in Short relationships, coefficients in the Long relationship subsample are statistically insignificant and very small in magnitude. That is, after the adverse cultural shock, firms reduce trade credit to customers with which they have limited history, but do not change trade credit policy towards their most established customers. Differences across subsamples are both statistically and economically significant, regardless of fixed effect structure.

TABLE 5 ABOUT HERE

Second, a negative social capital shock may not damage relationship collaboration if geographic proximity between partners makes monitoring easier. For example, Costello (2013) finds that supply contracts are looser when geographic proximity between partners makes monitoring less costly. In a similar vein, we expect that an adverse social capital shock damaging counterparty trust will have little or no effect when partners are geographically close. In Panel B, we subsample by distance between firm and customer headquarters.

[38] We exclude observations in which the first observed transaction between firm and customer is *after* the shock.

"Close" relationships denote a distance between headquarters below the 25th percentile (336 miles) and "Distant" relationships are above this cutoff. We find a strong reduction in trade credit in partnerships that are geographically distant (and thus, ex ante, relatively more reliant on trust vs. monitoring), but no such effect for partners that are geographically close, where the sign of the triple-difference estimator flips, depending on the fixed effects structure.

In both comparisons, firms affected by the scandals significantly decrease their provision of trade credit toward distant firms (Columns (2) and (4)). This decrease stands in contrast with the coefficient for geographically closer firms, which is positive (statistically insignificant) in the first specification (Column (1)), and less than half the magnitude (and statistically insignificant) of the coefficient on distant firms in the second comparison (Column 3). In both comparisons, the magnitudes of the coefficient estimates across subsamples are economically different, with a statistically significant difference in the comparison between Columns (1) and (2). These results are consistent with firms deliberately reducing their provision of trade credit to customers they are less able to monitor.

Finally, supply chain cooperation is more costly in cases where the potential for supply chain holdup problems is greater. Holdup costs are more severe when partners transact complex products, since their development requires collaboration and creates relationship rents, which are difficult to contractually share due to incomplete contracting (Grossman and Hart, 1986). Following the literature (e.g., Fee et al., 2006; Kale and Shahrur, 2007; Dass et al., 2015), we proxy for product complexity with the firm industry's (ex ante) R&D intensity. In the absence of complex products ("Low" complexity relationships, below the 25th percentile of industry R&D/assets), vertical frictions are weaker and we expect limited

effects from a social capital shock, in comparison to relationships involving complex products ("High" complexity, above the 25th percentile). Consistent with Panels A and B, we only observe a reduction in trade credit when the relationship involves complex goods and no change otherwise. Differences between the subsample splits are economically and statistically significant.

Together, results from Table 5 indicate a reduced willingness for firms to cooperate with difficult-to-monitor counterparties, resulting in lower trade credit extension to customers in relationships involving greater relationship-specific investments or more costly monitoring.[39] These findings support the social capital channel, since the effects only hold in relationships where trust is expected to be important.[40]

6.1.1 Shared Cultural Ties

Results from Table 5 are tentatively consistent with a shift in focus toward the familiar, since we do not observe trade credit reductions when customers are long-term partners or are geographically proximate. Another aspect of familiarity potentially relevant in the context of our empirical setting is similarity in cultural institutions, begging the question of whether treated firms differentially shift trade credit policy towards customers in non-

[39] In Table D3 of Appendix D, we report results replicating all three panels of Table 5 with median subsample splits, rather than splits at the upper or lower quartile. In all specifications, results are reassuringly consistent, but sometimes statistically weaker: Regarding relationship length, this suggests that only the longest-term customers are shielded from the trust shock; for geographic distance, the median distance (824 miles) is almost certainly not close enough to provide a meaningful monitoring benefit; and for R&D intensity, given that firms with major customers tend to have very high R&D intensity (Cen and Dasgupta, 2021), relationships are reasonably complex for all but the lowest-R&D firms.

[40] One may be concerned that the subsample splits of harder-to-trust customers correlate with customers being less important to the supplier. However, customers categorized as hard-to-trust in these three tests are not systematically less important: In ex ante comparisons (untabulated), suppliers sell a slightly *lower* proportion of their output to long-term customers and a slightly *higher* proportion to geographically close customers; there is no difference across high- and low-R&D industry suppliers. The mixed signs from these comparisons suggest the cross-sectional patterns in Table 5 are not spuriously driven by the sales importance of the customer.

Catholic areas relative to those headquartered in regions where Catholicism is also a locally important social institution.

We examine this in Appendix D (Table D4), where we split the sample based on whether the customer is headquartered in a community that is highly *Catholic*. Results do not support a cultural familiarity effect, as coefficients are identical in the first comparison (Columns (1) and (2)) and not statistically different from each other in the second comparison (Columns (3) and (4)). It appears that operational factors related to supply chain cooperation affect trade credit after the shock more than this aspect of cultural familiarity.

6.1.2 Financial Sophistication

We might expect older firms or larger firms to be more financially sophisticated and for their credit policies to be less affected by the cultural environment. In Table D5 of Appendix D, we show that results are indeed stronger for young firms and undetectable among older firms. Results are also greater in magnitude among smaller firms. While these measures are only rough proxies, results are consistent with a role of financial sophistication.

6.2 Alternative Channels

Section 3.2 discussed two alternative explanations for why the Catholic clergy scandals might affect firms' provision of trade credit: Firms may update their priors about the probability of being taken advantage of by a counterparty, indicating a risk aversion channel. Alternatively, the scandals may have damaged the financial standing of the firms (perhaps due to reduced local economic activity) such that they were less financially equipped to provide trade credit, which could lead to a reduction in trade credit as a side effect. We

examine these plausible alternative mechanisms and find no supporting evidence.

First, firms may contract credit terms with customers if they become more risk averse after the scandals. If this channel drives the observed reduction in trade credit, we expect the contraction to be most pronounced toward customers with riskier credit. In Table 6, we split the sample by customers' (ex ante) financial condition, labeling them constrained or unconstrained, based on three separate financial metrics: In Columns (1) and (2), constrained customers are those without an investment grade credit rating, while investment grade customers are considered unconstrained; in Columns (3) and (4), constrained (unconstrained) customers are those with a cash-to-asset ratio below (above) the 25th percentile; and in Columns (5) and (6), constrained (unconstrained) customers have a probability of default above (below) 1% (Bharath and Shumway, 2008; Merton, 1974). Results are never statistically different between the subsamples. Additionally, coefficients are generally very similar in magnitudes across the subsample splits. With similar results among customers that are more financially secure, we do not find evidence supporting the risk aversion channel.

Second, firms in affected Catholic communities may experience a negative shock to their financial ability to extend credit. This could plausibly occur via reduced economic activity in the area due to, e.g., suppressed consumer demand or business investment interest. To explore whether this channel may be at play, we apply our triple difference-in-difference framework to firm-level financial outcomes, using the Segment sample. If firms reduce trade credit due to a decline in financial standing, we would expect to see evidence of worse financial outcomes for treated firms in affected Catholic areas after the shocks. Table 7 reports results where the dependent variable is the (supplier) firm's cash-to-asset ratio, profitability, ratio of accounts payable to cost of goods sold, total book leverage, or short-term debt. We

find no effects of the triple-difference estimator on any of these financial outcomes, other than a modest increase in cash holdings, suggesting that any adverse effects on the overall financial status of firms resulting from the scandals were limited. Thus, we find no evidence of financial decline as a mechanism driving the results.

Overall, results support a social capital channel, with the negative cultural shock to affected Catholic communities leading to a reduced willingness to cooperate, affecting interfirm relationships.[41]

7 Additional Robustness Tests

In this section, we conduct various analyses to verify robustness of the main results along several dimensions. First, in Table 8, we verify consistent results on trade credit across cohort years. Particularly, we repeat our baseline (Table 2) results for the 2002 shock (Columns (1) and (2)) and for the 2003 and 2004 shocks (Columns (3) and (4)) separately. We combine the 2003/2004 events due to limited sample sizes; because a large proportion of the clergy removals were announced in 2002, the number of treated observations is smaller in the later event-years. Accordingly, statistical significance is strong for the 2002 cohort and weaker for the later cohorts, but, reassuringly, magnitudes are generally consistent across all specifications. The consistent magnitudes across event-years alleviate concerns about any contemporaneous shocks that might conflate results.[42]

[41] Also consistent with the social capital channel, in untabulated results we find stronger effects for firms located in areas characterized by higher ex ante social capital as measured by lower violent crime rates.

[42] The coefficients are slightly larger for the 2002 cohort than for the later ones, hinting at the possibility that earlier instances of the scandals at the outset of the *Boston Globe* investigation were slightly more impactful than later occurrences. However, the signs and magnitudes are consistent with previous findings that the scandal had a local impact on each affected area (e.g., Hungerman, 2013).

TABLE 8 ABOUT HERE

Second, in Table 9, we conduct robustness tests related to the data structure. In Panel A, we repeat the baseline results with entropy-balancing weights, based on ex ante characteristics of the treated and control groups. We continue to observe a statistically and economically significant decrease in trade credit after entropy balancing, with coefficients similar to those reported in Table 2. While Table 1 revealed the groups to be similar across nearly all dimensions, with only a couple of industry-driven differences that should be absorbed by our use of industry-year fixed effects, entropy balancing provides further assurance that ex ante characteristics are not causing a spurious correlation from time-varying patterns across groups. Panel B reports consistent results if we define *Catholic* counties as being above the sample median (rather than the national population-weighted median).

TABLE 9 ABOUT HERE

Taken together, results in Tables 8 and 9 suggest that the effects of the shock are qualitatively similar across cohort years and are not driven by differences in sample composition in the pre- and post-event periods.

8 Conclusion

Firm behavior is influenced by the behavioral norms of the people (managers) making firm decisions. Accordingly, local culture, which plays a significant role in how individuals

perceive others and interact with them, undoubtedly affects firm behavior. However, empirically, we know very little about how culture shapes corporate decision-making. While the literature acknowledges the role of culture in shaping firm outcomes, concrete evidence of its effects is limited and largely relies on cross-sectional cultural patterns. Our study shows significant impacts of a firm's cultural environment on its relationships with counterparties. Particularly, we examine the role of local culture in shaping interfirm relationships by studying how a firm's cultural environment affects its interactions with vertical trade partners. Since culture is notoriously endogenous to firm characteristics, we exploit plausibly exogenous shocks to local religious institutions to identify causal effects. We find that negative shocks to trust in these important cultural institutions have spillover effects on local firms' willingness to collaborate with supply chain partners. Firms headquartered in communities adversely affected by a religious scandal reduce their provision of trade credit to customers, evidencing a reduced willingness to lend and financially cooperate with them. These results concentrate in supply chain relationships where trust is more important, lending support to a social capital channel.

Our paper highlights the role of local culture in shaping firms' interactions with stakeholders. When a firm's community experiences an adverse trust shock, the firm behaves less trustingly toward trade partners, suggesting that the stability of social institutions influences perceptions of trade partners' reliability. Our findings also shed light on the role a firm's environment has in its supply chain interactions. Finally, our results demonstrate the importance of social capital in trade credit decisions.

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Figures and Tables

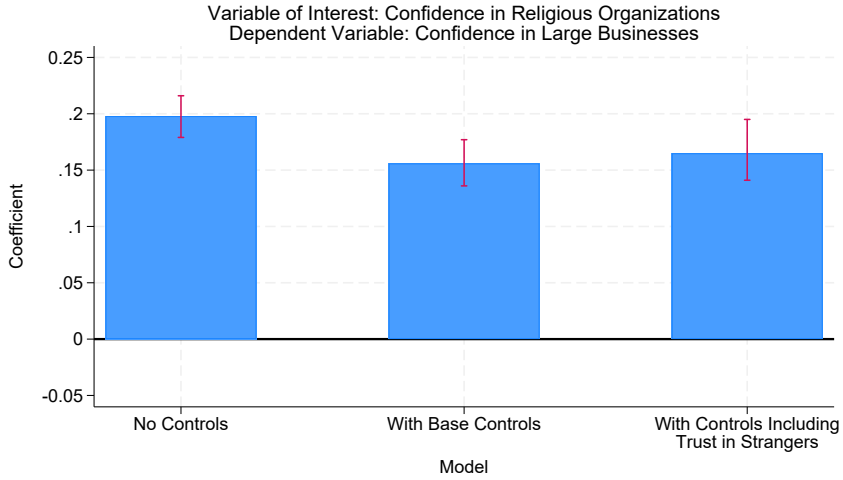

Figure 1. Spillover Between Trust in Religious Institutions and Trust in Businesses
Point estimates (blue bars) with 90% confidence intervals for the coefficients on confidence in religious institutions, following Equation 1: $Confidence\_Bus_{i,t} = \alpha_c + \alpha_t + \beta \times Confidence\_Rel_{i,t} + \gamma \times X_{i,t} + \varepsilon_{i.t}$, for respondent i in year t, between 1998 and 2006. The dependent variable captures confidence in large business organizations. We include year and county (c) fixed effects based on the GSS restricted localization data. In the "Base Controls" specification, $X_{i,t}$ is a vector of controls including the respondent's age, demographic characteristics (gender, education, and race as well as marital, employment, and health statuses), political preference, and (quintiles of) income and socio-economic position. On the right-hand side, we also include respondents' propensity to trust strangers in the controls. All estimates use year and county fixed effects.

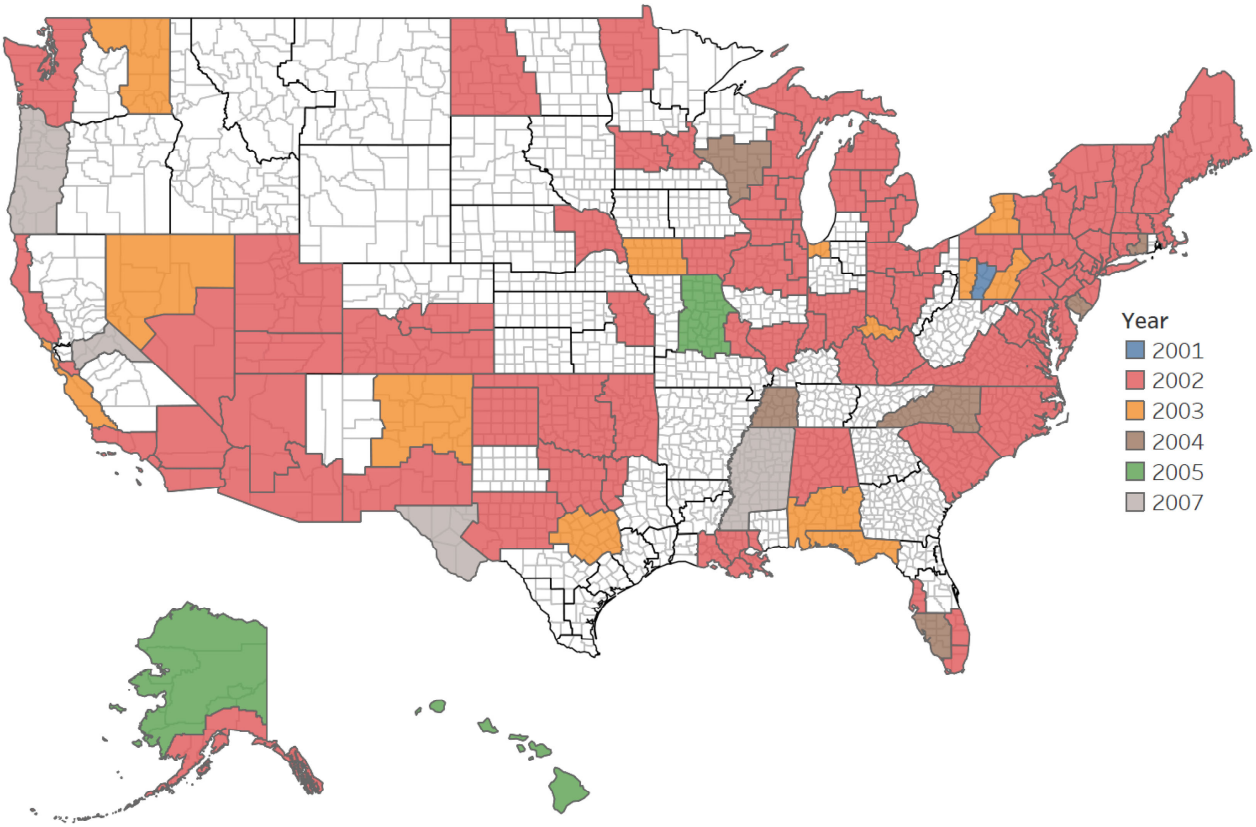

Figure 2. Map of Dioceses Treated by the *Removal* Measure
This map displays dioceses for which the measure of *Removal* turns to 1. Each color indicates the first year for which the indicator turns on. Years are indicated by the legend on the right.

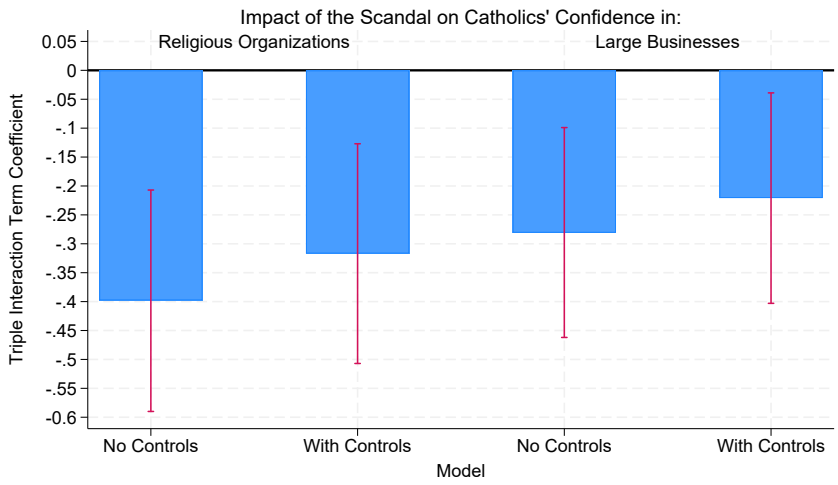

Figure 3. Impact of the Scandal on Trust in Religious Organizations and in Large Businesses in Catholic Counties

Point estimates (blue bars) with 90% confidence intervals for the coefficients on the triple interaction term of interest ($b1$) in Equation 2: $Confidence\_I_{c,t} = a_{ce} + a_{te} + b_1 Post_{c,t} \times Catholic_c \times Removal_{c,e} + b_2 Post_{c,t} \times Catholic_c + b_3 Post_{c,t} \times Removal_{c,e} + c * X_{c,t} + \epsilon_{c,t,e}$, for respondent c in year t, in event cohort e. The index I on the dependent variable captures the type of institution concerned, namely churches and religious organizations (two left-most bars) or large business organizations (two right-most bars). The unit of observation is a county-year, with interpolated values between survey years. We include event-year (te) and event-county (ce) fixed effects based on the GSS restricted localization data. $X_{i,t}$ is a vector of controls including the age, demographic characteristics (gender, education, and race, as well as marital, employment, and health statuses), political preference, and (quintiles of) income and socio-economic position averaged by county-year.

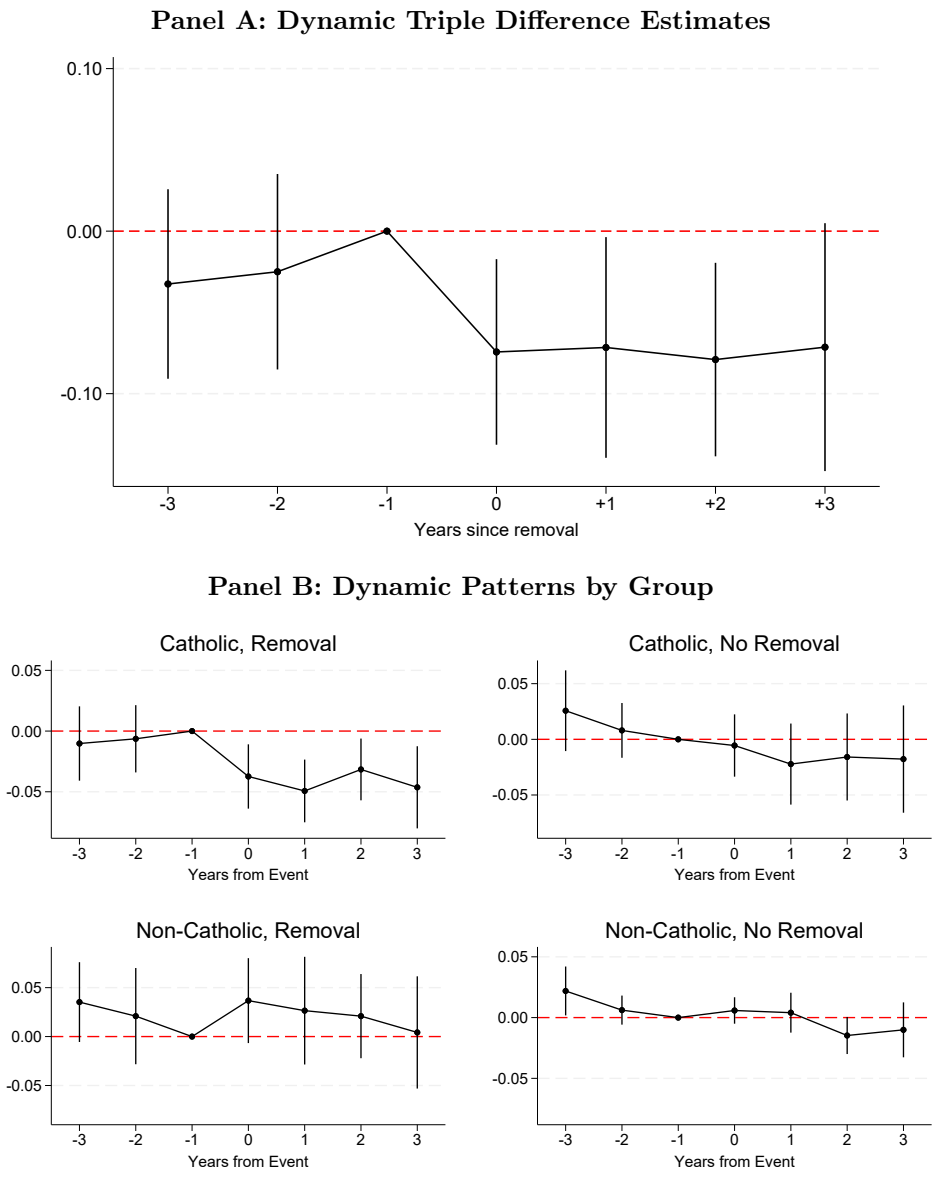

Figure 4. Dynamic Effects Around Event-Year.
Panel A reports Dynamic treatment effect coefficients from a triple difference-in-difference regression identical to the results reported in Column 3 of Table 2, except the *Post* interaction is replaced with indicators for years around the event as in Equation 4. Panel B reports coefficient estimates of event-year indicators for regressions splitting the sample based on whether the firm is in a *Catholic* county and whether it is in a *Removal* diocese, following Equation 5. In both panels, year t-1 is the base level. 90% confidence intervals are plotted.

Table 1. Summary Statistics

Panel A reports summary statistics comparing pre-event characteristics of treated vs. control pairs. Observations represent averages for the firm-customer pair in the three years preceding the event year. Treated pairs involve firms (suppliers) in dioceses with a clergy removal, while control pairs involve firms (suppliers) that are never treated in the event window. Panel B reports summary statistics for the full sample. Continuous variables are winsorized at the 1st and 99th percentiles. Variables are as defined in Appendix A.*, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Pre-Event Characteristics for Treated and Control Groups

	Treated		Control		
	Pairs	Mean	Pairs	Mean	Difference
Firm characteristics:					
HHI	274	0.124	170	0.157	-0.034***
Size	274	4.786	170	4.878	-0.092
Age	274	2.228	170	2.238	-0.010
R&D	274	0.090	170	0.103	-0.012
Leverage	274	0.162	170	0.194	-0.033
Profitability	274	-0.010	170	0.006	-0.017
Customer characteristics:					
Customer HHI	274	0.174	170	0.170	0.004
Customer Size	274	9.674	170	9.867	-0.193
Customer Age	274	3.083	170	3.132	-0.049
Customer R&D	274	0.031	170	0.027	0.004
Customer Leverage	274	0.213	170	0.243	-0.029*
Customer Profitability	274	0.123	170	0.109	0.015*
Trade Credit:					
Trade Credit	274	0.190	170	0.157	0.033**
Trade Credit, Ind.Adj.	274	-0.006	170	-0.013	0.007

Panel B: Full Sample Summary Statistics

	Observations	Mean	St. Dev.	25th pctl	Median	75th pctl
Firm characteristics:						
HHI	2,696	0.139	0.140	0.059	0.097	0.155
Size	2,696	5.053	1.735	3.827	4.895	6.185
Age	2,696	2.444	0.643	1.946	2.398	2.833
Profitability	2,696	0.025	0.226	-0.036	0.080	0.151
R&D	2,696	0.097	0.126	0.000	0.056	0.138
Leverage	2,696	0.164	0.207	0.000	0.089	0.264
Customer characteristics:						
Customer HHI	2,696	0.193	0.183	0.068	0.124	0.263
Customer Size	2,696	9.909	1.779	8.859	10.072	11.256
Customer Age	2,696	3.195	0.725	2.639	3.367	3.912
Customer Profitability	2,696	0.122	0.078	0.072	0.122	0.164
Customer R&D	2,696	0.029	0.041	0.000	0.005	0.049
Customer Leverage	2,696	0.213	0.162	0.091	0.193	0.280
Trade Credit:						
Trade Credit	2,696	0.167	0.139	0.080	0.134	0.208

Table 2. Baseline Results

This table reports results from the triple difference-in-difference test in Equation 3, using a stacked sample across event cohorts. The dependent variable is *Trade Credit*, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Catholic* denotes a firm headquartered in a county with an above-median percentage of Catholic residents. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	All Customers		Ex Ante Customers	
	(1)	(2)	(3)	(4)
Post×*Catholic*×*Removal*	-0.059**	-0.053**	-0.060**	-0.053**
	(-2.21)	(-2.14)	(-2.22)	(-2.15)
Post×*Catholic*	-0.002	0.005	-0.001	0.006
	(-0.15)	(0.31)	(-0.09)	(0.35)
Post×*Removal*	0.014	0.006	0.015	0.006
	(0.73)	(0.27)	(0.75)	(0.28)
Size	0.010	0.004	0.008	0.003
	(1.26)	(0.42)	(0.90)	(0.25)
Profitability	-0.025	-0.033*	-0.019	-0.025
	(-1.49)	(-1.73)	(-1.16)	(-1.26)
HHI	-0.013	-0.117	-0.024	-0.180*
	(-0.23)	(-1.15)	(-0.40)	(-1.95)
Leverage	-0.031	-0.024	-0.028	-0.018
	(-1.33)	(-0.78)	(-1.21)	(-0.57)
R&D	-0.208***	-0.217***	-0.202***	-0.201**
	(-3.46)	(-3.06)	(-3.00)	(-2.40)
Age	-0.116***	-0.075*	-0.118***	-0.076
	(-3.00)	(-1.93)	(-2.78)	(-1.66)
Customer Size	0.005	0.011	0.004	0.009
	(0.54)	(1.06)	(0.37)	(0.81)
Customer Profitability	0.076	0.093	0.082	0.097
	(1.39)	(1.40)	(1.39)	(1.47)
Customer HHI	-0.036	-0.020	-0.080	-0.051
	(-0.35)	(-0.24)	(-0.72)	(-0.56)
Customer Leverage	-0.001	0.003	-0.005	-0.007
	(-0.03)	(0.09)	(-0.15)	(-0.18)
Customer R&D	-0.225	-0.086	-0.109	0.102
	(-0.71)	(-0.26)	(-0.33)	(0.29)
Customer Age	-0.006	0.003	-0.013	-0.005
	(-0.11)	(0.05)	(-0.25)	(-0.08)
Pair-Event FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes	
Industry-Event-Year FE		Yes		Yes
R^2	0.532	0.518	0.531	0.501
Observations	2,696	2,681	2,372	2,350

Table 3. Addressing Potential Confounding Events

This table reports robustness tests to address the potential for confounding events. Panel A addresses the possibility that firms in *Catholic* counties or regions with a *Removal* may fundamentally differ from other firms, collapsing the sample to simple difference-indifference (DiD) estimations: Columns (1) and (2) restrict the sample to firms headquartered in *Catholic* counties and report a DiD estimator based on whether the county experienced a clergy removal, while Columns (3) and (4) restrict the sample to firms in *Removal* counties and report a DiD estimator based on whether the county is *Catholic*. Panel B alleviates concerns about any state-level legal or regulatory changes that could plausibly confound interpretation of the main results by repeating the baseline results of Table 2 with the inclusion of state-event-year FE. Panel C addresses a specific contemporaneous event, the implementation of the Sarbanes-Oxley Act (SOX) in 2002. Columns (1) and (2) exclude firms that ever in the sample period have a "not effective reason phrase" from Audit Analytics containing the word "receivable," while Columns (3) and (4) interact an indicator for these firms with *Post*. The dependent variable is Trade Credit, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are as in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Collapsing to DiDs

	Catholic Firms		Removal Firms	
	(1)	(2)	(3)	(4)
Post×*Removal*	-0.058*	-0.042*		
	(-1.98)	(-1.91)		
Post×*Catholic*			-0.061**	-0.044*
			(-2.46)	(-1.69)
Controls	Yes	Yes	Yes	Yes
Firm-Event FE				
Customer-Event FE				
Pair-Event FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes	
Industry-Event-Year FE		Yes		Yes
R^2	0.506	0.494	0.464	0.460
Observations	1,785	1,753	1,681	1,619

Panel B: Controlling for Time-Varying State-Level Factors

	(1)	(2)	(3)	(4)
Post×*Catholic*×*Removal*	-0.095***	-0.115***	-0.094***	-0.115***
	(-3.08)	(-3.68)	(-2.97)	(-3.55)
Post×*Catholic*	0.038**	0.076***	0.036**	0.075***
	(2.56)	(4.02)	(2.37)	(3.85)
Post×*Removal*	0.028	0.053*	0.029	0.053*
	(1.02)	(1.76)	(1.04)	(1.71)
Controls	Yes	Yes	Yes	Yes
Pair-Event FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes	
Industry-Event-Year FE		Yes		Yes
State-Event-Year FE		Yes		Yes
R^2	0.500	0.479	0.502	0.467
Observations	2,614	2,595	2,283	2,254

Panel C: Receivables-Related SOX404 Issues

	(1)	(2)	(3)	(4)
Post× *Catholic*× *Removal*	-0.072***	-0.060**	-0.061**	-0.052**
	(-2.73)	(-2.48)	(-2.28)	(-2.13)
Post× *Catholic*	0.016	0.018	-0.000	0.005
	(1.32)	(1.05)	(-0.01)	(0.27)
Post× *Removal*	0.022	0.014	0.015	0.006
	(1.03)	(0.63)	(0.75)	(0.27)
Post× *RecIssue*			-0.020	-0.019
			(-0.70)	(-0.59)
Controls	Yes	Yes	Yes	Yes
Pair-Event FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes	
Industry-Event-Year FE		Yes		Yes
R^2	0.520	0.506	0.532	0.518
Observations	2,526	2,506	2,696	2,681

Table 4. Headquarters Location Robustness Tests

This table reports robustness tests around the headquarters location used for treatment identification throughout the paper. Particularly, we address the implicit assumption that trade credit decisions are made at the headquarters location. Columns (1) and (2) exclude observations where the headquarters location was based on Compustat's current data (rather than historical header information, available for the vast majority of the sample). Columns (3) and (4) ((5) and (6)) exclude firms reporting more than one operating (geographic) segment in the year prior to the event cohort. The dependent variable is Trade Credit, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are as in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Filter:	*Historical Headquarters*		*Single Op. Segment*		*Single Geo. Segment*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post×*Catholic*×*Removal*	-0.059**	-0.058**	-0.064**	-0.049*	-0.096**	-0.110**
	(-2.16)	(-2.31)	(-2.46)	(-1.94)	(-2.45)	(-2.44)
Post×*Catholic*	-0.001	0.007	0.003	0.001	-0.007	0.011
	(-0.08)	(0.44)	(0.23)	(0.05)	(-0.40)	(0.38)
Post×*Removal*	0.012	0.010	0.011	0.000	0.018	0.017
	(0.59)	(0.45)	(0.59)	(0.02)	(0.57)	(0.43)
Pair-Event FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes		Yes	
Industry-Event-Year FE		Yes		Yes		Yes
R^2	0.534	0.520	0.518	0.507	0.538	0.508
Observations	2,634	2,619	2,529	2,501	1,580	1,529

Table 5. Cross-Sectional Variation on Social Capital Importance

This table reports results from subsample splits along characteristics related to the importance of social capital for supply chain cooperation. Panel A splits the sample based on relationship length at the time of the event, with "Long" denoting relationship lengths above the 75th percentile and "Short" denoting below-75th percentile. Panel B splits the sample based on geographic distance between the firm and customer, with "Close" denoting pairs having below-25th percentile distance and "Distant" denoting pairs with above-25th percentile distance. Panel C splits the sample based on industry R&D intensity (industry R&D/ industry total Assets) in year event-1, with "Low" denoting industry R&D below the 25th percentile and "High" denoting above the 25th percentile. Control variables are as reported in Table 2. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Differences by Relationship Length

	Long (1)	Short (2)	Difference (1) - (2)	Long (3)	Short (4)	Difference (3)-(4)
Post×*Catholic*×*Removal*	0.006	-0.095**	0.101**	-0.006	-0.084**	0.078*
	(0.28)	(-2.60)	(2.31)	(-0.27)	(-2.43)	(1.89)
Post×*Catholic*	0.004	-0.007		0.002	-0.004	
	(0.30)	(-0.38)		(0.09)	(-0.19)	
Post×*Removal*	-0.002	0.022		-0.003	0.018	
	(-0.08)	(0.81)		(-0.11)	(0.58)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.581	0.521		0.522	0.483	
Observations	688	1,684		615	1,614	

Panel B: Differences by Geographic Distance

	Close (1)	Distant (2)	Difference (1) - (2)	Close (3)	Distant (4)	Difference (3)-(4)
Post×*Catholic*×*Removal*	0.035	-0.096***	0.131***	-0.041	-0.088**	0.047
	(1.19)	(-2.86)	(2.95)	(-0.62)	(-2.21)	(0.62)
Post×*Catholic*	0.011	0.002		0.009	0.011	
	(0.54)	(0.13)		(0.23)	(0.43)	
Post×*Removal*	-0.040*	0.018		0.041	-0.005	
	(-1.94)	(0.68)		(0.61)	(-0.18)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.526	0.567		0.435	0.555	
Observations	592	1,656		505	1,622	

Panel C: Differences by Relationship Complexity (R&D)

	Low (1)	High (2)	Difference (1) - (2)	Low (3)	High (4)	Difference (3)-(4)
Post×*Catholic*×*Removal*	-0.006	-0.094***	0.089*	0.006	-0.086***	0.092**
	(-0.17)	(-2.80)	(1.86)	(0.17)	(-2.65)	(-2.05)
Post×*Catholic*	-0.003	0.005		-0.012	0.010	
	(-0.12)	(0.37)		(-0.34)	(0.52)	
Post×*Removal*	-0.038***	0.048*		-0.024*	0.029	
	(-3.69)	(1.69)		(-1.92)	(0.91)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.646	0.506		0.617	0.501	
Observations	607	2,022		579	1,999	

Table 6. Risk Aversion Tests: Customer Financial Constraints

This table subsamples the set of customers based on ex ante measures of financial constraints (measured one year before the cohort year). In each panel, "Constr." indicates the subsample of customers appearing financially constrained, while "Unconstr." indicates the subsample not appearing financially constrained. In Columns (1) and (2), constrained customers have non-investment grade (vs. investment grade) credit ratings or no rating; in Columns (3) and (4), constrained customers have a low (below 25th percentile) ratio of cash to assets; and in Columns (5) and (6), constrained customers have a high (above 1%) probability of default, following Bharath and Shumway (2008) and Merton (1974). Control variables are as in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Customer Financial Constraints

	Investment Grade			*Cash/Assets*			*Probability of Default*		
	Constr.	Non-Constr.	Difference	Constr.	Non-Constr.	Difference	Constr.	Non-Constr.	Difference
	(1)	(2)	(1-2)	(3)	(4)	(3-4)	(5)	(6)	(5-6)
Post×*Catholic*×*Removal*	-0.051	-0.049	0.025	-0.059	-0.061**	0.002	-0.062*	-0.055*	-0.007
	(-0.62)	(-1.51)	(0.39)	(-1.01)	(-2.20)	(0.03)	(-1.68)	(-1.71)	(-0.15)
Post×*Removal*	-0.011	0.029		0.073*	0.003		-0.002	0.038	
	(-0.12)	(1.50)		(1.69)	(0.19)		(-0.10)	(1.62)	
Post×*Catholic*	-0.056**	-0.001		-0.038	0.009		0.053	-0.010	
	(-2.57)	(-0.07)		(-1.12)	(0.58)		(1.44)	(-0.54)	
Controls	Yes	Yes		Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes		Yes	Yes		Yes	Yes	
R^2	0.521	0.553		0.552	0.529		0.545	0.522	
Observations	763	1,845		671	2,019		470	1,997	

Table 7. Firm Financial Outcomes

This table reports results of regressing firm financial measures on the triple-difference estimators of Table 2. The dependent variable in Columns (1)-(5), respectively, is the cash-to-asset ratio, profitability, payables-to-COGS ratio, total leverage, and short-term debt as a proportion of assets. Control variables are as defined in Appendix A and generally include *Size, Profitability, HHI, Leverage, R&D* and *Age*; *Profitability* is excluded in Column (2) and *Leverage* is excluded in Columns (4) and (5). t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Cash/Assets (1)	Profitability (2)	Pay./COGS (3)	Leverage (4)	ST Debt (5)
Post×*Catholic*×*Removal*	0.031*	0.002	0.022	0.024	0.010
	(1.91)	(0.17)	(1.49)	(1.47)	(0.97)
Post×*Catholic*	-0.026**	0.007	-0.021**	-0.002	-0.004
	(-2.41)	(0.94)	(-2.13)	(-0.24)	(-0.51)
Post×*Removal*	0.005	0.001	-0.004	-0.010	-0.019**
	(0.38)	(0.06)	(-0.30)	(-0.72)	(-2.11)
Controls	Yes	Yes	Yes	Yes	Yes
Firm-Event FE	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes	Yes	Yes	Yes
R^2	0.829	0.759	0.692	0.746	0.475
Observations	9,318	9,319	9,311	9,319	9,319

Table 8. Cohort Comparisons: 2002 vs. Later Cohorts

This table repeats the baseline specifications using only the 2002 cohort (Columns (1)-(3)) and using the 2003 and 2004 cohorts (Columns (4)-(6)). The dependent variable is *Trade Credit*, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Catholic* denotes a firm headquartered in a county with an above-median percentage of Catholic residents. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are as in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	2002 Cohorts		2003/2004 Cohorts	
	(1)	(2)	(3)	(4)
Post×*Catholic*×*Removal*	-0.076**	-0.056**	-0.044	-0.045
	(-2.49)	(-2.33)	(-0.79)	(-1.07)
Post×*Catholic*	0.015	0.003	-0.007	-0.002
	(0.95)	(0.13)	(-0.43)	(-0.12)
Post×*Removal*	0.028	0.030	-0.011	-0.021
	(1.05)	(1.26)	(-0.75)	(-0.91)
Controls	Yes	Yes	Yes	Yes
Pair-Event FE	Yes	Yes	Yes	Yes
Event-Year FE	Yes		Yes	
Industry-Event-Year FE		Yes		Yes
R^2	0.502	0.482	0.605	0.619
Observations	1,843	1,828	853	853

Table 9. Robustness Tests

This table reports robustness tests to the baseline findings in Table 2. In Panel A, entropy-balanced weights are used in the regressions, based on ex ante characteristics. Panel B alters the definition of *Catholic* to indicate firms with an above in-sample median Catholic population share, rather than using the nation-wide population median. The dependent variable is Trade Credit, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are as in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Entropy-Balanced Controls

	(1)	(2)
Post×*Catholic*×*Removal*	-0.067**	-0.074***
	(-2.39)	(-2.76)
Post×*Catholic*	0.001	0.020
	(0.07)	(0.90)
Post×*Removal*	0.013	0.012
	(0.63)	(0.59)
Controls	Yes	Yes
Pair-Event FE	Yes	Yes
Event-Year FE	Yes	
Industry-Event-Year FE		Yes
R^2	0.583	0.569
Observations	1,692	1,660

Panel B: Alternate Definition of *Catholic*

	(1)	(2)
Post × *Catholic* × *Removal*	-0.066***	-0.051*
	(-3.25)	(-1.88)
Post × *Catholic*	0.016	0.013
	(1.36)	(0.59)
Post × *Removal*	0.016	0.013
	(0.11)	(-0.27)
Controls	Yes	Yes
Pair-Event FE	Yes	Yes
Event-Year FE	Yes	
Industry-Event-Year FE		Yes
R^2	0.532	0.518
Observations	2,696	2,681

Appendix A Variable Definitions

Variable	Definition	Source
Catholic	An indicator for firms headquartered in counties with a proportion of Catholic residents above the population-weighted median	ARDA Religious Congregation and Membership Survey 2000
Removal	An indicator equal to 1 for firms headquartered in counties that experienced a clergy removal in 2002, 2003, or 2004.	BishopsAccountability.org
Post	An indicator for a firm-year ending at least 30 days after removal news.	BishopsAccountability.org
Trade Credit	Pair-level receivables scaled by pair-level sales	10-Ks, via SEC Edgar
Trade Credit, Ind. Adj.	*Trade Credit* minus its 2-digit SIC average	10-Ks, via SEC Edgar
HHI	Industry sales concentration, measured at the 3-digit SIC level	Compustat
Size	Logarithm of total assets	Compustat
Age	Logarithm of number of years firm has appeared in Compustat	Compustat
Leverage	Short-term debt + long-term debt, scaled by total assets	Compustat
Profitability	Operating income before depreciation scaled by total assets	Compustat
R&D	Research and development expense (set to 0 if missing), scaled by total assets	Compustat
Investment Grade	An indicator for the firm having an S&P credit rating of BBB- or higher	Capital IQ
Cash/Assets	Ratio of cash to total assets	Compustat
Probability of Default	Merton (1974)'s probability of default, following Bharath and Shumway (2008).	Compustat
Pay./COGS	Accounts payable, scaled by cost of goods sold.	Compustat
ST Debt	The ratio of short-term debt to total assets	Compustat
Confidence_Bus	Individuals' confidence in large businesses: the variable set to 1/0/-1 if the survey respondent answers "A great deal"/"Only some"/"Hardly any" (respectively)	GSS
Confidence_Rel	Individuals' confidence in churches and religious institutions: the variable is set to 1/0/-1 if the survey respondent answers "A great deal"/"Only some"/"Hardly any" (respectively)	GSS
RecIssue	An indicator for the firm reporting a "note effective reason phrase" containing the word "receivable	Audit Analytics

Appendix B Clergy Removals Data Collection

To collect data on the Catholic abuse scandal, we follow Dupont (2022). We use a news search from the *Factiva* database based on a broad set of keywords. We detail the data collection process in this data appendix. The first news story from the *Boston Globe*'s Spotlight series on the implication of local religious leaders (bishops) in the mishandling of abuse by the clergy they supervise dates from January 6, 2002. Accordingly, we begin our news search in 2001 to capture potential leaks from the Boston Globe's story.

The keyword search from *Factiva* consists of the key words "Catholic and (diocese or archdiocese) and (abuse or scandal)." This search yields yields over 27,000 articles between 2001 and 2006. We read each story and classify each set of allegations by date, diocese involved, nature of event (e.g. allegation, removal of clergy from ministry, etc.), and number of victims and perpetrators (and perpetrators' names) when mentioned. We exclude duplicates and stories with no identification information. The John Jay 2004 Report (John Jay College of Criminal Studies, 2004), a comprehensive study of U.S. clergy abuse related to the abuse crisis mentions that most of the allegations involved concern events from the 1960s and 1970s.

Among these articles, 410 concern removals from clergy ministry between 2001 and 2005. We collapse these data by diocese-year. One diocese, Greensburg, PA, faced revelations of removals of clergy from ministry in 2001, but this story is unrelated to the Boston Globe investigation and therefore we exclude this diocese all together. This leaves 113 out of 176 U.S. dioceses with removals from clergy ministry between 2002 and 2005. These dioceses are listed in Appendix C. The few dioceses impacted in 2005 (e.g., Fairbanks, AK; Honolulu, HI) are home to a very small number of firms in our trade credit database–hence we exclude those dioceses from the analysis as well.

While other papers use alternative measures of the Catholic clergy abuse scandal, viz., claims of abuse from the BishopAccountability database (Hungerman, 2013), our measure of the scandal offers several advantages. First, knowledge of (even rampant) abuse by some Catholic priests in the U.S. vastly predates 2002. Indeed, the *Boston Globe*'s Spotlight series was prompted by the follow-up-trial for former Boston-priest John Geoghan, who has already in jail for the abuse of dozens of minors at the time. The Spotlight series and the ensuing Catholic clergy abuse crisis focuses on the role of bishops in mishandling abusive clergy (Bruce, 2011). Our measure of removals deals precisely with the belated acknowledgment of the bishop's inaction regarding past claims of abuse against priests still in exercise.

Second, BishopsAccountability.com backfills their database. Abuses or removals are categorized not at the date that the public knew about them, but at the time contained in previously confidential files, which were made public in the course (and in the aftermath) of the crisis. Because we want to capture the impact of the scandal as news, we want to focus on a measure based on public disclosure. News articles, which most often come from local newspapers, allow us to capture the effect of the revelations of abuse on the local environment.

Third, as underscored by Bottan and Perez-Truglia (2015), allegations are reported regardless of their veracity. Using removals of clergy members rather than allegations against them ensured that the clergy's misbehavior is serious and warrants disciplinary action.

These advantages notwithstanding, two factors make removals from active ministry an imperfect measure of the scandals: First, this approach requires the offending priests to still be alive (while most offenses took place some 30 prior to the scandal). Second, these presumably older clergy members have to be in active ministry at the time of the removal (i.e., not retired and still members of the clergy). Both factors mean that our measure might underestimate the impact of the crisis, and that the criteria for treated areas create a high bar to clear in order to find significant effects for the shock.

Appendix C List of Shocked Dioceses

Diocese	State	*Scandal* On
Albany	NY	2002
Allentown	PA	2002
Altoona-Johnstown	PA	2003
Amarillo	TX	2002
Anchorage	AK	2002
Arlington	VA	2002
Austin	TX	2003
Baltimore	MD	2002
Baton Rouge	LA	2002
Belleville	IL	2002
Birmingham	AL	2002
Bismarck	ND	2002
Boston	MA	2002
Bridgeport	CT	2002
Brooklyn	NY	2002
Buffalo	NY	2003
Burlington	VT	2002
Camden	NJ	2004
Charleston	SC	2002
Charlotte	NC	2004
Chicago	IL	2002
Cincinnati	OH	2002
Cleveland	OH	2002
Colorado Springs	CO	2002
Columbus	OH	2002
Covington	KY	2003
Crookston	MN	2002
Dallas	TX	2002
Davenport	IA	2002
Des Moines	IA	2003
Detroit	MI	2002
Evansville	IN	2002
Fairbanks	AK	2005
Fall River	MA	2002
Fort Worth	TX	2002
Gary	IN	2003
Gaylord	MI	2002
Grand Rapids	MI	2002
Green Bay	WI	2002
Greensburg	PA	2001
Harrisburg	PA	2002
Hartford	CT	2002
Honolulu	HI	2005
Houma-Thibodaux	LA	2002
Indianapolis	IN	2002
Jefferson City	MO	2005
Joliet	IL	2002
Juneau	AK	2002
Kansas City	KS	2002
La Crosse	WI	2004
Lafayette	LA	2002
Lansing	MI	2002
Las Cruces	NM	2002
Las Vegas	NV	2002
Lexington	KY	2002
Los Angeles	CA	2002
Louisville	KY	2002
Madison	WI	2002
Manchester	NH	2002
Marquette	MI	2002
Memphis	TN	2004
Metuchen	NJ	2002
Miami	FL	2002
Milwaukee	WI	2002
Mobile	AL	2003
Monterey	CA	2003
New Orleans	LA	2002
New Ulm	MN	2002
New York	NY	2002
Newark	NJ	2002
Norwich	CT	2004
Ogdensburg	NY	2002
Oklahoma City	OK	2002
Omaha	NE	2002
Orange	CA	2002
Palm Beach	FL	2002
Paterson	NJ	2002
Pensacola-Tallahassee	FL	2003
Peoria	IL	2002
Philadelphia	PA	2002
Phoenix	AZ	2002
Pittsburgh	PA	2003
Portland	ME	2002
Pueblo	CO	2002
Raleigh	NC	2002
Reno	NV	2003
Richmond	VA	2002
Rochester	NY	2002
Rockford	IL	2002
Rockville Centre	NY	2002
Saginaw	MI	2002
Salt Lake City	UT	2002
San Angelo	TX	2002
San Bernardino	CA	2002
San Diego	CA	2002
San Francisco	CA	2003
San Jose	CA	2002
Santa Fe	NM	2003
Santa Rosa	CA	2002
Scranton	PA	2002
Seattle	WA	2002
Spokane	WA	2003
Springfield	MA	2002
St. Louis	MO	2002
St. Paul and Minneapolis	MN	2002
St. Petersburg	FL	2002
Syracuse	NY	2002
Toledo	OH	2002
Trenton	NJ	2002
Tucson	AZ	2002
Tulsa	OK	2002
Venice	FL	2004
Washington	DC	2002
Wilmington	DE	2002
Worcester	MA	2002

Appendix D Supplemental Robustness Tests

Relationship Between Trust in Religious Institutions and Trust in Businesses

In Section 3.2, we present evidence of a relationship between trust in religious organizations and trust in businesses, based on tests of Equation 1, using the General Social Survey data over a period consistent with our baseline tests. In this section, we display the regression results for these tests.

In Table D1, the main variable of interest is the confidence in religion indicator. The construction of these indicators is explained in Section 3.2. The results from Models (1)-(3) offer evidence that high confidence in religious institutions correlates with high confidence in business institutions. Similarly, the coefficients of interest in Models (4)-(6) show that low confidence in religious institutions correlates with low confidence in business organizations.

Taken together, these results offer evidence that a scandal which decreases trust in religious institutions is likely to decrease trust in businesses. In our setting, this means that the Catholic abuse scandal might spill over to Catholics' trust in businesses, including their firms' supply chain partners.

Robustness: Cross-Sectional Variation

Table D3 replicates the results of Table 5 of the main text, but reports results based on subsample splits along the median of relationship length (Panel A), geographic distance (Panel B), and industry R&D intensity. In all panels, results are directionally consistent with the main tests, though differences are less stark. In light of this, we conclude that only the longest-term relationships are shielded from the adverse effects of the shock (Panel A), that firms must be reasonably geographically proximate for monitoring benefits to exist (e.g., the median distance in Panel B is 824 miles), and that most relationships involve sufficient complexity to make holdup costs a significant concern (Panel C).

Cultural Familiarity: Catholic Customers

As discussed in Section 3.2, we explore whether firms' contraction of trade credit differs depending on whether the customer is in a Catholic or non-Catholic area. Table D4 presents regressions from subsample splits along this criterion. We do not observe significant differences for Catholic vs. non-Catholic customers, suggesting that the relationship-specific factors examined in Section 3.2 are more relevant in the social capital channel.

Financial Sophistication: Age and Size

Table D5 repeats the baseline specification using subsample splits along firm age and firm size as proxies for financial sophistication. Columns 1 and 2 split the sample on the age of the firm, measured by the number of years since the firm appeared in Compustat as of the event-year. "Young" firms are below-median age as of the event date while "Old" firms are above-median age. "Large" firms are above the median of pre-event total assets, while "Small" firms are below the median. Column (1), reporting results for young firms, shows a significant negative coefficient for $Post \times Catholic \times Removal$, but Column (2) shows a smaller, statistically insignificant effect for older firms. The difference in magnitudes is statistically significant. Columns (3) and (4) split the sample on pre-event size. The pattern with size mirrors the age

results, albeit more weakly, showing a larger coefficient on the triple-difference estimator for smaller firms These results suggest that financial sophistication may mitigate firms' reaction to cultural shocks.

Robustness: Additional Financial Constraint Measures

Table D6 replicates the findings of Table 6 with additional measures of financial constraint: Columns (1) and (2) use Kaplan and Zingales (1997)'s KZ index, Columns (3) and (4) use Whited and Wu (2006)'s WW index, Columns (5) and (6) use Hoberg and Maksimovic (2015)'s HM index, and Columns (7) and (8) use Hadlock and Pierce (2010)'s HP (size-age) index. In all subsample splits, "Constr." indicates an above-median level of the index, while "Non-Constr." indicates a below-median level. Differences across subsamples are mixed: Based on the KZ index and HM index, coefficients on the triple-difference estimator ($Post \times Catholic \times Removal$) are stronger for non-constrained firms, while the WW index and HP index suggest the opposite. None of the differences are statistically significant and, given the mixed evidence, do not suggest a risk aversion channel at play, consistent with Table 6 of the main text.[43]

[43]For readability, we only present the triple-difference coefficient estimates.

Table D1. Spillover Effect Between Trust in Religious Organizations and Trust in Large Businesses

This table reports results on the underlying spillover effect between trust in religious organizations and trust in large businesses, using responses to the General Social Survey between 1998 and 2006. Each of the Confidence variables is set to 1 if the respondent has "a great deal" of confidence, 0 if (s)he answers "only some," and -1 if (s)he answers "hardly any" confidence. All models include county and year fixed effects. All estimations are with robust standard errors. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Confidence_Rel	0.198***	0.170***	0.165***
	(17.79)	(11.45)	(11.11)
Age		-0.002***	-0.002***
		(-3.134)	(-3.84)
Male		0.02	0.012
		(0.79)	(0.62)
Non_White		-0.076***	-0.064**
		(-2.74)	(-2.30)
College		0.066***	0.048*
		(2.65)	(1.93)
Married		0.008	0.000
		(0.30)	(0.02)
Unemployed		-0.033	-0.033
		(-0.56)	(-0.56)
Poorhealth		-0.161***	-0.143***
		(-3.19)	(-2.87)
Republican		0.159***	0.147***
		(7.78)	(7.14)
Income_quintile		0.036	0.018
		(1.38)	(0.67)
SocioEcon_Index_quintile		0.026**	0.013
		(3.47)	(1.47)
Trust			0.056***
			(5.14)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R^2	0.125	0.169	0.177
Observations	7,139	4,009	3,989

Table D2. Impact of the Scandal on Trust in Religious Organizations and Businesses
This table reports results on the impact of the scandal on local confidence in religious organizations and large businesses, using GSS data to estimate equation 2. The unit of observation is county-year. Confidence in each organization is set to 1 if a respondent answer "a great deal," 0 if (s)he answers "it depends," and -1 if (s) answers "hardly any." *Catholic* is set to 1 if a county has a higher share of Catholics than the population-weighted across U.S. counties. *Removal* is set to 1 if a county belongs to a diocese with a clergy removal. *Post* is set to 1 in the year of the initial removal; that year is dropped from the estimation. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Confidence in Religious Organizations		Confidence in Large Businesses	
	(1)	(2)	(3)	(4)
Post×*Catholic*×*Removal*	-0.398***	-0.317***	-0.281**	-0.221**
	(-3.43)	(-2.76)	(-2.554)	(-2.01)
Post×*Catholic*	0.195**	0.139*	0.297***	0.256***
	(2.21)	(1.68)	(3.89)	(3.52)
Treat×*Post*	0.207**	0.197**	0.201**	0.150*
	(2.58)	(2.56)	(2.35)	(1.74)
Age		0.007**		0.001
		(2.47)		(0.56)
Male		-0.292**		-0.004
		(-2.47)		(-0.04)
Non_ White		-0.076		0.042
		(-0.82)		(0.54)
College		-0.256**		0.165
		(-2.27)		(1.62)
Married		0.068		-0.077
		(0.71)		(-0.82)
Unemployed		-0.262		0.152
		(-1.26)		(0.73)
Poorhealth		0.014		-0.142
		(0.09)		(-0.78)
Republican		0.121		0.124
		(1.31)		(1.07)
Income_ quintile		0.020*		0.049***
		(1.69)		(4.94)
SocioEcon_ Index_ quintile		-0.003		0.005
		(-0.22)		(0.37)
Observations	1,820	1,817	1,819	1,816
R^2	0.619	0.648	0.609	0.635
Controls	No	Yes	No	Yes
Event-Year FE	Yes	Yes	Yes	Yes
Event-County FE	Yes	Yes	Yes	Yes

Table D3. Robustness: Cross-Sectional Variation on Social Capital Importance
This table reports results from subsample splits along characteristics related to the importance of supply chain cooperation, and is identical to Table 5, but reports results for above/below median splits instead of the 25th/75th percentile splits in the main text. Panel A splits the sample based on firm-customer relationship length at the time of the event, with "Long" denoting relationship lengths above the median and "Short" denoting below-median. Panel B splits the sample based on geographic distance between the firm and customer, with "Close" denoting pairs having below-median distance and "Distant" denoting pairs with above-median distance. Observations in which the firm and customer are in the same diocese are excluded. Panel C splits the sample based on industry R&D intensity (industry R&D/ industry total Assets) in year event-1, with "Low" denoting industry R&D below the median and "High" denoting above the median. Control variables are as reported in Table 2. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Differences by Relationship Length

	Long	Short	Difference	Long	Short	Difference
	(1)	(2)	(1) - (2)	(3)	(4)	(3)-(4)
Post×*Catholic*×*Removal*	-0.028	-0.087**	0.059	-0.027	-0.072*	0.045
	(-1.02)	(-2.21)	(1.23)	(-0.95)	(-1.97)	(1.07)
Post×*Catholic*	-0.002	-0.006		-0.016	0.016	
	(-0.22)	(-0.27)		(-1.09)	(0.69)	
Post×*Removal*	0.006	0.008		0.000	0.034	
	(0.24)	(0.26)		(0.01)	(1.02)	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.459	0.554		0.410	0.531	
Observations	976	1,396		910	1,321	

Panel B: Difference by Geographic Distance

	Close	Distant	Difference	Close	Distant	Difference
	(1)	(2)	(1) - (2)	(3)	(4)	(3)-(4)
Post×*Catholic*×*Removal*	-0.032	-0.105**	0.073	-0.037	-0.067	0.030
	(-0.95)	(-2.44)	(1.34)	(-0.93)	(-1.42)	(0.48)
Post×*Catholic*	0.011	0.002		-0.014	-0.007	
	(0.69)	(0.09)		(-0.64)	(-0.27)	
Post×*Removal*	0.002	-0.000		0.024	-0.035	
	(0.09)	(-0.01)		(0.72)	(-0.83)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.516	0.587		0.449	0.605	
Observations	1,113	1,135		1,064	1,076	

Panel C: Difference by Relationship Complexity (R&D)

	Low (1)	High (2)	Difference (1) - (2)	Low (3)	High (4)	Difference (3)-(4)
Post×*Catholic*×*Removal*	-0.022	-0.075**	0.053	-0.035	-0.095**	0.060
	(-0.71)	(-2.03)	(1.11)	(-1.18)	(-2.34)	(1.20)
Post×*Catholic*	-0.020	0.007		-0.007	0.010	
	(-1.00)	(0.33)		(-0.36)	(0.47)	
Post×*Removal*	-0.015	0.058*		-0.014	0.071**	
	(-0.73)	(1.68)		(-0.74)	(2.01)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.492	0.551		0.506	0.562	
Observations	1,320	1,309		1,320	1,309	

Table D4. Customer Effects: Comparison of Catholic vs. Non-Catholic Customers
This table reports results from subsample splits based on whether the customer firm is headquartered in a Catholic county. The dependent variable is *Trade Credit*, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Catholic* denotes a firm headquartered in a county with an above-median percentage of Catholic residents. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are as reported in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Catholic (1)	Non-Catholic (2)	Difference (1)-(2)	Catholic (3)	Non-Catholic (4)	Difference (3)-(4)
Post×*Catholic*×*Removal*	-0.069	-0.062	-0.007	-0.043	-0.091***	0.049
	(-1.60)	(-1.66)	(-0.12)	(-1.17)	(-2.70)	(1.00)
Post×*Catholic*	0.002	-0.002		0.004	-0.001	
	(0.12)	(-0.12)		(0.16)	(-0.02)	
Post×*Removal*	-0.000	0.020		-0.020	0.010	
	(-0.01)	(0.58)		(-0.60)	(0.26)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes				
Industry-Event-Year FE				Yes	Yes	
R^2	0.498	0.556		0.476	0.517	
Observations	1,377	769		1,316	687	

Table D5. Financial Sophistication: Age and Size

This table reports from tests for differential effects based on firm age and size (as proxies for financial sophistication). Columns (1) and (2) show differences across Young (below-median age as of the event date) and Old (above-median) firms, while Columns (3) and (4) show differences between Small (below-median pre-event firm assets) and Large (above-median) firms. The dependent variable is *Trade Credit*, the pair-level trade credit extended by the firm to the customer, scaled by annual pair-level sales. *Catholic* denotes a firm headquartered in a county with an above-median percentage of Catholic residents. *Removal* denotes a county in a diocese that experienced a clergy removal. *Post* denotes an observation after the removal news event, from t=0 to t+3 years (equal to zero in the three years preceding the event). Control variables are the same as those in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors computed from standard errors clustered at the county-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Age and Size

Sample:	*Age*			*Size*		
	Young	Old	Diff (1-2)	Small	Large	Diff (3-4)
Post× *Catholic*× *Removal*	-0.104**	0.002	-0.106**	-0.063*	-0.033	-0.031
	(-2.70)	(0.07)	(-2.26)	(-1.72)	(-0.77)	(-0.57)
Post× *Catholic*	0.001	-0.021		-0.014	-0.017	
	(0.05)	(-0.99)		(-0.81)	(-0.51)	
Post × *Removal*	0.012	-0.001		0.034	0.005	
	(0.36)	(-0.04)		(1.01)	(0.20)	
Controls	Yes	Yes		Yes	Yes	
Pair-Event FE	Yes	Yes		Yes	Yes	
Event-Year FE	Yes	Yes		Yes	Yes	
R^2	0.488	0.596		0.470	0.589	
Observations	1,313	1,316		1,305	1,316	

Table D6. Robustness: Risk Aversion Tests with Other Constraint Measures

This table subsamples the set of customers based on ex ante measures of financial constraints (measured one year before the cohort year). In each panel, "Constr." indicates the subsample of customers appearing financially constrained, while "Unconstr." indicates the subsample not appearing financially constrained, where constrained is defined as an above-median level of the KZ (Kaplan and Zingales, 1997, Columns (1) and (2)), WW index (Whited and Wu, 2006, Columns (3) and (4)), HM index (Hoberg and Maksimovic, 2015, Columns (5) and (6)), or HP index (Hadlock and Pierce, 2010, Columns (7) and (8)). Control variables are the same as those in Table 2 and defined in Appendix A. t-statistics are shown in parentheses, computed from standard errors clustered at the diocese-event level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Customer Financial Constraints

	KZ Index		*WW Index*		*HM Index*		*HP Index*	
	Constr.	Non-Constr.	Constr.	Non-Constr.	Constr.	Non-Constr.	Constr.	Non-Constr.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post×*Catholic*×*Removal*	-0.045	-0.099**	-0.093**	-0.027	-0.084	-0.162***	-0.114**	-0.043*
	(-1.27)	(-2.19)	(-2.11)	(-0.68)	(-1.27)	(-2.71)	(-2.44)	(-1.75)
Difference, Constr.-Non-Constr.:		0.054		-0.066		0.078		-0.071
		(0.95)		(-1.12)		(0.91)		(-1.53)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair-Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.514	0.573	0.547	0.550	0.520	0.434	0.557	0.528
Observations	1,201	1,201	1,275	1,277	709	713	1,241	1,348