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ORIGINAL PAPER

Trust and Contracting: Evidence from Church Sex Scandals

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Abstract
Firms located in communities in which people are, on average, more trusting enjoy some benefits in terms of the power of CEO contracts. We present two pieces of empirical evidence to support this claim: (1) higher average trust in a county is associated with "flatter" executive contracts and (2) when an exogenous shock occurs (such as a scandal involving an important social institution), both trust and contracting move in similar directions. We obtain the first result in a panel specification and the second in a "difference-in-difference" specification that uses the revelation of sex scandals involving the Catholic Church across different U.S. localities.

Keywords Church scandals · Community trust · Firm management

Introduction

The extant literature highlights the costs of agency conflicts when executives (i.e., agents) have information that employers (i.e., principals) do not (e.g., Lambert, 2001). Although providing executives with incentives tied to performance can alleviate these conflicts, doing so exposes executives to a high level of personal risk that may lead them to engage in unethical actions (Dewatripont & Tirole, 2019; Kulshreshtha, 2005). In this study, we examine whether trust is associated with the presence of an alternative way to address agency conflicts and is correlated with the design of executive compensation contracts. We find that firms located in communities in which people are, on average, more trusting enjoy some benefits in terms of the power of CEO contracts. We present two pieces of empirical evidence to support this claim: (1) higher average trust in a county is associated with "flatter" executive contracts and (2) when an exogenous shock occurs (such as a scandal involving an important social institution), both trust and contracting move in similar directions.[1]

Specifically, we find in a first series of tests that firms located in U.S. counties where the average trust is higher are more likely to employ flatter compensation schemes. We show that, despite the use of less stringent contracts, trust is correlated with less moral hazard in these firms and with higher corporate valuation (controlling for various fixed effects and for a vector of observable variables).

To further address concerns that our results might be confounded by unobservable social and economic characteristics, we use a "difference-in-difference" specification that examines the effect of a sudden, negative, and—arguably—exogenous shock associated with the average trust in the community.[2] In 2002, the *Boston Globe* publicly uncovered a massive child-molestation scandal involving priests and a cover-up by the Catholic hierarchy. Newspapers such as the *Los Angeles Times* noted that the "Scandal [was] Shaking Catholicism to [its] Core." Other commentators predicted that the scandal would "hit every major diocese in the country."[3] Consistent with this view, we find that the average trust

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[1] A "flat" contract is a contract that has low power; in other words, a "flat" contract is less sensitive to measures of performance and closer to a "flat" salary.

[2] The difference-in-differences approach is typical in disciplines such as accounting, economics, or finance. See, among others, Angrist and Krueger (1999) for a description of the difference-in-differences approach. They indicate that the approach "is well-suited to estimating the effect of sharp changes in the economic environment or changes in government policy. The [difference-in-differences] method has been used in hundreds of studies in economics, especially in the last two decades, but the basic idea has a long history" (p. 1296).

[3] http://articles.latimes.com/2002/mar/13/news/mn-32586.

in counties affected by the sexual abuse decreased by 6% in the years following the revelation of the scandal compared to the years before the abuse was revealed. Furthermore, our results show no evidence of a different evolution of trust before the scandal in communities that were affected versus communities that were not affected. We then re-estimate our baseline specifications using a difference-in-differences analysis.

We find that the benefits correlated with a high average trust in the county dissipated when revelations of the abuses eroded this average trust. Furthermore, we find that the data support the parallel trend assumption, and that firms do not change their behavior in anticipation of the disclosure of a sex scandal. In other words, the mishandling of trust by those perceived to be beacons of ethics in the community is correlated with a decrease in the average trust in the county and with a contemporaneous revision to compensation and investment behavior by firms located in that county (controlling for firm and year-fixed effects and for a vector of observable variables).

Our paper contributes to the literature in at least two ways. First, we show that average county trust is correlated with a solution to incompleteness in contracts and, thus, with an efficient mitigation of moral hazard in firms.[4] Bicchieri and Sontuoso (2020, p. 241) note that "While neoclassical economics traditionally conceived of institutions as exogenous constraints, research in political economy has generated new insights into the study of endogenous institutions." We show that trust correlates with the presence of a valuable resource for firms that are located in the appropriate environment. Second, we examine what empirically happens when stewards of trust breach community expectations. Our results show that a high-profile behavior that is inconsistent with established ethical standards is correlated with a rapid and material erosion in average trust in the county and with a dissipation of the advantages associated with this high average trust for the management of firms.

The remainder of the paper proceeds as follows. We discuss the prior literature and develop our hypothesis in Sect. "Hypothesis Development". We present our research design and data in Sect. "Research Design and Data", and review our main empirical results in Sect. "Difference-in-Difference Estimation". We conduct additional analyses in Sect. "Additional Empirical Analyses". We provide discussion and conclusion in Sect. "Discussion and Conclusion".

[4] In economics, an incomplete contract is one that cannot specify what is to be done in every possible contingency (e.g., Grossman and Hart 1986; Hart 1995; Hart and Moore 1990). The remaining ambiguities have to be resolved through renegotiation or through the intervention of a third party (e.g., a court). This resolution may impose an additional cost on the different parties.

Hypothesis Development

Average Trust in a Community and Corporate Behavior

We start with the standard agency problem (e.g., Jensen, 1986). In this setting, a moral hazard problem arises because the principal is the residual claimant, whereas the agent, who is both effort and risk averse, is paid to execute a task on behalf of the principal.[5] When the actions of the agent (i.e., the executive) post-contracting are hidden from the principal (i.e., the employer), the model suggests that the principal accurately expects the agent to take advantage of this information asymmetry to appropriate or dissipate resources entrusted to them by the principal (e.g., Jensen & Meckling, 1976).

To mitigate this problem, the principal and the agent can establish a contract to increase the alignment of their interests; because the latter's effort is not directly observable, contracts are designed to compensate the agent based on outcomes rather than on the behavior. An increase in the power of incentives can be associated with greater effort from the agents while increasing the risk they are facing. However, agents' risk aversion can make these contracts prohibitively expensive and can lead the agent to commit unethical acts (Kulshreshtha, 2005). As such, a contingency contract is costly for both parties, and an alternative form of contracting that solves the agency problem is valuable for both the principal and the agent.

The prior literature (e.g., Aghion & Bolton, 1992; Chami & Fullenkamp, 2002) suggests that if that the principal and the agent operate under the belief that the agent will not harm the principal, the principal can offer a "flatter" contract that is less risky for the agent and is associated with better outcome for the principal. Since Gambetta (1988) defines such subjective probability that an individual assigns to the event that a potential counterparty will perform an action that is beneficial—or at least not harmful—to that individual as trust, we examine whether there is a correlation between the level of average trust in a given county and the empirical manifestations of efficient forms of contracting. Based on this analysis, we form our first hypothesis:

Hypothesis 1 Firms located in U.S. counties with a higher average trust are more likely to experience empirical manifestations of efficient contracts.

We contextualize our hypothesis by focusing on some of the traditional manifestations of the agency problem. If high

[5] When moral hazard is applied to executive employment, it is often referred to as the agency problem (Jensen 1986).

average trust is associated with the presence of a superior way to mitigate moral hazard, then average trust may be negatively correlated with level of empire building. Since one of the manifestation of corporate empire building is overinvestment, the correlation may also be negative with the level of plant, property, and equipment (PPE). Further, if the principal is more risk neutral than their risk-averse agent, the realized firm-risk appetite may be too low—another manifestation of agency conflict. Average trust may be associated with an increase in the average level of corporate risk tolerated by the agent without providing any further incentives to take risks. In contrast, the traditional contracting approach would mitigate this issue by increasing the Delta and the Vega of managerial compensation (i.e., the sensitivity of compensation to change in the volatility of the firm's stock price). Finally, if average trust is associated with a beneficial situation for the firm, this benefit may be reflected in a positive association with profitability and incorporated into stock prices (e.g., Tobin's Q).[6]

Shock to the Average Trust in a Community

In the development of our first hypothesis, we treat average trust in a community as an exogenous construct (with respect to firm behavior). We revisit the importance of this assumption in our second hypothesis. To investigate the possibility that a negative shock correlated with the average trust in a community may also be correlated with the dissipation of the benefit associated with a high average trust, we focus on a specific empirical setting.

On January 6, 2002, the *Boston Globe* published the first in a series of articles in its "Spotlight" column revealing a massive scandal involving the molestation of children by Catholic priests and the subsequent cover-up by local archdioceses. These articles shook the Catholic Church to its core.[7] In 2001, the newspaper assigned a team of journalists to investigate allegations against John Geoghan, a defrocked priest accused of molesting more than 80 boys. The investigation later revealed that when Catholic priests were found to have abused children, the Archdiocese of Boston went to extraordinary—and costly—lengths to cover up the scandal and to maintain secrecy for years. Although Geoghan's case was significant because of the number of victims, he was not the first priest to have molested children. In many cases, offending clergymen were quietly transferred or placed on leave, and settlements were confidential. Litigation following the Boston scandal forced the disclosure of thousands of pages of secret church records that documented how much bishops knew about the abuse. As the crisis unraveled nationally, many dioceses were sued and some sought bankruptcy protection to cover the cost of litigation and settlements. Appendix 2 presents a list of counties in which the media has reported the Catholic Church scandals.[8]

The revelation of these scandals shocked local communities and attracted widespread media attention. Both domestic and international media, including CNN, the Associated Press Newswire, Reuters News, *The Wall Street Journal*, *The Cincinnati Post*, *The Sunday Herald*, *The Philadelphia Daily News*, NPR, ABC News, and *The Birmingham Post* reported on the events in January 2002 alone. In 2002, a *Wall Street Journal*-NBC poll showed that 64% of respondents thought priests "frequently" abuse children.[9] A news article published by the *Boston Globe* on May 12, 2002, summarized the impact on local communities:

> Most of these secular authority figures are the children, grandchildren, or great-grandchildren of immigrants who owed much to the church for giving them a foothold and a place in the New World. But in many cases, that sense of unquestioning *trust* has been replaced by moral outrage […].

We expect that revelation of scandal is correlated with a powerful effect on average trust in communities that were directly affected by the scandal.

Hypothesis 2a Communities affected by church scandals are more likely to experience a decrease in average trust in the community after the revelation of the scandal.

If our Hypotheses H1 and H2a are true, we also expect the following hypothesis to be true:

Hypothesis 2b Firms located in U.S. counties with a higher level of average trust before a scandal are more likely to lose the benefits of this trust after the revelation of a scandal than those located in counties with an equally high level of trust that are unaffected by a scandal.

[6] This prediction does not necessarily imply that trust is an optimal form of contracting in the sense that it may be sub-optimal for the principal to incur costs to build trust. Rather, we hypothesize that organizations endowed with trust may be able to capitalize on this advantage.

[7] https://www.bostonglobe.com/news/special-reports/2002/01/06/church-allowed-abuse-priest-for-years/cSHfGkTIrAT25qKGvBuDNM/story.html.

[8] We conducted a search of the newspapers for the first mention of the scandal in different counties. We identified a few cases that may have indeed been revealed at a later date. In these cases, we used the date when the scandal was first reported.

[9] http://www.bostonmagazine.com/news/blog/2012/10/31/catholic-church-priest-sex-abuse-scandal/.

Research Design and Data

Data Source

We use the General Social Survey (GSS, Smith et al. 1972–2014) prepared by NORC (formerly the National Opinion Research Center) to measure the average level of trust in a county. NORC is the oldest and largest university-based survey research organization in the United States (Lavrakas, 2008). NORC incorporates methodological experiments into each year of the GSS data collection. These experiments have involved question wording, context effects, the use of various types of response scales, and random probes and other assessments of validity and reliability. NORC indicates that "the GSS is widely regarded as the single best source of data on societal trends." In fact, it is the second most frequently analyzed source of information for the social sciences in the United States after the U.S. Census.[10] The average response rate for the GSS is approximately 76%.[11] Cook and Ludwig (2006, p. 381) indicate that the GSS "is capable of providing representative samples at the national or census region or even [the] division level." The GSS covers 333 counties, representing approximately one-half of total market capitalization and one-half of the U.S. population. NORC uses "a repeated cross-sectional survey of a nationally representative sample of non-institutionalized adults who speak either English or Spanish."[12] The details of the GSS methodology are relatively technical, and more information can be found at GSS website.[13]

Essentially, the GSS asks whether people can be trusted, and respondents answer from among the responses "can be trusted" (assigned a value of 3), "can't be trusted" (assigned a value of 1), or "depends or don't know" (assigned a value of 2). We then average across all respondents to obtain a county-level measure of trust for a given year. Information on trust at the county level is available for every other year from 1992 until 2010 (the poll was also conducted in both 1993 and 1994), albeit not consecutively for every county. Other dimensions of trust (e.g., trust across racial lines, trust across socioeconomic status, trust in the federal government, and so forth) are also measured, but much more haphazardly. In our main tests, we follow previous studies (e.g., Hilary & Hui, 2009) and linearly interpolate the data to obtain values for the missing years. Approximating Trust linearly increases the power of our tests and gives us the opportunity to study the time-series properties of our setting; but, as discussed below, the results also hold when we do not linearly interpolate Trust. To gauge the accuracy of our interpolation, we conduct the following test. For all counties in which we have more than two data points, we linearly interpolate the data using the two extreme observations. We then compare the interpolated values with the actual ones. A *t* test indicates that the means of the two populations are not significantly different (p value = 0.75), suggesting that linearly interpolated measures provide a close approximation of actual values. As a further robustness test, we show that our conclusions are unchanged when we re-estimate our results but drop the years for which we interpolate the trust measures (we provide more details on this test in Sect. "Average Trust in a Community and Corporate Behavior").

Following the previous literature (e.g., Coval & Markowitz, 1999; Ivkovic & Weisbenner, 2005), we define a firm's location as the location of its headquarters. As noted by Pirinsky and Wang (2006, p. 1994), this approach appears "reasonable given that corporate headquarters are close to corporate core business activities." We extract historical headquarters location from previous 10-K filings available on the Electronic Data Gathering, Analysis, and Retrieval system (Edgar). If the data are not available on Edgar, we utilize the value in the closest year for which data are available. We then examine the effect of trust on firm-specific characteristics such as contractual intensity, monitoring, investment, and valuation.

We obtain most of the financial and accounting data from Compustat and the Center for Research in Security Prices (CRSP) database. We remove firms from the financial sectors [with Standard Industrial Classification (SIC) codes between 60 and 69] because they operate in a very different regulatory and economic environment. We also exclude penny stocks (priced at less than one dollar) from our analysis. Our sample period for the baseline test (H1) is from 1992 to 2010.[14] Our sample period for the difference-in-difference

[10] https://en.wikipedia.org/wiki/NORC_at_the_University_of_Chicago.

[11] http://publicdata.norc.org:41000/gss/.%5CDocuments%5CCodebook%5CA.pdf (pp. 2112–2113).

[12] http://gss.norc.org/Get-Documentation.

[13] More technical information on the survey can be found at http://www3.norc.org/GSS+Website. The General Social Survey (GSS) is a project of the independent research organization NORC at the University of Chicago, with principal funding from the National Science Foundation. We acknowledge that NORC at the University of Chicago, and all parties associated with the General Social Survey (including the principal investigators), offer the data and documentation "as is" with no warranty and assume no legal liability or responsibility for the completeness, accuracy, or usefulness of the data, or fitness for a particular purpose. Some of the data used in this analysis are derived from Sensitive Data Files of the GSS, obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the authors. Persons interested in obtaining GSS Sensitive Data Files should contact the GSS at GSS@NORC.org.

[14] This sample starts in 1992 because compensation-related variables are only available from 1992 onwards from the Execucomp database. It stops in 2010 to offer a balance period around the shock (i.e., we use nine years before the scandal to nine years after the scandal) from the period covered in the main sample (1998–2005).

Table 1 Summary statistics

Panel A: dependent variables						
	N	Mean	Median	Std	P25	P75
Delta	8199	1.481	1.245	1.063	0.675	2.056
Vega	8199	0.653	0.453	0.615	0.187	0.954
PPE Growth	28,192	0.020	0.007	0.081	−0.010	0.037
Tobin	27,835	2.543	1.549	3.182	1.097	2.637
Panel B: independent variables						
	N	Mean	Median	Std	P25	P75
Firm Age	29,529	2.472	2.398	0.845	1.792	3.045
Firm Size	29,529	5.300	5.178	2.109	3.806	6.690
Leverage	29,529	0.229	0.163	0.271	0.009	0.354
ROA	29,529	−0.036	0.103	0.595	−0.025	0.184
Capex/AT	29,529	0.057	0.037	0.064	0.018	0.070

The sample period is from 1998 to 2005. We define all the variables in Appendix 1

test (H2a and H2b) is 1998 to 2005 (4 years before and after the 2002 scandal). We classify 1998–2001 as the pre-scandal period and 2002–2005 as the post-scandal period.

Descriptive Statistics

Panel A of Table 1 provides descriptive statistics for the dependent variables. The first two variables measure the explicit sensitivity of CEO compensation to firm performance. Delta measures the dollar change in wealth associated with a 1% change in the firm's stock price; Vega measures the dollar change in wealth associated with a 1% change in the standard deviation of the firm's return (Coles et al., 2013).[15] PPE Growth is the change of plant, property, and equipment (PPE). Tobin is the measure of Tobin's *Q* defined as the ratio of the market-to-book value of assets (as calculated by Kaplan & Zingales, 1997). These variables are defined in greater detail in Appendix 1. Untabulated results indicate that firms located in high-trust (i.e., above-median) counties experience significantly lower average Delta, Vega, and PPE Growth and Tobin than those located in low-trust (i.e., below-median) counties.

Panel B provides summary statistics for different independent variables. We note that the mean and median values of Trust are approximately 1.8, suggesting that the U.S. population is marginally distrustful of its neighbors (with 2 being the neutral view). Untabulated results suggest that the level of trust is generally higher near the Canadian border. For example, out of 46 states for which we have data on trust, Wisconsin ranks third and Minnesota ranks fourth. The level is intermediate on the coasts (California ranks 28th, and New York State ranks 23rd). It is lower near the Mexican border (e.g., New Mexico ranks 43rd) and in the South (e.g., Arkansas ranks 42nd and Mississippi ranks 45th). Following Coles et al. (2006), we control for firm life cycle (Firm Age), firm size (Firm Size), leverage ratio (Leverage), accounting performance (ROA), and capital investment (Capex/AT). We define these variables in Appendix 1. The values in Table 1 are consistent with the prior literature (e.g., Hilary & Hui, 2009).

Table 2 provides the univariate correlations between Trust and the different variables. We present univariate correlations at the firm-year level in Panel A. Trust is associated with a lower likelihood of empire building (i.e., the correlation with PPE growth is negative). Consistent with trust being a positive attribute for firms, we find a positive correlation between Trust and Tobin. Finally, Trust is positively associated with measures of contractual intensity (Delta, Vega). Although this result is not supportive of our hypothesis, univariate analysis does not account for other variables that may confound the relationship. We will turn to multivariate analyses in the next section. Untabulated results demonstrate a low univariate correlation among the different control variables. Nevertheless, we verify below that multicollinearity does not drive our results. Panel B shows the univariate correlation between trust and various county-level social and economic variables. Importantly, social characteristics tend to be clustered at the national level and are influenced by key variables such as wealth. In contrast, correlation across U.S. counties is much weaker, particularly for trust.

[15] Both *Delta* and *Vega* are computed utilizing the Execucomp Database. We thank Lalitha Naveen for making these data available to us.

Table 2 Correlations

Panel A: correlations

		[1]	[2]	[3]	[4]
[1]	Trust	1.000			
[2]	Delta	0.025	1.000		
[3]	Vega	0.001	0.566	1.000	
[4]	PPE Growth	−0.004	0.124	0.007	1.000
[5]	Tobin	0.038	0.354	0.093	0.030

Panel B: correlation with social-demographic variables

		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
[1]	Trust	1.00							
[2]	% Catholic	0.03	1.00						
[3]	% Female	−0.10	0.33	1.00					
[4]	Unemploy Rate	−0.02	−0.04	−0.15	1.00				
[5]	Education	0.21	0.02	−0.07	−0.02	1.00			
[6]	Income	0.18	0.07	−0.10	0.08	0.85	1.00		
[7]	Ethnicity	0.09	−0.08	−0.24	−0.12	−0.12	−0.02	1.00	
[8]	% Vote Democrats	0.05	0.45	0.40	0.02	0.25	0.19	−0.64	1.00

The sample period is from 1998 to 2005. We define all the variables in Appendix 1. Panel A reports correlations between Trust and dependent variables at the firm-year level. Panel B reports correlation between Trust and social-demographic variables at the county-year level

Average Trust in a Community and Corporate Behavior

We investigate the hypothesis that average trust mitigates agency problems in steady state. We extend our analysis of the univariate correlations in Table 2 by employing regressions that control for multiple variables. Our model is the following:

$$\begin{aligned} \mathrm{FLC}_{i,t} = {} & \alpha_1 + \beta_1 \mathrm{Trust}_{i,t-1} + \delta^k \mathrm{Controls}_{i,t-1} \\ & + \varphi^t \mathrm{YearFE}_t + \psi^j \mathrm{IndFE}_j + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where i indexes the firm, t indexes years, j indexes the industry, and FLC is the set of firm-level characteristics defined in the prior section (Delta, Vega, PPE Growth, and Tobin). Control is a vector of firm-specific control variables in our baseline model. We also include a vector of county-specific controls in a second extended model. We lag these control variables by one period to mitigate any endogeneity issues (we further address this issue in Sect. 4). All our variables are winsorized at the 1% level. YearFE and IndFE are vectors of year and industry (SIC 2-digit level) indicator variables, respectively. To mitigate concerns that our results are driven by underlying socio-demographic characteristics, we further control for seven county-level socio-demographic control variables (listed in Appendix 1). Consistent with prior studies (e.g., McGuire et al., 2012), we do not make predictions about the association between our demographic control variables and the various dependent variables that we examine. For example, the workforce's educational level may be associated with managerial compensation, but the direction of this association is not clear ex ante. All the standard errors are robust and corrected for the clustering of observations by firm. Untabulated results indicate that the variance inflation factors (VIF) are all below 2 for the tabulated results.

The results presented in Table 3 support the hypothesis that average trust in the community reduces both contractual power and the degree of internal monitoring. We present the results from our baseline model in Panel A. They are consistent with our predictions in both cases. Specifically, columns (1) and (2) of Panel A in Table 3 show that Trust is negatively associated with the power of the compensation contract (both Delta and Vega). The t-statistics are − 3.08 and − 2.91. We consider the level of investment in column (3). The results are consistent with our predictions, and Trust is negatively associated with PPE growth. The t-statistic equals − 3.36. In column (4), Trust is positively associated with valuation (measured by the Tobin's Q). The t-statistic equals 3.94. Most of the socio-demographic control variables are insignificant, suggesting that these additional socio-demographic controls have little impact on the design of compensation contracts.

In Panel B, we re-estimate our baseline model using a hierarchical model instead of our fixed-effect models (Trust is treated as a second-level predictor). Mixed-models include random effects components. Random effects may increase the efficiency of the estimation but rely on more stringent

Table 3 Trust, incentive, and monitoring

Panel A: baseline model				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.080*** **	**−0.038*** **	**−0.003*** **	**0.141*** **
	(0.026)	**(0.013)**	**(0.001)**	**(0.036)**
Firm Age	−0.193***	−0.010	−0.011***	−0.036
	(0.024)	(0.011)	(0.001)	(0.027)
Firm Size	0.309***	0.209***	0.001***	−0.241***
	(0.013)	(0.007)	(0.000)	(0.015)
Leverage	−0.614***	−0.164***	−0.020***	0.419***
	(0.097)	(0.043)	(0.002)	(0.139)
ROA	0.653***	0.164***	0.004***	−1.151***
	(0.107)	(0.036)	(0.001)	(0.082)
Capex/AT	1.084***	0.174	0.249***	1.513***
	(0.251)	(0.116)	(0.010)	(0.289)
% Catholic	0.268	0.526***	−0.033***	−0.507
	(0.424)	(0.196)	(0.010)	(0.473)
% Female	−1.306	−3.101***	0.164***	−6.516**
	(2.064)	(1.019)	(0.055)	(2.721)
Unemploy Rate	0.945	1.626**	−0.004	3.407*
	(1.244)	(0.632)	(0.041)	(1.797)
Education	0.196	−0.002	−0.008	0.339
	(0.404)	(0.198)	(0.009)	(0.548)
Income	0.069	0.022	0.000	0.102
	(0.055)	(0.027)	(0.001)	(0.075)
Ethnicity	−0.301*	−0.101	−0.000	−0.164
	(0.166)	(0.079)	(0.004)	(0.212)
% Vote Democrats	−0.173	0.061	−0.014**	0.250
	(0.213)	(0.103)	(0.006)	(0.269)
Observations	16,709	16,709	54,401	55,624
R^2	0.2991	0.4102	0.1283	0.1744
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Panel B: hierarchical model				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.063** **	**−0.051*** **	**−0.003***	**0.153*** **
	(0.029)	**(0.016)**	**(0.002)**	**(0.042)**
Controls	Yes	Yes	Yes	Yes
Observations	16,709	16,709	54,401	55,624
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Panel C: re-define trust				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.071*** **	**−0.033*** **	**−0.003*** **	**0.141*** **

Table 3 (continued)

Panel C: re-define trust				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
	(0.024)	**(0.012)**	**(0.001)**	**(0.034)**
Controls	Yes	Yes	Yes	Yes
Observations	16,709	16,709	54,401	55,624
R^2	0.2990	0.4100	0.1283	0.1744
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Panel D: model without interpolation				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.109*****	**−0.035****	**−0.004*****	**0.210*****
	(0.032)	**(0.017)**	**(0.001)**	**(0.048)**
Controls	Yes	Yes	Yes	Yes
Observations	7651	7651	25,266	25,756
R^2	0.2976	0.3976	0.1256	0.1609
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Panel E: MICE				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.071*****	**−0.037*****	**−0.003*****	**0.134*****
	(0.026)	**(0.013)**	**(0.001)**	**(0.034)**
Controls	Yes	Yes	Yes	Yes
Observations	16,572	16,572	54,401	53,465
R^2	0.2977	0.4099	0.1283	0.1591
Industry * Year FE	Yes	Yes	Yes	Yes
Panel F: Industry * Year fixed effects				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.067****	**−0.036*****	**−0.003*****	**0.137*****
	(0.027)	**(0.014)**	**(0.001)**	**(0.036)**
Controls	Yes	Yes	Yes	Yes
Observations	16,709	16,709	54,401	55,624
R^2	0.3461	0.4422	0.1718	0.1971
Industry * Year FE	Yes	Yes	Yes	Yes
Panel G: entropy balance				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.091****	**−0.027*****	**−0.002*****	**0.158*****
	(0.032)	**(0.014)**	**(0.001)**	**(0.040)**
Controls	Yes	Yes	Yes	Yes
Observations	16,709	16,709	54,401	55,624
R^2	0.3112	0.3988	0.1182	0.1656

Table 3 (continued)

Panel G: entropy balance				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
Industry * Year FE	Yes	Yes	Yes	Yes

Panel H: include financial firms		
	(1)	(2)
	Delta	Vega
Trust	**−0.101*** **	**−0.052*** **
	(0.025)	**(0.013)**
Controls	Yes	Yes
Observations	19,248	19,248
R^2	0.2978	0.3907
Industry FE	Yes	Yes
Year FE	Yes	Yes

Panel I: bootstrapped standard errors				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.080*** **	**−0.038*** **	**−0.003*** **	**0.141*** **
	(0.018)	**(0.009)**	**(0.001)**	**(0.024)**
Controls	Yes	Yes	Yes	Yes
Observations	16,709	16,709	54,401	55,624
R^2	0.2991	0.4102	0.1283	0.1744
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Panel J: exclude firms that relocated				
	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
	Delta	Vega	PPE Growth	Tobin
Trust	**−0.102*** **	**−0.047*** **	**−0.003*** **	**0.112*** **
	(0.029)	**(0.014)**	**(0.001)**	**(0.038)**
Controls	Yes	Yes	Yes	Yes
Observations	13,937	13,937	45,594	46,445
R^2	0.3056	0.4066	0.1370	0.1594
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

The sample period is from 1992 to 2010. We define all the variables in Appendix 1. We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity and cluster observations at the firm level. Unless otherwise indicated, we include the full set of control variables in all panels but tabulate the full sets of coefficients only in Panel A

Bold values indicate treatment variable

assumptions than fixed effect models to be valid. Our conclusions are unaffected.[16]

Our trust variable is based on responses that are coded 1, 2, or 3. In Panel C, we drop responses with a value of 2 because people that answer "yes" or "no" may actually be different in trusting attitudes or behavior from the people that answer "don't know." Results are more significant.

In Panel D, we drop the interpolated data. In other words, we only use the actual trust measure provided by the GSS. Results show that the statistical significance is essentially unaffected, while the point estimates of the coefficients are larger for Delta and Tobin.

In Panel E, we use the Multivariate Imputation by Chained Equations (MICE) algorithm instead of the interpolation (Rubin, 1976, 1996) to impute missing values. Our conclusions are unaffected.

In Panel F, we incorporate industry * year-fixed effects to account for time-varying change industry practices or norms. Our results remain robust to this specification.

In Panel G, we use an entropy balanced sample (Hainmueller, 2012). Our results remain robust to this specification.

In Panel H, we add firms from the financial sector. However, we note that compensation may be materially different from that in other sectors. Further, PPE Growth is not a significant concern in this industry, and Tobin's Q is not conceptually defined; hence, we only re-estimate our results for the first two columns. Our conclusions are not affected. Untabulated results show that the relationship between Trust and both Delta and Vega is stronger when we focus exclusively on this sector.

In Panel I, we bootstrap the standard errors to investigate the robustness of our estimation. Our conclusions are not affected.

In Panel J, we exclude firms that relocated during the sampling period. Our conclusions are not affected.

Overall, our results suggest that average trust in the community is associated with flatter executive contract, lower moral hazard, and higher valuation. These different attributes represent benefits for firms located in high-trust environments and provide strong support for Hypothesis H1.

Difference-in-Difference Estimation

Scandal and Average Trust in the Community

We next examine the 2002 revelation of sex scandals in the Catholic Church as an exogenous shock to average trust. Specifically, we estimate the following difference-in-differences regression at the county-year level:

$$\begin{aligned}\text{Trust}_{c,t} =& \beta_{0,c\,t} + \beta_{1,c,t}\text{Post}_t \times \text{Treat}_c + \gamma' X_{c,t} \\ &+ \text{YearFE} + \text{CountyFE} + \varepsilon_{c,t},\end{aligned} \tag{2}$$

where c and t index counties and time, respectively. X represents a vector of control variables. YearFE denotes year-fixed effects, which are included to abstract away from systematic temporal effects. CountyFE denotes county-fixed effects, which absorb unobservable and slow-moving differences in different counties.[17] Post is an indicator variable that equals one from the date when news was first reported and zero otherwise. Treat_c is an indicator that equals one for counties in which sexual abuse cases are discovered and reported by the media.

We start with a simple specification in which we do not include any control variables other than fixed effects to avoid potential bias to the treatment coefficient caused by "bad controls" (e.g., Angrist & Pischke, 2008). The results in column (1) of Panel A of Table 4 show that average trust decreased among treated counties relative to control counties. The economic magnitude of the relative reduction in trust among treated counties equates to 6% of the sample mean. To mitigate concerns that changes in trust level might be spuriously correlated with other socio-demographic variables, we include a vector county–year control in addition to year- and county-fixed effects in column (2) of Panel A. The results are generally unchanged when we include additional controls. These results provide strong support for Hypothesis H2a.

Difference-in-differences analyses rely on the common (or parallel) trend assumption, which in our case means that the average trust of treated counties would have had the same change as the untreated counties had they not received the treatment. To investigate this premise, we estimate a specification that is analogous to Eq. (2) at the county-year level, except that we replace the Post indicator with separate indicators for each of the two years preceding the discovery of the church scandal, the year of discovery, and the 2 years following it: $D(t=-2)$, $D(t=-1)$, $D(t=0)$, $D(t=1)$, and $D(t\geq 2)$. Table 4, Panel B, reports the results. None of the pre-event variables is significant at conventional levels across three specifications, suggesting that treated and control counties exhibit parallel trends in trust before the shock. In other words, the decline in average trust started with the revelation of the scandal. As a robustness check, we remove responses in the original survey with a value of two and

[16] However, we note that ordinary least squares (OLSs) estimations (such as the one used in Table 3 Panel A) yield the Best Linear Unbiased Estimators (BLUE), while hierarchical models only yield the Best Linear Unbiased Predictors (BLUP).

[17] Note that the main effects of *Post* and *Treat* are absorbed by the time- and county-fixed effects, respectively.

Table 4 Test of Hypothesis H2a

Panel A: shock and trust		
	(1)	(2)
	Trust	Trust
Post * Treat	−0.109***	−0.103***
	(0.033)	(0.034)
% Catholic		−1.369
		(1.576)
% Female		−7.771
		(10.626)
Unemploy Rate		−3.940**
		(1.532)
Education		−6.086**
		(2.518)
Income		0.754***
		(0.239)
Ethnicity		−0.600
		(1.150)
% Vote Democrats		−0.521
		(0.485)
Observations	1728	1718
R^2	0.7124	0.7203
Year FE	Yes	Yes
County FE	Yes	Yes
Panel B: parallel trend trust		
	(1)	(2)
	Trust	Trust
$D(t=-2)$ * Treat	0.061	0.054
	(0.061)	(0.061)
$D(t=-1)$ * Treat	−0.019	−0.030
	(0.051)	(0.051)
$D(t=0)$ * Treat	−0.119*	−0.124**
	(0.063)	(0.063)
$D(t=1)$ * Treat	−0.101**	−0.100**
	(0.049)	(0.049)
$D(t\geq 2)$ * Treat	−0.087*	−0.083*
	(0.046)	(0.047)
% Catholic		−1.353
		(1.600)
% Female		−7.727
		(10.665)
Unemploy Rate		−4.017***
		(1.537)
Education		−6.147**
		(2.522)
Income		0.756***
		(0.240)
Ethnicity		−0.588
		(1.154)
% Vote Democrats		−0.533

Table 4 (continued)

Panel B: parallel trend trust		
	(1)	(2)
	Trust	Trust
		(0.487)
Observations	1728	1718
R^2	0.7128	0.7208
Year FE	Yes	Yes
County FE	Yes	Yes

The sample period is from 1998 to 2005. We estimate the regressions at county-year level. Panel A reports difference-in-differences analysis. Panel B tests parallel trend assumption at county-year level. We define all the variables in Appendix 1. We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity and cluster observations at the county level

recalculate our measure of trust. Untabulated results yield similar conclusions.

Scandal and Corporate Behavior

Having established that average trust level declined among treated communities following the discovery of the scandal, we examine in our next set of tests whether CEO incentive contracts, investment, or corporate performance changed following the event. To do so, we estimate the following difference-in-differences specification using firm-year observations:

$$Y_{it} = \beta_{0,it} + \beta_{1,it}\text{Post}_t \times \text{Treat}_i + \gamma' X_{it} + \text{FirmFE} + \text{YearFE} + \varepsilon_{it}, \quad (3)$$

where i, c, and t index firms, counties, and time, respectively. Treat_i is an indicator variable that equals one for firms located in counties in which sexual abuse cases were discovered and reported by the media. X represents a vector of control variables. FirmFE denotes firm-fixed effects, which are included to control for cross-sectional differences in dependent variables. YearFE denotes year-fixed effects, which are included to abstract away from systematic temporal effects.[18] Post is an indicator variable that equals one from the date when news was first reported and zero otherwise, delineating the post-scandal period. Treat is an indicator that equals one for firms headquartered in the counties in which sexual abuse cases were discovered and reported by the media. The β_1 coefficient in Eq. (3) captures the average treatment effect on the treated (ATT) and provides an estimate of the effect of a decline in average trust on firms' compensation contract, investment and performance.

Column (1) of Table 5 examines how the scandal affected the sensitivity of CEOs' equity portfolio values to changes in stock price, or Delta. The coefficient on Post * Treat is positive and statistically significant (t-statistic of 2.31), indicating that the magnitude of CEOs' equity incentives is increasing among treated firms relative to control groups. We find similar results for Vega: the coefficient on Post * Treat is positive and significant (t-statistic of 2.62). The coefficient estimates in columns (1) and (2) suggest that, following the discovery of the scandal, the CEOs in our sample experienced a relative increase in their portfolio Delta (Vega) of 4.8% (6.3%) relative to its mean among treated firms relative to control groups. This increase in equity incentives is consistent with our prediction that the power of managerial contracts increases when trust is lower. Column (3) reports estimates for PPE Growth. The coefficient on Post * Treat is positive and significant at the 5% level (t-statistic of 1.96), suggesting that the extent of empire building is increasing among treated firms. Together with the results in the first three columns, column (4) shows the performance impact when average trust is exogenously reduced. The coefficient on Post * Treat is negative and significant at the 10% level (t-statistic of − 1.88), indicating that treated firms, on average, perform poorly following the discovery of Catholic Church scandal.

In Panel B, we incorporate industry * year-fixed effects to absorb any effect from industry trend or changes. Our results remain robust to these additional controls.

Next, to address the possibility that our treatment effect might reflect substantial heterogeneity between firms located in different counties, we create a propensity score matched (PSM) sample. To generate a set of matched firms, we first regressed an indicator variable for the presence of a scandal on our firm and county-level control variables as per Eq. (1). We use information in the year immediately before the shock to create a matched

[18] Note that the main effects of *Post* and *Treat* are absorbed by the time- and firm-fixed effects, respectively.

Table 5 Difference-in-Differences—Hypothesis H2b

Panel A: difference-in-difference estimates

	(1) Delta	(2) Vega	(3) PPE Growth	(4) Tobin
Post * Treat	**0.071****	**0.041*****	**0.003****	**−0.114***
	(0.031)	**(0.016)**	**(0.002)**	**(0.061)**
Firm Age	−0.703***	−0.148***	−0.039***	−1.912***
	(0.112)	(0.046)	(0.003)	(0.163)
Firm Size	0.374***	0.256***	0.029***	−1.194***
	(0.029)	(0.015)	(0.001)	(0.073)
Leverage	−0.750***	−0.180***	−0.007	0.133
	(0.099)	(0.046)	(0.005)	(0.201)
ROA	0.764***	0.164***	−0.002*	−0.967***
	(0.265)	(0.038)	(0.001)	(0.130)
Capex/AT	1.062***	0.478***	0.852***	1.100**
	(0.275)	(0.124)	(0.022)	(0.506)
% Catholic	−1.422	−1.458	−0.076	5.112
	(2.085)	(1.120)	(0.104)	(4.249)
% Female	11.274	−4.748	−0.586	−67.031**
	(15.514)	(7.865)	(0.752)	(28.628)
Unemploy Rate	−2.920	−2.045**	−0.146	−3.659
	(1.878)	(0.957)	(0.095)	(3.795)
Education	−0.640	−2.168	0.043	−8.059
	(4.595)	(1.738)	(0.197)	(6.392)
Income	−0.443	0.191	0.004	−0.949**
	(0.300)	(0.122)	(0.011)	(0.374)
Ethnicity	0.969	−0.899	−0.006	14.879***
	(1.394)	(0.757)	(0.064)	(2.774)
% Vote Democrats	0.065	−0.233	0.060**	−4.095***
	(0.693)	(0.355)	(0.030)	(1.154)
Observations	8174	8174	28,081	27,716
R^2	0.8224	0.8555	0.5926	0.6656
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

Panel B: industry * Year fixed effects

	(1) Delta	(2) Vega	(3) PPE Growth	(4) Tobin
Post * Treat	**0.095*****	**0.045*****	**0.003****	**−0.095***
	(0.031)	**(0.016)**	**(0.002)**	**(0.055)**
Controls	Yes	Yes	Yes	Yes
Observations	8174	8174	28,081	27,716
R^2	0.8418	0.8662	0.6126	0.6942
Industry * Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

Table 5 (continued)

Panel C: propensity score matched sample

	(1) Delta	(2) Vega	(3) PPE Growth	(4) Tobin
Post * Treat	**0.303*** **	**0.084*** **	**0.004** **	**−0.216*** **
	(0.047)	**(0.025)**	**(0.002)**	**(0.066)**
Controls	Yes	Yes	Yes	Yes
Observations	10,995	10,995	32,671	32,201
R^2	0.7722	0.7325	0.6664	0.5669
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Matching Pair FE	Yes	Yes	Yes	Yes

Panel D: include financial firms

	(1) Delta	(2) Vega
Post * Treat	**0.089*** **	**0.057*** **
	(0.029)	**(0.016)**
Controls	Yes	Yes
Observations	9360	9360
R^2	0.8282	0.8538
Firm FE	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

Panel E: bootstrapped standard errors

	(1) Delta	(2) Vega	(3) PPE Growth	(4) Tobin
Post * Treat	**0.075*** **	**0.033** **	**0.003***	**−0.109***
	(0.024)	**(0.015)**	**(0.002)**	**(0.058)**
Controls	Yes	Yes	Yes	Yes
Observations	8174	8174	28,081	27,716
R^2	0.8185	0.8518	0.5863	0.6605
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

The sample period is from 1998 to 2005. We exclude financial industries (SIC 6000–6999) and penny stocks (stock price < 1 dollar). We estimate the regression at the firm-year level. We define all the variables in Appendix 1. We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity and cluster observations at the firm and pre-/post-scandal level

Bold values indicate treatment variable

sample that has similar firm and county characteristics immediately before the revelation of the church scandal. We then match all firms shocked by a scandal (i.e., treated firms) with the closest matched observation based on PSM score. To gauge the quality of our matching, we conduct a *t* test for the difference in PSM score between treated and matched control firms based on the matched sample. The *t* test shows a *p* value of 0.99, suggesting that our treated and matched control firms have a similar likelihood of experiencing a shock, given the various firm and county characteristics. In other words, the matched sample creates pairs of firms that are otherwise similar along the

set of control variables, except that treated firms experience the shock later. The matched samples may exacerbate the effect of unobservable omitted variables. To address this issue, we followed the procedure outlined in Heider and Ljungqvist (2015) to incorporate matched-pair fixed effects. We then re-estimate our difference-in-difference specification based on the PSM sample. Results in Panel C show that our inference remains the same but with an increase in statistical significance. Further, untabulated results indicate that our conclusions are unaffected if we use an entropy balance approach.

Taken together, the only remaining omitted variables that could confound our results are those that vary within counties, industry, year, and firms and behave in a manner that is collinear with the changes in average trust and with the occurrence of church scandals across cities but are unrelated to both. This strikes us as implausible. Across all specifications, we continue to find consistent results supporting H2b.

In Panel D, we add the firms from the financial sector. We continue to find similar results for incentive compensation.

In Panel E, we bootstrap the standard errors. Our inference remains unaffected.

Our inferences also rely on the parallel assumption that firms did not adjust their pre-treatment outcomes in anticipation of receiving treatment. This assumption appears realistic in our setting. To assess the validity of these assumptions further, we examine whether firms located in different counties with different trust levels did in fact exhibit parallel trends before the church scandals. To do so, we estimate a specification that is analogous to Eq. (3), except that we replace the Post indicator with separate indicators for each of the two years preceding the discovery of the church scandal, the year of the discovery, and the 2 years following it: $D(t=-2)$, $D(t=-1)$, $D(t=0)$, $D(t=1)$, and $D(t\geq 2)$.

We present the results of this specification in Table 6. None of the pre-event variables is significant at conventional levels, a finding that is consistent with our assumption that firms did not change their corporate behavior in anticipation of the event. This finding also suggests that treated and control firms had similar—and therefore, parallel—trends prior to the discovery of the church scandal. In other words, the decline in the benefits of average trust started with the revelation of the scandal.

Table 6 Parallel trend outcome

	(1) Delta	(2) Vega	(3) PPE Growth	(4) Tobin
$D(t=-2)$ * Treat	0.040	0.024	0.001	−0.066
	(0.041)	(0.021)	(0.002)	(0.117)
$D(t=-1)$ * Treat	0.033	0.039	0.004	−0.163
	(0.048)	(0.026)	(0.003)	(0.116)
$D(t=0)$ * Treat	**0.097****	**0.043***	**0.004**	**−0.188***
	(0.047)	**(0.025)**	**(0.003)**	**(0.102)**
$D(t=1)$ * Treat	**0.089***	**0.066****	**0.005***	**−0.145**
	(0.048)	**(0.026)**	**(0.003)**	**(0.104)**
$D(t\geq 2)$ * Treat	**0.092***	**0.071****	**0.006****	**−0.233****
	(0.054)	**(0.028)**	**(0.002)**	**(0.108)**
Firm Age	−0.701***	−0.147***	−0.039***	−1.915***
	(0.111)	(0.046)	(0.003)	(0.163)
Firm Size	0.374***	0.256***	0.029***	−1.194***
	(0.029)	(0.015)	(0.001)	(0.073)
Leverage	−0.750***	−0.186***	−0.007	0.135
	(0.099)	(0.046)	(0.005)	(0.201)
ROA	0.761***	0.157***	−0.002*	−0.964***
	(0.265)	(0.037)	(0.001)	(0.130)
Capex/AT	1.065***	0.454***	0.851***	1.103**
	(0.276)	(0.124)	(0.022)	(0.507)
% Catholic	−1.483	−1.345	−0.071	4.980
	(2.117)	(1.133)	(0.104)	(4.246)
% Female	11.903	−5.631	−0.708	−62.844**
	(16.052)	(8.108)	(0.763)	(28.928)
Unemploy Rate	−2.687	−1.953*	−0.122	−4.808
	(1.916)	(1.187)	(0.098)	(3.791)
Education	−0.616	−2.091	0.033	−7.685
	(4.614)	(1.748)	(0.198)	(6.446)
Income	−0.445	0.185	0.005	−0.988***
	(0.301)	(0.123)	(0.011)	(0.377)
Ethnicity	1.014	−0.928	−0.013	15.120***
	(1.404)	(0.764)	(0.064)	(2.784)
% Vote Democrats	0.039	−0.229	0.058**	−3.989***
	(0.693)	(0.361)	(0.030)	(1.148)
Observations	8174	8174	28,081	27,716
R^2	0.8224	0.8549	0.5927	0.6657
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

The sample period is from 1998 to 2005. We exclude financial industries (SIC 6000–6999) and penny stocks (stock price < 1 dollar). We estimate the regression at the firm-year level. We define all the variables in Appendix 1. We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity and cluster observations at the firm and pre/post-scandal level

Bold values indicate treatment variable

Additional Empirical Analyses

Lastly, our empirical design relies on the use of average trust in a county to understand firm behavior, which implies that the preferences of shareholders, directors, executives, and community members are congruent. The empirical literature has already established some of this congruence.

For example, Massa et al. (2020) find that investor attitude toward trust, investor decisions to invest in mutual funds, fund attitude toward trust, and fund decisions to invest in corporations are congruent. The well-documented home bias (e.g., Coval & Markowitz, 1999; Ivkovic & Weisbenner, 2005) indicates that many shareholders belong to the local community and naturally share that community's social preferences.

Trust and Directors

Knyazeva et al. (2013) show that firms tend to hire directors from the region in which the company's headquarters is located. To investigate this dimension further, we measure a director's "intrinsic" level of trust for a subsample of firms for which we can obtain the directors' outside employment location. For example, if a director sits on the board of focal firm A and works as an executive or director of firm B, we use the community average trust of the headquarters location of firm B as a measure of the director's intrinsic trust. When directors have multiple positions of outside employment, we average trust across all locations. We then calculate TrustBd as the average trust of all directors with available outside employment information. We obtain data for 2962 firms for which we identify at least one director with outside employment information. For these firms, we obtain a measure of intrinsic trust for 52% of directors. To reflect this sampling composition, we use weighted least square regression where the weight of the regression is inverse to the percentage of directors for whom we can identify employment information from Boardex.[19] We find that TrustBd has a 64% correlation with Trust, further supporting the presence of congruence between the culture of the firm's location and its directors. We present results in Table 7 Panel A. Our main conclusions are not affected.

Trust and CEOs

Tsai (2011) shows that organizational culture and leadership behavior are positively correlated and that a lack of alignment reduces job satisfaction. To investigate this dimension further, we examine a sample of 107 CEOs who changed employers between 1993 and 2010.[20] We regress the average trust of the county in which the new employer is located (Trust_Joining) on the average trust of the county in which the former employer is located (Trust_Leaving). If aversion to distrust is a stable parameter for CEOs, we expect CEOs to operate in similar environments, and we predict that the two measures of trust will be positively related. We employ three specifications. The first specification regresses Trust_Joining on Trust_Leaving, controlling for other differences in socio-demographic variables. The second specification further controls for joining-state and leaving-state time-invariant characteristics through state-level fixed effects. In the third specification, we add leaving-firm characteristics.

The results in Table 7, Panel B, indicate that the average trust of the county in which the former employer is located is a predictor of the average trust of the county in which the new employer is located. This finding holds in all three specifications, with *t*-statistics ranging from 2.85 to 3.62, and is consistent with the observation that CEOs consistently choose to work for organizations that are likely to exhibit the same environment. The other demographic variables are mostly statistically insignificant. Hilary and Hui (2009) obtain similar results with corporate risk aversion, which shows that CEOs have intrinsic preferences for certain social environments and strongly suggests that CEO and community average trust are congruent.

These different elements suggest that there should be congruence in environmental preferences between shareholders, directors, executives, and the community.

Discussion and Conclusion

Trust is a complex concept that has been examined by many scholars from different disciplines, using various settings and methodologies (Thielmann & Hilbig, 2015). Kramer (1999, p. 571) notes that "a concise and universally accepted definition [of trust] has remained elusive." Indeed, different authors in different disciplines have defined and operationalized it in different ways. For example, management scholars (e.g., Colquitt et al., 2007; Ferguson & Peterson, 2015) distinguish "trust" (the intention to accept being vulnerable to a trustee based on positive expectations of his or her actions) from "trustworthiness" (the ability, benevolence, and integrity of a trustee) and "trust propensity" (a dispositional willingness to rely on others). Mayer et al., (1995, p. 712) define trust as "the willingness of a party to be vulnerable to the actions of another party based on the expectation that the other will perform a particular action important to the trustor, irrespective of the ability to monitor or control that other party."

This definition of trust is broadly consistent with what some economists have in mind. For example, in their survey of the literature on trust and growth, Algan and Cahuc (2010, p. 522) rely on the definition provided by Coleman (1990): "individuals trust if they voluntarily place resources at the disposal of another party without any legal commitment

[19] Untabulated results from an OLS estimation are similar.

[20] We identify 2037 CEO turnover events from Execucomp over the 1993–2010 period. When we impose the further constraint that departing CEOs join another firm in the Execucomp universe, we are left with 107 events.

Table 7 Director, executive, and shareholder congruence

Panel A: trust and directors

	(1)	(2)	(3)	(4)
	Delta	Vega	PPE Growth	Tobin
TrustBD	**−0.186****	**−0.152*****	**−0.010*****	**0.198***
	(0.075)	**(0.053)**	**(0.002)**	**(0.113)**
Firm Age	−0.109***	0.030*	−0.006***	−0.046
	(0.027)	(0.017)	(0.001)	(0.039)
Firm Size	0.286***	0.206***	−0.001**	−0.180***
	(0.013)	(0.010)	(0.000)	(0.019)
Leverage	−0.365***	−0.145**	−0.019***	0.100
	(0.096)	(0.062)	(0.003)	(0.177)
ROA	1.015***	0.345***	0.015***	−0.770***
	(0.204)	(0.097)	(0.002)	(0.196)
Capex/AT	0.565	−0.072	0.242***	2.277***
	(0.375)	(0.221)	(0.022)	(0.454)
Observations	7402	7402	14,816	14,862
R^2	0.3519	0.3766	0.1819	0.1806
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Panel B: trust and CEOs

	(1)	(2)	(3)
	Trust Joining	Trust Joining	Trust Joining
Trusting Leaving	**0.328*****	**0.491*****	**0.487*****
	(0.112)	**(0.128)**	**(0.138)**
Diff in Religiosity	−0.704	−0.335	−0.527
	(0.458)	(1.006)	(1.169)
Diff in % Catholic	0.803	−0.171	0.145
	(0.856)	(2.400)	(2.642)
Diff in Population	0.020	0.073*	0.081*
	(0.023)	(0.037)	(0.042)
Diff in % Female	−0.017	3.087	1.434
	(5.295)	(10.545)	(11.493)
Diff in Unemploy Rate	−0.011	0.021	0.022
	(0.025)	(0.033)	(0.035)
Diff in Education	0.007	0.028	0.024
	(0.009)	(0.019)	(0.021)
Diff in Income	0.012	−0.014	0.040
	(0.110)	(0.223)	(0.252)
Leverage Leaving			−0.102
			(0.392)
ROA Leaving			−0.003
			(0.562)
Capex/AT Leaving			−0.257
			(1.121)
Observations	107	107	107
R^2	0.2929	0.7750	0.7680
Year FE	Yes	Yes	Yes
Leaving-state FE	No	Yes	Yes
Joining-state FE	No	Yes	Yes

Table 7 (continued)

(A) The sample period is from 1992 to 2010. We exclude financial industries (SIC 6000–6999) and penny stocks (stock price < 1 dollar). We define all the variables in Appendix 1 We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity and cluster observations at the firm level

(B) The sample period is from 1992 to 2010. The dependent variable is the trust of the county in which the new employer is located (Trust Joining). Diff in *XX* is the difference in the county-level characteristics *XX* between the joining firm and the leaving firm. *XX Leaving* is the firm-level characteristics *XX* of the leaving firm. We define all other variables in Appendix 1. We include but do not report constants in the regressions. We present the standard errors beneath the coefficients within parentheses

*, **, and ***Significance at the 10%, 5%, and 1% level, respectively. We correct standard errors for heteroscedasticity

Bold values indicate treatment variable

from the latter, but with the expectation that the act of trust will pay off." In this context, one definition that fits well with the contracting literature can be found in Gambetta (1988). It defines trust as the subjective probability that an individual assigns to the event that a potential counterparty will perform an action that is beneficial—or at least not harmful—to that individual.

To bridge from theoretical construct to empirical measure, one may consider the expected probability that the counterparty is "helpful" (conditional on belonging to a county) to postulate a framework. In this theoretical environment, this conditional expected probability is unique (in a mathematical sense), but, naturally, this does not mean that everyone adheres to the expectation (put differently, the realized value is not always equal to its expectation). Empirically, one estimates an expected value by using the in-sample mean of its realization, which is essentially what we do in our empirical part (adjusting for various econometric issues).

Economists often think about probabilities using a Bayesian framework. Thus, one way to think about trust (as a probability) is in terms of prior/posterior distributions in a Bayesian framework. Individuals have subjective probabilities (their prior beliefs) that they adjust based on what they observe. One question is where these priors come from, and different antecedents have been proposed in the economic and management literature. Importantly, these antecedents are plausibly orthogonal to what we consider in our study. We use this property in Table 3.

A second issue is how people update their priors (i.e., the process that generates the posterior probability). Within this theoretical framework, Table 4 can be seen as the empirical examination of this process, while Table 5 focuses on its consequences. Bayesian updating implies that the expected value is changed as agents receive more information. To a large extent, our focus is on the conditional expectation (the average trust). A natural way to think about the interplay between individual and community beliefs is again a game-theoretical framework. For example, Bicchieri and Sontuoso (2020, p. 244) define norms as "behavioral regularities emerging in a mixed-motive (i.e., social dilemma) game, as a result of preferences for conformity conditional on an endogenous set of beliefs and expectations." A game-theoretic approach generates individual beliefs that are mutually consistent. For example, the belief that people will reciprocate may lead individuals to help those in need. Prior research suggests the existence of common beliefs at the regional level. Controlling for parental attitudes, Dohmen et al. (2012) find that trust attitudes for children are significantly related to the prevailing attitude in a region, even after controlling for parental influence and other factors. These beliefs can be stable over long period of time. For example, Algan and Cahuc (2010) show that inherited beliefs of descendants of U.S. immigrants is significantly influenced by the country of origin and the timing of arrival of their forebears. This stability provides the empirical foundation for our first hypothesis, but naturally, this does not mean that these attitudes are not completely rigid. In our context, an observable shock to the community behavior is associated with a change in the individual behavior of firms in the county.[21] Tables 5 and 6 can be understood as the empirical analysis of this change.

How does this framework relate to the thinking in the management literature? The definition of trust in Gambetta seems close to what the management literature commonly uses. For example, Mayer et al., (1995, p. 712) indicate that "The definition of trust proposed in this research is the willingness of a party to be vulnerable to the actions of another party based on the expectation that the other will perform a particular action important to the trustor, irrespective of the ability to monitor or control that other party." The key difference between these two definitions is that "probability" is replaced by "willingness." While the former is a specific mathematical concept, the latter seems to invoke a state of mind. Economists may prefer this first approach for at least two reasons. A first advantage of framing the definition in terms of probabilities (and later in terms of conditional

[21] Studies such as Gavrilets and Richerson (2017) and Morsky and Akçay (2019) offer theoretical models for these interactions.

probabilities) is that we can directly use statistical theory to go from the construct definition to its empirical analog. In contrast, the operationalization of a "willingness" may be subject to greater debate. A second advantage is that such framing allows for an easier derivation of an optimal contract from a theoretical point of view. For example, Chami and Fullenkamp (2002) propose a theoretical agency model that relies on these expectations and mitigates the need for monitoring. This model suggests that, while the principal decreases the power of the employment contract, the agent still cares more about the principal, and firms enjoy higher profits.[22] These results are consistent with the view that the presence of these expectations is associated with optimized contracts and operations, and more generally with the view that incomplete contracts may dominate complete contracts when managers are ethical (e.g., Allen & Gale, 1992; Carlin & Gervais, 2009; Falk & Kosfeld, 2006). In contrast, scholars in management may be interested in a richer understanding of the complex relationships between social activities, psychological states, social and moral norms, and institutions. In this context, the use of a summary statistic to describe the notion of trust may prevent a fuller characterization of these different relations. Expanding on these differences could be a very fruitful avenue for further cross- and interdisciplinary research and discussion.

We also note that the notion of prior belief is also present in the management literature (albeit under a different name).[23] For example, Mayer et al., (1995, p. 89) indicate that the "Propensity to trust is proposed to be a stable within-party factor that will affect the likelihood the party will trust. People differ in their inherent propensity to trust. Propensity might be thought of as the general willingness to trust others." Although not couched as such, this view is reminiscent of the notion of prior beliefs (i.e., the baseline trust before it is contextualized through the reception of subsequent information).[24] Another notion used in the management literature is the attribution of trustworthiness (Colquitt et al., 2007), which we may call the formation of game-theoretical beliefs in our framework. Thus, thinking about these different notions using an economic structure may offer a unified framework. We note, however, the use of Bayesian framework is not unknown to management scholars. For example, early on, Solomon (1960) described the effects of reputation on trust utilizing a prisoner's dilemma. More recently, Thielmann and Hilbig (2015, p. 260) note that "To specify how exactly individuals form this probability estimate—and thus bridge the noteworthy gap between trust research and judgment research—remains a vital quest for future research (cf. Table 1). However, recent developments in (probabilistic) modeling of epistemic trust in children based on Bayesian inference (Shafto et al., 2012) seems to provide a valuable starting point and a fruitful (statistical) approach to close this gap." To summarize, a framework based on conditional expectations unifies different concepts present in the management literature using statistical theory: trust as a probability, an expected behavior in a community as an expected probability, antecedents of trust (and trust propensity) as prior probability distributions, or attribution of trustworthiness as a Bayesian updating to form posterior probability distributions. This approach also directly motivates our empirical proxies.

Aside from these conceptual similarities, partially masked by differences in language, there are dissimilarities, or at least, aspects outside the scope of the reductionist view that we take in this study. For example, researchers (e.g., McKnight & Chervany, 2001) have distinguished between low trust and mistrust. In contrast, we consider only high and low level of trust and treat them as polar opposites of a unique dimension. In that, we join Schoorman et al., (2007, p. 349), who hold the "(more traditional) view that trust and distrust are the opposite ends of the same continuum." Further, the management and the psychology literature is careful to note that there are important differences on a psychological and behavioral level between a low trust and the betrayal of trust (e.g., Fitness, 2001; Kramer, 1999). This conceptual segmentation, reminiscent of prospect theory in economics, is not without merit.[25] However, this distinction does not yield predictions that we can easily discriminate in our empirical setting. To do this, we would need a sudden spike in average trust without obvious economic implications. Unfortunately, sudden exogenous shocks that show that community leaders should be trusted more than anticipated are rare.

Lastly, we use Bayesian updating as an approximation of the cognitive processes used to form expectations. Naturally, a vast literature in psychology and management as well as in economics shows that this is, indeed, just an approximation. Cognitive biases and emotions, for example, have been shown to matter in many contexts. We ignore the neurological and psychological processes that lead to the formation

[22] Conversely, Al-Najjar and Casadesus-Masanell (2002) indicate that the presence of these beliefs is a necessary condition to work under incomplete contracts and that there is a monotonic relationship between the principal's ability to hold these beliefs and their expected profit.

[23] While economists often think in terms of prior and posterior beliefs, scholars in management may refer to the trust among strangers as "initial trust" (McKnight et al., 1998; Thielmann and Hilbig, 2015).

[24] Although exploring the antecedent of trust can be a very interesting topic, it is beyond the scope of this study.

[25] See also older economic models (e.g., Kreps and Wilson 1982; Milgrom and Roberts 1982) that offers a similar view of trust betrayal without relying on prospect theory.

of expectations to keep our analysis tractable and because our empirical setting is not optimal to investigate these issues. Given the extensive empirical procedures that we employ, we do not have reasons to believe that analyzing these processes with a greater granularity would invalidate our conclusions. However, as always in social science, the presence of an unspecified correlated omitted variable is still possible. As such, causality cannot be established in our findings in the strictest sense, and the different associations we report may or may not be a reflection of a direct relationship between trust and contracting.

Appendix 1

Variable definitions

Variables	Definition
Trust	Trust constructed from the GSS. The survey asks whether people can be trusted, to which respondents answer "can be trusted" (assigned a value of 3), "can't be trusted" (assigned a value of 1) or "depends or don't know" (assigned a value of 2). We then average across all respondents from one county to obtain a county-level measure of trust for each year. When the trust measure is not available for that year, we interpolate the value from the most recently available value
Delta	Log of the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's returns. Obtained from Coles et al. (2013)
Vega	Log of the dollar change in wealth associated with a 1% change in the firm's stock price. Obtained from Coles et al. (2013)
PPE Growth	Change in PPE over lagged assets
Tobin	Market value of equity plus the book value of assets minus the sum of the book value of common equity and deferred taxes, all divided by the book value of assets
Firm Age	Log of firm age, where age is calculated as number of years since a firm first appeared in the CRSP
Firm Size	Log of total assets
Leverage	Short-term debt plus long-term debt, divided by total assets
ROA	Operating income before depreciation expenses over lagged total assets
Capex/AT	Capital expenditure over total assets
%Catholic	Percentage of Catholic population at the county level. When the measure is not available in that year, we interpolate the value from the most recently available value
% Female	Percentage of females in the county-level population. When the measure is not available in that year, we interpolate the value from the most recently available value
Education	Percentage of population with at least a bachelor's degree at the county level. When the measure is not available in that year, we interpolate the value from the most recently available value
Income	Income per capita at the county level. When the measure is not available in that year, we interpolate the value from the most recently available value
Ethnicity	Percentage of white population at the county level. When the measure is not available in that year, we interpolate the value from the most recently available value
% Vote Democrats	Percentage of vote cast for Democratic president. When the measure is not available in that year, we interpolate the value from the most recently available value
Post	Indicator equal to one for observations from 2002 onward and zero otherwise
Treat	Indicator equal to one for firms located in counties affected by a scandal in the Catholic
$D(t=-2)$	Indicator equal to one if it is 2 years prior to the report of the Catholic church scandal and zero otherwise
$D(t=-1)$	Indicator equal to one if it is 1 year prior to the report of the Catholic church scandal and zero otherwise
$D(t=0)$	Indicator equal to one if it is the year during which the Catholic church scandal was reported and zero otherwise
$D(t=1)$	Indicator equal to one if it is 1 year after the report of the Catholic church scandal and zero otherwise
$D(t\geq 2)$	Indicator equal to one if it is two or more years after the report of the Catholic church scandal and zero otherwise

Appendix 2

List of counties with scandals where Catholic Church scandals have been reported

State names	County names	First news report
Alaska	Anchorage	28Apr2002
Arizona	Maricopa	07Jun2002
Arizona	Pima	13Dec2001
California	Alameda	11Apr2002
California	Los Angeles	28Apr2002
California	Orange	21Aug2001

State names	County names	First news report
California	Sacramento	28Jan2002
California	San Bernardino	13Feb2002
California	San Diego	31Jan2002
California	Santa Barbara	04Mar2002
California	Santa Clara	27Mar2002
California	Sonoma	15Mar2002
Colorado	Denver	20Mar2002
Connecticut	Fairfield	19Mar2002
Delaware	Newcastle	07Apr2002
Florida	Palm beach	11Feb2002
Illinois	Cook	15Mar2002
Illinois	Will	17Apr2002
Indiana	Marion	20Mar2002
Kansas	Wyandotte	18Jun2002
Kentucky	Kenton	19Mar2002
Maryland	Baltimore	26Jan2002
Massachusetts	Norfolk	03Feb2002
Massachusetts	Suffolk	08May2001
Massachusetts	Worcester	31Aug2001
Michigan	Kent	18May2002
Michigan	Wayne	29Mar2002
Minnesota	Ramsey	13Apr2002
Missouri	Jackson	03Jul2002
Missouri	St. Louis	03Jul2001
New Jersey	Camden	12Apr2002
New York	Kings	23May2002
New York	Monroe	27Feb2002
New York	Nassau	15Mar2002
New York	New York	28Apr2002
New York	Orange	21Aug2001
North Carolina	Wake	25Apr2002
Ohio	Cuyahoga	29Mar2002
Ohio	Hamilton	18Mar2002
Oregon	Multnomah	06Nov2001
Pennsylvania	Allegheny	18Apr2002
Pennsylvania	Philadelphia	05Feb2002
South Carolina	Charleston	12Mar2002
Tennessee	Davidson	18Mar2002
Tennessee	Shelby	30Apr2002
Texas	Bexar	23Apr2002
Texas	Dallas	17Mar2002
Texas	Tarrant	15Aug2002
Vermont	Chittenden	12Apr2002
Washington	King	10Apr2002

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Declarations

Conflict of interest The authors have not identified any conflict of interests.

Ethical Approval The research did not involve human participants or animals.

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