Local religiosity and stock market liquidity

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The data that support the findings of this study are obtained from several sources. Market data were obtained from Refinitiv Eikon and Refinitiv Datastream. These data are available from the authors with the permission of Refinitiv. Other data are obtained from public sources (U.S. Census Bureau, ARDA, Guttmacher Institute, Bureau of Economic Analysis, National Institute on Alcohol Abuse and Alcoholism, MIT, NRCRD, Stephen Brown's website, Anthony D'Agostino) and are available on request.

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Abstract

This paper investigates whether religiosity affects liquidity for a broad sample of U.S. listed companies. Over the period 1997–2020, we find a negative and statistically significantly relation between religiosity and the bid-ask spread. Further, we document that firms located in more religious areas tend to have smaller price impact of trades and lower probability of information-based trading. The relation between religiosity and liquidity is of particular interest when liquidity providers face high information asymmetry and rely on religiosity as a commitment device conveying the firms' willingness to conduct business reliably, predictably, and conservatively. Finally, the negative relation tends to be more relevant for NASDAQ firms. Overall, our findings highlight the importance of soft information in enhancing stock market liquidity.

Keywords: Religiosity; Liquidity; Information Asymmetry; Bid-Ask Spread; Geographic Location; Religious Social Norms

JEL classification: G31; G32; G40; G41; Z12

1 Introduction

"Culture, more than book rules, determines how an organization behaves" (Buffet, 2011).[1] This statement underpins the importance of considering culture as a relevant determinant of corporate decisions and performance. Also, Hirshleifer (2015) further concludes in his comprehensive review on behavioral finance that scholars need to pay more attention to social finance, which includes the consideration of social norms, moral attitudes, religions, and ideologies in the context of financial behavior. Recently, Graham et al. (2017) document that 92% of executives believe that improving corporate culture is an important factor for increasing their firm's value. Indeed, a growing number of studies empirically investigate the link between culture and firm outcomes (e.g., Kanagaretnam et al. 2014; Guiso et al., 2016; Hilary and Huang, 2021).[2] Of particular interest in this context is the local religiosity of the county where a firm is located (Jiang et al., 2018).

We start with Weber (1930), who already concluded in the early 20th century that religiosity has a positive effect on economic growth (see, also, Williamson, 2010; El Ghoul et al., 2012; and the references inhere). While the investigation of the impact of religiosity on individuals has begun in the early 80s (e.g., Chiswick 1983, 1985; see, Iannaccone 1998 for a comprehensive overview), the consideration of religiosity on the firm level, however, has only gained importance in recent years. Literature reveals that risk aversion (e.g., Hilary and Hui, 2009; Adhikari and Agrawal, 2016) and trust (e.g., Grullon et al., 2010; McGuire et al., 2012) are the key traits that are associated with religiosity on firm level. According to social norm theory, the prevailing set of behaviors and

[1] Also cited in Cantrell and Yust (2018).

[2] Even in times of machine learning and big date, corporate culture is perceived to be a key factor for many business decisions, and corporate success and failure (Goldstein et al., 2021). Using a semi-supervised machine-learning approach, Li et al. (2021) for example, try to quantify the strength of corporate culture by investigating earnings call transcripts.

values in an area influences religious and non-religious individuals in a similar vein (e.g., Hilary and Hui, 2009; Cantrell and Yust, 2018). Consequently, despite the impact of local religiosity on individual and firm behavior, it also affects how an organization is viewed by corporate outsiders (e.g., Callen and Fang, 2015; Jiang et al., 2018). Our study extends existing research and raises the question, what role does religiosity play in the context of stock market liquidity.

The idea of linking liquidity to factors that go beyond common stock attributes, such as price, trading volume, or return volatility (e.g., Harris, 1994; Huang and Stoll, 1996; Chung and Charoenwong, 1998), is not new in the literature. Several studies provide empirical evidence that liquidity relates to stocks' visibility (Grullon et al., 2004), familiarity (Loughran and Schultz, 2005), and ownership structure (Attig et al., 2006). Moreover, political stability and judicial efficiency (Eleswarapu and Venkataraman, 2006), internal corporate governance (Chung et al., 2010), or the education of the CEO (Pham, 2020) also plays an important role for stock market liquidity. In contrast to the above-mentioned studies, we are aiming at exploring the difference in liquidity due to religiosity as a type of soft information (Jiang et al., 2018).

The rationale behind the linkage of religiosity and stock market liquidity is summarized as follows. Liquidity providers demand compensation for both the inventory risk they bear and the adverse selection risk they face, respectively. In an incomplete market setting, where market makers cannot fully observe firms' activities and corporate information, religiosity with its antimanipulative ethos can serve as a commitment device conveying the firms' willingness to conduct business reliably, predictably, and conservatively (Callen and Fang, 2015; Jiang et al., 2018). Accordingly, liquidity providers trust in quality of information disclosure, morality, and risk aversion by the firms. Thus, we conjecture that the firms located in more religious areas will have enhanced liquidity, because market makers appreciate the antimanipulative ethos inheriting religious values and norms. Moreover, the prevailing set of behaviors caused by religiosity may be of particular interest in situations, when market makers face high information asymmetry, and little is known about the firm. Therefore, we further expect that the impact of religiosity on liquidity will be stronger in such circumstances. Due to differences in firm characteristics, such as firm size or coverage by analysts, and the associated availability of information, we also hypothesize that the impact of religiosity could be different for firms listed on NASDAQ and NYSE/AMEX. Taken together, however, if market makers do not acknowledge the value of soft information, which inherits religiosity, there is ex ante no clear expectation as to why religiosity should be associated with liquidity.

We measure religiosity as the fraction of religious adherents to the total population of a county, in which the firm is headquartered (Hilary and Hui, 2009). By using a broad sample of U.S. listed firms for the period from 1997 through 2020, we then demonstrate in our baseline analysis, which controls for firm as well as demographic characteristics, that firms located in more religious areas tend to have lower bid-ask spreads. The results are also economically significant, comparable to existing studies such as Chung et al. (2010) in terms of magnitude. One of the important points we further raise is that our results are robust to the inclusion of several further variables, such as governance, visibility, or additional demographic factors, and to a battery of different model specifications. Additionally, we show that our results are robust to alternative measures of liquidity and information asymmetry, such as Amihud illiquidity (Amihud, 2002)[3] or the probability of information-based trading (Easley et al., 2002; Brown and Hillegeist, 2007).

In our next step, we analyze the causal link between religiosity and liquidity. Of course, we acknowledge the fact that establishing causality in the setting of religiosity is a challenging and

[3] We use Amihud (2002) measure as a proxy for the price impact of trades in our empirical analysis (e.g., Goyenko et al., 2009; Edmans et al., 2013).

almost unsolvable task (Cantrell and Yust, 2018). Besides numerous demographic county-variables or other factors that may be correlated with religiosity, which we not aware of, the slow changing behavior of religious norms makes it difficult to, at least partially, rule out endogeneity since statistical techniques (especially fixed-effects regression) are not applicable in such settings (e.g., Zolotoy et al., 2019; Cantrell and Yust, 2018). However, in our causality section we conduct several tests, which helps us to confirm our baseline results and establish a potential causal link between religiosity and liquidity.

To see whether religiosity plays an emphasized role for firms facing high information asymmetry, we create subsamples based on four commonly used proxies for information asymmetry, that are analyst coverage, S&P 500 membership, firm size, and firm location. Our results indicate that the value of religiosity is more pronounced for high information asymmetry firms. Finally, further analyses reveal that the effect of religiosity is also stronger for firms listed on NASDAQ.

We extend existing research in several ways. First, we provide evidence that religiosity with its antimanipulative ethos affects the way how an organization acts and is perceived by corporate outsiders, i.e., market makers and investors, respectively. Honesty and conservativity enhance the information environment of firms, which are headquartered in more religious areas, thus increasing trust in firms' corporate actions. Accordingly, for these companies, liquidity providers post lower spreads, which, in turn, improves market liquidity.[4] Second, based on this notion, we also add evidence that liquidity providers not only consider hard information in their decision process. Thus,

[4] We note that there is one related study to ours. Blau (2018) also examines the effect of religiosity on liquidity. However, the author does not investigate local religiosity, since he focuses on a cross-country setting. Moreover, by using a different source of data and a deviating methodological approach, we reason that our study is unique.

we sharpen the understanding of the mechanisms of soft information in capital markets. Third, in a broader setting, we add evidence to the determinants of stock market liquidity.

The remainder of this paper is organized as follows. Section 2 summarizes literature and develops testable hypotheses. In section 3, we present the data and summary statistics. Section 4 consists of the model used to estimate our baseline model and discusses our empirical results. Section 5 focuses on the establishment of a cautious causal link between liquidity and religiosity, and reports results. Section 6 discusses potential channels for our findings, while we conclude in Section 7.

2 Theoretical background and hypothesis development

Culture is defined as the "transmission from one generation to the next […], of knowledge, values and other factors that influences behavior" (Boyd and Richerson, 1985, p. 2), and as "the collective programming of the mind that distinguishes the members from one category of people from those of another" (Hofstede and Bond, 1988, p. 6). Literature documents that culture affects firm behavior and decisions in various ways, which, in turn, have an impact on economic outcomes (Williamson, 2010; Jiang et al., 2018). Empirical studies on the relation between corporate culture and economic outcomes has received growing attention in recent years. For example, among many other studies, Kanagaretnam et al. (2014) document that national culture affects bank accounting conservatism and risk-taking. Guiso et al. (2016) further show that cultural differences between countries of the EU can result in a political impasse. This could affect economic actions, such as the management of the European sovereign debt crisis. Additionally, Hilary and Huang (2021) provide empirical evidence that trust can efficiently mitigate moral hazard in firms. One particular source of general commonality of a group or society is religion (Stulz and Williamson, 2003; Williamson, 2010). Religiosity as an incremental part of corporate culture creates reliability and trustworthiness in companies' corporate actions.

Starting with social norm theory, norms rule the way of social interaction between members of a group, even if they are not stated explicitly or come with any sanctions when deviating from them (Cialdini and Trost, 1998). Thus, the set of norms inheriting religiosity, particularly morality and risk aversion, affecting the behavior and values of religious and non-religious individuals in the same way (e.g., Hilary and Hui, 2009; Cantrell and Yust, 2018). Consequently, individuals in and around the firm follow the cultural norms in the county where the company is headquartered, no matter if they are actually religious or not. Therefore, we posit that acting according to local religious norms and beliefs by employees and stakeholders should be reflected in market liquidity. Thus, liquidity providers value the soft information content that underlies religiosity.

Our argumentation implicitly assumes that market makers do not only act purely on hard information (such as, financial statements or analysts' reports; Meshcheryakov and Winters, 2019). Since religiosity is a type of soft information, which can be relatively easily observed by local and non-local liquidity providers as well as liquidity providers with and without inhouse research (e.g., Madureira and Underwood, 2008; Meshcheryakov and Winters, 2019), we assume that the information-content of religiosity affects decisions of these types of liquidity providers in a similar vein.[5] For example, the consideration of soft information by financial institutions is also well documented in the credit market (e.g., Butler and Cornaggia, 2012; Jiang et al., 2018).

[5] We acknowledge the possibility that it is the stronger social connectivity (e.g., through geographic proximity) between liquidity providers and firms, which improves market liquidity. For example, liquidity providers located in areas with higher religiosity could choose firms only, which share the same set of social norms. Since we do not have data on markets makers, we cannot make direct inference on this notion.

As mentioned earlier in this section, morality and risk aversion are the fundamental values underlying religiosity. Based on this idea, Hilary and Hui (2009) show that firms located in more religious areas tend to have lower risk. In a subsequent study, Adhikari and Agrawal (2016) and Chircop et al. (2020) provide additional evidence for the negative relation between religiosity and risk-taking for a sample of public banks and venture capital investments, respectively, while Cantrell and Yust (2018) find that religiosity is positively associated with asset risk-taking for private banks. Furthermore, in terms of trust in information disclosure, Grullon et al. (2010) report that religiosity is associated with fewer incidences of inappropriate corporate behavior, such as option backdating, aggressive earnings management, or being target of class action securities lawsuits. Moreover, Dyreng et al. (2012) as well as McGuire et al. (2012) document that companies headquartered in areas with strong social religious norms experience fewer financial reporting irregularities. Additionally, Omer et al. (2018) also show that the degree of religiosity affects the audit quality positively. While these studies focus on community religiosity, Cai et al. (2019) investigate the CEOs' personal religious beliefs. The authors find that firms with religious CEOs are associated with significantly less earnings management. Callen and Fang (2015) provide evidence that religiosity helps to curb bad-news-hoarding activities by managers. Thus, the authors find that companies headquartered in more religious areas reveal a lower level of stock price crash risk. Also, stronger social religious norms are associated with lower cost of equity (El Ghoul et al., 2012), better ratings and lower cost of debt (Jiang et al., 2018; Cai and Shi, 2019), and higher workplace safety (Amin et al., 2021).

Based on the above considerations and the presented literature, we posit that liquidity providers will post lower spreads for firms headquartered in more religious areas compared to firms located in areas with low level of religiosity. Since reliability and trustworthiness inherits religiosity, liquidity providers trust in quality of information and risk aversion of the company, which reduces uncertainty. As a result, firms located in areas with higher religiosity will reveal lower bid-ask spreads. Thus, our first hypothesis is formulated as follows:

Hypothesis 1: Firms located in more religious areas have lower bid-ask spreads.

We also examine the relation between religiosity, and liquidity and information asymmetries in a more direct way by considering two additional measures: Amihud illiquidity (Amihud, 2002) as a proxy for price impact (Goyenko et al., 2009; Edmans et al., 2013), as well as the probability of information-based trading (Easley et al., 2002; Brown and Hillegeist, 2007). Chung et al. (2010) provide empirical evidence that reduced information asymmetry through improved internal corporate governance alleviate information-based trading and price impact of firms. Thus, taking further into account the sincerity and conservativity of companies located in more religious areas, we also expect a negative relation between religiosity and price impact as well as between religiosity and the probability of information-based trading:

Hypothesis 2: Firms headquartered in counties with higher levels of religiosity reveal lower price impact of trades as well as lower probability of information-based trading.

Our next hypothesis is related to the findings of Jiang et al. (2018). The authors address the question whether the function of religiosity as a commitment device differs according to the levels of information asymmetry. Considering that large companies with high visibility and dense analyst coverage are screened more intensively than smaller and less covered firms, information asymmetry probably plays an important role in the mechanism between religiosity and liquidity. Based on this notion, we expect that religiosity as commitment device is more pronounced for firms with high information asymmetry, since these firms could hide bad news or conduct inappropriate actions more easily. Our second hypothesis is:

Hypothesis 3: The relation between religiosity and liquidity is more emphasized when information asymmetry is high.

Finally, it is well known that companies list on NASDAQ, on average, are smaller and have less analyst coverage, thus probably having higher information asymmetry. Also, the market structure difference between NASDAQ and NYSE/AMEX may impact the relation between religiosity and liquidity. Therefore, we expect that the trust in information disclosure and risk aversion inheriting religiosity is stronger for firms that are listed on NASDAQ compared to NYSE/AMEX firms.

Hypothesis 4: The effect of religiosity on liquidity is more relevant for firms that list on NASDAQ.

3 Data and summary statistics

3.1 Firm level sample

Our sample construction begins with all active and dead U.S. companies traded on the NYSE, AMEX[6], or NASDAQ, which are included in Refinitiv Datastream from 1973 until 2020 (22,420 companies).[7] We then apply the data filtration process proposed by Porter and Ince (2006), and Landis and Skouras (2021). Specifically, we first exclude all non-ordinary shares and minor shares,

[6] Datastream reports the old exchange name ("NYSE MKT") for AMEX firms. Therefore, we replace the exchange listing manually from "NYSE MKT" to "AMEX" for the respective companies.

[7] Our download procedure in Datastream is summarized as follows. First, we choose as market "United States" and set the currency to "United States Dollars". We then select all "Active" and "Dead" firms from all industries of type "Equity", and consider "Major" shares with "Primary" codes only. Our base date is 2020, which means that all years equal 2020 and before are considered.

and shares with missing ISIN code from our list (10,767 stocks). After removing firms with missing SIC codes (1,326 stocks) (e.g., Ma et al., 2021), we further restrict the sample to firms, which are headquartered in the U.S., and for which we have complete information on their headquarter's location (1,049 stocks are deleted). Furthermore, the utility sector (2-digit SIC code 49) is excluded since it appears as being different from other industries, at least partly, due to regulatory issues. We also omit financials (2-digit SIC codes 60-69) because their balance sheets are different from those of other firms (e.g., Hilary, 2006; Jiang et al., 2018). Finally, we delete all firm-years if any variable of our baseline model is missing, if we observe implausible balance sheet data (e.g., negative book values of equity), or if the yearly average price is below $5 (e.g. Grullon et al., 2004; Chung et al., 2010; Cai and Shi, 2019).[8] The final sample consists of 5,365 unique firms for a total of 46,201 firm-year observations for the period from 1997 through 2020.[9]

3.2 County-level variables

We measure the strength of local religious norms, our variable of interest, retrieving data from the Association of Religion Data Archive (ARDA). We use the longitudinal version of the religious congregations and membership files, which contains the adherent and congregation counts of 302 religious groups that participated in at least one the of the 1980-2010 data collections.[10] We construct the religiosity ratio by summing the number of adherents of all religious denomination in a

[8] We note that the results of our baseline model (see equation 1) are robust when retaining firms with average stock prices above $1 instead of $5, or when omitting any price filtration. The results also hold when we keep the companies from the financial and utility sector.

[9] Note that information on institutional ownership from Refinitiv Ownership Profile (ROP) is available from 1997. Since we detect one singleton observation, our sample size reduces to 46,200 and 5,364 firms in our baseline regression analysis.

[10] Data are available at https://www.thearda.com/Archive/Files/Descriptions/RCMSMGCY.asp.

county and dividing it by the total population (*REL*). A higher level of *REL* indicates stronger religious social norms. In further analyses, we additionally consider religious subgroups, i.e., the number of protestant (*PROT*), catholic (*CATH*), mainline protestant (*MPRT*), or evangelical protestant (*EVAN*) adherents to the total population in the respective county.

Surveys are conducted at approximately ten-year intervals: 1980, 1990, 2000, and 2010. As social norms, and in particular religious adherence tend to change slowly over time, we follow previous studies (e.g., Hilary and Hui, 2009; Jiang et al., 2018; Zolotoy et al., 2019) by linearly interpolating missing values to obtain values for non-survey years, i.e., 1991-1999, and 2001-2009. Since our sample period is from 1997 through 2020, we apply the religious ratios in 2010 for the 2011-2020 period (Shu et al., 2012). An alternative method is to fill in the data for missing years using the survey value in the preceding year in which the data are available; for example, we fill in missing values from 1991 to 1999 using the religious ratios in 1990. Hasan et al. (2017a and 2017b) apply this practice for data on social capital. As discussed in the following, the second method of filling missing values do not have an impact on our results.

Following previous studies (Pirinsky and Wang, 2006; Hilary and Hui, 2009) we define a firm's location as the location of its headquarters. Refinitiv reports for the (current) location of each firm's headquarter zip (zone improvement plan) codes only, while ARDA use fips (federal information processing standard) codes to identify the location of a county. To overcome this issue, we use U.S. Census and HUD ZCTA crosswalk files to convert zip codes to fips codes.[11]

Additionally, we consider a set of county-level demographic factors as these characteristics might affect the degree of religiosity in a county (e.g., Hilary and Hui, 2009). The intention of

[11] We thank Anthony D'Agostino for providing the Stata routine. The code is available at https://gist.github.com/a8dx/7e9d5af24101fc66aafa739577713b59.

including these variables is to ensure that the effect of *REL* on liquidity is not contaminated by these factors (Hasan et al., 2017a). We control for the size of the population in a county (*TOTPOP*); the county population divided by its area size (*DENSITY*); the percentage of residents aged 25 years or older who hold a bachelor's, graduate, or professional degree (*EDUCATION*);[12] the median age of people in the county (*AGE*); the fraction of married people in a county (*MARRIAGE*); the fraction of non-white people in a county (*MINORITY*); the male population to the female population (*MF_RATIO*); and the proportion of republican votes during presidential elections (*ELEC*). We obtain these variables from the 1990, 2000, 2010, and 2020 (at time of writing only *TOTPOP* and *MINORITY* were available from 2020 survey) surveys of the U.S. Census Bureau, while we collect the latter from MIT election data lab (https://electionlab.mit.edu/data).[13] The choice of the demographic control variables closely follows Hilary and Hui (2009), Kumar et al. (2011), and Shu et al. (2012).

3.3 Dependent variable - bid-ask spread

We use bid-ask spread as our main measure of liquidity. Bid-ask spread has been widely used in prior studies, when determinants of liquidity are investigated (e.g., Grullon et al., 2004; Loughran and Schultz, 2005; Attig et al., 2006; Chung et al., 2010). The bid-ask spread posted by a market maker represents the maximum round-trip trading costs for investors and is determined by three

[12] Although data on per-capita income are available, we do not include this variable as it is highly correlated with *EDUCATION* (correlation between education proxy and the natural logarithm of income is 0.88). Kumar et al. (2011) also omit the average income due to high correlation with education.

[13] We download demographic variables using NHGIS data finder, which is available at https://data2.nhgis.org/main (Manson et al., 2021).

factors: inventory holding costs, order processing costs[14], and adverse selection costs (e.g., Attig et al., 2006; Lee and Chung, 2018). Since our arguments on the relation between religiosity and liquidity are based on informational and non-informational aspects of trading costs, the bid-ask spread is a suitable measure of liquidity in our research setting. The bid-ask spread (*BAS*) represents the yearly average of daily bid-ask spreads calculated as (Ask – Bid)/ ((Ask + Bid)/2).[15] To mitigate the potential effect of data errors and outliers, we exclude all daily *BAS* that are negative, are greater than 50% of the midpoint, or are greater than $5 (e.g., Chung et al, 2010; Chung and Zhang, 2014). *BAS* indicates illiquidity, thus larger *BAS* implies lower liquidity in the underlying stock. Data on daily bid prices and daily ask prices are retrieved from Refinitiv.

3.4 Firm level controls

Our aim is to isolate the effect of *REL* on *BAS*. Therefore, we consider a variety of control variables in our analyses that have been identified as relevant in the context of liquidity (e.g., Grullon et al., 2004; Loughran and Schultz, 2005; Hilary, 2006; Attig et al., 2006; and Chung et al., 2010). These variables are firm size, stock price, number of analysts, Tobin's Q, capital expenditures, research and development, turnover, standard deviation of daily returns, leverage, cumulative returns, profitability, membership in the S&P 500, exchange listing, institutional ownership, and insider ownership.

[14] We capture the order-processing costs by implementing a dummy variable (*NASD*). We explain this in more depth in Section 2.4.

[15] Due to limited data availability, we were not able to retrieve intraday data for calculating the *BAS*. However, as shown by literature (see, e.g., Chung and Zhang, 2014; Fong et al., 2017), daily *BAS* are highly correlated with intraday-based spreads.

Theory suggests that bigger firms reveal greater visibility and provide better information, thus reducing adverse selection risk (e.g., Brennan and Subrahmanyam, 1995; Easley et al., 1998; McInish and Van Ness, 2002; Kedia and Zhou, 2011). Therefore, we control for firm size with total assets (*SIZE*), the average share price (*PRICE*) as well as the number of analysts following a firm (*ANALYST*).[16] Accordingly, high-growth firms may have higher stock liquidity due to higher attention from both media and investors (e.g., Chung et al., 2010; Gopalan et al., 2012). Growth options and investment opportunities are proxied by Tobin's Q (*Q*), capital expenditures (*CAPEX*), and research and development (*RnD*). We further include share turnover (*TURNOVER*) as we expect that increased investors' interest and high trading activity leads to lower spreads.[17] Moreover, risk has an impact on liquidity since higher risk is related to higher spreads (e.g., Grullon et al., 2004). For this purpose, we include the yearly standard deviation of daily returns (*RISK*) and leverage (*LEVERAGE*) to proxy for risk. To control for the performance of firms, we further consider profitability (*ROA*) and cumulative stock returns (*CUMRET*) because investors are likely to be attracted by successful firms (Grullon et al., 2004).

Prior empirical work has also shown that the ownership structure, i.e., percentage of shares held by institutional and insider investors, of a company affects liquidity (e.g., Attig et al., 2006; Chung et al., 2010). The process of constructing institutional ownership is summarized as follows. At the

[16] Following previous literature (e.g., Chang et al., 2006; Guo et al., 2019) we set missing analyst firm years to zero. Thus, we implicitly assume that missing earnings forecasts is due to no analyst coverage (e.g., Chan and Hameed, 2006; Chang et al., 2006).

[17] Turnover is defined as the annual average of total monthly number of shares traded divided by shares outstanding (Grullon et al., 2004). Following Lesmond (2005), we determine the shares outstanding and the respective adjustment factor annually, i.e., we keep the value constant throughout the year. Since our winsorized raw turnover measure is highly skewed, we use its natural logarithm in the following calculations (e.g., Chung and Charoenwong, 1998; Chordia et al., 2001; Grullon et al., 2004; Jayaraman and Milbourn, 2012). To ensure comparability between NYSE/NYSE MKT and NASDAQ firms, we follow Gao and Ritter (2010) by adjusting the trading volume prior to 2004 for firms listed on NASDAQ. Please refer to Gao and Ritter (2010, p. 51-52) for a detailed explanation. Additionally, we also account for different regimes in market microstructure on NASDAQ and NYSE/AMEX by adding time-fixed effects.

end of each quarter, we identify all 13(f) institutions that are invested in the firm and calculate institutional ownership as the sum of all holdings in the firm divided by the shares outstanding in the respective quarter. From this, we receive our yearly ownership variable (*OWN_INST*) by taking the average of the quarterly data over the calendar year. As pointed out by Baghdadi et al. (2018), focusing on the average level of institutional ownership reduces an impact by periodic sharp increases or decreases at a specific point in time. Following prior literature (e.g., Gompers and Metrick, 2001; and Ferreira and Matos, 2008), we set *OWN_INST* to zero if a stock is not held by any institution, i.e., if institutional ownership is missing. In special cases, we observe, however, that our institutional ownership variable exceeds 100%.[18] In such circumstances, we set the maximum ownership proportion of institutions at 100% (e.g., done in Lewellen, 2011; Striewe et al., 2016). We furthermore include shares held by closely related investors (*OWN_INSIDER*) to proxy for insider ownership (e.g., Ferreira et al., 2008 and 2010; Chung et al., 2010).

Moreover, following Hilary (2006), we include an indicator variable (*NASD*) to control for the exchange the firm is traded on. The dummy variable *NASD* takes the value of one if the respective firm is traded on NASDAQ and zero otherwise. We consider this variable to control for systematic microstructure differences between exchanges since past research reports that firms traded on NASDAQ are associated with higher spreads (e.g., Huang and Stoll, 1996; Hilary, 2006). This dummy variable also helps us to account for institutional features of the exchanges (e.g., Gao and Ritter, 2010).[19]

[18] See Striewe et al. (2016) for a detailed discussion of possible reasons.

[19] More recent research, however, find that the difference in NYSE and NASDAQ average spreads diminished after market reforms on NASDAQ started in 1997 (e.g., Weston, 2000; Dang et al., 2018). Our baseline results are also robust when we add an additional indicator for AMEX listed firms.

Lastly, to capture the effect of being included in a leading stock index and to control for possible industrial differences, we follow Agarwal (2007) and Chung et al. (2010) by considering a dummy variable for firms included in the S&P 500 index[20] (*SP500*) and dummy variables for two-digit SIC codes. We also include time dummies to control for time trends in liquidity, e.g., resulting from changes in minimum tick size, i.e., decimalization period. All presented control variables are collected from Refinitiv, while ownership data comes from Refinitiv Ownership Profile (ROP). Table A1 in the Appendix provides detailed variable definitions.

3.5 Descriptive statistics

Table 1 provides descriptive statistics of our variables presented in Sections 3.1-3.4. Our liquidity measure (Section 3.3) and control variables (Section 3.4) are winsorized at the upper and lower 1-percentile by year to reduce the effect of outliers.[21]

[Insert Table 1 about here]

As Panel A in Table 1 shows, the mean value of *BAS* in our sample is 0.0077, indicating round trip trading costs of 77 basis points (bps) on average. The mean value and standard deviation (0.0134) are comparable to the quoted spread measures reported in existing studies (e.g., Chung et al., 2010; Gopalan et al., 2012).

[20] Each end of the year, we check the constituents list of S&P 500 companies. We then get a time series of yearly constituents, which we merge to our main dataset.

[21] Implementing alternative methods of winsorizing, i.e., over the whole sample period or by year-industry, or omitting winsorization, do not affect our conclusions.

REL reveals a mean value and standard deviation of 0.5163 and 0.1072, respectively, similar to the statistics documented in Callen and Fang (2015), and Jiang et al. (2018), among others. Unreported results show that the most religious state is Utah (*REL* equals 0.7558 with 50 companies headquartered here), while the least religious state is Maine (*REL* is 0.3189; number of companies is 7). We further observe that our companies are headquartered in 485 counties, while most of the companies are located in Santa Clara (298, California), followed by Middlesex (254, Massachusetts) and New York (220, New York). These counties reveal religiosity ratios of 0.4344, 0.6523, and 0.5325, respectively.

With regards to our controls, the average firm in our sample is covered by 7.70 analysts (untabulated) and its institutional ownership (insider ownership) is 0.6448 (0.1933).[22] The average price of firms is $42 with total assets of $4.3 B (untabulated) and a mean *Q* of 2.30, indicating the coverage of rather larger firms (Agarwal, 2007). Taken together, our firm controls and demographic variables are mostly comparable to those reported in existing studies (e.g., Grullon et al., 2004; Chung et al., 2010; Kumar et al., 2011; Callen and Fang, 2015; Jiang et al., 2018; Zolotoy et al., 2019; Albuquerque et al, 2019).

A Pearson correlation matrix for the variables used in our baseline analysis (see Table IA1a, Panel A in the Internet Appendix, IA), reveals that the correlation between *BAS* and *REL* is slightly positive (0.0207) and statistically significantly at the 1% level, which contrasts with our first hypothesis. However, we should be cautious of overinterpreting this simple pairwise correlation since it does not control for the impact of other effects (e.g., the decimalization period). Furthermore, the

[22] Note that the average analyst coverage (institutional ownership) is 8.14 (0.6644), when we omit companies with zero analysts following (zero institutional ownership). In our sample, 91.24% (99.61%) of firms are covered by at least one analyst (one institutional investor). Additionally, we document 6.97% of firm-years, in which institutional ownership exceeds 100%.

level of pooled correlation between our controls is generally moderate (< 0.50). We observe some exceptions: log(*SIZE*) shows higher correlations with log(1+*ANALYST*), and *SP500* at around 0.7076 and 0.6167, respectively. Also, *ROA* reveals a negative correlation with *RnD* of -0.6918. Moreover, for *INST_OWN* and log(1+*ANALYST*) we document a correlation of 0.5567. Further results of correlations among demographic variables (see Table IA1a, Panel B in the IA) indicate that *REL* is significantly correlated with the different demographic controls, with exception of *EDUCATION* and log(*AGE*). The correlations among demographic variables are reasonably moderate. We observe the highest correlation (in absolute terms) between log(*DENSITY*) and *MARRIAGE*, which is -0.6042.[23]

4 The relation between religiosity and liquidity

4.1 Baseline regression results

To study the relation between religiosity and liquidity, we estimate our baseline model using ordinary least squares (OLS) regression technique with standard errors adjusted for heteroscedasticity and firm clustering.[24] Specifically, we employ the following empirical model (hereafter "baseline model") to test our first hypothesis:

$$BAS_{i,t} = \beta_0 + \beta_1 REL_{i,t} + \delta' \mathbf{FIRM\_CONTROLS}_{i,t} + \gamma' \mathbf{DEMO}_{i,t} + \sum_j IND_j + \sum_t YEAR_t + \varepsilon_{i,t} \qquad (1)$$

[23] The average variance inflation factor value is 2.10.

[24] Our baseline results are robust if we adjust for year clustering, county-level clustering, or year and county double clustering.

$BAS_{i,t}$ denotes the dependent variable representing our main liquidity measure, the bid-ask spread. $REL_{i,t}$ is the main variable of interest, computed as the sum of all adherents divided by the total population in the county of the firm's headquarters location. Higher levels of $REL_{i,t}$ corresponding to stronger religious norms. Based on our first hypothesis we expect a negative relation between $REL_{i,t}$ and $BAS_{i,t}$. The vector **FIRM_CONTROLS** captures the firm attributes discussed in Section 3.4, controlling for the effect of firm characteristics: log($TURNOVER_{i,t}$), log($PRICE_{i,t}$), $NASD_{i,t}$, log($SIZE_{i,t}$), $RISK_{i,t}$, $CUMRET_{i,t}$, $CAPEX_{i,t}$, $RnD_{i,t}$, $LEVERAGE_{i,t}$, $Q_{i,t}$, $ROA_{i,t}$, log(1+$ANALYST_{i,t}$), $SP500_{i,t}$, $OWN\_INST_{i,t}$, and $OWN\_INSIDER_{i,t}$. **DEMO** is a vector containing the following county-level demographic attributes (Section 3.2): log($TOTPOP_{i,t}$), log($DENSITY_{i,t}$), $EDUCATION_{i,t}$, log($AGE_{i,t}$), $MARRIAGE_{i,t}$, $MF\_RATIO_{i,t}$, $MINORITY_{i,t}$, *and* $ELEC\_REP_{i,t}$. $\sum IND_j$ denotes industry fixed effects based on the 2-digit SIC,[25] $\sum YEAR_t$ represents year fixed effects, and $\varepsilon_{i,t}$ is the error term.[26]

The regression results are reported in Table 2. We estimate six specifications of the OLS regression. Column (1) reports the results of a reduced model, which is intended to minimize potential concerns arising from spuriously correlated independent variables (e.g. Hilary, 2006). In Column (2) we control for all firm characteristics presented in Section 3.4. In Column (3) we estimate our baseline model (see equation 1). Column (4) documents the results of a restricted sample to the two years for which we have direct survey data to mitigate any concerns of systematic noise due

[25] The results are quantitatively and qualitatively the same when we rerun our baseline model using 1-digit SIC or 4-digit SIC codes rather than 2-digit SIC codes.

[26] For the sake of brevity, we omit subscripts in the following.

to linear interpolation. Finally, in column (5) and (6), we estimate our baseline model for the survey years separately.

[Insert Table 2 about here]

Across all specifications, the estimates on *REL* are negative and statistically significant. The point estimates are -0.0028, -0.0031, -0.0033, -0.0050, -0.0036 and -0.0054 for models (1) to (6), respectively. Our results are consistent with hypothesis 1 that is, firms located in U.S. counties with higher level of religiosity reveal significant lower bid-ask spreads.[27]

Our results are also economically significant. The estimate of the coefficient in our baseline model (3) suggests that a one standard deviation increase in the fraction of religiosity (0.1072) leads to a 0.035% (=0.1072×0.0033×100) decline in bid-ask spreads, c.p. To offer a bigger picture of the economic significance, we compare our economic magnitude of *REL* to that reported in Chung et al. (2010) for governance.[28] We follow their way of quantifying the economic impact, thus moving from the first quartile (0.4369) to the third quartile (0.5945) of *REL*, the bid-ask spread decreases by 0.052% (=0.0033×0.1576×100). This is approximately 6.75% (=0.00052/0.0077×100) of the mean spread for the average firm. Similar calculations in Chung et al. (2010) reveal that a raise in governance standards would decrease its quoted spread by 4.5% of the mean quoted spread. The authors conclude an economically significant effect.[29]

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[27] The application of the alternative method of constructing *REL* and the vector of demographic variables by keeping values constant between the survey years (see Section 3.2) does not affect our conclusion. The coefficient slightly decreases to -0.0031 (t-statistics = -3.38).

[28] Similarly done in Adhikari and Agrawal (2016).

[29] Of course, this way of quantifying the economic significance is not limited to governance, but is also consistent with several studies on religiosity (e.g., Callen and Fang, 2015; and Jiang et al., 2018).

We next turn our focus to the control variables of Model (3). The results are mostly consistent with our theoretical predictions and the effects documented in previous literature (e.g., Grullon et al., 2004; Hilary, 2006; Chung et al., 2010; Kedia and Zhou, 2011; Pham, 2020, among others). As expected, log(*TURNOVER*)[30], log(*SIZE*), *Q*, *RnD*, *CAPEX*, *ROA* as well as log(1+*ANALYST*) have negative coefficients. Furthermore, spreads are lower for firms with higher institutional ownership (*OWN_INST*), indicating that corporate monitoring is better. In turn, firms with higher risk (*RISK*), higher prices (log(*PRICE*))[31], and leverage (*LEVERAGE*) tend to have higher spreads. Spreads are also higher, when the firm is listed on S&P500 (*SP500*), which may be explained by poorer governance of these firms (Chung et al., 2010). Surprisingly is the lack of significance for the indicator variable for NASDAQ listings (*NASD*). However, since we already adjust for institutional features of the trading volume (Gao and Ritter, 2010), this may explain the insignificance of our dummy variable. Additionally, as shown by Weston (2000), differences in average spreads between NYSE and NASDAQ firms decreased after market reforms on NASDAQ started in 1997. Likewise, our variable on insider trading (*OWN_INSIDER*) is statistically insignificant, which may be explained that the information content is already captured by other variables (e.g., *OWN_INST*, *ANALYST*) included in our baseline model (similarly in Chung et al., 2010). With regards to our set of demographic controls, we find that log(*TOTPOP*) is negative and statistically significant at the 10% level.

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[30] Agarwal (2007), and Jayaraman and Milbourn (2012) point out that turnover itself may also be used as a measure of liquidity. Thus, we re-estimate our baseline model excluding log(*TURNOVER*). Our results are robust (coeff. -0.0029, t-statistics = -2.63), indicating a decrease by 0.046% in bid-ask spread, when moving from the first to the third quartile of REL.

[31] We note that the findings with respect to log(*PRICE*) among our different models are mixed. For example, we observe a negative and statistically significantly sign in Model (5), while the coefficient is positive in our baseline regression. Narayan et al. (2015, p. 4497-4498) provide a detailed discussion of arguments, which motivates both, positive and negative effects of price on spreads. In addition, these findings may be also explained by pre- and post-decimalization effects (e.g., Gibson et al., 2003).

Following Jiang et al. (2018), we additionally estimate the economic significance of our controls moving from the first to the third quartile of the respective variable. The economic impact of our controls are as follows: log(*TURNOVER*) (-0.628%), log(*PRICE*) (0.071%), log(*SIZE*) (-0.623%), *RISK* (0.430%), *CUMRET* (0.029%), *CAPEX* (-0.026%), *RnD* (-0.039%), *LEVERAGE* (0.187%), *Q* (-0.097%), *ROA* (-0.019%), log(1+*ANALYST*) (-0.096%), and *OWN_INST* (-0.216%). Thus, the economic impact of *REL* (-0.052%) is in the same range to that of *RnD* (-0.039%), however, slightly larger.

4.2 Robustness tests

In our robustness section, we conduct several analyses that establish robustness of our results reported in Section 4.1. First, we consider additional control variables to reduce the risk of correlated omitted variables.[32] Second, we carry out further sensitivity analyses, which capture different specifications of our baseline model, including alternative definitions of our control variables. Third, we examine whether our main results are robust to alternative proxies for *REL*. Fourth, we use religious subgroups to understand whether the type of religiosity matters. Fifth, we test our hypothesis 1 using alternative measures of liquidity, i.e., the volatility of daily *BAS* (*VOLA_BAS*), Amihud illiquidity (*AMIHUD*), the squared version of Amihud illiquidity (*AMIHUD_SQ*), and probability of information-based trading (*PIN*).[33]

[32] We do not include these additional controls in the main analysis because they mostly come at the cost of sample size reduction or are not commonly used in empirical work.

[33] We provide descriptive statistics for all additional variables used in this section in our Internet Appendix (Section IA1, Table IA1b).

4.2.1 Additional control variables

Previous studies document that firms with better governance have lower spreads (e.g., Chung et al., 2010). Although we capture some specific governance features in our main analysis (e.g. institutional ownership, number of analysts following a firm), we now explicitly control for further aspects of corporate governance, considering Refinitiv's governance pillar score (*GOV*)[34], whether the CEO is a board member (*CEO_DUAL*), whether the firm has a CEO-Chairman separation (*BINDEP*), or the board size (*BSIZE*). In addition, we rerun our baseline model by including an indicator variable, which is one if the respective firm is audited by a Big4 company (*BIG4*). Finally, we identify the first principal component extracted from the five above mentioned governance controls (e.g. Callen and Fang, 2020). Results are presented in Rows (1) to (6) in Table 3. We find that the coefficient on *REL* remains negative and statistically significant in all specifications, at least at the 10% level, although the sample sizes is much smaller for the tests using governance variables, with exception of Model (5). Thus, our findings are robust to controlling for a battery of governance metrics.

[Insert Table 3 about here]

Second, in a similar vein, Grullon et al. (2004) detect that the degree of visibility, proxied by advertising expenses, has an impact on liquidity. As an additional sensitivity test, we rerun our baseline model including selling, general, and administrative scaled by total assets (*SG&A*) to

[34] For a detailed description of the categories and governance standards, which are included in the score, see Benz et al. (2020). Note that our results remain unchanged, when we use the overall environmental, social and governance (ESG) score of a firm instead of the isolated governance metric.

proxy for advertising (e.g., Hawn and Ioannou, 2016). In Model (7), we show that our inferences on *REL* remain unchanged when controlling for advertising intensity.[35]

Third, we use two additional balance sheet controls that are asset tangibility (*TANG*) and segment concentration of a firm (*COMPLEX*). Prior studies suggest that the asymmetric information problem is reduced for firms with more tangible assets, while stronger industry concentration may be associated with an increase in adverse selection risk (e.g., Grullon et al., 2004; Hilary, 2006; Chung et al., 2010). The coefficient on *REL* (Model 8) remains negative and statistically significant at the 1% level.

Finally, social capital as a measure of non-religious trustworthiness (*SOCIAL*), the abortion rate (*ABORT*), the per capita alcohol consumption (*ALC*), and the state-GDP (*SGDP*) could also affect the degree of religiosity in a county (e.g. Hasan et al., 2017a; Jiang et al., 2018; Amin et al., 2021).[36] This test is intended to further ensure that the effect of *REL* on liquidity is no contaminated by these factors. Results are documented in Model (9). Again, our coefficient on *REL* is not affected by including additional demographic attributes.

Even when we put all presented additional control variables together in one model along with the variables used in our baseline regression, our result holds (Model 10). Collectively, additional control variables do not impact our conclusions drawn in the baseline analysis.

[35] The unreported coefficient on our advertising coefficient is positive and statistically significant, which is in contrast with Grullon et al. (2004). This result may be explained by the different sample period and the fact that we already control for a variety of visibility measures (e.g., *SP500*, log(*SIZE*)) in our baseline analysis.

[36] We obtain data on social capital from Northeast Regional Center for Rural Development (NRCRD; https://aese.psu.edu/nercrd/community/social-capital-resources), while we receive the other measures from the National Institute on Alcohol Abuse and Alcoholism (https://pubs.niaaa.nih.gov), from Guttmacher Institute (https://osf.io/u58vf/), and from bureau of economic analysis (https://www.bea.gov/data/gdp/gdp-state), respectively.

4.2.2 Different variable and model specifications

In this section, we examine whether our results are robust to different variable and model specifications of our baseline model. Table 4 reports the results.

Previous studies (e.g., Chung et al., 2010; Lee and Chung, 2018; Pham, 2020) proxy trading activity by using the natural logarithm of dollar trading volume (*TVOL*).[37] Additionally, research has shown (e.g., Harris, 1994) that the inverse of *PRICE* (*IPRICE*) captures more accurately the variation in tick size-induced binding constraints on spreads (e.g., Harris, 1994 Chung et al., 2010; Pham, 2020). For this purpose, we replace in our baseline model log(*TURNOVER*) by log(*TVOL*), or log(*PRICE*) by *IPRICE*, respectively. Table 4, Models (1) and (2) present the results on the alternative measure for trading activity and price. We find that the coefficient on *REL* is still negative and statistically significant at the 1% level. The coefficients are -0.0026 and -0.0034, indicating a decrease in *BAS* by at least 0.0041%, which is lower than the economic effect reported in Section 4.1. Our next test addresses the concern about biases stemming from our contemporaneous model. We rerun our baseline model by using lag one of continuous firm characteristics (e.g., Adhikari and Agrawal, 2016). The effect of religiosity is robust to these alternative specification (see Model 3), while the magnitude of the coefficient is close to that reported for the baseline regression. Finally, concerns may arise due to the replacement of missing values with zero for *RnD* and log(1+*ANALYST*). In the same vein, the adjustment of our ownership variables (*OWN_INST* and *OWN_INSIDER*) may bias our results. For this purpose, we rerun our baseline model simply drop-

[37] We do not use dollar trading volume in our baseline analysis since it captures the size effect, that is bigger companies have higher trading volume (e.g., Brennan et al., 2013). Consequently, our size variable is highly correlated with the dollar trading volume (correlation = 0.8182).

ping all missing observations, and additionally, all observations above 100% for ownership variables (Model 4). Although the sample size is reduced to 21,166 observations, our overall conclusion drawn in Section 4.1 still holds.

[Insert Table 4 about here]

The next set of model specifications tackles concerns rising from geographical clustering of our sample. We start by excluding all companies, which are headquartered in the most conservative states since states such as Utah tend to be more religious and less diverse than other states (Cai and Shi, 2019).[38] Besides excluding the most conservative states, we also rerun our baseline model omitting the five most and five least religious counties. As documented in Section 3.5, most companies are headquartered in California, Texas, and New York. Thus, in our subsequent test, we exclude all companies, which are headquartered in these states to ensure that our results are not driven by these states. In a similar vein, we create a subsample, which omits the five largest counties in terms of number of observations. We also address the possibility that our results are driven by rural companies (e.g., Loughran and Schultz, 2005). Therefore, we reestimate equation (1) using the ten largest metropolitan statistical areas only. Next, to capture differences in legal and social environment, we rerun our baseline model using state-level fixed effects along with the industry and year fixed effects (e.g., Hilary and Hui, 2009; Jiang et al., 2018; Cai and Shi, 2019). Finally, we follow Hilary and Hui (2009) and Cantrell and Yust (2018) by running a cross-sectional analysis on county level. We calculate the average value of each variable included in our baseline model

[38] Cai and Shi (2019) provide a list of the most conservative states, that are, Mississippi, Idaho, Alabama, Wyoming, Utah, South Dakota, Louisiana, North Dakota, South Carolina, Arkansas.

for each county and reestimate equation (1) with standard errors adjusted for heteroscedasticity and county clustering. This test should minimize the risk that our results are driven by a small number of counties. Results of geographical subsampling are reported in Table 4, Models (5) to (11). We find that our results remain robust. Turning our focus to the state fixed effects regression in Model (10) as expected, the effect is much weaker in terms of statistical significance since the religiosity ratio is relatively stable over time. However, the coefficient is still statistically significantly at the 10% level.[39]

We now focus on temporal subsampling. Results are reported in Table 4, Models (12) to (16). We first divide the sample into two equal subperiods, i.e., from 1997-2008 and 2009-2020. The respective coefficients and significances for *REL* are reported in Models (12) and (13). Additionally, we rerun equation (1) until the last survey year in 2010 or considering (omitting) the financial crisis period from 2007 through 2009 (see Models 14 and 15). Finally, we conduct a cross-sectional regression using the Fama/MacBeth-procedure to rule out the possibility that the results are driven by cross-sectional correlation in a few years (e.g., Cantrell and Yust, 2018). The coefficient on *REL* remains negative and statistically significantly, at least, at the 5% level, thus supporting our first hypothesis (Model 16).

[39] Due to little within-firm time series variation in religiosity ratios, it is inappropriate to conduct a firm-fixed effects regression in this setting (e.g., Adhikari and Agrawal, 2016; and Zolotoy et al., 2019; for a discussion). Frijns et al. (2016) note that firm-fixed effects may lose statistical power in settings, in which the effects of variables differ mostly in the cross-section rather than over time. This is probably the reason that, with firm-fixed effects, *REL* turns insignificant. However, the negative sign persists. Moreover, as pointed out by Cantrell and Yust (2018), due to (technically induced) little yearly time series variation of *REL*, a full changes model seems also not appropriate in this setting. This could explain the insignificant coefficient on the first difference of *REL* (t-statistic = -1.59), when we estimate a full changes specification until the last survey year in 2010.

4.2.3 Alternative measures of religiosity

Following McGuire et al. (2012) and Amin et al. (2021), we also consider an alternative measure of religiosity. We regress *REL* on the demographic controls, such as population, density, education, age, marriage, minority, election, social capital, alcohol consumption per capita, and abortion rate, respectively, and use the residuals as our revised measure of religiosity (*RES_REL*). The reestimation of our baseline model produces similar results to those reported in Table 4 (see Model 17). Additionally, we replace *REL* by an indicator variable, which takes the value of one, if *REL* is greater than the yearly sample median, and zero otherwise (*HIGH_REL*). We replicate this analysis by using the sample tercile by year as an alternative threshold (*HIGH_REL1*). Results are reported in Model (18) and (19). The results reveal that alternative definitions of our religiosity variable do not alter our conclusion drawn in Section 4.1.

4.2.4 Religious subgroups

So far, we investigate whether the overall religious attitude has an impact on stock market liquidity. However, an interesting question is related as to whether the type of religiosity also matters for liquidity. For this purpose, we replace the total religiosity ratio with catholic ratio (*CATH*), and protestant ratio (*PROT*), and in an additional analysis, we further decompose *PROT* into mainline protestants (*MPRT*) and evangelical protestants (*EVAN*). Jiang et al. (2018), for example, document that both protestant and catholic rate, respectively, have a significantly impact on the cost of debt, thus indicating that both subgroups matter in the same direction. Our analysis of religious subgroups reveals similar (unreported) results, that is, both catholics and protestants negatively affect the bid-ask spread (coefficient of *CATH* = -0.0021 with t-statistics = -1.82; coefficient of *PROT* =

-0.0028 with t-statistics = -2.14). A Wald-Test on the differences between coefficients of both religious groups furthermore indicate that *CATH* is not significantly different from *PROT*. When we additionally replace *PROT* by the protestant subgroups, i.e. *MPRT* and *EVAN*, we document a significantly negative coefficient for *CATH* and for *EVAN* at the 5% level, while *MPRT* is insignificant. However, Wald-Tests show that the coefficients are not statistically significantly different between religious subgroups. Taken together, our results indicate that especially the overall strength of local religiosity impacts liquidity.

4.2.5 Alternative measures of liquidity

Besides the *BAS*, we further apply four more measures as dependent variables to capture the multiple facets of liquidity: the volatility of daily *BAS* (*VOLA_BAS*), Amihud illiquidity measure (*AMIHUD*; Amihud, 2002), the square root version of Amihud illiquidity measure (*AMIHUD_SQ*; Gopalan et al., 2012), as well as the probability of information-based trading (*PIN*, see Easley et al., 1998; Easley et al., 2002; Brown and Hillegeist, 2007).[40] Detailed descriptions of the variable definitions are provided in Table A1. We regress *VOLA_BAS*, *AMIHUD*, *AMIHUD_SQ*, and *PIN*, respectively, on *REL* using the same control variables as in our baseline model (see equation 1). Table 5 reports the results.

[Insert Table 5 about here]

[40] Since details on the estimation procedure of *PIN* are quite complex, and for brevity of this paper, the interested reader is referred to the original study by Easley et al. (1998, 2002), and Brown and Hillegeist (2007). We thank Steven Brown for sharing the estimates on the adjusted PINs. We retrieve these data from https://terpconnect.umd.edu/~stephenb/.

Consistent with hypothesis 2, the estimates on alternative measures of liquidity are negative and statistically significant, at least at the 5% level, indicating that our finding on *BAS* is robust to alternative definitions of liquidity. Interpreting *AMIHUD* and *AMIHUD_SQ* as a measure of price impact (e.g., Goyenko et al., 2009; Edmans et al., 2013), our results thus suggest that firms located in more religious areas exhibit smaller price impacts of trades. This is likely to be driven, at least partially, through the channel of lower bid-ask spreads and higher trading volume. Additionally, since the price impact mostly captures the informational component of the trading costs (Lee and Chung, 2018), liquidity providers probably trust in religiosity as a self-commitment device of firms located in more religious areas. For their governance sample, Chung et al. (2010) point out that smaller average price impact of trades could also be motivated by their smaller information-based trading. Congruent with this notion, our estimate on *PIN* is also negative and statistically significant. Because *PIN* captures the probability of trading against a superiorily informed trader, we posit that religiosity impacts the information environment of a firm positively in terms of credibility and trustworthiness (e.g., McGuire et al., 2012; Callen and Fang. 2015).[41] Consequently, since local religiosity represents the moral standards in and around the firm (Jiang et al., 2018), market makers face less risk of trading against better informed investors. Taken together, the results are consistent with the view that companies headquartered in more religious areas exhibit smaller price impacts and lower probability of information-based trading through the antimanipulative ethos inheriting religiosity.

[41] Chung and Li (2003) show that the adverse selection component of the bid-ask spread is positive and significantly related to *PIN*.

5 Potential causality

We interpret religiosity as an exogenous antimanipulative factor that serves as a commitment device conveying honesty and credibility of firms' corporate actions, thus producing an enhanced information environment, and which has an impact on how a firm is recognized by outside parties. Our results so far suggest that companies headquartered in areas with stronger religiosity exhibit lower bid-ask spreads. Although we present several robustness tests on omitted variables in Section 4.2, the relation between religiosity and liquidity may be still spurious due to further omitted correlated variables that we are not aware of. Therefore, this section aims at establishing a potential causal link between religiosity and liquidity by conducting a set of causality tests (e.g., Adhikari and Agrawal, 2016; Jiang et al., 2018; Cai and Shi, 2019; Cai et al., 2019; Zolotoy et al., 2019). The results are presented in Table 6.

5.1 Endogeneity

We start our endogeneity section by focusing on reverse causality, that is, the notion that the change in religiosity of the headquarter county is due to firm-specific bid-ask spread. Numerous studies mentioned that relocations caused by changes in bid-ask spreads are very unlikely (e.g., Callen and Fang, 2015; Cai and Shi, 2019). However, as pointed out by Cantrell and Yust (2018), firms may select headquarters that are common with their social norms. Thus, it is possible that liquidity could affect religiosity. For this purpose, we conduct two additional analyses to mitigate any concerns of biases arising from endogeneity.[42]

[42] Note that we already, at least partially, address the concern of reverse causality in our robustness section by considering lagged dependent variables (see Section 4.2.2).

First, following John et al. (2011), and Cai and Shi (2019), we re-run our baseline model using a subsample of firms, which operate in the manufacturing, mining, and agriculture sectors (SIC codes 100-3999). These firms are more likely to choose their headquarters location based on production and supply considerations rather than on religiosity. Second, following Cantrell and Yust (2018), and Callen and Fang (2020), we re-estimate our baseline model using the fitted values of *REL* within a two-stage least squares approach (2SLS). These are estimated from the first stage regression of *REL* on our instrument, that is the county-level religiosity in 1980[43], including all other variables used in our baseline model (see equation 1). Since the religiosity ratios in 1980 are 17 years before our sample period starts, it seems very unlikely that the 1980s values of county religiosity are correlated with current firm liquidity (exclusion condition). However, as religiosity ratios tend to change very slowly over time, the values in 1980 should meet the relevance condition, i.e., should be sufficiently correlated with *REL*.[44]

Model (1) and (2) in Table 6 report the results. Consistent with our previous findings, the coefficient on *REL* remains negative and statistically significant, at least at the 10%, thus alleviating concerns of potential endogeneity in our setting.

[43] We do not use the religiosity ratio in 1971, since we use the adjusted longitudinal file in our main analysis (see Section 3.2).

[44] The unreported first stage regression reveals that, as expected, our instrument is highly predictive of *REL*. The coefficient is 0.6375, significant at the 1% level, which is in line with results reported in Cantrell and Yust (2018). The first stage F-statistic is also highly significant (p-value = 0.000), indicating that the weak instrument problem is not an issue. Moreover, the Hausman test (Hausman, 1978) fail to reject the exogeneity of *REL* (similar results in Callen and Fang, 2020). In an unreported robustness test, we follow an alternative strategy proposed by Hilary and Hui (2009), and conducted in Callen and Fang (2015) and Jiang et al. (2018), among others, by taking lag 3 of *REL* and lag 3 of log(*TOTPOP*) as our internal instruments. The second stage result of the fitted coefficient on *REL* is -0.0023, being significant at the 5% level (t-statistics = -2.10). The overidentification test in this setting shows that our instruments are valid.

5.2 Placebo test

Our next attempt to address concerns of omitted variables is to conduct a placebo test (e.g., Zolotoy et al., 2019; Ma et al., 2021). This method is indented to further examine whether our main findings (see Section 4.1) are solely an artefact of omitted county variables. As described in Ma et al. (2021), we randomly shuffle religiosity ratios among each of our sample counties. For example, we replace the "true" religiosity ratios for years 1990, 2000, and 2010 of county A with the "false" ratios of county B; we then substitute the "true" ratios of county B with the "false" fractions of county C.[45] If there are omitted county-level variables driving our results, we would expect that the coefficients on pseudo-*REL* are still negative and statistically significantly, i.e., pseudo t-values would be smaller than -1.65, since the distribution of *REL* itself is not altered with this test. We repeat this procedure 500 times and plot the distribution of the pseudo t-values in Figure 1 (Ma et al., 2021).

[Insert Figure 1 about here]

Evidently, the pseudo t-values are distributed around zero, thus indicating that the coefficients of pseudo-*REL* are mostly not statistically significantly different from zero. This finding is supported by an unreported t-test on the pseudo t-values, which fails to reject the null hypothesis that the pseudo t-values are statistically significantly different from zero (t-statistic = 0.86). Furthermore, the t-value of *REL* estimated in our baseline model (see Table 2, Column 3), represented by

[45] We also apply this strategy to our demographic controls, except for *ELEC_REP*, since it is measured on state-level. However, the results still hold when we use pseudo values for *REL* only.

the vertical solid line, lies outside the distribution of the pseudo t-values. Taken together, the placebo test implies that our findings are unlikely to be driven by omitted county-level variables.

5.3 Entropy Balancing and Propensity Score Matching

To further improve identification between *REL* and *BAS*, we conduct entropy balancing (Hainmueller, 2012; and Hainmueller and Xu, 2013) like Jiang et al. (2018), Cai et al. (2019), and Mayberry (2020). We group firms into high- or low-religiosity strength by constructing an indicator variable (*HIGH_REL_DUMMY*), which is one if a firm's religiosity ratio is within the top tercile during the year (=treatment group), and zero if it is in the bottom tercile during the year (=control group). The implementation of entropy balancing is summarized as follows. First, we match our treatment and control group via the maximum-entropy reweighting scheme (Hainmueller and Xu, 2013) based on the first moment (Jiang et al., 2018; Cai et al., 2019) of all covariates used in our baseline model, that is, firm characteristics (Section 3.4) as well as county attributes (Section 3.2). Specifically, entropy balancing computes weights for every control observation such that the average equals those of the treatment observation. The reweighting scheme ensures that the first moment condition of the treatment group and the reweighted control group are virtually equal, and has the advantage that all control firms remain in the sample (e.g., Hainmueller, 2012; Hainmueller and Xu, 2013; Jiang et al., 2018; and Mayberry, 2020).[46] In the second step, we then use the weights obtained in the first step for the regression analysis with the treatment indicator (*HIGH_REL_DUMMY*) as the main explanatory variable, including all key covariates used in our

[46] See Hainmueller (2012), Hainmueller and Xu (2013), Jiang et al. (2018), and Mayberry (2020), for the main advantages of using entropy balancing over other conventional preprocessing schemes, such as propensity score matching.

baseline analysis.[47] Test diagnostics with respect to the means after the matching procedure reveal that we observe no statistically significantly difference in the means of firm characteristics as well as county characteristics after the matching procedure (see Table IA2a, Panel A in the IA). Turning our focus to the results for the weighted regression (Table 6, Model 3a), we continue to find a negative and statistically significantly effect, indicating that bid-ask spread decreases with the level of religiosity for firms that are virtually identical in other firm characteristics as well as county attributes.[48] In a subsequent test, we also verify that our results are not driven by extreme balancing weights (see, Hainmueller, 2012; Mayberry, 2020). For this purpose, we trim the extreme percentiles of our balancing weights, i.e., the 1% and 99% percentiles. As shown in Table 6 (Model 3b) our results are not affected by this adjustment.

For robustness purposes, we also conduct propensity score matching. Following Adhikari and Agrawal (2016), our first step is to estimate a logit model that regresses *HIGH_REL_DUMMY* on the set of firm characteristics described in Section 3.4, including time-fixed and industry-fixed effects.[49] We then use one firm located in a high religious area to match it to the closest firm headquartered in the low religious group without replacement and a caliper of 0.00001. Although the choice of the matching parameters comes heavily at the cost of losing observations, potential causal inferences can be only made when reasonable balancing is achieved. Our test diagnostics show that all covariates exhibit standardized biases lower than 3.1% after matching. Caliendo and Kopeinig

[47] We use Stata's option *[pweight=_webal]* in combination with *reghdfe* for the regression analysis in the second step (see, Hainmueller and Xu, 2013, p. 13, for similar procedure with respect to the weight option).

[48] Our results are robust if we match on higher moments, i.e., variance or skewness (p-values < 0.10). However, when matching on the third moment, we excluded *MF_RATIO* from the matching scheme in the first step due to near collinearity with other demographic covariates (see IA, Table IA2a, Panel B)

[49] Unlike with entropy balancing, we omit county attributes for propensity score matching in the first step to obtain reasonable balancing of the covariates (e.g., similarly done in Cai et al., 2019). Results of the first stage logit model can be found in the internet appendix (Table IA2b, Panel A).

(2005) interpret a standardized bias below 3% or 5% as sufficient. Moreover, all covariates are not statistically significantly different for our treatment and control group after conducting propensity score matching (see Table IA2b, Panel B in the IA).[50] As shown in Table 6 (Model 4), we continue to find a negative relation between religiosity and liquidity for the propensity matched sample, although our sample size is severely reduced.[51] Our results are robust to the alternative matching procedure, thus not affecting our conclusions drawn from entropy balancing.

[Insert Table 6 about here]

5.4 Company Relocation

In the context of local social norms, it is a challenging task to find an appropriate exogenous shock to provide additional evidence of potential causality (Amin et al., 2021). However, we argue that a headquarter change may act as a sufficient exogenous event that drives changes in religiosity. For this purpose, we use historic headquarter changes of a company as a quasi-exogenous shock to *REL* (e.g., Chhaochharia et al., 2012; and Hasan et al., 2017a, 2020; in alternative settings).

As noted earlier, Refinitiv reports the current location of a firm's headquarter only, not its historic headquarter. To remedy this issue, we follow recent studies (e.g., Hasan et al., 2017a and 2020; Chow et al., 2021) and extract historic headquarter addresses from Securities and Exchange

[50] We use Stata's user-written command *pstest* to perform variables balance check before and after the matching procedure. For the second-stage regression, we use all treated observations that are on common support.

[51] We additionally carried out the following matching procedures: Matching with replacement, one nearest neighbor, and caliper of 0.0001 (e.g., Adhikari and Agrawal, 2016); matching without replacement, three nearest neighbors (e.g., Cai et al., 2019); and one-to-one matching with replacement, and caliper of 0.01 (e.g., Mayberry, 2020). As done in Mayberry (2020), in these auxiliary analyses, we employ frequency weights to accommodate that one control firm can match with multiple treatment firms. Our Internet Appendix (Table IA2b, Panel C) reports the results.

Commission (SEC) filings. We use the headquarters relocations data of Loughran-McDonald augmented 10-X header data.[52] The dataset captures all information in the header section of 10-K/Qs (and all variants) filed on EDGAR. In total, there are 1,285,447 filings for 42,368 firms with a unique Central Index Key (CIK) for the period from 1994 to 2018. We follow the procedure described in Garcia and Norli (2012), and Heider and Ljungqvist (2015) to further extract the filings and to obtain a time series of headquarter locations for every company in our sample. We limit our dataset to a period starting in 1997 (first year of available observations on institutional ownership) and ending in 2018 (last year of HQ addresses from Loughran-McDonald data).[53]

Our next step is to construct a sample of headquarter changes, which is summarized as follows. We define a headquarter relocation event when a firm reveals a change in the corporate headquarters' fips code located across two different states in two consecutive years (Chhaochharia et al., 2012). A company is required to have at least two observations before and after the relocation occurs (Hasan et al., 2017a).[54] Because the full sample covers the period from 1997-2018 and we

[52] We thank Loughran for sharing their headquarters relocation data, https://sraf.nd.edu/data/augmented-10-x-header-data/.

[53] Another important issue of data preparation relates to the mapping of company identifiers between Refinitiv and SEC. Refinitiv provides a crosswalk for merging ISIN codes (our company identifier) with their respective CIK code (SEC company identifier). Unfortunately, the matching procedure leads to a significant loss of firms compared to our original sample (see Section 3.1). This may have at least two reasons. First, CIK number is generally not available in Refinitiv. Second, we are aware of the fact that not all companies that offer stocks must file electronically prior to 2008. For example, certain small companies are excluded from regular SEC reporting prior to 2008, thus having no CIK number. In total, we cannot match 968 firms to their respective ISIN code. This is also the main reason, why we use the SEC dataset for the HQ changes test only. Nonetheless, we estimated our baseline model (see equation 1) with this reduced-firm dataset for the period 1997-2018. The results are robust (t-statistics = -2.16), while the economic significance is lower than reported in Section 4.1 (the *BAS* decreases by 0.0038% when moving from the first to the third quintile of *REL*). The partially loss of economic significance could be explained by the exclusion of smaller firms. For example, Jiang et al. (2018) congruently find that the effect of *REL* on the cost of private and public debt is much stronger for smaller firms, i.e., if its asset value is below the sample median.

[54] Besides the fact that we closely follow existent literature (Hasan et al., 2017a), this choice is motivated as follows: First, a one-year threshold inherits the risk of considering companies that directly went inactive (e.g., due to mergers and acquisitions) in the year after the relocation occurred. Thus, the relocation would not represent a "true" picture of changes in social norms. Second, requiring more than two years before and after the relocation comes heavily at the cost of observations. Thus, two years seems appropriate in our setting.

require a two-year window around the relocation event, relocations are detected in the 1999-2016 period. Moreover, we omit firms with multiple relocations to avoid any confounding events (Hasan et al., 2017a). With this strategy, we identify 178 relocation events in total, while the most take place in 2003 (13 relocations), and the least in 1999 and 2001 (5 relocations).

In the next step, we measure the "strength" of changes in religiosity due to the HQ relocation as the difference between the *REL* in the post- and pre-relocation period. Specifically, it thus represents the difference in *REL* one year after the relocation and one year before the relocation. Subsequently, we create two dummy variables to pin headquarter changes involving a "measurable" change in religiosity (in a similar vein done in Adhikari and Agrawal, 2016): *ADH_INCR*, which equals one if the HQ change comes along with a change in religiosity that lies within the top quintile, zero otherwise; and *ADH_DECR*, which equals one if the HQ change is accompanied by a change in religiosity that is in the bottom quintile, zero otherwise. We choose the top/bottom quintiles for two reasons. First, we do not expect that a change in *REL* that is close to zero has a "visible" effect on liquidity. Second, we do not choose more extreme percentiles to ensure a sufficient variation in our indicator variables. Finally, following Cai et al. (2019), we regress the changes of *BAS* on the indicator variables *ADH_INCR* and/or *ADH_DECR*, including the changes[55] and the levels of all our control variables (see Sections 3.2 and 3.4) in the regression models. Since a firm appears only once in our sample, we replace within firm clustering by standard errors clustered at the county level.[56] Table 6 presents the results from the multivariate regressions based on HQ changes.

[55] The change variables are defined in a similar way to the changes in *REL* over the same time.

[56] Note that we observe nine singleton observations, which are dropped in the regression analysis. This results in 167 observations for our regression analyses. However, the inclusion of the singleton observations does not alter our conclusions. Likewise, results are robust if we cluster standard errors by year, county and year, or by year and industry, or when we exclude the vector of demographic variables (as done in Cai et al., 2019).

In Model 5a, we investigate HQ changes that comes along with a "measurable" increase of *REL*. Based on our hypothesis, we would expect that such HQ changes are related to a decrease in *BAS*. Indeed, we find a negative and statistically significantly coefficient. In Model 5b, where we consider "measurable" decreases of *REL* only, we observe no association with *BAS*. When we consider both types together (Model 5c), we obtain similar results as with the solely considerations. Additionally, the null hypothesis that these coefficients are equal is rejected at the 10% level (p-value = 0.06; not reported in Table 6).

Although, we find a negative and statistically significantly coefficient for "measurable" adherence increasing HQ changes, these findings do not necessarily demonstrate causality, thus our results may be still spurious. However, this analysis serves as another important component to further support our initial finding, that is, changes in religiosity caused by headquarter relocations contribute to changes in liquidity.

6 Information asymmetry and exchange listing

In this section, we run several tests to examine the channel through which religiosity is related to liquidity. Again, it is of course a challenge to provide definitive proof of the underlying mechanism(s) through which religiosity enhances liquidity. Thus, our tests are only suggestive and should be interpreted cautiously.

6.1 Information asymmetry and religiosity

As hypothesized in the introduction, the importance of religiosity as an indicator of trustworthiness and enhanced information environment is even more pronounced for liquidity, when information asymmetry is high, and market makers have limited information about the firm. To test this hypothesis, we use four proxies for firm level information asymmetry, and create two subsamples having high/low information problems (e.g., Callen and Fang, 2015; Jiang et al., 2018). The investigation of the results of each subsample separately ensures a more nuanced interpretation of the coefficients (Callen and Fang, 2015).

Our first proxy for information asymmetry is analyst coverage (e.g., Duarte et al., 2008; Jiang et al., 2018). Previous studies document that analysts play an effective role of external monitoring, thus reducing potential information problems because information is more widely distributed (e.g., Duarte et al., 2008; Bradley et al., 2021). A high information asymmetry firm is classified as one with no analyst coverage. The second proxy relates to the visibility of a firm. S&P 500 companies are more likely to be visible to market participants and media, thus having lower information asymmetry (e.g., Jiang et al., 2018). Based on this notion, non-S&P500 firms face higher information asymmetry. A closely related measure for visibility refers to company size. Literature suggests that bigger companies reveal improved corporate visibility (e.g., Dang et al., 2018; Jiang et al., 2018). A firm is categorized as having high information asymmetry if its total assets are below the sample median by year (Jiang et al., 2018). The last proxy is based on firm location (e.g., Kedia and Zhou, 2011; El Ghoul et al., 2013). Loughran and Schultz (2005) find evidence that firms located in urban areas have a larger investor base since they are local stocks for many people. This, in turn, reduces information asymmetry. Therefore, if a firm is located more than 100 miles (approximately 161 km) away from the nearest city center of the six financial centers (Boston, Chicago, Los Angeles, New York, Philadelphia, and San Francisco), we categorize this "far away" firm as one facing high information asymmetry (e.g., Loughran and Schultz, 2005; El Ghoul et al., 2013). The main advantage of our first two measures of information asymmetry is that they are externally determined, which avoids having to set a threshold for defining high/low information groups, while the latter ones are created directly from our sample and consider additional aspects of possible sources of information asymmetry, i.e., company size and geographic proximity.[57]

[Insert Table 7about here]

Table 7 shows that for each subsample of firms within the high information asymmetry cluster (Models (1), (3), (5), and (7), respectively), the coefficient on *REL* is always negative and statistically significantly at the 1% level. Turning our focus to the low information problems firms (Models (2), (4), (6), and (8), respectively), coefficients are insignificant, except for the analyst coverage sample. In all models, the magnitude of the coefficients for each pair of subsamples is statistically significantly different for the high information asymmetry group compared to the low information asymmetry group (see row "Differences in coefficients"), at least at the 10% level. The difference is especially pronounced for the analyst and S&P500 subsamples.

As conducted in Callen and Fang (2015), we also interact *REL* with an indicator variable (*ADUM*) that takes the value of one for companies that are not covered by any analysts, zero otherwise. Untabulated results show that the coefficient on the interaction term *REL*ADUM* is negative and statistically significantly at the 5% level (t-statistics = -2.32), supporting our findings that the effect is more pronounced for firms with no analyst following the firm. The comparison of the

[57] In our Internet Appendix (Table IA3, Panel A), we present additional analyses on subsamples that are based on institutional monitoring and governance metrics. Since monitoring and governance, respectively, may be also related to information asymmetry, we conduct these analyses to further deepen our understanding of the effect of religiosity in the context of information asymmetry. Although the differences between high and low groups are less pronounced for the monitoring/governance samples, collectively, the additional results support the conclusions drawn in this section. Moreover, to further investigate the relation between holdings of different types of investors and liquidity, we additionally decompose institutional ownership to different investor groups (see Table IA3, Panel B in the IA).

stand-alone coefficient of *REL* with the interaction term *REL*ADUM* further shows that the impact is remarkably higher for firms with no analyst coverage (coefficient of *REL*ADUM* = -0.0107; coefficient of *REL* = -0.0022). In a similar vein, we conduct this analysis for our other externally determined dummy (*NONSP500*). We receive similar results with respect to statistical significance, while the magnitude of the stand-alone coefficient on the interaction term (*REL*NONSP500*) is only slightly higher than the stand-alone coefficient of *REL* (coefficient of *REL*NONSP500* = -0.0034, and coefficient of *REL* = -0.0029).

Overall, these findings support our third hypothesis that the effect of religiosity on liquidity is stronger for firms with high information asymmetry. Our results are consistent with previous literature (e.g., Jiang et al., 2018), which detect that the strength of the impact of religiosity is dependent on the information environment of the firm.

6.2 NASDAQ vs. non-NASDAQ firms

Another interesting question is whether the exchange on which the firm is listed matters for the relationship between religiosity and liquidity. Although we already capture microstructure differences between NASDAQ and NYSE/AMEX firms in our baseline model, we now estimate regressions for NASDAQ and NYSE/AMEX firms separately to further sharpen our understanding of the connection between religiosity and liquidity.

Interestingly and consistent with hypothesis 4, our results for the separated regressions reveal that religiosity is an important determinant for NASDAQ firms but not for stocks that list on

NYSE/AMEX (not tabulated).[58] The insignificant result for NYSE/AMEX firms may be explained as follows. The first argument refers to the way of how these exchanges operate. NYSE/AMEX firms have one specialist, while NASDAQ stocks have varying numbers of market makers (e.g., Loughran and Schultz, 2005). Thus, since market makers are more familiar (or local) to the stocks they support on NYSE/AMEX (Loughran and Schultz, 2005), it is more likely that religiosity with its antimanipulative ethos is less relevant for these firms. Second, NYSE/AMEX firms may reveal higher visibility, because these firms are often larger and/or have more analyst coverage relative to firms traded on NASDAQ (e.g., Loughran and Schultz, 2005; Chung et al., 2010; Dang et al., 2018). Information for these firms is more widely distributed among investors, which could dampen the effect of religiosity. Overall, these results are consistent with the view that religiosity as a substitute mechanism for reliable information disclosure is more pronounced for NASDAQ firms, since they may reveal higher information asymmetry (see Section 6.1).

Another interesting point in this context relates to the findings of Chung et al. (2010), who show that NYSE/AMEX firms may have better internal corporate governance structures than firms listed on NASDAQ.[59] This raises the question if religiosity may be a relevant factor for NYSE/AMEX firms, when market makers can observe that internal governance is weak, and the risk of fraudulent activities is increased for these companies (e.g., Callen and Fang, 2015)? To further test this notion, we reuse Refinitiv's governance pillar score (see Section 4.2.1), splitting the sample in high-quality and low-quality governance by the yearly sample terciles for NYSE/AMEX firms. Our untabulated results show a significantly negatively coefficient of *REL* for the weak governance group (within

[58] The coefficient of *REL* is -0.0034 (t-statistics = -2.46) for NASDAQ firms, while the coefficient of *REL* is -0.0020 (t-statistics = -1.45) for firms listed on NYSE/AMEX.

[59] The authors show in summary statistics that the average governance score is significantly higher for NYSE/AMEX firms than for firms traded on NASDAQ. This is also confirmed by our data when using Refinitiv's governance pillar score.

the bottom tercile of the governance score), while the coefficient of *REL* for the good governance cluster (within the top tercile of the governance score) is insignificant. The difference in coefficients is significant at the 5% level, indicating that religiosity also plays an important role for NYSE/AMEX firms, when internal corporate governance is weak.

7 Conclusion

This study investigates the relation between religiosity and market liquidity for a broad sample of U.S. listed firms. Collectively, we find strong support that firms located in more religious areas tend to have lower bid-ask spreads. This negative relation remains statistically significantly, even if we control for a battery of other factors that may influence both liquidity and/or religiosity, respectively, or for different model specifications. Also, firms headquartered in U.S. counties with a high level of religiosity reveal lower price impact and probability of information-based trading. Finally, we show that the impact of religiosity is especially emphasized when little information about the firm is conveyed, and market makers face high information asymmetry, specifically for NASDAQ firms.

The present study contributes to and elaborates on existing literature on the effects of religiosity on the trustworthiness and reliability of firms that are headquartered in more religious areas. Consistent with previous literature (e.g., Hilary and Hui, 2009), religiosity as an important ethical factor not only matters for inside corporate behavior and culture, but particularly plays a crucial role of how a company is viewed by third parties, such as market makers. We acknowledge that there may be more “soft” factors besides religiosity that affect corporations, which are worth recognizing and tackling in further studies (Callen and Fang, 2015). Taken together, our results should be of interest for investors and liquidity providers.

Table A1 : Variable definition

This table provides a summary of the variables used in our study.

Variable		Definition
Panel A: Main dependent variable		
Bid-Ask Spread	*BAS*	Adjusted ask price (Refinitiv Eikon item TR.ASKPRICE) minus adjusted bid price (Refinitiv Eikon item TR.BIDPRICE) divided by the spread midpoint, which is the sum of the adjusted ask price (Refinitiv Eikon item TR.ASKPRICE) and the adjusted bid price (Refinitiv Eikon item TR.BIDPRICE) divided by two. We then take the average of daily figures to receive our yearly variable.
Panel B: Variable of interest		
Religiosity	*REL*	The number of religious adherents in a county divided by the county population in a year (ARDA). Religiosity is linear interpolated between the survey years (https://www.thearda.com/Archive/Files/Descriptions/RCMSMGCY.asp).
Panel C: Firm controls		
Analysts	log(1+*ANALYST*)	Number of analysts forecasting earnings per share for the following year (Datastream EPS1NE). We define the variable as the arithmetic mean number of monthly earnings forecasts during each calendar year. Firm-year in which a firm is not covered by any analysts, we set these values to zero (e.g., Chang et al., 2006; Bradley et al., 2021). Finally, we take the natural log of one plus the annual value (e.g., similarly done in Chan and Hameed, 2006).

Capital Expenditures	*CAPEX*	Capital Expenditures (Worldscope item 04601) divided by total assets (Worldscope item 02999).
Closely Held Shares	*OWN_INSIDER*	Number of shares held by insiders as a proportion of the number of shares outstanding (Worldscope item 08021). We set closely held shares to zero, if we observe a missing firm-year in our dataset. This variable has been used to proxy for insider holdings by researchers such as Ferreira et al. (2008), and Ferreira et al. (2010).
Cumulative return	*CUMRET*	Calculated as the cumulative firm-specific daily returns (calculated from Datastream item RI) during a calendar year.
Institutional ownership	*OWN_INST*	Institutional Ownership by all institutions (13F filings) (Refinitiv Ownership Profile item TR.FilingType) as a percentage of shares outstanding (Refinitiv Ownership Profile item TR.PctOfSharesOutHeld). All values above 100% are set to 100% (e.g., Lewellen, 2011). Also, we set institutional ownership to zero if a stock is not held by any institution (see Gompers and Metrick, 2001).
NASDAQ listing	*NASD*	Indicator variable that is one, if the firm is listed on NASDAQ, zero otherwise.
Price	log(*PRICE*)	It is the natural logarithm of the mean daily stock price (Datastream item P) during a calendar year (e.g., McInish and Van Ness, 2002).
Research and Development	*RnD*	Research and development expenditures (Worldscope item 01201) divided by total assets (Worldscope item 02999). Following prior studies, we set missing values to zero

		(e.g. Chung et al., 2010; Lewis and Tan, 2016).
Return on assets	*ROA*	Ratio of operating income (Worldscope item WC01551) divided by total assets (Worldscope item 02999).
S&P membership	*SP500*	Indicator variable, which equals one if a firm is a member of the S&P 500, zero otherwise. This dummy is generated from yearly constituents lists of the S&P 500 (e.g., Datastream item LS&PCOMP1220 for the constituents list as of December 2020). Empirically, each end of the year, we check the constituents list of S&P 500 companies. We then get a time series of yearly constituents, which we merge to our main dataset.
Tobin's Q	*Q*	Total assets (Worldscope item 02999) plus market value of equity (Worldscope item 08001) minus book value of equity (Worldscope item 03501) divided by total assets (Worldscope item 02999).
Total Assets	log(*SIZE)*	Natural logarithm of annual total assets in thousands of dollars (Worldscope item 02999).
Total Risk	*RISK*	The standard deviation of daily stock returns (calculated based on Datastream item RI) during a calendar year.
Turnover	log(*TURNOVER)*	Monthly share volume (Datastream item VO) divided by adjusted shares outstanding (Datastream items NOSH/AF; Ferreira and Matos, 2008). The shares outstanding are determined at the beginning of each year and kept constant for each day of the year (similarly done in Lesmond, 2005). We then take

		the mean from monthly turnover during a calendar year. At the end, we take the natural log of the average turnover.
Panel D: Demographic controls		
Age	log(*AGE*)	The natural logarithm of the median age of the population in a county (U.S. Census Bureau). Age is determined by linear interpolation between the survey years.
Education	*EDUCATION*	Education is defined as the fraction of county's population that is 25 years or older and hold a bachelor's degree or higher (U.S. Census Bureau). Education is determined by linear interpolation between the survey years.
Male-female ratio	*MF_RATIO*	The ratio of male population to female population in a county (U.S. Census Bureau). Missing values between survey years are obtained by linear interpolation.
Marriage	*MARRIAGE*	The percentage of married households in a county (U.S. Census Bureau). Marriage is determined by linear interpolation between the survey years.
Minority	*MINORITY*	The percentage of non-white population in a county (U.S. Census Bureau). Missing values between survey years are obtained by linear interpolation.
Population Density	log(*DENSITY)*	The natural logarithm of the total population to the land area in the respective county. Density is determined by linear interpolation between the survey years (U.S. Census Bureau).
Presidential election by republicans	*ELEC_REP*	The proportion of votes received by the republican candidate (https://electionlab.mit.edu/data).

Total population	log(*TOTPOP*)	The natural logarithm of the total population in a county (U.S. Census Bureau). Total population is determined by linear interpolation between the survey years.
Panel E: Variables used in auxiliary analyses		
Abortion Rate	*ABORT*	Represents the abortion rate for women aged 15 to 44 (https://osf.io/u58vf/; accessed via https://www.guttmacher.org/public-use-datasets).
Advertising Expenses	*ADV*	Advertising expenses are defined as selling, general & administrative expenses (Worldscope item WC01101) divided by total assets (Worldscope item WC02999); Hawn and Ioannou (2016).
Alcohol Consumption	*ALC*	Is defined as the per capita alcohol consumption rate (https://pubs.niaaa.nih.gov).
Amihud Illiquidity	*AMIHUD*	The absolute daily return calculated from adjusted prices (Datastream item P) scaled ty the daily dollar volume (Datastream items P*VO) and multiplied by 1,000,000. The average of daily figures is calculated during a calendar year.
Amihud Illiquidity squared	*AMIHUD_SQ*	The square root version of our daily *AMIHUD* measure (Gopalan et al., 2012). We then take the average of our daily measure to receive our yearly variable.
Asset Tangibility	*TANG*	Asset Tangibility is defined as property, plant, and equipment (Worldscope item WC02501) divided by total assets (Worldscope item WC02999)
Audit Company	*BIG4*	Represents an indicator variable that is one, if a firm is audited by one of the big four audit

		companies, zero otherwise (Worldscope item WC07800).
Board Size	*BSIZE*	Represents the number of board members at the end of the fiscal year (Refinitiv Datastream item CGBSDP060).
Board Independence	*BINDEP*	Indicator variable, which equals one if the company has a policy regarding the independence of its board, zero otherwise (Refinitiv Datastream item CGBSDP0012).
Catholic adherence	*CATH*	The number of catholic adherents in a county divided by the county population in a year (ARDA). Catholic adherence is linear interpolated between the survey years.
CEO Duality	*CEO_DUAL*	Indicator variable, which equals one if the CEO simultaneously chair the board or the chairman has been the CEO of the company, zero otherwise (Refinitiv Datastream item CGBSO09V).
Complexity	*COMPLEX*	The Herfindahl-Hirschman Index based on production segments (calculated from Worldscope items WC19501-WC19591).
Dollar Trading Volume	log(*TVOL)*	The natural logarithm of daily price times (Datastream item P) times daily trading volume (Datastream item VO).
Evangelical protestants	*EVAN*	The number of evangelical protestants including black protestant divided by the county population in a year (ARDA). *EVAN* is linear interpolated between the survey years.
Governance Pillar Score	*GOV*	Weighted average relative rating of a company based on the reported governance information and the resulting three governance category scores (Refinitiv Datastream item CGSCORE).

Inverse of price	*IPRICE*	It is the inverse of the mean daily stock price (Datastream item P) during a calendar year.
Probability of Information Based Trading	*PIN*	Robust version of EKO PIN (Stephen Brown's website https://terpconnect.umd.edu/~stephenb/).
Protestant adherence	*PROT*	The number of protestant adherents in a county divided by the county population in a year (ARDA). Protestant adherence is linear interpolated between the survey years.
Mainline protestants	*MPRT*	The number of mainline protestants in a county divided by the county population in a year (ARDA). Mainline protestants is linear interpolated between the survey years.
Social Capital	*SOCIAL*	The first principal component from principal component analysis based on PVOTE, RESPN, NCCS, and ASSN (NRCRD at Pennsylvania State University; available at https://aese.psu.edu/nercrd/community/social-capital-resources). Hasan et al. (2017a), among others, provide a comprehensive description of constructing the *SOCIAL* variable. PVOTE = Percentage of voters who voted in presidential elections RESPN = Response rate to the Census Bureau's decennial census NCCS = Sum of tax-exempt nonprofit organizations divided by populations per 10,000 ASSN = Sum of social organizations divided by populations per 100,000
State-GDP	*STATE_GDP*	Is defined as the natural logarithm of the gross domestic product by state (Bureau of Economic Analysis).

Volatility of Bid-Ask Spread	*VOLA_BAS*	The standard deviation of daily *BAS* during a calender year (Refinitiv Eikon item).
Panel F: Supplemental data		
Exchange Listing		Current exchange, on which the company is listed.
FIPS	*fips*	FIPS stands for "federal information processing standard". The 5-digit fips code is used by ARDA and US Census Bureau to determine the location of a county, while the first two digits represent the state, where the county is located. ZIP and FIPS are used to merge company data with data on religiosity and demography.
Industry dummies	*COMPANY_IND*	Based on two-digit standard industry classification (SIC) codes (Worldscope item 07021). We use the first SIC code, which is assigned to a company, i.e., this represent the business segment which provided most revenue.
ISIN	*ISIN*	ISIN stands for "international securities identification number". This is the main identifier of the companies in our sample.
ZIP code	*ZIP*	ZIP stands for "zone improvement plan". It is used to determine the location of a company's headquarter (Worldscope item 06025).

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Figure 1: Distribution of pseudo t-values of REL

This figure plots the t-values on pseudo-*REL* obtained from 500 regressions of our baseline model (see equation 1). In each regression, we replace *REL* and all demographic controls (with exception of *ELEC_REP*) by randomly assigned values of another county. The solid line represents the t-value of *REL* estimated in our baseline model (see Table 2, Column 3). The solid curve overlays the distribution represents the normal density curve.

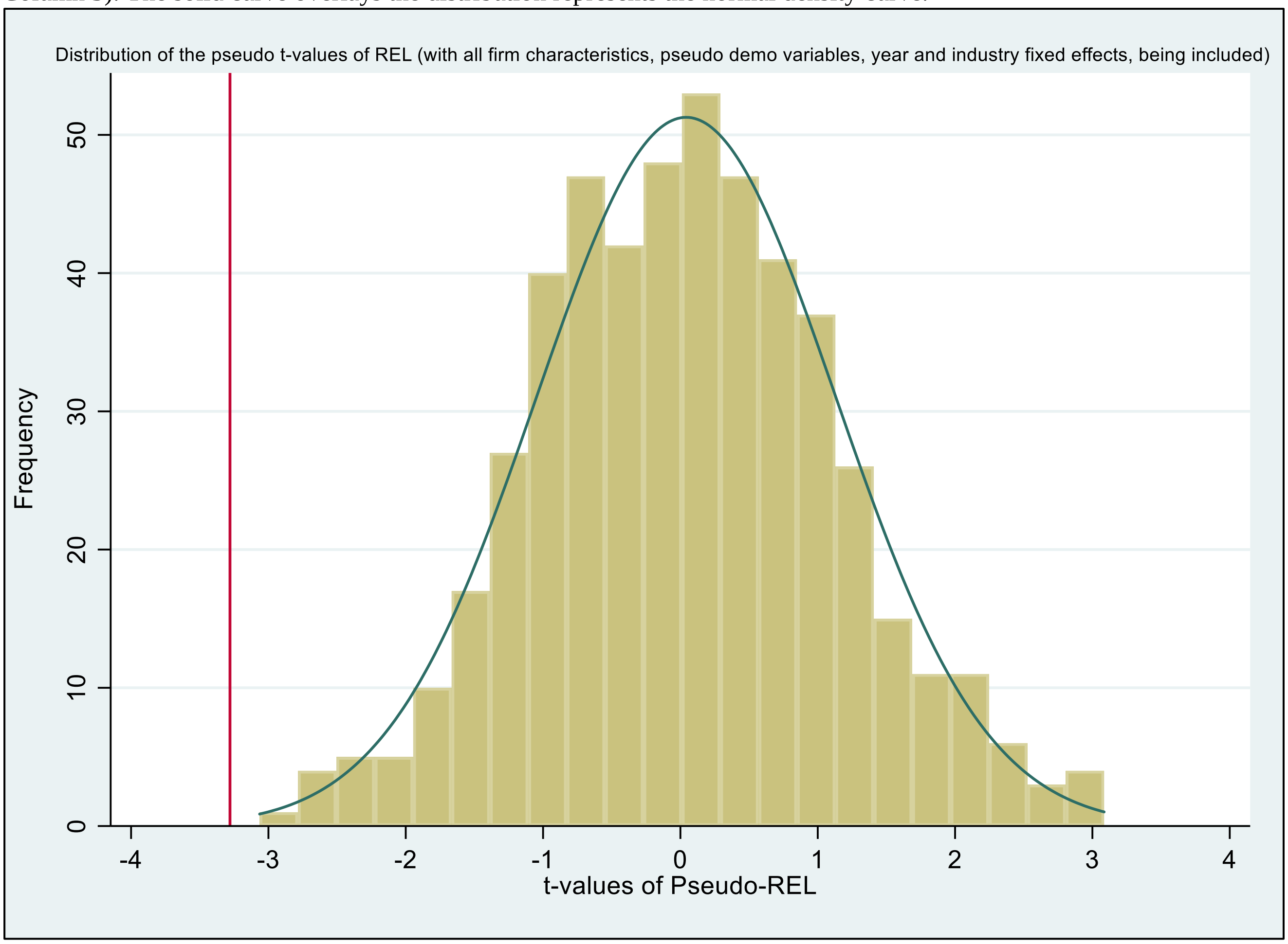

Table 1: Summary statistics

This table reports descriptive statistics metrics of key variables used in our baseline analysis. The sample covers firm-year observations with nonmissing values for all variables from 1997 to 2020. All continuous variables are winsorized at the 1%/99% level. Appendix A1 provides a detailed description of the variables. The data are from Refinitiv Eikon, Refinitiv Datastream, ROP database, U.S. Census Bureau, and MIT Election Lab.

				Percentiles				
	N	Mean	Std.Dev.	5th. Pctl.	25th. Pctl.	Median	75th. Pctl.	95th. Pctl.
Panel A: Bid-ask spread								
BAS	46,201	0.0077	0.0134	0.0003	0.0009	0.0023	0.0083	0.0336
Panel B: Variable of interest								
REL	46,201	0.5163	0.1072	0.3464	0.4369	0.5177	0.5945	0.7014
Panel C: Firm characteristics								
log(TURNOVER)	46,201	-2.0960	0.9780	-3.8780	-2.6335	-1.9918	-1.4590	-0.6821
log(PRICE)	46,201	3.0901	0.9235	1.8047	2.4165	2.9959	3.6155	4.6859
NASD	46,201	0.5577	0.4967	0.0000	0.0000	1.0000	1.0000	1.0000
log(SIZE)	46,201	6.5347	1.9032	3.5233	5.1947	6.45555	7.7877	9.8495
RISK	46,201	0.0316	0.0172	0.0132	0.0199	0.0273	0.0385	0.0638
CUMRET	46,201	0.1726	0.6739	-0.5964	-0.1894	0.0797	0.3775	1.1927
CAPEX	46,201	0.0504	0.0554	0.0042	0.0166	0.0329	0.0621	0.1628
RnD	46,201	0.0551	0.1131	0.0000	0.0000	0.0058	0.0664	0.2472
LEVERAGE	46,201	0.1974	0.1859	0.0000	0.0098	0.1692	0.3207	0.5496
Q	46,201	2.3029	1.9540	0.8834	1.2316	1.6898	2.6076	5.8232
ROA	46,201	0.0353	0.2198	-0.3460	0.0204	0.0773	0.1274	0.2318
log(1+ANALYST)	46,201	1.7976	0.9179	0.0000	1.2040	1.8845	2.4918	3.1781
SP500	46,201	0.1607	0.3672	0.0000	0.0000	0.0000	0.0000	1.0000
OWN_INST	46,201	0.6448	0.2803	0.1057	0.4423	0.7110	0.8799	1.0000
OWN_INSIDER	46,201	0.1933	0.2084	0.0010	0.0209	0.1258	0.2959	0.6401
Panel D: County-level attributes								
log(TOTPOP)	46,201	13.7593	1.0485	11.8495	13.2569	13.7602	14.3530	15.4631

Table 1 continued

log(DENSITY)	46,201	6.3466	1.2837	4.3130	5.6488	6.4127	6.8317	8.5320
EDUCATION	46,201	0.3601	0.1025	0.2074	0.2819	0.3454	0.4400	0.5496
log(AGE)	46,201	3.5774	0.0809	3.4530	3.5232	3.5774	3.6322	3.7040
MF_RATIO	46,201	0.9609	0.0354	0.8990	0.9375	0.9626	0.9869	1.0177
MARRIAGE	46,201	0.4144	0.0468	0.3244	0.3889	0.4213	0.4435	0.4838
MINORITY	46,201	0.3147	0.1430	0.0857	0.2000	0.3095	0.4335	0.5304
ELEC_REP	46,201	0.4423	0.0893	0.3132	0.3712	0.4436	0.4999	0.5930

Table 2: Baseline regressions

This table documents results of our baseline regressions. Model (1) and (2) includes controls for firm attributes. Model (1) is a reduced model, while Model (2) considers all firm controls. Model (3) is the baseline model presented in equation (1). It consists of firm characteristics, demographic controls as well as industry and year fixed effects. Finally, Model (4) reports the results for the survey years, i.e. 2000 and 2010, only, while Model (5) and Model (6) considers both survey years separately. Across all models, the dependent variable is BAS, which is calculated as (Ask – Bid)/((Ask + Bid)/2). Appendix A1 defines all other variables in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering. *t*-statistics are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable: BAS					
Variables	(1) Reduced Model	(2) All Firm Controls	(3) Baseline Model	(4) Survey Years	(5) Yr 2000	(6) Yr 2010
Variable of interest						
REL	-0.0028***	-0.0031***	-0.0033***	-0.0050***	-0.0036*	-0.0054**
	(-2.59)	(-3.32)	(-3.28)	(-3.23)	(-1.75)	(-2.37)
Firm characteristics						
log(TURNOVER)	-0.0049***	-0.0053***	-0.0053***	-0.0056***	-0.0067***	-0.0048***
	(-23.18)	(-22.74)	(-22.73)	(-12.82)	(-11.46)	(-7.06)
log(PRICE)	0.0001	0.0006***	0.0006***	-0.0004*	-0.0014***	0.0009***
	(0.36)	(4.08)	(4.01)	(-1.89)	(-4.66)	(2.86)
NASD	0.0008**	0.0004	0.0004	-0.0006	-0.0011*	-0.0008
	(2.47)	(1.58)	(1.60)	(-1.29)	(-1.81)	(-1.21)
log(SIZE)	-0.0029***	-0.0024***	-0.0024***	-0.0028***	-0.0031***	-0.0020***
	(-25.67)	(-18.74)	(-18.69)	(-13.51)	(-11.94)	(-5.69)
RISK		0.2289***	0.2289***	0.1921***	0.2503***	0.2498***
		(18.77)	(18.74)	(7.56)	(7.01)	(3.83)
CUMRET		0.0005***	0.0005***	0.0004	0.0001	0.0002
		(5.61)	(5.57)	(1.30)	(0.31)	(0.24)
CAPEX		-0.0058***	-0.0057***	-0.0038	-0.0028	-0.0093
		(-3.58)	(-3.58)	(-1.13)	(-0.76)	(-1.46)
RnD		-0.0059***	-0.0060***	-0.0031	-0.0106***	-0.0044
		(-3.99)	(-4.01)	(-1.19)	(-3.37)	(-1.06)
LEVERAGE		0.0060***	0.0060***	0.0096***	0.0102***	0.0051**
		(10.08)	(10.06)	(7.63)	(7.18)	(2.37)
Q		-0.0007***	-0.0007***	-0.0010***	-0.0008***	-0.0008***
		(-13.90)	(-13.91)	(-11.08)	(-8.11)	(-3.62)
ROA		-0.0018**	-0.0018**	-0.0004	0.0007	-0.0014
		(-2.38)	(-2.40)	(-0.37)	(0.61)	(-0.66)
log(1+ANALYST)		-0.0008***	-0.0008***	-0.0016***	-0.0024***	-0.0008
		(-4.59)	(-4.64)	(-4.82)	(-5.77)	(-1.47)
SP500		0.0053***	0.0052***	0.0069***	0.0067***	0.0055***
		(17.10)	(16.91)	(12.42)	(10.06)	(6.86)
OWN_INST		-0.0050***	-0.0050***	-0.0056***	-0.0023*	-0.0092***
		(-9.44)	(-9.49)	(-5.85)	(-1.87)	(-6.16)
OWN_INSIDER		-0.0001	-0.0000	-0.0014	-0.0010	-0.0026
		(-0.11)	(-0.08)	(-1.49)	(-0.92)	(-1.65)
Demographic controls						
log(TOTPOP)			-0.0003*	-0.0001	-0.0001	0.0000
			(-1.82)	(-0.27)	(-0.56)	(0.04)
log(DENSITY)			0.0003	0.0002	0.0003	-0.0000
			(1.45)	(0.63)	(0.99)	(-0.02)
EDUCATION			-0.0001	0.0018	0.0022	0.0034
			(-0.07)	(0.82)	(0.80)	(0.93)
log(AGE)			-0.0026	-0.0082***	-0.0075**	-0.0028
			(-1.25)	(-2.58)	(-1.98)	(-0.60)
MF_RATIO			0.0033	-0.0087	-0.0147*	0.0081

Table 2 continued

			(0.74)	(-1.37)	(-1.80)	(0.69)
MARRIAGE			0.0019	0.0098	0.0055	0.0046
			(0.41)	(1.47)	(0.74)	(0.41)
MINORITY			-0.0007	-0.0006	-0.0024	0.0015
			(-0.63)	(-0.33)	(-1.00)	(0.71)
ELEC_REP			-0.0019	-0.0054**	0.0004	-0.0026
			(-1.13)	(-2.09)	(0.11)	(-0.71)
INTERCEPT	0.0173***	0.0096***	0.0183*	0.0536***	0.0537***	0.0123
	(16.82)	(7.08)	(1.88)	(3.80)	(3.02)	(0.55)
Year FE	YES	YES	YES	YES	NO	NO
Industry FE	YES	YES	YES	YES	YES	YES
Observations	46,200	46,200	46,200	3,776	1,940	1,830
R-squared	0.4432	0.5397	0.5403	0.5950	0.6907	0.4506

Table 3: Additional control variables

This table documents results of considering additional control variables for different dimensions (Model 1 to Model 9). All models include the variables used in our baseline analysis. In Model 10, we put all presented additional control variables, i.e., the first principal component of multiple governance variables, advertising expenses, Herfindahl index, capital intensity, social capital, abortion, alcoholism, and state-GDP, together in one model along with the variables used in our baseline analysis. All models are estimated with year and industry fixed effects. Across all models, the dependent variable is *BAS*, which is calculated as (Ask – Bid)/((Ask + Bid)/2). Appendix A1 defines all other variables used in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	REL			
Dimension	Coefficient	t-statistics	*N*	Adj. R^2
Corporate governance dimension				
Model 1: Refinitiv Governance Pillar Score	-0.0021**	-2.14	13,766	0.1478
Model 2: CEO duality	-0.0021**	-2.12	15,289	0.1962
Model 3: Board Size	-0.0017*	-1.74	15,184	0.1765
Model 4: Board Independence	-0.0019*	-1.90	15,204	0.1884
Model 5: Big4	-0.0033***	-3.30	46,200	0.5409
Model 6: First principal component of multiple governance variables	-0.0019**	-2.00	13,740	0.1318
Balance sheet dimension				
Model 7: Advertising expenses	-0.0035***	-3.37	45,357	0.5409
Model 8: Herfindahl Index & Capital Intensity	-0.0032***	-3.13	45,586	0.5448
Demographic dimension				
Model 9: Social Capital Index, Abortion, Alcoholism, state-GDP	-0.0033***	-3.19	46,200	0.5406
Model 10: All variables together	-0.0019**	-2.08	13,310	0.1150

Table 4: Alternative definitions and model specifications

This table documents results of alternative definitions of variables and model specifications for different dimensions. All models include the variables used in our baseline analysis, and are estimated with year and industry fixed effects, with exception of Model (10) and (11). Across all models, the dependent variable is *BAS*, which is calculated as (Ask – Bid)/((Ask + Bid)/2). Appendix A1 defines all other variables used in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering, with exception of Model (11). *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	REL			
Dimension	Coefficient	t-statistics	*N*	Adj. R^2
Dimension: control variables				
Model 1: Dollar trading volume (*TVOL*)	-0.0026***	-2.66	46,200	0.5529
Model 2: Inverse of price (*IPRICE*)	-0.0034***	-3.29	46,200	0.5391
Model 3: Lagged firm characteristics	-0.0030***	-2.83	39,339	0.4875
Model 4: Exclude all replaced values	-0.0032***	-3.02	21,166	0.4925
Geographic dimension				
Model 5: Excluding most conservative counties	-0.0030***	-2.82	44,802	0.5397
Model 6: Excluding counties with 5 highest and lowest RELs	-0.0032***	-3.04	46,032	0.5398
Model 7: Omitting CA, TX, and NY	-0.0040***	-3.56	33,546	0.5527
Model 8: Excluding five largest counties (in terms of number of obs.)	-0.0035***	-2.99	37,300	0.5487
Model 9: Only urban companies	-0.0046**	-2.48	17,668	0.5175
Model 10: State-fixed effects	-0.0032*	-1.82	46,200	0.5424
Model 11: County level estimation	-0.0056*	-1.89	485	0.7029
Temporal dimension				
Model 12: period 1997-2008	-0.0035***	-2.90	23,820	0.5881
Model 13: period 2009-2020	-0.0026**	-2.06	22,379	0.4389
Model 14: Financial crisis period (2007-2009)	-0.0066***	-2.66	5,776	0.4408
Model 15: Excluding financial crisis period	-0.0029***	-3.11	40,423	0.5652
Model 16: Fama/MacBeth-procedure	-0.0030***	-4.84	46,201	0.5425
Dimension: variable of interest				
Model 17: *RES_REL*	-0.0033***	-3.18	46,200	0.5402
Model 18: *HIGH_REL*	-0.0005***	-2.68	46,200	0.5401
Model 19: *HIGH_REL1*	-0.0007***	-2.74	30,612	0.5399

Table 5: Alternative liquidity measures

This table documents results of alternative liquidity measures. All models include the variables used in our baseline analysis, and are estimated with year and industry fixed effects. Appendix A1 defines all other variables used in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	(1)	(2)	(3)	(4)
Variables	*VOLA_BAS*	*AMIHUD*	*AMI-HUD_SQ*	*PIN*
Variable of Interest				
REL	-0.0026**	-0.3245**	-0.0631**	-0.0207***
	(-2.39)	(-2.15)	(-2.37)	(-3.00)
Baseline controls included	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	46,195	46,200	46,200	24,834
Adj. R-squared	0.3400	0.1830	0.5744	0.7042

Table 6: Estimates from reverse causality and endogeneity tests

This table documents results from tests of reverse causality and endogeneity. Across models (1) to (4), the dependent variable is *BAS*, which is calculated as (Ask – Bid)/((Ask + Bid)/2). In Models (5a) to (5c) we use the change of *BAS* as the dependent variable, where the change is measured from *t*-1 to *t*+1. All models are estimated with year and industry fixed effects. Appendix A1 defines all variables used in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering, with exception of Models (5a) to (5c). *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Variables	Model (1) SIC 100-3999	Model (2) 2SLS	Model (3a) Entropy Balancing	Model (3b) Entropy Balancing	Model (4) Propensity Score Matching	Model (5a) HQ relocation	Model (5b) HQ relocation	Model (5c) HQ relocation
Variable of Interest								
REL	-0.0030** (-2.29)	-0.0028* (-1.85)						
HIGH_REL_DUMMY			-0.0007*** (-2.73)	-0.0009*** (-3.26)	-0.0009*** (-2.92)			
ADH_INCR						-0.0073** (-2.59)		-0.0078** (-2.54)
ADH_DECR							-0.0003 (-0.13)	-0.0022 (-0.78)
Baseline controls included	YES	YES	YES	YES	YES	YES	YES	YES
Changes in baseline controls included	NO	NO	NO	NO	NO	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	26,822	46,179	30,612	29,998	9,671	169	169	169
R-squared	0.5422	0.4700	0.5620	0.5560	0.5746	0.695	0.667	0.697

Table 7: High and low information asymmetry firms

This table documents results from the effects of *REL* on subsamples, which are constructed based on measures of information asymmetry. Across all models, the dependent variable is *BAS*, which is calculated as (Ask – Bid)/((Ask + Bid)/2). The left panels, i.e., models (1), (3), (5), and (7), respectively, represent the subsamples of firms, which face high information asymmetry. All models are estimated with year and industry fixed effects. Appendix A1 defines all variables used in detail. Standard errors are adjusted for heteroscedasticity and within-firm clustering. In the "Differences in coefficients" column, we test the null hypothesis of the equality between the coefficients of *REL* across the subsamples. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Variables	ANALYST		S&P500		SIZE		LOCATION	
	(1) NOT COVERED	(2) COVERED	(3) NON-S&P500	(4) S&P500	(5) BELOW MEDIAN	(6) ABOVE MEDIAN	(7) FAR AWAY	(8) CLOSE
Variable of Interest								
REL	-0.0127***	-0.0023***	-0.0036***	0.0000	-0.0040***	-0.0013	-0.0042***	0.0006
	(-3.02)	(-2.75)	(-3.19)	(0.52)	(-2.90)	(-1.21)	(-2.92)	(0.28)
Baseline controls included	YES	YES	YES	YES	YES	YES	YES	YES
Differences in coefficients (p-value)	0.01		0.00		0.09		0.07	
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	4,044	42,156	38,773	7,424	23,106	23,093	29,448	16,749
R-squared	0.574	0.492	0.534	0.656	0.593	0.300	0.540	0.554

Internet Appendix to:

Local religiosity and stock market liquidity

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Abstract

This Internet Appendix (IA) contains additional results that supplements our main paper. It consists of three sections: The first section (Section IA1) provides a Pearson correlation matrix for the variables used in our baseline model as well as descriptive statistics of variables used in auxiliary analyses (Section 4.2 in the main paper). The second part (IA2) contains test diagnostics and further results from Entropy Balancing (EB) and Propensity Score Matching (PSM), respectively. Lastly, the third section (IA3) provides additional analyses of the effect of different subsamples based on monitoring/governance metrics and investor types.

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Section IA1: Pearson correlation matrix and descriptive statistics

Table IA1a: Pearson correlation matrix of variables used in our baseline analysis

Panel A presents a Pearson correlation matrix of the firm attributes, while Panel B documents correlations among demographic variables, used in our baseline analysis. Appendix A1 (Panel A to Panel D) in the main paper provides a detailed description of variables. * indicates p-values > 0.10.

Panel A: Correlations among firm attributes used in our baseline analysis

	BAS	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
REL (2)	0.0207															
log(TURNOVER) (3)	-0.4926	-0.0970														
log(PRICE) (4)	-0.2512	-0.0198	0.2226													
NASD (5)	0.2121	-0.0916	-0.0364	-0.1679												
log(SIZE) (6)	-0.5248	-0.0015*	0.2850	0.3772	-0.4450											
RISK (7)	0.3507	-0.0456	0.1783	-0.1790	0.2969	-0.4376										
CUMRET (8)	-0.0222	-0.0106	0.0495	-0.0243	0.0239	-0.0197	0.0297									
CAPEX (9)	0.0292	0.0121	-0.0299	0.0124	-0.0480	0.0200	0.0022*	-0.0397								
RnD (10)	0.0853	-0.0951	0.1235	-0.0078	0.2932	-0.3310	0.3223	-0.0327	-0.1511							
LEVERAGE (11)	-0.0822	0.0103	0.0367	0.0764	-0.2690	0.3853	-0.1262	-0.0344	0.0901	-0.2448						
Q (12)	-0.0701	-0.0476	0.1232	0.1781	0.1616	-0.1616	0.1058	0.4032	-0.0252	0.2885	-0.2122					
ROA (13)	-0.1944	0.0584	-0.0801	0.0281	-0.2208	0.3507	-0.4339	0.0919	0.1196	-0.6918	0.0954	-0.1097				
log(1+ANALYST) (14)	-0.5256	-0.0563	0.4973	0.3243	-0.2191	0.7076	-0.2455	-0.0410	0.0669	-0.0480	0.1608	0.1031	0.1817			
SP500 (15)	-0.2156	0.0202	0.1105	0.2765	-0.2576	0.6167	-0.2526	-0.0198	-0.0083	-0.0961	0.1145	0.04171	0.1691	0.5119		
OWN_INST (16)	-0.5668	-0.0382	0.4988	0.2739	-0.1993	0.5348	-0.3518	-0.0289	-0.0624	-0.1555	0.1482	-0.0435	0.2508	0.5567	0.1757	
OWN_INSIDER	0.3263	0.0224	-0.3896	-0.2186	0.0864	-0.3001	0.1595	-0.0117	0.0782	-0.0392	-0.0412	-0.0351	-0.0223	-0.3496	-0.2406	-0.4596

Table IA1a continued

Panel B: Correlations among demographic variables used in our baseline analysis

	REL	log(TOTPOP)	log(DENSITY)	EDUCATION	log(AGE)	MF_RATIO	MARRIAGE	MINORITY
log(TOTPOP)	0.0358							
log(DENSITY)	0.1755	0.5370						
EDUCATION	-0.0047*	0.1075	0.4351					
log(AGE)	-0.0005*	-0.2612	-0.0917	0.1460				
MF_RATIO	-0.3048	0.0472	-0.4150	-0.0665	-0.3579			
MARRIAGE	-0.0105	-0.3593	-0.6042	-0.0185	0.3544	0.3113		
MINORITY	-0.1321	0.5632	0.5438	0.1539	-0.3439	0.0045*	-0.5808	
ELEC_REP	-0.0229	-0.1734	-0.3389	-0.2990	-0.2837	0.1897	0.0928	-0.0970

Table IA1b: Summary statistics of variables used in auxiliary analyses

This table reports descriptive statistics metrics of variables used in auxiliary analyses of Section 4.2 in the main paper. The sample covers firm-year observations with nonmissing values for all variables from 1997 to 2020. All continuous variables are winsorized at the 1%/99% level. Appendix A1 (Panel E) in the main paper provides a detailed description of the variables. The data are from Refinitiv Eikon, Refinitiv Datastream, U.S. Census Bureau, and Northeast Regional Center for Rural Development.

				Percentiles				
	N	Mean	Std.Dev.	5th. Pctl.	25th. Pctl.	Median	75th. Pctl.	95th. Pctl.
GOV	13,766	46.1326	22.2752	11.2000	28.0200	45.7600	63.9800	82.2200
CEO_DUAL	15,289	0.6414	0.4796	0.0000	0.0000	1.0000	1.0000	1.0000
BINDEP	15,204	0.8950	0.3065	0.0000	1.0000	1.0000	1.0000	1.0000
BSIZE	15,184	9.4394	2.2265	6.0000	8.0000	9.0000	11.0000	13.0000
BIG4	46,201	0.7786	0.4152	0.0000	1.0000	1.0000	1.0000	1.0000
ADV	45,358	0.3046	0.2782	0.0336	0.1229	0.2369	0.4022	0.7854
TANG	46,071	0.2419	0.2223	0.0187	0.0747	0.1674	0.3406	0.7492
COMPLEX	45,701	0.7114	0.2860	0.2558	0.4869	0.7411	1.0000	1.0000
SOCIAL	46,201	-0.5170	0.7624	-1.7536	-1.0882	-0.5017	-0.0449	0.5783
ABORT	46,201	18.9297	7.7692	8.7000	13.6000	17.3000	24.0000	34.8400
ALC	46,201	2.2954	0.3137	1.8900	2.1400	2.2600	2.3900	2.7700
SGDP	46,201	13.2121	0.9209	11.7153	12.5850	13.1522	13.9722	14.6636
log(TVOL)	46,201	8.4726	2.3599	4.2915	6.8508	8.6321	10.1996	12.1564
IPRICE	46,201	0.0634	0.0472	0.0092	0.0269	0.0500	0.0892	0.1645
RES_REL	46,201	0.0000	0.0935	-0.1453	-0.0654	0.0040	0.0637	0.1579
HIGH_REL	46,201	0.4943	0.5000	0.0000	0.0000	0.0000	1.0000	1.0000
HIGH_REL1	30,612	0.4940	0.5000	0.0000	0.0000	0.0000	1.0000	1.0000
CATH	46,201	0.2500	0.1311	0.0652	0.1533	0.2285	0.3507	0.4887
PROT	46,201	0.2036	0.1232	0.0747	0.1068	0.1644	0.2926	0.4578
MPRT	46,201	0.0736	0.0485	0.0225	0.0398	0.0638	0.0922	0.1728
EVAN	46,201	0.1300	0.0986	0.0275	0.0574	0.0989	0.1705	0.3215
VOLA_BAS	46,196	0.0060	0.0129	0.0001	0.0005	0.0016	0.0063	0.0245
AMIHUD	46,201	0.2277	1.3075	0.0001	0.0007	0.0044	0.0388	0.9963
AMIHUD_SQ	46,201	0.1644	0.2938	0.0081	0.0241	0.0590	0.1656	0.7098
PIN	24,836	0.1681	0.0970	0.0604	0.1023	0.1415	0.2099	0.3636

Section IA2: Test diagnostics and further results

Table IA2a: Test diagnostics and further results from EB

Panel A reports the difference in characteristics before and after the matching procedure for the treatment and control group. Panel B presents the regression results from matching on higher moment conditions, i.e. mean, variance, and skewness. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Difference in characteristics before and after entropy balancing (EB)

Variables	Main Sample			EB Sample (after matching)		
	High REL	Low REL	Diff. (1-2)	High REL	Low REL	Diff. (1-2)
log(TURNOVER)	-2.159	-1.967	-0.192***	-2.159	-2.159	0.000
log(PRICE)	3.089	3.073	0.016	3.089	3.089	-0.000
NASD	0.509	0.659	-0.150***	0.509	0.509	0.000
log(SIZE)	6.566	6.406	0.160***	6.566	6.566	-0.000
RISK	0.031	0.034	-0.003***	0.031	0.031	0.000
CUMRET	0.160	0.188	-0.028***	0.160	0.160	-0.000
CAPEX	0.050	0.048	0.002***	0.050	0.050	0.000
RnD	0.047	0.077	-0.030***	0.047	0.047	0.000
LEVERAGE	0.201	0.184	0.0167***	0.201	0.201	-0.000
Q	2.200	2.541	-0.341***	2.200	2.200	-0.000
ROA	0.042	0.011	0.031***	0.042	0.042	-0.000
log(1+ANALYST)	1.758	1.851	-0.093***	1.758	1.759	-0.000
SP500	0.169	0.151	0.018***	0.169	0.169	-0.000
OWN_INST	0.645	0.641	0.004	0.645	0.645	-0.000
OWN_INSIDER	0.198	0.194	0.004*	0.198	0.198	-0.000
log(TOTPOP)	13.837	13.787	0.050***	13.837	13.836	0.001
log(DENSITY)	6.616	6.081	0.535***	6.616	6.615	0.001
EDUCATION	0.366	0.368	-0.002*	0.366	0.366	-0.000
log(AGE)	3.582	3.578	0.004***	3.582	3.582	0.000
MF_RATIO	0.949	0.976	-0.027***	0.949	0.949	0.000
MARRIAGE	0.412	0.417	-0.005***	0.412	0.412	-0.000
MINORITY	0.296	0.338	-0.042***	0.296	0.296	0.000
ELEC_REP	0.436	0.430	0.006***	0.436	0.436	0.000
Number of treated units	15,122					
Number of control units	15,490					
Number of observations	30,612					

Panel B: Results from matching procedure on higher moments

Variables	EB on mean and variance	EB on mean, variance, and skewness (excluding MF_RATIO) from the matching scheme
Variable of Interest		
HIGH_REL	-0.0014*	-0.0014*
	(-1.91)	(-1.68)
Baseline controls included	YES	YES
Year FE	YES	YES
Industry FE	YES	YES
Observations	30,612	29,998
R-squared	0.5620	0.5560

Table IA2b: First stage logit model, test diagnostics, and further results from PSM

Panel A documents results from the first stage logit model, which is used to predict the probability of a firm being located in the top tercile by county-level religiosity. In Panel B, we report results of the bias and the differences in characteristics before and after the matching procedure for the treatment and control group. We use Stata's *pstest* after matching (Stata command *psmatch2*) to receive test diagnostics for PSM sample. The matching procedure is based on matching with no replacement, one nearest neighbor, and a caliper of 0.00001. Panel C reports results from alternative matching parameters, which are based on previous studies. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: First stage logit model

Variables	HIGH_REL
log(TURNOVER)	-0.153***
	(-3.431)
log(PRICE)	0.00164
	(0.0356)
NASD	-0.474***
	(-4.466)
log(SIZE)	-0.0466
	(-1.069)
RISK	-3.144
	(-1.512)
CUMRET	-0.00346
	(-0.140)
CAPEX	-1.585**
	(-2.284)
RnD	-1.514***
	(-3.775)
LEVERAGE	0.0387
	(0.188)
Q	-0.0355**
	(-2.034)
ROA	-0.148
	(-0.835)
log(1+ANALYST)	-0.0987
	(-1.543)
SP500	0.199
	(1.392)
OWN_INST	0.316
	(1.600)
OWN_INSIDER	0.0132
	(0.0792)
INTERCEPT	-1.667
	(-1.594)
Observations	30,541
Year FE	YES
Industry FE	YES
Area under ROC Curve	0.680

Panel B: Test diagnostics of PSM sample

Variables	%bias			Mean difference of PSM Sample		
	%bias (un-matched)	%bias (matched)	%reduc-tion of |bias|	High REL	Low REL	Diff. (1-2)
log(TURNOVER)	-19.9	0.1	99.6	-2.080	-2.081	0.001
log(PRICE)	1.8	-1.6	9.4	3.083	3.098	-0.015
NASD	-30.9	3.1	90.0	0.614	0.599	0.015
log(SIZE)	8.4	2.1	75.4	6.467	6.427	0.040
RISK	-17.5	0.2	99.0	0.032	0.032	0.000
CUMRET	-3.9	0.7	82.5	0.157	0.152	0.005
CAPEX	3.0	1.7	42.7	0.044	0.043	0.001
RnD	-26.1	-1.5	94.1	0.057	0.059	-0.002
LEVERAGE	9.0	1.6	82.4	0.192	0.189	0.003
Q	-16.8	-1.3	92.4	2.307	2.333	-0.026
ROA	13.6	2.2	83.6	0.034	0.029	0.005
log(1+ANALYST)	-10.1	-0.1	99.4	1.800	1.800	0.000
SP500	4.8	-0.3	93.8	0.158	0.159	-0.001
OWN_INST	1.4	-1.8	-29.2	0.645	0.650	-0.005
OWN_INSIDER	2.1	-0.8	63.7	0.190	0.192	-0.002

Panel C: Alternative matching parameters

Variables	Adhikari and Agrawal (2016)	Cai et al. (2019)	Mayberry (2020)
HIGH_REL_DUMMY	-0.0008***	-0.0008***	-0.0008***
	(-2.63)	(-2.90)	(-2.62)
Baseline controls included	YES	YES	YES
Caliper	0.0001	NA	0.01
Replacement	YES	YES	YES
Neighbors	1	3	1
Highest %bias	3.6	6.8	6.3
Observations	14,922	25,215	15,154
R-squared	0.551	0.551	0.554

Section IA3: Investigation of governance subsamples and different investor types

Following the strategy proposed in our main paper, we create two subsamples having good/weak monitoring and governance, respectively. We proxy institutional monitoring and governance quality by using the number of blockholders (*BLOCK*), the percentage held by the largest shareholder (*LAR*) as well as the governance pillar score by Refinitiv (*GOV*).[66] We split the sample by the yearly sample terciles of the respective variable. The "lower tercile" groups indicate weak monitoring/governance (see Table IA3, Panel A).

Since different types of investors may also be related to monitoring/governance behavior, and thus having a different impact on liquidity, we additionally analyze institution types as reported by Refinitiv Ownership Profile (ROP). Particularly, we replace institutional ownership by specific types of owners. In Table IA3, Panel B, we tabulate the coefficients and respective t-statistics for the specific average ownership type. Our results indicate that investor holdings of different groups are broadly negatively associated with *BAS*, with exception of research firms and sovereign wealth funds.

[66] Following Fich et al. (2015), we define number of blockholders as the number of institutions whose ownership in the target firm is at least 5% of the target's shares outstanding, while LAR represents the ownership controlled by the largest institutional investor in the target. Appendix A1 in the main paper defines GOV.

Table IA3: Monitoring/governance subsamples and investor types

Panel A documents results from the effects of *REL* on subsamples, which are constructed based on measures of governance and monitoring. Across all models, the dependent variable is *BAS*, which is calculated as (Ask – Bid)/((Ask + Bid)/2). The "lower tercile" panels, i.e., models (1), (3), and (5), respectively, represent the subsamples of firms, which reveal weak monitoring/governance. In the "Differences in coefficients" column, we test the null hypothesis of the equality between the coefficients of *REL* across the subsamples. Panel B analyzes the effect of different investor types. All models are estimated with year and industry fixed effects. Standard errors are adjusted for heteroscedasticity and within-firm clustering. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Good and weak monitoring/governance firms

Variables	BLOCK		LAR		GOV	
	(1) LOWER TERCILE	(2) UPPER TERCILE	(3) LOWER TERCILE	(4) UPPER TERCILE	(5) LOWER TERCILE	(6) UPPER TERCILE
REL	-0.0051***	-0.0029***	-0.0052***	-0.0039***	-0.0061**	-0.0005
	(-2.60)	(-2.89)	(-2.72)	(-3.11)	(-2.21)	(-1.40)
Baseline controls included	YES	YES	YES	YES	YES	YES
Differences in coefficients (p-value)	0.32		0.56		0.04	
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	15,874	15,029	15,366	15,346	4,595	4,582
R-squared	0.566	0.524	0.575	0.498	0.176	0.148

Panel B: Decomposition of institutional ownership

Investor types	Coefficients	t-statistics
Bank and Trust	-0.0074	-1.39
Endowment Fund	-0.0936***	-3.41
Hedge Fund	-0.0017	-1.30
Investment Advisor	-0.0039***	-4.75
Insurance Company	-0.0309***	-4.31
Private Equity	-0.0099***	-5.62
Pension Fund	-0.0200***	-4.28
Investment Advisor/Hedge Fund	-0.0053***	-5.07
Research Firm	0.0315***	4.80
Sovereign Wealth Fund	0.1866***	11.28
Venture Capital	-0.0149***	-8.55
Independent Research Firm	-1.5360	-1.61
REL	-0.0032***	-3.19
Baseline controls included	YES	
Year FE	YES	
Industry FE	YES	

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