CONSCIOUS CAPITAL

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Abstract

My overarching research ambitions have been to understand the good and bad sides of finance and the market forces that enable or inhibit activities associated with them. While the three chapters in this thesis might seem different from one another, they reflect my continuous development and fundamentally showcase my overarching interests for the creation of a better welfare-optimal world. The best way to characterize this dissertation is to use the acronym commonly referred to in public discourse amongst academics and practitioners, namely, ESG. ESG stands for Environmental, Social and Governance factors that investors, businesses and households are using as a framework for understanding the challenges and opportunities that come with our continuously evolving capital market regime. Households begun using environmental factors to associate themselves with banks that are aligned with their societal preferences. Impact funds are incorporating social factors in their investment strategies to identify the most promising projects for the maximization of social returns. Asset managers are using governance factors to determine, which companies are operationally ready and adaptable to manage themselves towards a long-term sustainable future. Overall, Chapter 1. reflects the Environmental dimension, Chapter 2. reflects the Governance dimension and Chapter 3. reflects the Social dimension and together, they reflect my ESG aspirations.

Chapter 1. asks whether depositors react to non-financial information about their banks. By using branch level data for the United States, I show that banks, that financed the highly controversial Dakota Access Pipeline, experienced significant decreases in deposit growth, especially in branches located closest to the pipeline. These effects were greater for branches located in environmentally or socially conscious counties, and data suggests that savings banks were among the main beneficiaries of this depositor movement. Using a global hand-collected dataset on tax evasion, corruption, and environmental scandals related to banks, I show that negative deposit growth as a reaction to scandals is a widespread phenomenon. Overall, this is the first thorough documentation of the non-financial preferences of household financial investment decisions. The paper contributes to policy discussions surrounding investment externalities (e.g. environmental pollution), tax evasion and corruption. Lastly, it contributes to a broader theoretical discussion on the appropriate objectives of the firm. In other words, should firms maximize value or welfare? The results of this paper showcase that from here on out, banks must condition their corporate policies on the non-financial preferences of their creditors, i.e. the depositors.

Chapter 2. investigates whether there exist universally applicable corporate governance practices. In this paper, we construct various measures of firm- and country-level corporate governance, including a "global entrenchment index". We then test their relation with firm value on a large sample of more than 20,000 firms across 47 countries. We find substantial heterogeneity in the relation between some governance practices—especially those related to corporate rules constraining insider entrenchment—and firm value across countries, which is contingent on firms' ownership structure and institutional environments. In contrast, higher institutional ownership is unconditionally correlated with higher firm valuation. Our results cast doubt on the universality of rule-based corporate governance practices, and yield important policy recommendations for future discussions on corporate governance reforms.

Chapter 3. explores the empirical interaction between firm growth, financing constraints, and job creation. Using a novel small-business survey from Uganda, we find that the extent to which small businesses expand skilled employment as their sales and profits increase is significantly related to access to external funding, while the hiring of casual and family workers is not. This paper is an important contribution to policy discussions pertaining to youth and skilled unemployment in developing countries and, therefore, identifies a partial mechanism for addressing these policy relevant concerns.

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Abstract

Do depositors react to negative non-financial information about their banks? By using branch level data for the United States, I show that banks, that financed the highly controversial Dakota Access Pipeline, experienced significant decreases in deposit growth, especially in branches located closest to the pipeline. These effects were greater for branches located in environmentally or socially conscious counties, and data suggests that savings banks were among the main beneficiaries of this depositor movement. Using a global hand-collected dataset on tax evasion, corruption, and environmental scandals related to banks, I show that negative deposit growth as a reaction to scandals is a widespread phenomenon.

Keywords: Depositor Discipline, Bank Scandals, Environment, Tax Evasion, Corruption.

JEL Classification Codes: G21, G41, M14.

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1.1 Introduction

Understanding depositor behavior has been fundamental for evaluating the existence of market discipline in banking. As deposit institutions finance their operations with stable sources of financing, depositors serve a monitoring and disciplining role for these banks. The attention has so far concentrated on the perception of bank fundamentals, such as solvency and probability of default; hence, once banks are deemed too risky, depositors discipline banks by either withdrawing their funds or by demanding higher interest rates. To this date, however, there has been little to no research on whether depositors react to information that goes beyond financial fundamentals. Moreover, if depositors do in fact react to such information, is it because of financial motives or something else, such as social conscience? To shed light on these new ideas, this paper will attempt to test these hypotheses by examining the effects of bank scandals on depositor movement.

An extensive literature has established the importance of the banking system for the financing of the real economy. In the recent decade however, the banking sector has been under scrutiny as it has been perceived as a major conduit of business activities deemed unsustainable for the global economy. Even to this day, banks continue financing major coal and carbon intensive projects that undermine the Paris Agreement's aim of limiting global warming to 1.5 C above pre-industrial levels (Bank Track, 2017). In addition, banks have been identified as some of the largest enablers of tax avoidance, thereby contributing to the $21-$32 trillion of private financial wealth invested in tax havens (Henry, 2012), further generating obstacles for economic development (Alstadsæter et al., 2017). In order to force banks to internalize the costs of funding these activities, do we have to rely purely on regulatory measures or can we rely on disciplining by depositors?

This paper is the first to test depositor reactions to bank scandals. As such, it is the first thorough attempt of examining whether depositors discipline banks based on other sources of information than just financial health. With branch level data from the United States, I make use of the 2016 Dakota Access Pipeline (DAPL) protests. DAPL was a highly controversial project that was financed by, among others, nine major banks in the United States. These banks were highly criticized by activist groups because the pipeline was intended to cross major rivers as well as ancient burial grounds. The results from the empirical analysis show that banks involved in financing the DAPL had significant decreases in deposit growth and that the effect was stronger for branches located in states where the pipeline was present. These results indicate that depositor movement was heavily influenced by people's actual proximity to the scene of the controversy. In addition, the results show that the effects were stronger for environmentally as well as socially conscious counties, highlighting other non-financial drivers of depositor movement. The results also show that savings banks, which tend to be more localized institutions with more transparent asset allocations relative to larger banks, were among the main

beneficiaries of this unanticipated depositor movement. I find that savings banks located in counties with proportionally more DAPL banks had significantly higher deposit growth rates, which is in line with prior findings (Brown et al., 2017). I further establish whether this new channel of depositor discipline holds for a multinational setting by using quarterly bank level data and a hand-collected dataset on bank level scandals. I find that total deposit growth decreases when banks are caught in tax evasion, corruption, and environmental scandals. Furthermore, I find some evidence that, on average, larger banks that have not been involved in these scandals are rewarded with higher deposit growth rates.

The first contribution of this paper relates to the evolving literature on corporate social responsibility and ESG (environmental, social, and governance) finance. While there is much evidence that investors and corporations are significantly more active in addressing and pricing positive and negative externalities, there remains much debate as to what exactly these value adding or decreasing mechanisms are.[3] Therefore, the first contribution of this paper is documenting a new and novel channel by which tractable ESG risks affect a firm's (in this case bank's) bottom line. While many papers have attempted to distinguish these channels, this is among the first that can clearly identify a business cost for not addressing ESG risks. Furthermore, this paper contributes to a much broader debate on the purpose of the firm. Hart and Zingales (2017) re-evaluate the purpose by making a crucial distinction that considers how individuals place different weights on the choice to take a socially efficient action. For them, this is highly dependent on the degree to which an individual feels responsible for the action in question. I directly tackle this novel distinction by showing that an individual's degree of responsibility (proxied by deposit ownership, social norms, and climate change beliefs) has a direct effect on the choice to do the right or socially efficient action. In line with Hart and Zingales's conclusion that corporations should maximize shareholder welfare, this paper is the first to show that it makes financial sense to optimize corporations' strategies, conditional on the non-financial preferences of their creditors.

This paper further contributes to a range of literature on depositor behavior. In general, studies have shown that depositors discipline banks by either withdrawing deposits or by requiring higher interest rates (Martinez Peria and Schmukler, 2001; Maechler and McDill, 2006). While depositors have traditionally been seen as reactive to fundamental information (Saunders and Wilson, 1996; Schumacher, 2000; Goldberg and Hudgins, 2002; Schnabel, 2009), recent evidence has indicated that they are sensitive to other sources of information (e.g., negative press rumors and regulatory signals),

[3] There is an extensive literature documenting these new developments: Krüger (2015); Dimson et al. (2015); Ferrell et al. (2016); Lins et al. (2017); Liang and Renneboog (2017); Hartzmark and Sussman (2018), just to name a few. Servaes and Tamayo (2017) and Kitzmueller and Shimshack (2012) also provide useful literature reviews.

mechanisms (e.g., banking relationships and social networks), and bank characteristics (e.g., Euro-area affiliation and perception of too-big-to-fail) (Hasan et al., 2013; Correa et al., 2013; Iyer et al., 2013; Oliveira et al., 2014; Iyer and Puria, 2012). While this paper contributes to the findings of "other sources of information," the novelty here comes from the information beyond financial fundamentals. Even though these papers demonstrate behavioral frictions may cloud depositor judgment, behavior is still largely founded on the perception of financial loss. As a counter example, Brown et al. (2017) examine Swiss depositor movement from the two largest banks in Switzerland during the financial crisis and find that the role of switching costs in deterring deposit withdrawals was independent of deposit insurance. They argue that it is reasonable to assume that the withdrawals of deposits were at least partly driven by disagreement with the bank's corporate policy, rather than by fear about losing savings.[4] While their paper provides indicative evidence of the non-financial preferences of depositors, as of yet there exists no systematic analysis nor understanding of the other motivators of depositor discipline. This paper's next contribution is filling this gap, specifically, the other non-financial sources of depositor discipline.

This paper also contributes to the rising literature on corporate social responsibility in banking. While there is some evidence of misbehavior in the retail banking channel (Halan et al., 2016; Bursztyn et al., 2018; Fecht et al., 2018), on the corporate banking channel, a range of studies has documented that banks punish socially irresponsible firms by charging higher loan spreads and award responsible firms with lower loan spreads (Goss and Roberts, 2011; Hasan et al., 2014; Chava, 2014; Cheng et al., 2014; Kleimeier and Viehs, 2016). This paper further contributes to a much broader literature on corporate fraud (Dyck et al., 2010; Liu, 2016), tax evasion (Bennedsen and Zeume, 2017; Hasan et al., 2014; Johannesen, 2014; Johannesen and Zucman, 2014; Chernykh and Mityakov, 2016), and bribery (Zeume, 2017). Along with the essence of these papers, this study will increase our understanding of the nuances of corporate malpractice while providing further insights on how to tackle them.

Direct policy recommendations from this paper are difficult to justify, since there exist endless nuances as to what kinds of regulation are better fit for addressing various externalities. Nonetheless, the findings of this study show that depositor discipline can play an important role in forcing banks to internalize non-financial externalities on society from their risk decisions. While the findings of this study cannot infer the relative importance of regulatory and market responses to such externalities, they clearly show that reliance purely on government intervention might not be necessary.

[4] Blickle (2017), further document this movement extensively.

The remainder of the paper is structured as follows: Section 2 elaborates on the composition of the data and section 3 presents the main empirical findings. Section 4 tackles a series of additional findings. Section 5 takes a broader view by analysing global bank scandals, and section 6 concludes.

1.2 Data

The Dakota Access Pipeline protests were grassroots movements that began early 2016 in reaction to an approved oil pipeline project in the northern United States. The pipeline begins in the Bakken shale oil fields in northwest North Dakota and continues through South Dakota and Iowa, ending in Illinois. The pipeline sparked a lot of controversy from environmental activists as well Native Americans because the pipeline was intended to cross both the Missouri and Mississippi Rivers as well as ancient burial grounds. There was a total of 17 banks directly funding the construction of the DAPL and the banks that had a significant proportion of branches in the United States were Bank of Tokyo Mitsubishi UFJ, BBVA, BNP Paribas, Citigroup, SunTrust Robinson Humphrey, TD Bank, Mizuho Bank, SMBC, and Wells Fargo.[5] The protests themselves were large in scale, but the rather surprising outcome was the attention on banks as well as the financial coordination among activists. By February 2017, over 700,000 people had signed one of six petitions addressed to banks financing the DAPL. Individuals who signed the petition collectively reported having over $2.3 billion invested in these banks through checking, mortgage, and credit card accounts. They threatened to divest their wealth if the banks continued financing DAPL, and by then thousands had already closed their accounts, removing over $55 million from these banks (BankTrack, 2017). While the true extent of this movement is difficult to document, it is very likely that these actions and associated reputational costs were significant both in the United States and across the globe. Many banks, including ABN Amro and ING were quick to make public statements as a reaction to the scandal. They were publicly re-evaluating their commitments to the project; by March 2017, ING had sold its stake in the DAPL loan (ING, 2017). Soon after, other banks, including DNB ASA and BNP Paribas, had sold their stakes as well. Interestingly, public pressure further increased and was not only directed at those financing the pipeline directly, but also those who provided corporate financing to the pipeline companies. Furthermore, Seattle ended up cutting ties with Wells Fargo, Los Angeles moved to divest from Wells Fargo, San Francisco moved to divest $1.2 billion from companies financing the DAPL, Norwegian wealth fund stated its intent to drop fossil energy investments, and numerous Norwegian pension funds divested from companies behind DAPL. Interestingly, U.S. Bank stated its intent to stop financing pipeline projects, though, later retracted, and Nordea (the Nordic Banking and Investment group) had decided

[5] The 17 banks were Bank of Tokyo Mitsubishi UFJ, BayernLB, BBVA, BNP Paribas, Citigroup, Crédit Agricole, DNB ASA, ICBC, ING, Intesa Sanpaolo, Mizuho Bank, Natixis, SMBC, Société Générale, SunTrust Robinson Humphrey, TD Bank, and Wells Fargo. The energy and pipeline companies involved in the project were Dakota Access, LLC, a company owned by Philips 66, Energy Transfer Partners LP, and Sunoco Logistics Partners LP. At a later stage, stakes in the pipeline were bought by MarEn Bakken Co LLC, which was a joint venture by Enbridge Incorporated and Marathon Petroleum Corporation.

to exclude three companies behind DAPL, which was partially due to the unwillingness of those companies to talk about these issues.

To clearly identify depositor movement, I first collect the Federal Deposit Insurance Corporation (FDIC) summary of deposits (SOD) data for years 2012 - 2017. The data is based on an annual survey of branch office deposits as of June 30 for all FDIC-insured institutions, including insured US branches of foreign banks. All institutions with branch offices are required to submit the survey, and all responses are required by July 31. While the DAPL protests began April 2016, the attention on banks started around September 2016 as indicated by the timeline in figure 1. For this reason, the analysis assumes that the main shock took place in 2017 (i.e., July 2016 - June 2017). However, many regressions will be accounting for any effects that might have already risen in 2016 (i.e. July 2015 - June 2016).[6]

Figure 1-1: Timeline of Events

Overall, the dataset has detailed information on total deposits and other branch characteristics (including location) for over 100,000 bank branches across the United States. In the analysis, I only consider branches that have less than $1.0 billion and more than $100,000 in deposits, since larger branches often house deposits from all over the country (including corporate, municipal, and nonlocal retail consumers), while deposit growth rates for smaller branches might mislead the analysis with

[6] Interestingly, the pressure on banks did not peak in 2017. Protests have continued since June 2017 and with an even broader focus, e.g., with the inclusion of Tar Sand projects and the Keystone XL pipeline. The current banks are still being targeted since the June 2017 petitions, and more banks have been included in subsequent petitions. NGOs have reported that financial activism continues to this day and there have been no signs of these protests stopping as of yet.

abnormally high or low growth rates. I also exclude banks that had been acquired in either 2017, 2016, or 2015 to retain the focus of the analysis on established branches. Overall, these exclusions remove less than 1.6% of the total sample. Furthermore, as certain regressions will attempt to identify non-financial determinants of depositor behavior, it will be important to rule out alternative explanations that might be correlated with locational characteristics. Therefore, later analyses will include county level data on education, specifically, the percentage of people with a bachelor's degree or higher. The data is for 2012 - 2016 and collected from the *United States Department of Agriculture* county-level data sets. The analysis will also incorporate the percentage of the county that voted for Barack Obama in the 2012 presidential elections as a measurement for political affiliation. The data for this measurement was collected from *the Guardian*. In addition, the analysis will control for the county population, which is collected from the *Northeast Regional Center for Rural Development*. Lastly, in order to investigate the non-financial determinants of depositor behavior, the analysis will make use of county level climate change beliefs as well as proxies for social preferences. This data will be further discussed in later sections.

The summary statistics can be found in table 1, and panel B presents the two-sample *t*-tests for equal means. The population of banks is split between those banks that financed the DAPL and those that did not. The results show that branches whose parent banks financed the DAPL had higher levels of deposits and slightly lower deposit growth rates. Furthermore, DAPL financing branches were located in relatively populous, educated, and pro-Democratic counties. It was mainly larger commercial banks that financed the pipeline, and data suggests that these characteristics were reflected in the branch level data. Panel C provides bank level summary statistics on the number of branches as well as the distribution of deposit growth data.

Table 1-1: Summary Statistics

(a) Branch Controls

VARIABLES	(1) N	(2) mean	(3) sd
Branch Data and Controls			
Annual branch level deposit growth, winsorized at the 1st and 99th percentile	416,594	0.0862	0.230
Factor variable that defines the type of service the branch office provides	416,594	11.18	1.254
Industry classification grouping which indicates the institution's primary asset specialization	416,594	4.853	2.399
Factor variable that indicates major groupings of the institution	416,594	2.330	1.810
Total assets of the institution	416,594	3.930e+08	6.689e+08
Total deposits of the institution	416,594	2.543e+08	4.229e+08
Treatment Variables			
Equal to one if the branch financed the Dakota Access Pipeline	416,594	0.131	0.337
Equal to one if the branch financed the Dakota Access Pipeline and the year is 2016	416,594	0.0260	0.159
Equal to one if the branch financed the Dakota Access Pipeline and the year is 2017	416,594	0.0253	0.157
County Level Information			
Percentage of the county that thinks global warming is happening	415,533	69.43	6.287
Percentage of the county that thinks global warming is caused mostly by human activities	415,533	52.62	6.006
Number of non-profit organizations (without including those with an international approach)	416,578	3,853	6,533
Percent of adults with a bachelor's degree or higher, 2012-2016	416,578	30.16	11.32
Percentage of the county that voted for Barack Obama in the 2012 presidential elections	400,110	47.44	16.84
Population 2014	416,578	898,249	1.649e+06

(b) Two-Sample *t*-Test for Equal Means

VARIABLES	(1) N	(2) Mean Did not Finance DAPL	(3) Mean Financed DAPL	(4) *t*-test
Branch Deposits	416594	63250	95241	***
Annual branch level deposit growth, winsorized at the 1st and 99th percentile	416594	0.087	0.083	***
Percent of adults with a bachelor's degree or higher, 2012-2016	416578	29.64	33.56	***
Percentage of the county that voted for Barack Obama in the 2012 presidential elections	400110	46.96	50.66	***
Population 2014	416578	830554	1347573	***

(c) Deposit Growth by Bank

BANKS	(1) Number of Branches	(2) N	(3) mean	(4) sd
MITSUBISHI UFJ FINANCIAL GROUP, INC.	351	1,475	0.118	0.257
BANCO BILBAO VIZCAYA ARGENTARIA, S.A.	653	3,278	0.0973	0.219
BNP PARIBAS	526	2,747	0.101	0.227
CITIGROUP INC.	714	4,030	0.105	0.254
MIZUHO FINANCIAL GROUP, INC.	1	5	0.199	0.735
SUNTRUST BANKS, INC.	1395	7,124	0.0466	0.191
TORONTO-DOMINION BANK, THE	1237	6,302	0.126	0.268
WELLS FARGO & COMPANY	5937	29,538	0.0744	0.149
SUMITOMO MITSUI FINANCIAL GROUP, INC. / TRUST HOLDINGS, INC.	10	51	0.0456	0.215

Notes: Branch level data and controls are all collected from the *FDIC* for 2012-2017. County level education information come from the *United States Department of Agriculture* county-level data sets. County-level information on climate change beliefs come from the *Yale Program on Climate Change Communication*. Population data and number of non-profit organizations come from the *Northeast Regional Center for Rural Development* website. Lastly, the full US 2012 election county-level results is collected online from the *the Guardian*. Variable descriptions can be found from section 2.

1.3 Empirical Results

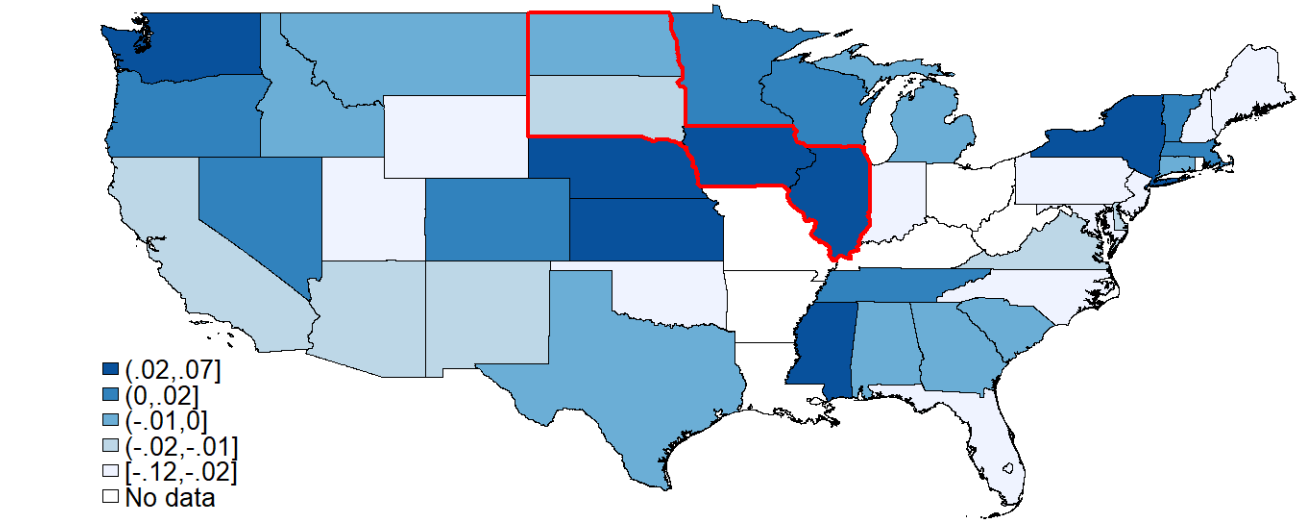
1.3.1 Main Results

To document the effects of the DAPL scandal on depositor behavior, I begin by visualizing the phenomenon with heat maps shown in figure 2. The first heat map shows the deposit growth rates for 2015 and the second one for 2016. Values are based on the average deposit growth rates for branches that financed the DAPL minus average state level deposit growth rates.[7] The darker colors in the heat maps translate to higher than state average deposit growth rates for the treated banks. As one can see, the areas closest to the pipeline, turn increasingly lighter in 2016 (first year of the scandal). This change means that on average, banks who financed the DAPL became more likely to have lower than state average deposit growth rates during the first year of the scandal. Furthermore, once you take a glimpse into 2017, the changes look even starker. A clear majority of the states turn lighter, indicating that depositor movement had become a nationwide phenomenon.

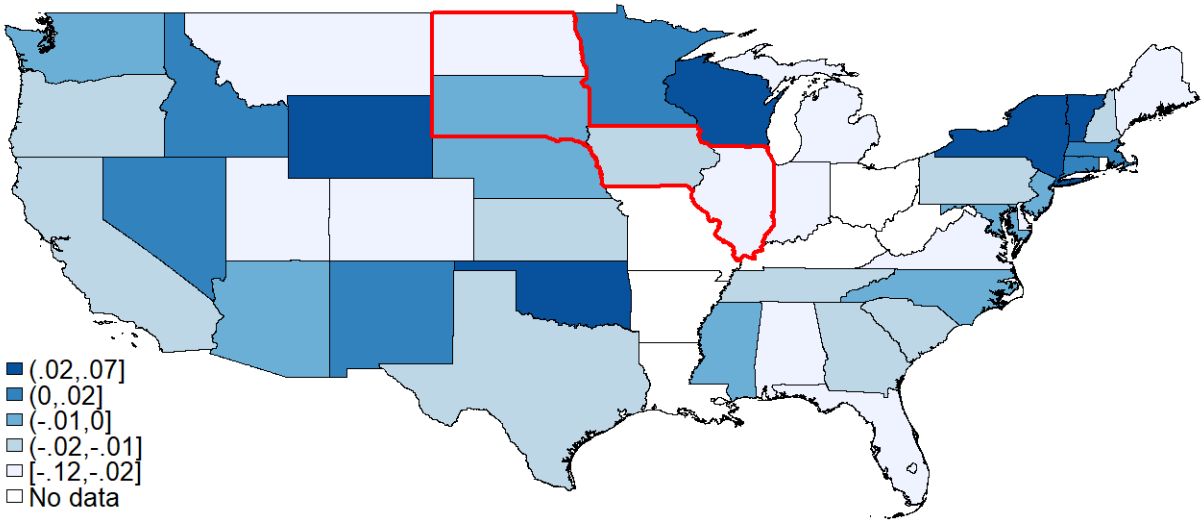
[7] Kentucky, Louisiana, Ohio, and Rhode Island are the only states in which these banks did not have any significant operations; hence, there is no branch level information for them. This is why the states are white (i.e., "No data").

Figure 1-2: Dakota Access Pipeline: State-Average Adjusted Deposit Growth Rates for Treated Banks

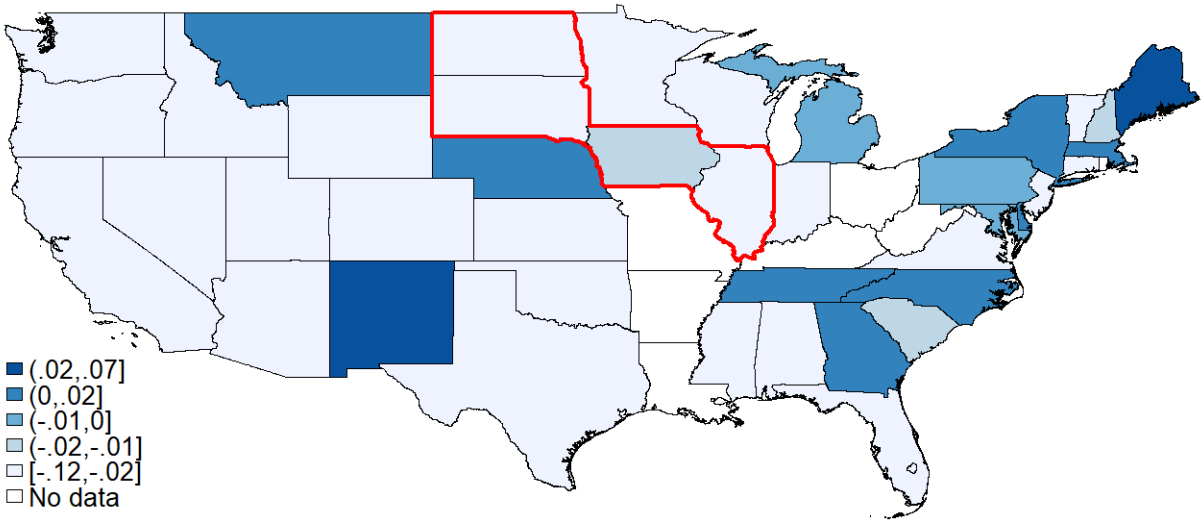
(a) July 2014 - June 2015



(b) July 2015 - June 2016



(c) July 2016 - June 2017



Notes: The red line highlights the pipeline states. Kentucky, Louisiana, Arkansas, Missouri, West Virginia, Ohio and Rhode Island are categorized as "No data", because these states had either zero or less than eight branches in total from the treatment group. Legends are determined by the five quartiles from 2015.

To better identify the effects of the scandal, I continue the analysis by estimating a simple diff-in-diff style analysis shown below. The treatment is equal to one if the year was 2017 and the bank was involved in financing the DAPL. Since the assignment to the treatment group is not random by nature, this is not a pure diff-in-diff analysis. Therefore, it will be important to control for a host of factors. For the majority of base results, all regressions will include bank, state, and year fixed effects to be assured that the results are not driven by any year or state level shocks nor any bank specific characteristics. In addition, the regressions will include a range of bank and branch specific controls as described in table 1.[8]

$$DepositGrowth_{it} = \alpha_0 + \beta_1 FinancedDAPL_i * 2017_i + \lambda X_{it} + \alpha_i + \theta_t + \varepsilon_{it} \quad (1)$$

The regression results for this exercise are shown in table 2. All the columns include the full sample of US states and show that financing the DAPL had a significant negative effect on deposit growth. Overall, financing the DAPL project had cost the affected banks 1.5 - 2.2 percentage point decrease in deposit growth. The economic effects of the event are quite substantial, considering that the average deposit growth rate for the full sample is 8.6% and for the treated banks is 8.3%. The results demonstrate that the incident was indeed a nationwide phenomenon as already evidenced by the high level of public awareness and engagement. To account for any time-varying, county-level, demand-side shocks or branch specific characteristics, columns 3-4 report the interaction results with the inclusion of bank fixed effects (at the institution level), branch fixed effects, and county-year fixed effects. The results hold after including these exhaustive controls. Overall, the results remain significant across the specifications, and the magnitudes change very little.

[8] The majority of the results control for total assets of the institution, total domestic deposits of the institution, asset specialization (international, agricultural, credit-card, commercial lending, mortgage lending, consumer lending, other specialized under 1 billion, all other under 1 billion, and all other over 1 billion), type of branch service (brick and mortar, retail, cyber, military, drive through, mobile/seasonal, and trust), major institution grouping (national member bank, state member bank, state nonmember bank, savings banks and savings and loans, state stock savings and loans, and other insured institution).

Table 1-2: DAPL Main Results

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth
Financed DAPL * 2017	-0.020***	-0.018***	-0.022***	-0.015***	-0.015***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Financed DAPL	-0.022***	-0.022***			
	(0.002)	(0.002)			
Observations	416,594	416,594	416,513	412,557	411,930
Controls	Yes	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes	Yes
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	Yes	Yes	Yes	No
County*Year FE	No	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) on branch level deposit growth. The variable “Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

To get a better sense of the real impacts of the DAPL, I calculate the approximate loss of deposits for the affected group. In other words, I estimate the losses for the 10 902 (treated) branches in 2017, which had average branch level deposits of $101 million in 2016. Using the previously identified economic effect of -0.015% (and -0.020%) deposit growth rate, this would imply that the bank branches lost approximately $101 million * 0.015 (0.020) * 10 902 = $16.5 ($22.0) billion in total deposits in 2017 alone. While this is a large economic effect, generating reliable loss estimates is difficult, and one might argue that if all these effects were caused by switchers, the estimates would be inflated due to double counting (though unlikely, since losses due to any unmet new deposit demand surely played a role as well). In other words, a “deposit loss" for treated banks naturally means “deposit gain" for other banks (due to switching), and this puts an upward bias in the diff-in-diff estimates. This means the estimates may double-count the effect by taking the difference between these two types of banks. Therefore, with full-switching, we can assume that these banks lost at least $8.25 - $11 billion in total deposits as a result of the scandal. This estimate is by far the most conservative, considering that the analysis also does not incorporate the largest US bank branches (those with over $1 billion in deposits), which undoubtedly suffered from deposit losses as well. In addition, the FDIC does not collect data on credit unions, which were primary locations to which NGOs instructed their petitioners to transfer their funds. This biases against finding a result and implies that the deposit loss estimates would be understated. Later analysis incorporating bank level credit union data from call reports provides strong support for this claim.

While it is difficult to establish how many people might have moved their deposits, the results and anecdotal evidence may provide some indications. As stated earlier, 700,000 petitioners collectively reported having over $2.3 billion invested in these banks through checking, mortgage, and credit card accounts. They threatened to divest their wealth if the banks continued financing DAPL (BankTrack, 2017). Furthermore, in the second Signforgood petition, 150,000 petitioners had pledged to divest $4.4 billion. While it is difficult to get a sense of the actual amount of people who were responsible for the overall deposit losses, comparing petitioner statements with the estimated $8.25 - $22.0 billion change, gives us a glimpse of the extent and potential. Overall, the results highlight a large cost of doing business for these banks, yet we must be aware that this analysis is unable to capture further business losses as a result of employee morale or lower demand for other consumer products, which surely had some impact on these banks as well.

To further evaluate whether these changes were driven by traditional retail clients, figure 4 in the appendix reports the uninsured deposit growth rates of the treated and non-treated banks. The information was collected from the Federal Financial Institutions Examination Council (FFIEC) Central Data Repository's Public Data Distribution web site and more specifically, the Call Reports, which are available at the institution level (i.e., not the branch level). Bank branches can receive sizeable funding from large time deposits from US money market funds, so it is important to examine whether these changes might have impacted the overall deposit growth results. The figure fails to indicate any substantial changes in these markets. It seems that large time deposits were not the primary driver of depositor movement. Furthermore, it was difficult to find any mentions of this event in analyst reports, which further yields support for this claim. The figure also serves as a partial test for arguments in favor of financial motives. If anyone were to move their deposits due to fears of these banks facing future financial difficulties, it would have been the uninsured depositors, who again show no clear sign of movement. As a final point, those who might argue that financially less experienced retail clients might be biased in interpreting these events as a sign of future distress, depositors are insured by the FDIC up to at least $250,000. Therefore, it would be difficult to argue in favor of misguided movement in the retail deposit channel. Overall, the results indicate that retail depositors are, therefore, a likely candidate for driving the changes in deposit growth.

In order to alleviate concerns that the empirical analyses might not be identifying a unique event specific to these banks, figure 5 in the appendix reports the total deposit growth rates of the treated and non-treated banks. This data is also from the FFIEC Call Reports. While the graph provides some convincing evidence that these banks were facing abnormally lower deposit growth rates, as a reminder, the data is only available at the institutional level, so we must be careful before making any strong statements based on these results. Identification is further complicated by the fact that DAPL specific events took place across several accounting quarters and most of these banks experienced positive

deposit growth rates during the overall time frame. While the figure provides partial evidence that the treated banks witnessed abnormally lower deposit growth rates, in order to provide further evidence that the results are identifying a unique event, table 14 in the appendix provides branch level regressions, whereby the interaction *2017*FinancedDAPL* is kept as the base variable. As the results show, all the alternative year times *FinancedDAPL* interactions are positive and significant. These banks were doing strictly better across all years before and compared to 2017, further highlighting the importance and uniqueness of the DAPL events. As a final examination of the data, figure 6 provides graphical representation of the pre-treatment trends of the branch level FDIC data used in the main analyses. The graph does not suggest any clear violations of the parallel trend assumption.

1.3.2 Channels of Depositor Movement

Even though depositor reactions are unlikely to be motivated by financial concerns, it is just as important to further gauge the non-financials motivators of the unanticipated depositor movement. These extended analyses will serve to further alleviate concerns regarding the financial motives of depositors (assuming financial motives are not fully correlated with social motives) and will provide interesting insights into the factors that amplify pro-social depositor behavior. I begin this exercise by limiting the sample to only the states where the pipeline was actually present (i.e., North Dakota, South Dakota, Iowa, and Illinois). The motivation for running this analysis is to understand whether those who were located closer to the scene of the controversy were more likely to move their deposits. In these regressions, I also include an interaction with the year 2016 in case there were any effects that might have risen prior to June 2016. The results from this analysis are shown in table 3. The coefficients remain significant and negative, but most importantly, the coefficients are larger compared to the full sample results. This difference would suggest that people in these localities were more sensitive to the controversy and hence more driven to move their deposits.

Table 1-3: DAPL and Proximity: Subsample Analysis

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Financed DAPL * 2017	-0.044***	-0.040***	-0.028**	-0.057***	-0.051***	-0.044***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.014)
Financed DAPL * 2016				-0.050***	-0.045***	-0.042***
				(0.012)	(0.013)	(0.016)
Financed DAPL	-0.059***	-0.063***		-0.049***	-0.054***	
	(0.009)	(0.009)		(0.009)	(0.010)	
Observations	31,433	31,433	31,422	31,433	31,433	31,422
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	No	Yes
State FE	Yes	No	No	Yes	No	No
Year FE	Yes	No	No	Yes	No	No
State*Year FE	No	Yes	Yes	No	Yes	Yes
Sample	DAPL States	DAPL States	DAPL States	DAPL States	DAPL States	DAPL States

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and proximity on branch level deposit growth. The sample is restricted to the states where the pipeline is actually present. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

To better identify the proximity to pipeline effect, I run the same regression for the full sample of US states, while including a triple interaction term accounting for whether the branch was located in a pipeline state. The results from this analysis are shown in table 4 and further show that, on average, bank branches who financed the DAPL had suffered an additional 2.6 -2.3% negative deposit growth rate if they were located in the pipeline states. Even though the DAPL scandal was a nationwide phenomenon, the results further highlight that people who were closest to the scene of the controversy, and hence more likely to be impacted, were the ones who were more likely to move their deposits. These results are similar to findings by Levine et al. (2018), who document the increased migration of corporate executives after firms open industrial plants emitting toxic air pollutants. It is important to clarify that, in total, nine banks with branches in the United States financed the DAPL; however, not all of them had operations in the pipeline states. The banks that had a presence in these states were BNP Paribas, Citigroup, Wells Fargo, and Mitsubishi UFJ. In total, they collectively held 17% of all deposits in South Dakota, 12% in North Dakota, 9% in Iowa, and 3% Illinois. The results from this analysis are in line with earlier findings and further highlight an amplifying factor for depositor movement.

Table 1-4: DAPL and Proximity: Full Sample Analysis

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Financed DAPL * 2017 * DAPL State	-0.029**	-0.026**	-0.023*	-0.017
	(0.012)	(0.012)	(0.012)	(0.015)
Financed DAPL * 2016 * DAPL State	-0.041***	-0.036***	-0.033***	-0.034**
	(0.012)	(0.012)	(0.012)	(0.015)
Financed DAPL * 2017	-0.020***	-0.016***	-0.023***	-0.014***
	(0.002)	(0.003)	(0.003)	(0.003)
Financed DAPL * 2016	0.001	0.003	-0.004	0.002
	(0.002)	(0.002)	(0.003)	(0.003)
Finance DAPL * DAPL State	0.000	-0.001	0.011	
	(0.008)	(0.008)	(0.009)	
Financed DAPL	-0.022***	-0.022***		
	(0.002)	(0.002)		
Observations	416,594	416,594	416,513	411,930
Controls	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No
State FE	Yes	No	No	No
Year FE	Yes	No	No	No
State*Year FE	No	Yes	Yes	No

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and proximity on branch level deposit growth. The variable "DAPL State" takes a value of one if the branch is located in one of the pipeline states. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

While the locational effect of scandals partially determine, the severity of depositor discipline, I examine other non-financially motivated mechanisms. To do so, I collected county level data from the *Yale Program on Climate Change Communication* (YPCCC). This data is based on surveys that evaluate Americans' climate change beliefs, risk perceptions, and policy support (Howe et al., 2015). I use their data from 2016, and in the analysis I include a include a dummy, "Happening 70," which is equal to 1 if at least 70% of the county thinks that global warming is happening. Approximately 17% of US counties fall under this category. The dummy is used as a way to represent the counties where a clear majority of the population think climate change is happening. While incorporating a continuous variable might be of interest, the effects are mostly expected to appear in communities with relatively strong climate change beliefs as a result of the intense polarization of public opinion (Hoffman, 2011). Table 5 shows the results of this analysis. The interactions are negative and significant, demonstrating that changes in deposits were further aggravated by local beliefs in climate change. Bank branches who financed the DAPL had a greater negative deposit growth rate if they were located in a county with stronger beliefs in climate change. To make sure the effects are not driven by other factors that might be correlated with climate change beliefs, all regressions include county level data on education, specifically, the percentage of people with a bachelor's degree or higher, as well as county level population data for 2014. In addition, the results also include the percentage of the county that voted

for Barack Obama in the 2012 presidential elections as a measurement for political affiliation. All the results hold after controlling for these alternative determinants of depositor behavior. As a further test, the third column of the analysis includes bank-year and state-year fixed effects as a way to control for any other bank and state specific effects that might have risen in 2017. The results hold after including this conservative test, which yields strong support for the effects of DAPL and the additional drivers of depositor movement. As a final conservative test, the analysis makes use of the YPCCC data for 2014. One might argue that the DAPL event had an effect on local climate change beliefs, and therefore, it would be more appropriate to incorporate prior county-level beliefs on climate change. Table 21 in the appendix shows the results of this analysis. The findings remain the same and demonstrate that local climate change beliefs had an effect on depositor movement.

Table 1-5: DAPL & Climate Change Beliefs

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Happening 70 * Financed DAPL * 2017	-0.013***	-0.018***	-0.019***	-0.012**
	(0.005)	(0.005)	(0.005)	(0.005)
Happening 70 * 2017	-0.002	-0.005**	-0.003	-0.003
	(0.002)	(0.002)	(0.003)	(0.003)
Happening 70 * Financed DAPL	0.001	-0.022	-0.032	0.004
	(0.002)	(0.041)	(0.041)	(0.003)
Financed DAPL * 2017	-0.016***	-0.006	-0.004	
	(0.004)	(0.004)	(0.004)	
Happening 70	0.001	-0.020	-0.019	0.017***
	(0.002)	(0.028)	(0.028)	(0.001)
Percent of Adults With a Bachelor's Degree or Higher	0.002***	0.001	0.001	
	(0.000)	(0.001)	(0.001)	
Percent of Votes for Obama	0.000	0.002*	0.002*	
	(0.000)	(0.001)	(0.001)	
Population 2014	0.000***	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
Observations	398,980	395,158	395,158	407,998
Controls	Yes	Yes	Yes	Yes
Bank Fe	Yes	No	No	No
Branch FE	No	Yes	Yes	No
State FE	No	No	No	No
Year FE	Yes	Yes	No	No
State*Year FE	No	No	Yes	Yes
Bank*Year FE	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and environmental awareness on branch level deposit growth. "Happening 70" is equal to 1 if at least 70% of the county thinks that global warming is happening. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1.

The next stage of the analysis is to further gauge the effects of personal responsibility that depositors might feel due to the negative externalities caused by their banks (as proposed by Hart and Zingales, 2017). To further tackle this responsibility channel, the analysis includes a variable "Human 55," which is is equal to 1 if at least 55% of the county thinks global warming is caused mostly by human activities. Approximately 9% of US counties fall under this category. The results from this analysis remain similar to earlier findings and provide additional insights into the non-financial determinants of depositor discipline.

Table 1-6: DAPL & Climate Change and Human Responsibility

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Human 55 * Financed DAPL * 2017	-0.010**	-0.015***	-0.016***	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)
Human 55 * 2017	-0.004*	-0.008***	-0.007**	-0.006*
	(0.002)	(0.002)	(0.003)	(0.003)
Human 55 * Financed DAPL	0.003	-0.026	-0.036	0.006**
	(0.003)	(0.048)	(0.047)	(0.003)
Financed DAPL * 2017	-0.020***	-0.009***	-0.008**	
	(0.003)	(0.004)	(0.004)	
Human 55	0.004**	-0.022	-0.019	0.017***
	(0.002)	(0.034)	(0.034)	(0.002)
Percent of Adults With a Bachelor's Degree or Higher	0.002***	0.001	0.001	
	(0.000)	(0.001)	(0.001)	
Percent of Votes for Obama	-0.000	0.002*	0.002*	
	(0.000)	(0.001)	(0.001)	
Population 2014	0.000***	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
Observations	398,980	395,158	395,158	407,998
Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	No	No	No
Branch FE	No	Yes	Yes	No
State FE	No	No	No	No
Year FE	Yes	Yes	No	No
State*Year FE	No	No	Yes	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and environmental awareness on branch level deposit growth. "Human 55" is equal to 1 if at least 55% of the county thinks global warming is caused mostly by human activities. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1.

As another examination of non-financial drivers, the analysis makes use of the county level social capital data from Rupasingha et al. (2006). More specifically, the analysis will incorporate their 2014 data on the number of non-profit organizations within a county (without including those with an international approach) to proxy for county level willingness to tackle societal issues. Table 7 shows the results of this analysis. The interactions are negative and significant across the US, demonstrating that changes in deposit growth were further aggravated by local social norms. The results hold after controlling for county level education, population, and political affiliation.

Table 1-7: DAPL & Charitable Behavior

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Ln(Number of Non-Profits) * Financed DAPL * 2017	-0.004***	-0.004***	-0.005***	-0.007***	-0.005***	-0.006***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Ln(Number of Non-Profits) * 2017	-0.001***	-0.000	0.001	-0.002***	-0.000	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Number of Non-Profits) * Financed DAPL	0.000	-0.000	0.001	-0.014**	-0.000	-0.018***
	(0.001)	(0.001)	(0.001)	(0.006)	(0.001)	(0.006)
Financed DAPL * 2017	0.014	0.015	0.014	0.038***	0.013	0.035***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)
Ln(Number of Non-Profits)	0.010***	0.008***	0.008***	0.030*	0.009***	0.030*
	(0.001)	(0.001)	(0.001)	(0.016)	(0.001)	(0.016)
Financed DAPL	-0.020***	-0.021***				
	(0.007)	(0.007)				
Percent of Adults With a Bachelor's Degree or Higher	0.001***	0.001***	0.001***	-0.001	0.001***	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Percent of Votes for Obama	-0.000***	-0.000***	-0.000	0.001	-0.000***	0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Population 2014	0.000	-0.000	-0.000	-0.000*	0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	400,110	400,110	400,026	396,187	400,026	396,187
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	Yes	No
Branch FE	No	No	No	Yes	No	Yes
State FE	No	No	No	No	No	No
Year FE	Yes	No	No	Yes	Yes	No
State*Year FE	No	Yes	Yes	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and charitable behavior on branch level deposit growth. The variable "Number of Non-Profits" is equal to the number of domestically focused non-profits at the county level. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

As a final examination of the non-financial drivers of depositor movement, the analysis makes use of state level data from *Google Trends*. The motivation for this final analysis is to find a proxy that would best represent DAPL specific local support. While county level climate change attitudes and

other factors are good proxies for general social preferences, they might still be unable to fully capture area specific interests for supporting the DAPL movement. To address this concern, the analysis uses a variable *Donate to Standing Rock*, which is equal to the intensity of Google searches between 4/4/2016 and 11/9/2017 for the search term. Figure 3 in the appendix displays the heterogeneity of those searches across the United States, and table 8 shows the results of this analysis. The interactions are negative and significant, adding strength to earlier findings. As indicated by the figure, there are states across the United States that had missing values for this particular search term. To mitigate this concern, columns 1-3 show the results, while converting the missing values to zero, and columns 4-6 include the unconverted results. The results hold under both specifications. Furthermore, table 22 shows the results of the analysis using more generic terms, specifically *Standing Rock Indian Reservation"* and *Dakota Access Pipeline*, which provide more state level observations for the analysis. The results hold after using these alternative search queries as well. While this adds overall strength to the analysis, the earlier results are much more reliable since the query *Donate to Standing Rock* is a much better representation of local DAPL support. Lastly, table 23 shows the results of all four previous search query specifications, while including state-year fixed effects, branch fixed effects and bank-year fixed effects in all specifications. The results remain significant and negative. This is by far the most conservative test of this paper providing further strength to the analysis.

Table 1-8: DAPL & Donating to Standing Rock

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
	Converted Missing Values			Non-Converted Missing Values		
Ln(Donate to Standing Rock) * Financed DAPL * 2017	-0.003**	-0.003**	-0.006***	-0.019***	-0.020***	-0.029***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.004)	(0.005)
Ln(Donate to Standing Rock) * 2017	0.002***	0.002***	0.002***	0.007***	0.006***	0.008***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Ln(Donate to Standing Rock) * Financed DAPL	0.001	0.001	-0.040**	0.021***	0.020***	-0.042***
	(0.001)	(0.001)	(0.016)	(0.003)	(0.003)	(0.016)
Financed DAPL * 2017	-0.011**	-0.014**	0.003	0.052***	0.050***	0.091***
	(0.005)	(0.005)	(0.006)	(0.016)	(0.017)	(0.018)
Ln(Donate to Standing Rock)	-0.000	0.000	-0.012	-0.015***	-0.015***	0.081
	(0.000)	(0.000)	(0.021)	(0.001)	(0.002)	(0.104)
Financed DAPL	-0.021***			-0.103***		
	(0.003)			(0.010)		
Percent of Adults With a Bachelor's Degree or Higher	0.002***	0.002***	0.001	0.002***	0.001***	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Percent of Votes for Obama	-0.000***	0.000	0.002	-0.000**	0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Population 2014	0.000***	0.000***	-0.000	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	400,110	400,026	396,187	318,789	318,722	315,583
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	Yes	No	No	Yes	No
Branch FE	No	No	Yes	No	No	Yes
State FE	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and public opinion on branch level deposit growth. "Donate to Standing Rock" is equal to the search intensity of the Google search term using Google Trends. These are state specific values between 15 and 100. Columns 1-3 convert all missing values to zero and columns 4-6 are unconverted. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

1.4 Further Findings

1.4.1 Savings Banks

While deposit growth decreases for banks involved in the DAPL incident, it is worthwhile exploring whether the uninvolved banks enjoy any spillovers as a result of depositor movement. To do so, I create a dummy equal to one if the branch did not finance the DAPL and is a state-chartered savings

bank. This test is motivated by Brown et al. (2017) who found that deposit withdrawals from distressed banks in Switzerland were unrelated to household coverage by deposit insurance. They assume that deposit withdrawals from UBS (distressed bank that incurred investment losses in the wake of the US subprime crisis) were at least partly driven by disagreement with the bank's corporate policy, rather than by fear about losing savings. This view was further supported by the fact that while customer deposits declined strongly at the two large banks (UBS and Credit Suisse), deposits at the domestically focused cantonal banks and savings banks increased throughout the crisis. More specifically, there is additional documentation that local mortgage lenders (Raiffeisenbanks) were direct recipients of the new clients that migrated away from UBS (Blickle, 2017). In similar spirit, Giannetti and Wang (2016) find that in the United States, state-level corporate fraud, decrease stock market participation and, more interestingly, Gurun et al. (2017) find that residents who were exposed to the infamous Madoff Ponzi scheme, were more likely to withdraw their assets from investment advisors and subsequently increase their deposits at banks. Motivated by these findings and insights, I test whether US savings banks faced similar advantages during the DAPL incident.

The results in table 9 show that savings banks were the main beneficiaries of this unanticipated depositor movement. This is done by incorporating a triple interaction term with a variable measuring the proportion of DAPL branches in a given county. Intuitively, if the locality has more DAPL branches, the likelihood of savings banks exhibiting higher deposit growth should increase as a result of greater levels of depositor movement. The results demonstrate that savings banks in counties with more DAPL banks enjoyed higher deposit growth rates as a result of the scandal. In order to be assured that the results are not driven by any time-varying, state-level, demand-side shocks nor bank-year specific characteristics, column 10 reports the interaction results with the inclusion of state-year fixed effects as well as bank-year fixed effects. Overall, these results demonstrate the impacts of the pipeline controversy and its heterogenous effects on depositors.

Table 1-9: DAPL & Savings Banks

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth
Savings Bank * 2017 * Proportion of DAPL Banks	0.085**	0.090**	0.077*	0.135***	0.140**
	(0.040)	(0.041)	(0.042)	(0.051)	(0.071)
Savings Bank * 2017	-0.005	-0.003	-0.003	-0.012**	
	(0.005)	(0.005)	(0.005)	(0.006)	
Savings Bank * Proportion of DAPL Banks	-0.027	-0.027	-0.001	0.212***	
	(0.023)	(0.023)	(0.028)	(0.052)	
Proportion of DAPL Banks * 2017	-0.045***	-0.033***	-0.021*		
	(0.008)	(0.012)	(0.013)		
Savings Bank	-0.013***	-0.013***	0.019**		
	(0.003)	(0.003)	(0.009)		
Proportion of DAPL Banks	0.104***	0.098***	0.079***		
	(0.007)	(0.007)	(0.007)		
Observations	416,594	416,594	416,513	411,930	408,123
Controls	Yes	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes	No
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	Yes	Yes	No	No
Year*County FE	No	No	No	Yes	Yes
Bank*Year FE	No	No	No	No	Yes

Notes: This table shows the effect of the Dakota Access Pipeline (DAPL) on branch level deposit growth for savings banks. The variable “Savings Bank" takes a value of one if the branch is classified as a savings bank. The variable “Proportion of DAPL Banks" is equal to the percentage of DAPL financing branches at the county level. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

1.4.2 Wells Fargo

During the DAPL scandal, Wells Fargo was going through a series of corruption scandals unrelated to the DAPL incident. The bank had created (without customers’ permission) millions of fraudulent accounts as sales staff desperately tried to hit unreasonable sales targets. Furthermore, thousands of auto loan customers were charged for car insurance that they did not need (Fox and Duren, 2017). As a result of all these scandals, 18% of Wells Fargo’s branches lost deposits, while competitor deposit growth rates had improved during the same time frame (Tor, 2017). These incidents do not necessarily go against the main findings of the extended analysis; however, to establish that changes in deposits were partially driven by the pipeline scandal as well as the proximity to the pipeline, I exclude the Wells Fargo branches from the sample. Wells Fargo had the largest amount of branches in the treatment group, so excluding them serves as a conservative approach to the analysis. The results in table 15 in the appendix show that branches that financed the DAPL had incurred significantly greater deposit losses, even after excluding the branches owned by Wells Fargo.

1.4.3 Credit Unions

The overall results of this study are most likely understated. As mentioned earlier, the FDIC does not cover nor insure credit unions, which hold a non-negligible amount of $1 trillion in total deposits. Credit unions are covered by the National Credit Union Administration (NCUA), and as a comparison, the FDIC covers approximately $11.1 trillion in total deposits. Overall, not including these banks in the analysis biases against finding any results as NGOs quite often instructed petitioners to move their funds to more local institutions (i.e., savings banks, and more relevantly, credit unions). Unfortunately, this paper cannot include credit union data in the main analysis. Though branch level data is provided by the NCUA, their data does not provide deposit information at the branch level. Therefore, this analysis will incorporate call report data at the institutional level as an alternative. This data is added to the main dataset, providing approximately 6,000 additional banks to the main analysis. The analysis will assume that each institution operates as its own individual branch, located at the headquarter's address. Overall, this is not a major problem for the analysis as most credit unions in the United States are small with few branches, which operate within a geographically close proximity. The main drawback is that the data cannot be used in the primary analyses of this paper as it would not allow for specific controls found in the FDIC dataset nor for bank-year fixed effects that are useful in the extended analyses of the paper.

Table 10 shows the main results after incorporating credit union data in the analysis. As a comparison, columns 3-4 show the very same results excluding credit unions. In line with prior expectations, the results show that branches which financed the DAPL had significant decreases in deposit growth. The results are greater in magnitude once the credit unions are incorporated in the analysis. This provides further evidence that the earlier results were indeed underestimating the effects of the DAPL incident on the treated branches.

Table 1-10: DAPL Main Results and Credit Unions

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
	With Credit Unions		Without Credit Unions	
Financed DAPL * 2017	-0.014***	-0.013***	-0.012***	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)
Financed DAPL	0.002	-0.032	0.000	-0.035
	(0.043)	(0.048)	(0.043)	(0.048)
Observations	443,145	442,521	412,562	411,935
Branch FE	Yes	Yes	Yes	Yes
State*Year FE	Yes	No	Yes	No
County*Year FE	No	Yes	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) on branch level deposit growth. The first two columns include credit unions in the sample and the last two columns exclude them. The variable “Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

As an additional test, table 16 in the appendix re-creates the earlier deposit windfall results for the non-treated branches. More specifically, the analysis tests whether savings and credit unions had higher deposit growth rates if there were more DAPL financing branches operating in the same county. The variable savings bank is a dummy equal to one if the entity is either a savings bank or a credit union. In line with earlier results, the findings show that these entities were in fact doing well if there were more DAPL financing branches in the vicinity. It would be interesting to test whether there were any heterogenous effects on these institutions (e.g., whether credit unions received more of the deposit windfall than savings banks did). Unfortunately, this analysis will not be able to make any further inference as the credit union data is only available at the institution level, which therefore limits the reliable identification strategies available for further tests.

1.4.4 Further Protests

Between September 2016 and June 2017, bank protests often concentrated on the “project" financing banks, as emphasized in earlier sections and analyses. However, as time went on, protests grew larger and so did the list of banks that were targeted. Among the major petitions, “DefundDAPL" and “Signforgood" began including banks that provided corporate loans to the companies in charge of the pipeline construction. These were known as the corporate financing banks. While these banks were not generally targeted until June 2017, some early stage protests took place already in February 2017. These were mainly as a result of the “DefundDAPL" movement that had started in November 2016. The banks that were in these extended lists included Citizens Bank, Comerica, U.S. Bank, PNC, JPMorgan Chase, Bank of America, RBC, Origin Bank and HSBC. While other banks were included in these lists as well, all of these banks specifically had major branch level presence in the United States.

Table 11 reports the main empirical results by incorporating the extended treatment group, i.e., by including both the project and corporate financing banks. The results are significant and provide evidence that even the extended group was affected during the treatment period. Interestingly, the overall results are weaker, which suggests that the larger and perceptionally vaguer group was not being targeted as successfully on average. Anecdotal evidence does suggest that some of the corporate financing banks also received much attention and public scrutiny. For example, U.S. Bank, Bank of America, and Citizens Bank experienced numerous branch level protests, and, after major public pressure, Citizens Bank ended up withdrawing from the pipeline loan in March 2018. While U.S. Bank received much attention from branch level protests, activists also climbed the U.S. Bank stadium in Minneapolis, Minnesota and hung large banners protesting the bank's commitment to financing the DAPL project. This pressure initially led U.S. Bank to publicly state that it would stop financing future pipelines; however, this later turned out to be false. U.S. Bank was later found financing new pipeline projects not too long after the protests.[9] While some banks faced immense public pressure, certain banks, including HSBC, which has large branch level presence in the United States, faced very limited public scrutiny. This disparity would indicate that there were some levels of heterogeneity as to which banks were ultimately targeted by local grassroots movements. Most likely, both the heterogeneity and the timing are partial explanations as to why the overall effect on deposit growth is smaller compared to earlier results. The next sections will test for whether the extended group was differentially affected by the non-financial motivators of depositor discipline, i.e., proximity to pipeline, climate change beliefs, and social values.

Table 1-11: DAPL Main Results and Extended Treatment Group

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Deposit Growth	Deposit Growth	Deposit Growth	Deposit Growth	Deposit Growth
Financed DAPL * 2017	-0.010***	-0.008***	-0.008***	0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Financed DAPL	-0.002	-0.002			
	(0.002)	(0.002)			
Observations	416,594	416,594	416,513	412,557	411,930
Controls	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	Yes	Yes	Yes	No

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) on branch level deposit growth. The variable "Financed DAPL" takes a value of one if the branch provided project or corporate financing to the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

[9] To be more specific, U.S. Bank had committed to cease project financing loans. The new loans that were then issued were corporate financing loans to pipeline companies for general use, which included pipeline construction.

Table 24 reports the results for branches located in pipeline states with the inclusion of the extended treatment group. Overall, the results are similar, but most importantly, the results are stronger compared to earlier findings. These coefficient results are greater not only in size but also in significance as they hold after accounting for branch and county-year fixed effects. This result highlights some of the earlier heterogeneity. While the extended group of banks were generally being targeted by protesters, it seems banks were targeted more in areas where it really mattered, in this case, the pipeline states.

While the pipeline state results in itself are interesting, perhaps there are other channels that show a similar story. Tables 25 and 26 take a further dive into the drivers of alternative sources of depositor movement while incorporating the extended treatment group. Table 25 shows the results of the analysis after the incorporation of the YPCCC data on climate change beliefs. While the coefficient signs are negative, the results remain insignificant. On the other hand, the effects of local societal attitudes proxied by county level number of NGOs, in table 26, show negative and significant results similar to earlier findings. What is most notable about these results is that while the coefficient size is smaller, the results are stronger. The results hold for all previous specifications, but also after the inclusion of state-year and bank-year fixed effects. Overall, the results highlight that while the masses did not pay as much attention to this larger list of banks, there was clear heterogeneity as to how and when these branches were targeted. Results suggest that proximity to the pipeline had a clear effect on all banks and that county level attitudes had further heterogeneous impacts on the extended treatment group.

As a sanity check, table 27 shows the results for savings banks after adjusting for the amount of extended DAPL group financing banks present in each county. It is likely that savings banks experienced higher deposit windfalls in counties with more extended group DAPL financing banks. Results are similar to earlier findings and showcase the phenomenon that savings banks that were located in DAPL heavy counties, benefited from the DAPL incident.

1.5 Global Scandals

As much as it is interesting to identify a new and significant incident (i.e., the DAPL), it is just as important to explore the external validity of this new depositor discipline finding. One might argue that the DAPL was a one-off incident and that on a global scale these effects are virtually non-existant or undetectable. While branch level analyses provide granularity to explore the drivers of depositor movement, a cross-country study will be important for broader implications. In addition, while the DAPL scandal was a very specific event, it is worthwhile to explore whether other types of non-financial scandals have an effect on depositor behavior. Therefore, to investigate the global implications of these

findings, this study incorporates a novel hand-collected dataset on global bank scandals, which include all major bank specific events on tax evasion, corruption and environmental scandals.

The data on bank scandals has been collected from a range of primary sources. The preliminary information was largely collected from major non-governmental organizations (NGOs) that deal with, among other things, the reporting of unethical bank behavior. These include the following organizations: *BankTrack*, *Global Witness*, *Greenpeace*, *Oxfam*, *Tax Justice Network*, and the *International Consortium of Investigative Journalists*. These institutions have either extensively documented or campaigned against commercial banks and their operations. With the purpose of covering globally significant scandals, these organizations have covered the majority of these distinct and high-profile events.

The majority of the scandals were collected by searching through all major NGOs' historical campaigns with the key word: "bank." The results were then refined manually to identify cases that explicitly targeted deposit-taking commercial banks or their financed operations. Therefore, even though in certain cases the operations themselves might have been the focus of a campaign, the scandal is included in the dataset if the bank itself was extensively highlighted during the scandal. The reason for collecting this data primarily via campaign information (relative to news-specific search engines) is because it is more likely that a globally-significant scandal is reported by at least one of these institutions (i.e., the NGOs). News organizations or mainstream media do not have similarly strict mandates for covering such events. However, if these events are also covered by the mainstream media, this additional coverage will give us an indication of the severity of these scandals as well as the extent to which they had entered public awareness.

The scandals are classified into three overall categories: i) Tax Evasion, ii) Environment, and iii) Corruption. Tax evasion scandals are defined as events in which banks were specifically targeted and identified as conduits of evasion or money laundering practices. These include well known events such as *Panama Leaks* and *Lux Leaks*, which caused wide-spread reputational shocks to the banks involved. Environmental scandals include bank financed operations that were deemed controversial by the public for being harmful to the environment. These include events such as the *Dakota Access Pipeline* and the controversial *Carmichael Coal Mine* project in Queensland, Australia. These events often cover multiple issues including the violations of indigenous peoples' territorial rights, but more often they revolve around the destruction of the local habitat and the environment (e.g., deforestation and the pollution of local rivers). Corruption events are loosely defined as all other events not covered by tax evasion and environmental scandals. These are high profile cases associated with corporate malpractice in the banking community. Cases vary from well-known events, such as the *Libor Scandal* (illegal manipulation of interbank lending rates), to the provision of banking services to corrupt

government officials (e.g. President Omar al-Bashir of Sudan and James Ibori, former governor of Nigeria's oil-rich Delta State) and the conducting of business activities with sanctioned nations.

One concern for identifying scandals is determining the relevant dates in which the news broke out. In many cases, the start date is the date at which the event gained international attention and news coverage. For example, *Panama Leaks* was a well-publicized scandal by investigative journalists, who made sure the information spread globally at the precise moment that they released the leaked documents. However, in other cases, such as the Dakota Access Pipeline, events and global attention slowly progressed as the localized protests grew from month to month. In addition, these as well as other scandals, progressed with geographic heterogeneity, whereby the scandal was first recorded in one country and later spread to other localities. For every scandal in the dataset, a search was conducted for the first mention of the scandal (whether it be via the NGO or news). This first mention was then used as the initial date of the scandal. This was done by both manual searches as well as Factiva searches aided by major key words associated with each scandal (which also included bank names). The dataset also includes a "high impact" date for each scandal. This second date was most often the date at which multiple newspapers covered the event. This date was determined in multiple ways with the aid of Factiva as well as Google Trends. There were rarely any difficulties determining a fair date for the "high impact" date and fortunately, for most events, the start and high impact dates are in the same reporting quarter. As a result, the analysis will not rely on potentially subjective measures of high impact dates. Nonetheless, this study acknowledges the potential conflicts that this measure might create, and therefore the main analysis will only incorporate "start date" as an indication for scandal specific time lines.

It is important to clarify that these events are by no means mutually exclusive. There are certain cases where Tax Evasion and Corruption are not clearly separable, (for example, in the case of money laundering services provided to corrupt officials in developing countries). Therefore, our analysis will mainly incorporate the variable *Scandal*, for whether the bank experienced any of the three types of events. At later stages, the categorical information will be utilized to further examine whether certain types of scandals are associated with greater losses in deposit growth. As a final refinement of the data, the dataset excludes all events that were only reported by the NGOs (i.e., the events for which no obvious media reporting was found).

Bank level information has been collected from the *SNL Financial* database, which provides detailed and standardized data on financial institutions in the United States, Europe, Africa and the Middle East. Most importantly, the database covers banks on a quarterly basis, thus providing much needed granularity to the analysis. Compared to other standard databases, including *Bankscope* (which only provide financial data on an annual basis), this quarterly data will bring more confidence to the

empirical findings. In the analysis, I exclude all unconsolidated banks as well as banks that have zero total deposits. All the bank level controls and deposit information are available on a quarterly basis.[10] As standard controls, all empirical tests will account for *Total Assets*, *Total Equity / Total Assets*, *Non-performing Loans / Total Loans*, and *Return on Assets*.[11] In total, the analysis will include approximately 2,100 banks from 31 countries for the years 2010 - 2016.

After merging the scandal dataset with the bank level balance sheet information, in total, the analyses include 26 unique events, which translate to 140 bank-specific scandals (most scandals involve multiple banks). This provides the econometric analysis with 150 scandal specific quarter-bank observations for which there is also deposit data available[12].

The summary statistics of the key variables are presented in table 17, and table 18 presents the correlations between key variables included in the regressions. On average, quarterly deposit growth is 1.4% during the sample period. Deposit growth is negatively correlated with the size of the bank (*Total Assets*) and, as one might expect, negatively correlated with under-performing banks (*NPLs / Total Loans*). On a similar note, less risky (*Total Equity / Total Assets*) and more profitable (*ROAA*) banks are positively correlated with deposit growth.

1.5.1 Scandals and Deposit Growth

To further establish the effect of scandals on depositor behavior, I begin the analysis by estimating the following empirical specification:

$$DepositGrowth_{it} = \alpha_0 + \beta_1 Scandal_{it} + \lambda X_{it} + \varepsilon_{it} \quad (2)$$

Scandals are quarter specific and are assumed to affect all banking entities under the same holding structure. The dependent variable will be *Total Deposit Growth*, and I regress scandals on the quarter level as well as their lags to further document the short and medium term effects of these events.

[10] *Total Deposits* are total deposits from customers. For US banks, this is the total deposits from customers and banks.

[11] *Total Assets* are all assets owned by the company as of the date indicated, as carried on the balance sheet and defined under the indicated accounting principles. *Tota Equity / Total Assets* is equal to equity as a percent of assets. *NPLS / Loans* is nonperforming loans, net of guaranteed loans, as a percent of loans before reserves. Lastly, *ROAA* is the return on average assets; net income as a percent of average assets.

[12] These observations correspond to the scandals incorporated in the main analysis for years after 2009 and for banks operating in OECD countries. These restrictions are in place so that the results do not pick up any unintended effects caused by the 2008 financial crisis nor any other effects that might rise due to the limited sample representation from non-OECD countries. Overall, the second exclusion removes less than 21% of the available observations. Furthermore, it is noteworthy that an important control, namely *Non-performing Loans / Total Loans*, reduces the sample size significantly. Omitting this control would provide the analysis with 33 unique events, translating to 179 bank scandals and 271 scandal-quarter-bank observations. The main findings of this section remain unchanged after the exclusion of this bank level control.

Scandals are reported across three distinct events: tax evasion, corruption, and environment. However, the analysis will mainly incorporate the *Scandal* variable, which equals one if the bank incurred any of these three distinct events.

The main results are reported in table 12. Overall, the results show that scandals have a negative effect on *Total Deposit Growth*. Once banks are involved in scandals, on average, deposit growth decreases by 1.5 - 2.1 percentage points the following quarter after the scandal. The regressions incorporate winsorized *Total Deposit Growth* (at the 1st and 99th percentiles) as well as bank fixed effects. This inclusion is to address potential effects of outliers and bank specific determinants of deposit growth. In addition, the second column of each variable controls for country-year fixed effects. This control is important as it accounts for any country-year specific changes in deposit growth. As one might expect, both the significance and economic magnitude decrease in the following quarter after the scandal.[13]

[13] Additional tests show that interactions between scandals and bank financial health (i.e., *Total Equity / Total Assets* and *NPLs / Total Loans*) do not significantly affect the changes in deposit growth. This suggests that changes in deposit growth are more likely to be driven by non-financial determinants. Furthermore, additional tests regressing scandals on banks controls show that standard bank level characteristics are not clear determinants as to whether banks were involved in scandals. This suggests that these scandals can be treated more or less as exogenous events. While banks are currently differentiating themselves across various initiatives and social corporate strategies, historically speaking, these types of scandals can be treated as previously un-anticipated reputational shocks. These results are not reported in this paper for brevity.

Table 1-12: Scandals & Deposit Growth

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Scandal Start	-0.003	-0.004				
	(0.007)	(0.007)				
L. Scandal Start			-0.021***	-0.015**		
			(0.007)	(0.006)		
L2. Scandal Start					-0.005	-0.002
					(0.009)	(0.010)
L. TA (W.01)	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
L. TE / TA (W.01)	0.005***	0.005***	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
L. NPLs / TL (W.01)	-0.003***	-0.002***	-0.003***	-0.002***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
L. ROAA (W.01)	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	36,431	36,431	36,431	36,431	36,087	36,087
Number of Banks	2,110	2,110	2,110	2,110	2,102	2,102
Countries	OECD	OECD	OECD	OECD	OECD	OECD
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No
Country*Year FE	No	Yes	No	Yes	No	Yes
Quarter Num FE	Yes	Yes	Yes	Yes	Yes	Yes
Years	>2009	>2009	>2009	>2009	>2009	>2009

Notes: This table shows the effect of scandals on bank level deposit growth. The variable “Scandal Start" takes a value of one for a specific quarter if the bank was involved in either a corruption, tax evasion or environmental scandal. The dependent variable is annual bank level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in 17. Standard errors are in parentheses and clustered at the bank level. *** p0.01, ** p0.05, * p0.1

To further document the heterogenous effects of different types of scandals, table 13 reports the regression results for each individual type of scandal. The table shows that environmental scandals are on average the costliest for banks, causing a negative deposit growth rate of 2.7%. Tax Evasion scandals are less costly, but they also cause a significant decrease for deposit growth rates totaling 1.8%. Interestingly, corruption scandals are insignificant under these specifications. One major reason for this insignificance is that corruption scandals are very heterogenous. Compared to the other types of scandals, corruption scandals have often very different start and peak dates, with the international press sometimes taking over two years to cover these incidents. Because of these inherent difficulties, start dates are perhaps not the best dates for determining a treatment quarter; therefore, in table 19 in the appendix, I incorporate the “peak date" as the main treatment quarter observation. The results from this analysis also show that corruption scandals can create significant depositor movement, but determining the precise impact dates can be challenging.

Table 1-13: Individual Scandals & Deposit Growth

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
L. Tax Evasion Start	-0.018*			-0.022**
	(0.010)			(0.011)
L. Corruption Start		0.014		0.016
		(0.011)		(0.012)
L. Environment Start			-0.027***	-0.026***
			(0.010)	(0.010)
L. TA (W.01)	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
L. TE / TA (W.01)	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
L. NPLs / TL (W.01)	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
L. ROAA (W.01)	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	36,431	36,431	36,431	36,431
Number of Banks	2,110	2,110	2,110	2,110
Countries	OECD	OECD	OECD	OECD
Bank FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Quarter Num FE	Yes	Yes	Yes	Yes
Years	>2009	>2009	>2009	>2009

Notes: This table shows the effect of scandals on bank level deposit growth. The variables “Tax Evasion Start", “Corruption Start" and “Environment Start" takes a value of one for a specific quarter if the bank was involved in either a corruption, tax evasion or environmental scandal. The dependent variable is annual bank level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in 17. Standard errors are in parentheses and clustered at the bank level. *** p0.01, ** p0.05, * p0.1

1.5.2 Non-scandalous Competitors

While deposit growth decreases for banks involved in scandals, it is worthwhile to explore whether banks uninvolved in scandals are rewarded for their relatively ethical behavior. To do so, I create a dummy equal to one for each quarter-country pair if the bank is the largest bank by total deposits and has never been involved in scandals. I also create this country-pair dummy for banks who are not characterized as a systematically important bank (SIB). These country-pair dummies provide additional insight into whether large established banks also enjoy the effects of a positive non-scandalous reputation. The results in table 20 in the appendix show that banks who are not involved in scandals enjoy higher total deposit growth rates. In addition, they confirm that the effect holds for large banks and not just local banks, which often enjoy competitive advantages from close client relationships influenced by relationship banking and social capital (Brown et al., 2017; Ostergaard et al., 2015; Jin et al., 2017).

1.6 Conclusion

The purpose of this study is to document a new and novel channel of depositor discipline. By using US branch level deposit data, I find that banks who financed the controversial Dakota Access Pipeline had significantly lower deposit growth rates, especially when branches were located closest to the pipeline and in environmentally and socially conscious counties. I find that local savings banks were among the major beneficiaries of this deposit movement, which is also in line with prior evidence. Furthermore, by using a hand-collected dataset on global bank scandals, I find that deposit growth decreases when banks are involved in tax evasion, corruption, or environmental scandals. This finding is consistent with the disciplining and monitoring role of depositors, while extending to non-financial conditions (e.g., bank financial health). Lastly, I find that depositors, on average, reward larger banks who are not involved in these types of scandals.

While one may still argue that these scandals have had little impact on the balance sheets of these banks, a closer examination of each event has revealed surprising bank level operational changes. After the DAPL incident, several banks re-evaluated their commitments to the pipeline loan, after which many ended up in fully selling their stakes in the project. Managerial layoffs are not uncommon after scandals, and sometimes even the composition of the board has been put into question. As a reaction to other environmental scandals, Santander was quick to discontinue all financing to a company driving deforestation in Indonesia, and, after another similar incident, both the Commonwealth Bank of Australia and Standard Chartered pulled out from the Carmichael coal mine project. While it is difficult to truly identify how and to what extent the depositor channel influenced these operational decisions, it has certainly played a role in all of them.

The results from this study highlight the importance of depositor activism on bank fundamentals. Bank involvement in perceptually non-ethical activities not only requires regulatory oversight; depositors can have real impact as well. As financial institutions are increasingly being evaluated for their financial and non-financial activities, I show a surprising, yet important, disciplinary channel for bank behavior.

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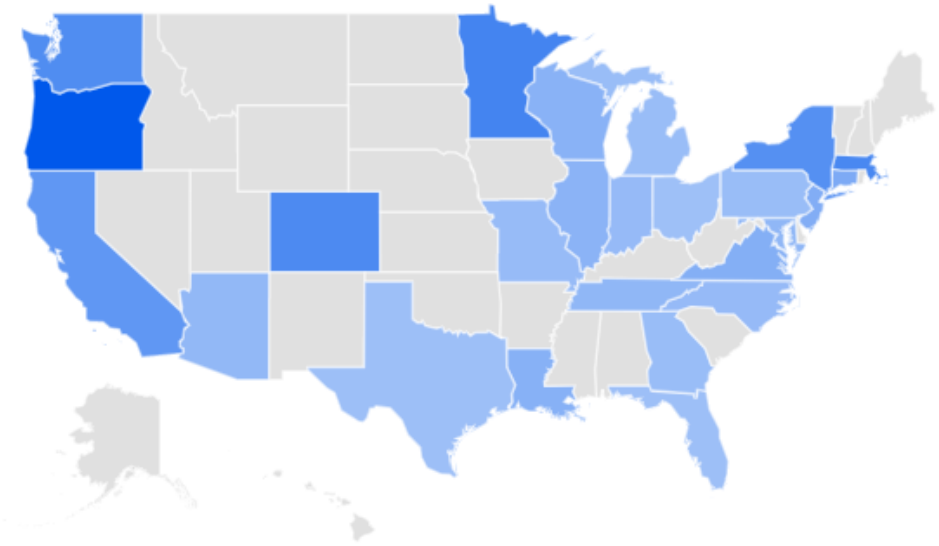
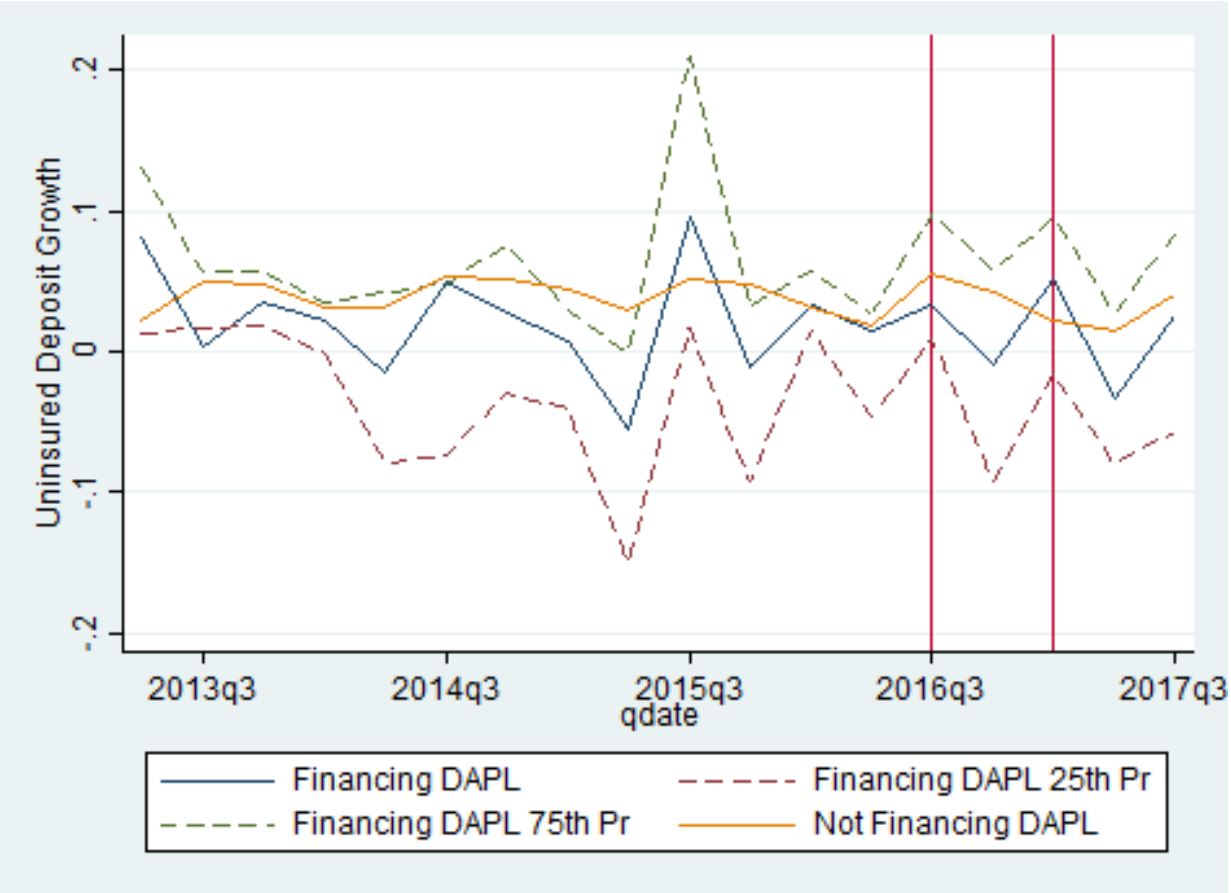
1.8 Appendix

Figure 1-3: Google Trends: “Donate to Standing Rock"



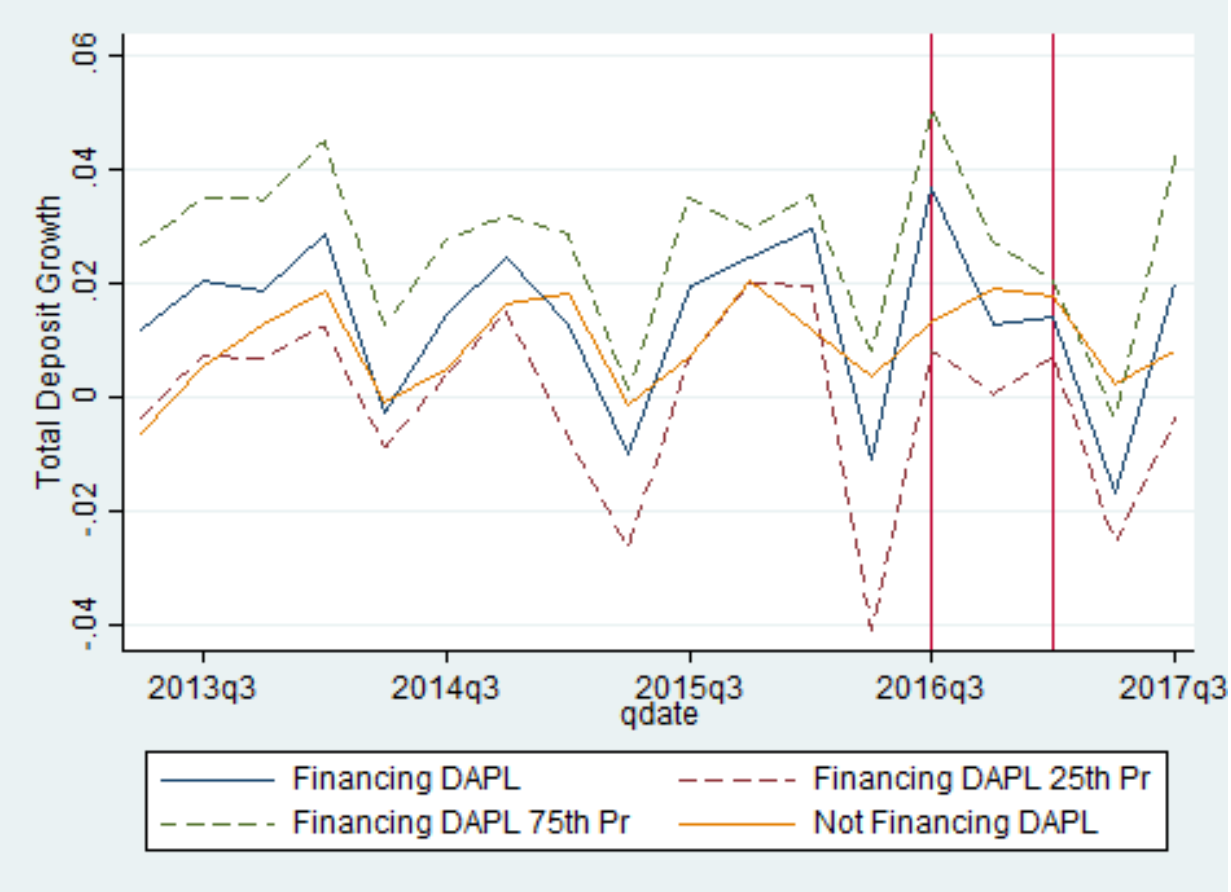
Notes: This figure shows the popularity of the search term “Donate to Standing Rock" across the US between 4/4/2016 and 11/9/2017. The darker the color, the more popular the search querie in *Google Search*.

Figure 1-4: Quarterly Uninsured Deposit Growth



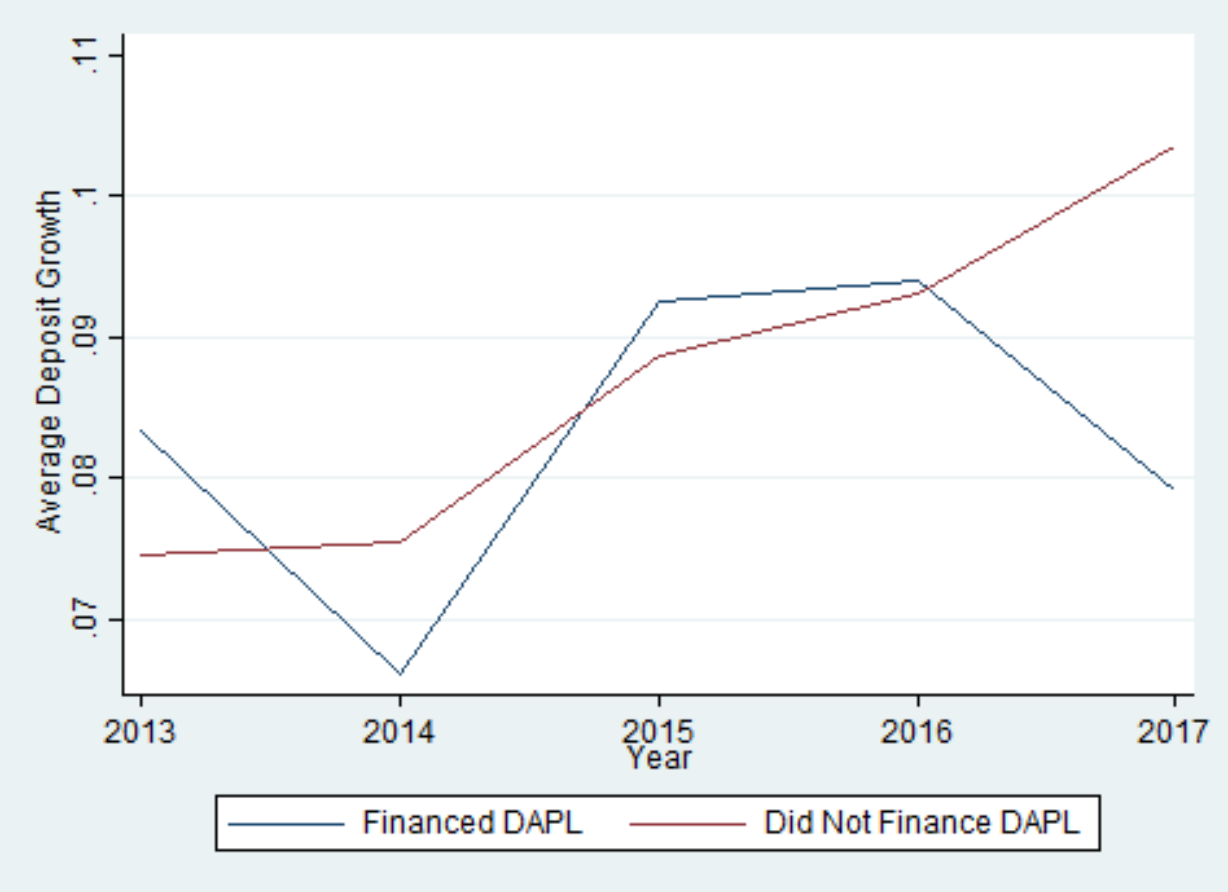
The graph includes uninsured deposit growth for banks who financed the DAPL and those who did not. The left red vertical line indicates the date at which banks were being targeted as a result of the DAPL scandal. The second vertical line indicates the date at which the 700,000 collective petition had come public. Uninsured deposit growth rates are winsorized at the 1% and 99% level.

Figure 1-5: Quarterly Deposit Growth



The graph includes deposit growth for banks who financed the DAPL and those who did not. Data for institutions that financed the DAPL include all institutions with $1 billion or more in total deposits and institutions that are associated with the majority of FDIC branches (e.g. the analysis excludes all cases where for example a Wells Fargo entity had only one branch). The left red vertical line indicates the date at which banks were being targeted as a result of the DAPL scandal. The second vertical line indicates the date at which the 700,000 collective petition had come public. Deposit growth rates are winsorized at the 1% and 99% level.

Figure 1-6: Annual Deposit Growth



This graph shows the pre-treatment trends of the treated and control group. The graph includes the mean deposit growth for banks who financed the DAPL and those who did not finance the DAPL at the annual level. This graph is made using the main FDIC branch level dataset used in all main analyses of this paper.

Table 1-14: Treatment Group Times Year Interactions

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth
Financed DAPL * 2016	0.020***	0.019***	0.019***	0.014***	0.015***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Financed DAPL * 2015	0.023***	0.017***	0.022***	0.015***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Financed DAPL * 2014	0.009***	0.009***	0.018***	0.009**	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Financed DAPL * 2013	0.026***	0.025***	0.036***	0.023***	0.022***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Financed DAPL	-0.041***	-0.040***			
	(0.002)	(0.002)			
Observations	416,594	416,594	416,513	412,557	411,930
Controls	No	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes	Yes
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	No	Yes	Yes	No
County*Year FE	No	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) on branch level deposit growth. The purpose of this test is to show that DAPL financing branches had on average, positive deposit growth rates before the year of the scandal (i.e. the base "Financed DAPL * 2017"). The variable “Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-15: DAPL & No Wells Fargo

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth
Financed DAPL * 2017	-0.024***	-0.023***	-0.031***	-0.022***	-0.021***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Financed DAPL	-0.006**	-0.006**			
	(0.002)	(0.002)			
Observations	387,056	387,056	386,975	383,094	382,411
Controls	Yes	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes	Yes
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	Yes	Yes	Yes	No
County*Year FE	No	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) on branch level deposit growth without including Wells Fargo branches in the sample. The variable “Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-16: DAPL, Savings Banks & Credit Unions - Full Sample Analysis

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
	With Credit Unions		Without Credit Unions	
Savings Bank * 2017 * Proportion of DAPL Banks	0.111***	0.124***	0.122**	0.147***
	(0.022)	(0.024)	(0.048)	(0.052)
Savings Bank * 2017	-0.008***	-0.003	-0.003	-0.010*
	(0.003)	(0.003)	(0.005)	(0.006)
Savings Bank * Proportion of DAPL Banks	0.018	0.222***	0.018	0.260***
	(0.031)	(0.068)	(0.033)	(0.078)
Observations	446,483	442,521	415,896	411,935
Controls	Yes	Yes	Yes	Yes
Branch FE	No	Yes	No	Yes
Bank FE	Yes	No	Yes	No
Year*County FE	Yes	Yes	Yes	Yes

Notes: This table shows the effect of the Dakota Access Pipeline (DAPL) on branch level deposit growth for savings banks and credit unions. The variable “Savings Bank" takes a value of one if the branch is classified as a savings bank or a credit union. The variable “Proportion of DAPL Banks" is equal to the percentage of DAPL financing branches at the county level. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-17: Summary Statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max	(6) p25	(7) p50	(8) p75
Total Deposit Growth (Winsor .01)	36,431	0.0139	0.0672	-0.196	0.486	-0.0156	0.00753	0.0339
Total Assets (Winsor .01)	36,431	2.102e+07	7.271e+07	36,885	4.717e+08	301,862	856,106	4.824e+06
Total Equity / Total Assets (Winsor .01)	36,431	10.12	3.865	2.510	35.14	8.029	9.805	11.76
NPLS / Total Loans (Winsor .01)	36,431	3.500	3.648	0	17.62	1.106	2.334	4.470
ROAA (Winsor .01)	36,431	0.469	1.176	-6.183	3.897	0.261	0.633	0.963

Table 1-18: Correlation Table

	Total Deposit Growth	Total Assets	Total Equity / Total Assets	NPLS / Total Loans	ROAA
Total Deposit Growth	1				
Total Assets	-0.0385***	1			
Total Equity / Total Assets	0.141***	-0.203***	1		
NPLS / Total Loans	-0.155***	0.0149**	-0.144***	1	
ROAA	0.0868***	0.00963	0.208***	-0.391***	1

*** p0.01, ** p0.05, * p0.1

Table 1-19: Corruption Scandals & Deposit Growth

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Corruption Peak	-0.007			-0.001
	(0.008)			(0.012)
L. Corruption Peak		0.003		-0.008
		(0.010)		(0.009)
L2. Corruption Peak			-0.030**	-0.031**
			(0.014)	(0.015)
L. TA (W.01)	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
L. TE / TA (W.01)	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
L. NPLs / TL (W.01)	-0.003***	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
L. ROAA (W.01)	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	36,431	36,431	36,087	36,087
Number of Banks	2,110	2,110	2,102	2,102
Countries	OECD	OECD	OECD	OECD
Bank FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Quarter Num FE	Yes	Yes	Yes	Yes
Years	>2009	>2009	>2009	>2009

Notes: This table shows the effect of scandals on bank level deposit growth. The variable “Corruption Peak" takes a value of one for a specific quarter if the bank was involved in a corruption scandal. The quarter is determined as the peak date involving publicity of the scandal. The dependent variable is annual bank level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in 17. Standard errors are in parentheses and clustered at the bank level. *** p0.01, ** p0.05, * p0.1

Table 1-20: Competitor Deposits

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Largest Non-Scandalous Bank	0.031***	0.025***		
	(0.009)	(0.009)		
Largest Non-Scandalous Bank (not SIB)			0.028***	0.022**
			(0.009)	(0.009)
L. TA (W.01)	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
L. TE / TA (W.01)	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
L. NPLs / TL (W.01)	-0.003***	-0.002***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
L. ROAA (W.01)	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	36,431	36,431	36,431	36,431
Number of Banks	2,110	2,110	2,110	2,110
Countries	OECD	OECD	OECD	OECD
Bank FE	Yes	Yes	Yes	Yes
Country FE	No	No	No	No
Year FE	Yes	No	Yes	No
Country*Year FE	No	Yes	No	Yes
Quarter Num FE	Yes	Yes	Yes	Yes
Years	2009	2009	2009	2009

Notes: This table shows the effect of being a non-scandalous bank on deposit growth. The variable "Largest Non-Scandalous Bank" is a dummy equal to one for each quarter-country pair if the bank is the largest bank by total deposits and has never been involved in scandals. The second variable is a similar country-pair dummy for banks who are not characterized as a systematically important bank (SIB). The dependent variable is annual bank level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in 17. Standard errors are in parentheses and clustered at the bank level. *** p0.01, ** p0.05, * p0.1

Table 1-21: Climate Change Beliefs Using 2014 YPCC Values

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Happening 70 * Financed DAPL * 2017	-0.004	-0.017**	-0.019***			
	(0.007)	(0.007)	(0.007)			
Happening 70 * 2017	-0.008***	-0.010***	-0.012***			
	(0.003)	(0.003)	(0.004)			
Happening 70 * Financed DAPL	0.013***	-0.050	-0.062			
	(0.004)	(0.051)	(0.051)			
Financed DAPL * 2017	-0.023***	-0.014***	-0.012***			
	(0.003)	(0.003)	(0.003)			
Happening 70	-0.002	0.021	0.028			
	(0.002)	(0.042)	(0.043)			
Human 55 * Financed DAPL * 2017				-0.001	-0.015**	-0.016**
				(0.006)	(0.007)	(0.007)
Human 55 * 2017				-0.012***	-0.016***	-0.016***
				(0.003)	(0.003)	(0.004)
Human 55 * Financed DAPL				0.004**	-0.045	-0.054
				(0.002)	(0.043)	(0.042)
Financed DAPL * 2017				-0.024***	-0.013***	-0.012***
				(0.003)	(0.003)	(0.003)
Human 55				-0.000	0.007	0.011
				(0.002)	(0.042)	(0.043)
Percent of Adults With a Bachelor's Degree or Higher	0.002***	0.001	0.000	0.002***	0.001	0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Percent of Votes for Obama	0.000	0.002	0.002	0.000	0.002*	0.002
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Population 2014	0.000***	-0.000	-0.000	0.000***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	400,026	396,187	396,187	398,980	395,158	395,158
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	No	No	Yes	No	No
Branch FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE	No	No	Yes	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and environmental awareness on branch level deposit growth. "Happening 70" is equal to 1 if at least 70% of the county thinks that global warming is happening in 2014. "Human 55" is equal to 1 if at least 55% of the county thinks global warming is caused mostly by human activities in 2014. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1.

Table 1-22: Alternative Google Trends Search Queries

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Ln(Standing Rock Indian Reservation) * Financed DAPL * 2017	-0.026***	-0.033***	-0.025***			
	(0.005)	(0.006)	(0.005)			
Ln(Standing Rock Indian Reservation) *	-0.000	0.001	-0.002			
	(0.002)	(0.002)	(0.002)			
Ln(Standing Rock Indian Reservation) * Financed DAPL	0.023***	-0.057**	0.025***			
	(0.003)	(0.023)	(0.003)			
Ln(Standing Rock Indian Reservation)	-0.013***	0.089	-0.014***			
	(0.001)	(0.064)	(0.002)			
Ln(Dakota Access Pipeline) * Financed DAPL * 2017				-0.034***	-0.039***	-0.033***
				(0.006)	(0.006)	(0.006)
Ln(Dakota Access Pipeline) * 2017				-0.007***	-0.006**	-0.009***
				(0.002)	(0.002)	(0.002)
Ln(Dakota Access Pipeline) * Financed DAPL				0.021***	-0.062**	0.028***
				(0.003)	(0.025)	(0.004)
Ln(Dakota Access Pipeline)				-0.009***	0.140**	-0.017***
				(0.002)	(0.059)	(0.003)
Financed DAPL * 2017	0.040***	0.061***	0.034***	0.044***	0.058***	0.039***
	(0.012)	(0.014)	(0.013)	(0.011)	(0.013)	(0.011)
Financed DAPL	-0.072***			-0.059***		
	(0.007)			(0.006)		
Percent of adults with a bachelor's degree or higher, 2012-2016	0.002***	0.001	0.002***	0.002***	0.001	0.002***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Percent of Votes for Obama	-0.000***	0.002	0.000	-0.000***	0.002	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Population 2014	0.000***	-0.000	0.000***	0.000***	-0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	400,110	396,187	400,026	400,110	396,187	400,026
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	No	Yes
Branch FE	No	Yes	No	No	Yes	No
State FE	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and public opinion on branch level deposit growth. "Standing Rock Indian Reservation" and "Dakota Access Pipeline" is equal to the search intensity of the Google search term using Google Trends. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-23: Google Trends Search Queries with Additional Fixed Effects

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Ln(Donate to Standing Rock) * Financed DAPL * 2017	-0.006***			
	(0.002)			
Ln(Donate to Standing Rock) * Financed DAPL * 2017		-0.017***		
		(0.006)		
Ln(Standing Rock Indian Reservation) * Financed DAPL * 2017			-0.020**	
			(0.008)	
Ln(Dakota Access Pipeline) * Financed DAPL * 2017				-0.020**
				(0.009)
Percent of adults with a bachelor's degree or higher, 2012-2016	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Percent of votes for Obama	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Population 2014	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	388,573	310,511	388,573	388,573
Controls	Yes	Yes	Yes	Yes
Branch FE	Yes	Yes	Yes	Yes
State*Year FE	Yes	Yes	Yes	Yes
Bank*Year FE	Yes	Yes	Yes	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and public opinion on branch level deposit growth. The variables are equal to the search intensity of the Google search term using Google Trends. The variable "Financed DAPL" takes a value of one if the branch financed the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-24: DAPL States Full Sample Analysis and Extended Treatment Group

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth
Financed DAPL * 2017 * DAPL State	-0.027***	-0.032***	-0.030***	-0.041***
	(0.007)	(0.007)	(0.007)	(0.009)
Financed DAPL * 2016 * DAPL State	-0.030***	-0.031***	-0.027***	-0.035***
	(0.007)	(0.007)	(0.007)	(0.009)
Financed DAPL * 2017	-0.013***	-0.011***	-0.015***	-0.001
	(0.002)	(0.002)	(0.003)	(0.003)
Financed DAPL * 2016	-0.013***	-0.015***	-0.017***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.003)
Finance DAPL * DAPL State	0.034***	0.036***	0.013**	-0.019
	(0.005)	(0.005)	(0.006)	(0.016)
Financed DAPL	-0.001	-0.001		
	(0.002)	(0.002)		
Observations	416,594	416,594	416,513	411,930
Controls	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes
Bank FE	No	No	Yes	No
State FE	Yes	No	No	No
Year FE	Yes	No	No	No
State*Year FE	No	Yes	Yes	No
County*Year FE	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and proximity on branch level deposit growth. The variable “DAPL State" takes a value of one if the branch is located in one of the pipeline states. The variable “Financed DAPL" takes a value of one if the branch provided project or corporate financing to the Dakota Access Pipeline. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-25: Climate Change Beliefs and Extended Treatment Group

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth
Happening 70 * Financed DAPL * 2017	-0.003	-0.002	-0.001			
	(0.004)	(0.004)	(0.004)			
Happening 70 * 2017	-0.003	-0.007***	-0.006**			
	(0.002)	(0.003)	(0.003)			
Happening 70 * Financed DAPL	-0.006**	-0.048***	-0.046**			
	(0.002)	(0.018)	(0.018)			
Financed DAPL * 2017	-0.008***	0.001	0.001			
	(0.003)	(0.003)	(0.003)			
Happening 70	0.004**	-0.013	-0.013			
	(0.002)	(0.027)	(0.027)			
Human 55 * Financed DAPL * 2017				-0.003	-0.004	-0.004
				(0.004)	(0.004)	(0.004)
Human 55 * 2017				-0.004	-0.010***	-0.009**
				(0.003)	(0.003)	(0.003)
Human 55 * Financed DAPL				-0.001	-0.054**	-0.052**
				(0.002)	(0.021)	(0.021)
Financed DAPL * 2017				-0.008***	0.002	0.003
				(0.003)	(0.003)	(0.003)
Human 55				0.005**	-0.013	-0.012
				(0.002)	(0.032)	(0.032)
Percent of Adults With a Bachelor's Degree or Higher	0.002***	0.001	0.001	0.002***	0.001	0.001
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Percent of Votes for Obama	0.000	0.002*	0.002*	-0.000	0.002*	0.002*
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Population 2014	0.000***	-0.000	-0.000	0.000***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	398,980	395,158	395,158	398,980	395,158	395,158
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	No	No	Yes	No	No
Branch FE	No	Yes	Yes	No	Yes	Yes
State FE	No	No	No	No	No	No
Year FE	Yes	Yes	No	Yes	Yes	No
State*Year FE	No	No	Yes	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and environmental awareness on branch level deposit growth. "Happening 70" is equal to 1 if at least 70% of the county thinks that global warming is happening in 2014. "Human 55" is equal to 1 if at least 55% of the county thinks global warming is caused mostly by human activities in 2014. The variable "Financed DAPL" takes a value of one if the branch provided project or corporate financing to the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1.

Table 1-26: Charitable Behavior and Extended Treatment Group

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth	(6) Deposit Growth	(7) Deposit Growth
Ln(Number of Non-Profits) * Financed DAPL * 2017	-0.003***	-0.002*	-0.001	-0.003**	-0.003**	-0.001	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Ln(Number of Non-Profits) * 2017	-0.001	-0.000	0.001	-0.003***	0.000	-0.003***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Number of Non-Profits) * Financed DAPL	-0.001	-0.001	-0.004***	-0.007***	-0.004***	-0.006***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Financed DAPL * 2017	0.016*	0.007	0.001	0.023**	0.011	0.011	
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	
Ln(Number of Non-Profits)	0.010***	0.008***	0.009***	0.029*	0.010***	0.029*	0.009***
	(0.001)	(0.001)	(0.001)	(0.016)	(0.001)	(0.016)	(0.001)
Financed DAPL	-0.000	0.001					
	(0.006)	(0.006)					
Percent of Adults With a Bachelor's Degree or Higher	0.001***	0.001***	0.001***	-0.001	0.001***	-0.001	0.001***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Percent of Votes for Obama	-0.000***	-0.000***	-0.000	0.001	-0.000***	0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Population 2014	0.000	0.000	-0.000	-0.000**	0.000***	-0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	400,110	400,110	400,026	396,187	400,026	396,187	392,560
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	Yes	No	No
Branch FE	No	No	No	Yes	No	Yes	No
State FE	No	No	No	No	No	No	No
Year FE	Yes	No	No	Yes	Yes	No	No
State*Year FE	No	Yes	Yes	No	No	Yes	Yes
Bank*Year FE	No	No	No	No	No	No	Yes

Notes: This table shows the effect of financing the Dakota Access Pipeline (DAPL) and charitable behavior on branch level deposit growth. The variable "Number of Non-Profits" is equal to the number of domestically focused non-profits at the county level. The variable "Financed DAPL" takes a value of one if the branch provided project or corporate financing to the Dakota Access Pipeline. The variable "2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

Table 1-27: Savings Banks and Extended Treatment Group

VARIABLES	(1) Deposit Growth	(2) Deposit Growth	(3) Deposit Growth	(4) Deposit Growth	(5) Deposit Growth
Savings Bank * 2017 * Proportion of DAPL Banks	0.058***	0.054***	0.038*	0.078***	0.060
	(0.020)	(0.020)	(0.021)	(0.026)	(0.038)
Savings Bank * 2017	-0.012**	-0.009	-0.005	-0.020***	
	(0.006)	(0.006)	(0.006)	(0.007)	
Savings Bank * Proportion of DAPL Banks	-0.054***	-0.053***	0.009	0.104***	
	(0.013)	(0.013)	(0.016)	(0.023)	
Proportion of DAPL Banks * 2017	-0.028***	-0.025***	-0.014**		
	(0.004)	(0.006)	(0.007)		
Savings Bank	-0.001	-0.001	0.016*		
	(0.004)	(0.004)	(0.009)		
Proportion of DAPL Banks	0.088***	0.087***	0.072***		
	(0.004)	(0.004)	(0.004)		
Observations	416,594	416,594	416,513	411,930	408,123
Controls	Yes	Yes	Yes	Yes	Yes
Branch FE	No	No	No	Yes	No
Bank FE	No	No	Yes	No	No
State FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
State*Year FE	No	Yes	Yes	No	No
Year*County FE	No	No	No	Yes	Yes

Notes: This table shows the effect of the Dakota Access Pipeline (DAPL) on branch level deposit growth for savings banks. The variable “Savings Bank" takes a value of one if the branch is classified as a savings bank. The variable “Proportion of DAPL Banks" is equal to the percentage of DAPL project and corporate financing branches at the county level. The variable “2017" is a dummy variable indicating the year of the DAPL scandal. The dependent variable is annual branch level deposit growth winsorized at the 1st and 99th percentiles. Fixed effects are reported in the bottom of the table and controls are summarized in Table 1. Standard errors are in parentheses and clustered at the branch level. *** p0.01, ** p0.05, * p0.1

2. UNIVERSAL CORPORATE GOVERNANCE

Mikael Homanen and Hao Liang [14]

ABSTRACT

A widely accepted principle in finance is that good corporate governance is associated with higher firm value. However, what is "good governance" and whether the same set of good governance practices can be universally adopted are fiercely debated. In this paper, we construct various measures of firm- and country-level corporate governance, including a "global entrenchment index". We then test their relation with firm value on a large sample of more than 20,000 firms across 47 countries. We find substantial heterogeneity in the relation between ***some*** governance practices—especially those related to corporate rules constraining insider entrenchment—and firm value across countries, which is contingent on firms' ownership structure and institutional environments. In contrast, higher institutional ownership is unconditionally correlated with higher firm valuation. Our results cast doubt on the universality of rule-based corporate governance practices.

Keywords: corporate governance, entrenchment index, institutional ownership, institutional environment, contingency

JEL Classifications: G30, G34, G23

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2.1 Introduction

Policymakers and investors around the world have been increasingly emphasizing the importance of effective corporate governance systems. Corporate governance refers to the structure of rights and responsibilities among the parties with a stake in the firm (Aoiki, 2001; Aguilera et al., 2008). Effective corporate governance uses mechanisms to align the interests of managers and of the company's shareholders and various stakeholders for them to act responsibly regarding the protection, generation, and distribution of wealth invested in the firm. It has been widely recognized that good corporate governance with regard to shareholder protection can lead to higher shareholder value and more efficient capital allocation (Gompers et al., 2003; Bebchuk et al., 2008), which are in turn associated with better economic outcomes (La Porta. Lopez-de-Silanes, Shleifer, Vishny, 1997, 1998, 2002; La Porta. Lopez-de-Silanes, Shleifer, 2008). There is also a considerable amount of evidence that governance practice can spill over around the world (Aggarwal, Erel, Stulz, Williamson, 2009; Aggarwal, Erel, Ferreira, Matos, 2011; Albuquerque, Brandao Marques, Ferreira, Matos, 2018). Despite the widespread recognition and spillover of governance practice, a central issue in this literature is the extent to which "good" governance practices are universal (one size mostly fits all) or instead depend on country and firm characteristics (Black, De Carvalho, Gorga, 2012). The latter refers to the interdependencies between organizations and diverse environments which may determine governance effectiveness (Aguilera & Jackson, 2003; Aguilera et al., 2008). For long, researchers have been arguing that there are systematic differences in corporate governance structures and practices across countries, which are related to law (La Porta et al., 1997, 1998), political institutions (Roe, 2003; Pagano and Volpin, 2005; Perotti and von Thadden, 2006), cultures and social norms (Stulz and Williamson, 2003), economic and financial development (Doidge, Karolyi, and Stulz, 2007) as well as other institutional factors (Aguilera, Filatotchev, Gospel & Jackson, 2008; Aguilera & Jackson, 2010). The voluminous studies on comparative corporate governance have also generated debates on which type of governance is "superior" in protecting investor rights and maximizing firm value, and whether there is an "end of history" for corporate law and corporate governance (Hansmann and Kraakman, 2001).[15]

[15] Hansmann and Kraakman (2001) argue that there is a strong likelihood of convergence toward a single governance model, and assert that the basic corporate form has already achieved a high degree of uniformity. They find that governance systems in Germany, Japan, and the U.S. show signs of convergence toward each other (e.g., German and Japanese firms are increasingly adopting a single-tier board model). The significant international evidence of privatizations further represents a move toward private ownership that characterizes the world's major economies. These developments suggest that the role of corporate governance may be conditioned by a range of outside factors, however, others have approached the debate assuming there exist objective standards. For example, Aggarwal, Erel, Stulz, and Williamson (2009) consider the United States as having very strong protection for investors and property rights and suggest that the internal governance of firms in the US should be the benchmark for all firms around the world if it were not constrained by weaker institutions and lower development.

These academic debates have triggered a moving landscape globally: in the past few decades, corporate governance reforms have taken place around the world. These reforms are mostly towards the Anglo-American governance model, featured by high proportion of independent directors on the board, elimination of provisions empowering managers, active market for corporate control, equity- and option-based compensation structure, dispersed ownership structure, and strong role of external governance forces such as auditors, analysts and institutional investors (Khanna, Kogan and Palepu, 2002; Denis and McConnell, 2003).[16] These developments have created an implicit quest for a single and universally "best" set of corporate governance practices. In turn, this has profoundly influenced decisions of policymakers and corporate executives.[17] Meanwhile, we have also witnessed the advent of new governance forms, such as the rise of sovereign wealth funds and the rise in investor activism around the world (Becht et al., 2017; Bebchuk et al., 2015; Dimson et al., 2015; also see an overview by Bebchuk and Weisbach (2010)). As the landscapes and corporate ownership and governance keep evolving (Bebchuk et al., 2017), it becomes even more challenging to formalize what good governance practices are. The governance issues today are no longer only about addressing agency problems between corporate managers and shareholders that result from the dispersion of ownership in large publicly traded corporations. These developments are further changing the way firms are governed and therefore will demand a closer inspection and re-evaluation of universality and standardization of good governance practices.

To enhance our current debate, understanding the *how* and *when* questions of corporate governance is of important policy relevance. Bebchuk & Hamdani (2009) argue that the quest for a universal set of global corporate governance standards is misleading, as the effectiveness of governance mechanisms crucially depend on companies' ownership structures. Ownership can vary immensely across countries and corporations (La Porta et al., 1999) and mechanisms that protect outside investors in a company without a controlling shareholder are often irrelevant or even harmful when it comes to investor protection. Even within the same company, Cremers et al., (2015) argue that different

[16] These mechanisms are in place to keep managers and corporate insiders on check, usually with the firm's financial performance as the benchmark (a practice commonly referred to as financialized corporate governance; see Admati (2017)).

[17] For example, corporations in China traditionally adopted the German-style corporate law and governance systems under the German civil law, such as a two-tier board structure and a significant influence of the government and banks. The Chinese reform has been moving toward the Anglo-American governance model, featured by a single-tier board with significant portion of independent directors. According to a recent Corporate Governance Report by MSCI, which uses the Anglo-American governance metrics as a benchmark for "good governance," Alibaba Group Holdings, one of the most valuable companies in China and in the world, is also considered as one of the worst-governed public companies in China due to its dual-class shares structure and the significant influence of the founder (See MSCI: Corporate Governance in China (September 2017): https://www.msci.com/documents/10199/1d443a3d-0437-4af7-aa27-ada3a2655f6d).

governance mechanisms can function in distinct ways under different circumstances.[18] The notion of a single set of criteria to evaluate firm-level governance around the world, i.e., a "universal corporate governance model," is undoubtedly appealing. Institutional Shareholder Services (ISS; formerly RiskMetrics) have issued guidance and metrics based on the renowned G-index (Gompers et al., 2003), E-index (Bebchuk et al., 2009), and the *GOV* index (Aggarwal et al., 2009) for institutional investors and companies worldwide to evaluate their corporate governance "quality." However, extant studies often focus on the classic agency theory, i.e., the separation of ownership (shareholders) and control (managers), while largely neglect the diversity of its institutional contexts. By contemplating differing origins of the systems, one can also appreciate what determines different forms of governance. The contingencies that affect governance outcomes are debatable, yet surprisingly, there exist only few studies investigating these topics (e.g., Pagano & Volpin, 2005; Griffin et al., 2017; Doidge et al., 2007). Motivated by all these developments, we try to empirically answer the following two questions in this paper. Are some sets of corporate governance practice universally effective across the world? If some governance practices are not unconditionally applicable, under what conditions are they effective?

To address these questions, we assemble information from all major international corporate governance databases, including RiskMetrics (ISS), Thomson Reuters, MSCI, and Factset, among others, and test their relation with firm value on a large sample of more than 20,000 firms across 47 countries. Empirically, to investigate the (non-)existence of a universal corporate governance model, we consider a few sets of "good governance" from the perspectives of corporate rules, ownership structure, and investor legal protection. In particular, we primarily focus on corporate rules which are more implementable and self-constructed a "global entrenchment index" (Global E-index) as our primary proxy for the (lack of) rule-based governance. We then compare it with the governance indices in existing studies (mostly based on US samples for firm-level governance) and the aggregate indices provided by major governance data providers (e.g., ISS Governance and MSCI Governance Metrics).[19] We find the relation between rule-based governance practices and firm value indeed vary substantially across countries. In many subsamples that are well studied in the literature (e.g., common law, French civil law, Western Europe, East Asia, developing counties, etc.) the results are simply not there. This result casts serious doubts on the existence of a universal corporate governance model, thus the universality of a standard set of corporate governance practices around the world. We further find that

[18] For example, staggered boards and other "entrenchment mechanisms" can in fact be value-enhancing. Meanwhile, as Goshen & Hannes (2018) have observed, the compositional shift in equity markets from retail to institutional ownership has relocated regulatory power over corporations from courts to markets.

[19] As explained in greater details in the next section, we argue that using an aggregate firm-level index containing various aspects of governance issues is by nature problematic, primarily because these different dimensions do not function to enhance investor protection in a linear way, but their effects intervene with each other. What's more problematic is that the role of governance varies significantly across countries and jurisdictions with dramatically different institutional environments, and the resulting ownership structures of public corporations. Separating their effects by different dimensions of governance and by different institutional backgrounds is not only an important consideration but also a necessity.

the effect of rule-based governance (i.e., Global E-index) on *q* is contingent on ownership structure (e.g., institutional ownership) and institutional environments (e.g., legal protections and political systems). In particular, its effect becomes stronger in firms with greater independent institutional ownership, stronger investor protection and rule of law, and under left-wing and majoritarian political regimes. In contrast, we find that (independent) institutional ownership is conditionally associated with higher *q* across all subsamples, consistent with the recent advocates on the unique role of institutional investors' activism in dealing with agency problems in their portfolio companies. This is further supported by our analysis on the voting outcomes of governance-related shareholder proposals during shareholder meetings under a regression discontinuity framework for global companies. We find that even within a small margin, the passage of corporate governance proposals which are usually sponsored by institutional shareholders is associated with positive stock market reactions.

We aim to make significant contributions to the academic literature and to policy debates. From the academic perspective, there have been numerous papers documenting the important of governance, and a standard set of implementable governance practices have been set by Gompers, Ishii, Metrick (2003) and Bebchuk, Cohen, Ferrell (2009) for U.S. firms. However, as argued by Black, De Carvalho, Gorga (2012), we still know relatively little about factors influence both which aspects of governance predict firm market value (Tobin's *q*) and for which firms. Beyond firm-level rule-based governance, which has been the focus of most governance studies, we also find that country (institutional) ownership structure and country-level minority shareholder rights all play a strong governance role. Even for the firm-level rule-based governance, we provide systematic evidence that its effect on firm valuation hinges on other external governance mechanisms and institutional environments. Although our empirical investigation is exploratory in nature, it extends the scope of empirical corporate governance research and can enhance the understanding of governance mechanisms at the theoretical level.

From the policy perspective, it is well known that a firm's governance is chosen by those who control the firm to maximize their welfare (Aggarwal, Erel, Stulz, Williamson, 2009). Historically, conflicts over corporate control in the U.S. frequently originated from hostile takeover attempts (Goshen & Hannes, 2018). Outside the U.S., control usually took the form of ownership concentration (e.g., in the hands of wealthy families or of the state) as well as regulations and laws. Such difference in historically-determined control arrangement implies that regulators should take different approaches of corporate governance (not merely legal rules versus comply-or-explain), instead of questing for global governance standards, as advocated by rating agencies (e.g., ISS) and international institutions (e.g., World Bank), but criticized by Bebchuk & Hamdani (2009). Our results using international sample and self-constructed global governance indices support this view and will have vast implications for the ongoing corporate governance reforms around the world.

2.2 Data

Before diving into details of the data, we begin this section by acknowledging and slightly elaborating on the past developments of corporate governance data collection efforts and the overall trend. To date, there have been a handful of international corporate governance metrics constructed by researchers and institutions. For example, the ISS measures on governance provide data on classic takeover defenses and other corporate governance provisions, including classified boards, cumulative voting, golden parachutes, poison pills, takeover laws, etc. However, ISS stopped providing global governance measures in 2008, and has recently developed a Governance Quality Score, which ranks global companies from 1 to 10 based on assessed risks in the areas of board structure, compensation programs, shareholder rights, and audit & risk oversight. Other international metrics include the extensive *GOV* measure by Aggarwal et al. (2009), which includes 41 governance attributes and is based on the ISS metrics. Another main dataset on firm-level corporate governance is from MSCI Governance Metrics, which is based on 96 unique governance and accounting metrics, organized into four individual scoring themes. They too provide a weighted-average aggregate score, *GOVSCORE*, based on their metrics. Finally, some researchers have begun to utilize the firm-level environmental, social, and corporate governance (ESG) ratings and extract the governance (G) component of ESG as a measure of corporate governance. For example, Albuquerque, Brandao-Marques, Ferreira, and Matos (2018) who constructed a governance measure using 16 governance attributes obtained from the Governance category of the Bloomberg ESG database.

While it is tempting to aggregate information from all these data sources into one single corporate governance measure—it is theoretically unclear why various dimensions can and should be combined in a linear way to capture governance quality. In addition, in the case of MSCI's *GOVSCORE*, its weighted-average composition is largely a "blackbox" to outside researchers. In fact, according to Bebchuk et al. (2009), most governance provisions are not strongly relevant to firm value, or at least are partly the endogenous product of the allocation of power within a firm set by other provisions. What's more worrying is that firm performance is usually hardwired into the aggregate score (as an "economic" dimension) when constructing such a comprehensive governance rating (e.g., *GOVSCORE*) by ESG data providers. Indeed, our conversations with various data providers confirm that this is a serious concern from researchers' point of view. Given all these theoretical and practical concerns, Bebchuk et al. (2009) caution against the "kitchen sink" approach of building ever-larger indexes of governance measures. Practically, the development and use of these indexes has put pressure on firms to adjust their governance arrangements in ways that would improve their index scores, which help them attract more institutional investors.

We concur with Bebchuk et al. (2009) and argue that there is significant value for using an index that is parsimonious and that investors care most about. The components of the Entrenchment Index (E-Index), which they constructed and are widely used in finance research, were motivated by the substantial sentiment of institutional investors. This is consistent with the observation that conflicts over corporate control in the U.S. frequently originated from hostile takeover attempts (Goshen & Hannes, 2018). However, "entrenchment" by managers, directors, and large shareholders are not only a US phenomenon, but has also been frequently mentioned as a major corporate governance issue around the world (e.g., Claessens, Djankov, Fan and Lang, 2002).

We have tackled an extensive data collection process to explore corporate governance to the fullest extent possible. To our knowledge, we are the first to assemble a large set of data from all major corporate governance databases including ISS (formerly RiskMetrics), Thomson Reuters, MSCI (ESG ratings and GovernanceMetrics), and Factset. We also collect country-level governance data from the World Bank and from La Porta et al. (2008). Lastly, we collected historical data on shareholder voting results from ISS Voting Analytics. To distinguish between the different forms of corporate governance and investor protection, as well as to better keep track of the composition of the data, we divide our governance variables into three distinct categories: (1) corporate rules, (2) ownership, and (3) institutional constraints (i.e., country-level governance). The following sub-sections further clarify on the collected information of each distinctive category and hypothesized effect on firm value.

2.2.1 Corporate Rules

The first and most direct form of corporate governance is often attributed with firm-level rules set up in the corporate charter or bylaw that dictate the roles and powers between managers and owners. There are many dimensions to this topic and we have collected the most relevant information available. Originally, the Governance Index (G-Index) was developed by Gompers et al. (2003), which consists of 24 governance rules measuring the degree of shareholder rights for US public companies, with higher index score representing worse corporate governance, which is in turn correlated with lower equity price and firm value. Bebchuk et al. (2009) find that only six provisions out of the 24 governance rules in the G-Index are strongly correlated with firm valuation, and constructed the well-known Entrenchment Index (E-Index) based on these six governance rules which are mostly anti-takeover defenses. Many subsequent studies further established and legitimized the negative association between the E-Index and firm valuation (e.g., Cremers & Ferrell, 2013; Bebchuk et al., 2013), and the E-Index has become a standard proxy for corporate governance in most studies. Recently, the discussion has further developed, whereby Cremers et al., (2015) argue that only certain components of the E-index (i.e., unilateral arrangements that do not require shareholder approval, such as poison pills and golden parachutes) negatively affect firm value, while components related to commitment (i.e., protective

components or bilateral protection arrangements that require shareholder approval, such as staggered board) are seen as value enhancing.[20] Even with these new developments, the E-Index is still generally perceived as a proxy for poor corporate governance that is associated with lower firm value (Cohen and Wang, 2013). For example, Masulis, Wang and Xie (2007) find that acquirers with more of such antitakeover provisions experience significantly lower announcement-period abnormal stock returns. Karpoff, Schonlau, Wehrly (2017) further demonstrate that these measures of takeover defenses are indeed significantly and negatively related to the likelihood of the company being acquired, justifying their construct validity.

Following these theoretical and empirical debates and justifications, we construct a global entrenchment index (Global E-Index) as our parsimonious proxy for the rule-based corporate governance. Our E-Index has five components consisting of (i) staggered board, (ii) poison pill, (iii) golden parachute, (iv) charter amendments, and (v) bylaw amendments. Similar to the original E-index by Bebchuk et al. (2009), a higher index value can be considered as management and other corporate insiders are more easily entrenched. We have collected this information from *MSCI Governance Metrics* database. For additional tests, we also constructed an E-Index using data from *Thomson Reuters Asset4 ESG* as well as the ISS database for comparative analyses. Our index is built for a global sample and therefore is slightly different to the original dataset (which was only for US firms)[21]. The missing component is the supermajority requirements for mergers, which MSCI Governance Metrics do not provide. While conflicts over corporate control in the U.S. frequently originated from hostile takeover attempts, this is usually not the case for firms outside the U.S. Therefore, it is not an exact replica of the original Bebhcuk et al. (2009) E-Index.[22] As one can observe, the variables are nearly the same, and we have re-created this index for a global sample to the best extent that is currently available. It should be noted that, similar to the original E-index, the values of our E-index components vary along the time series. That is, certain governance provisions such as staggered board may change over time: from non-staggered to staggered, and vice versa.

[20] Cremers et al. suggest that arrangements that can be unilaterally adopted by directors (i.e., "unilateral protection arrangements"), which include poison pills, golden parachutes, and supermajority requirements to amend the bylaws, are associated with decreased firm value and hence fit the entrenchment theory of incumbent protection. Conversely, protective arrangements that require shareholder approval (i.e., "bilateral protection arrangements"), including staggered boards, supermajority requirements to amend the charter and to approve mergers, are associated with increased firm value. Based on these arguments, they decompose the E-Index into a commitment index (C-Index), only including the E-Index's three bilateral provisions, and an incumbent index (I-Index), only including the E-Index's three unilateral provisions. The authors find that increased scores on the C-Index (i.e., more commitment) are associated with increases in firm value. Conversely, increased scores on the I-Index (i.e., more entrenchment) is associated with decreases in firm value. They also find that the use of bilateral protection arrangements is more valuable in firms where the limited commitment problem appears to be more severe.

[21] The appendix provides more information on the exact components of each index.

[22] The original E-Index by Bebchuk et al. (2009) consisted of six separate components; (i) staggered board (ii) limits to shareholder amendments or bylaws (iii) supermajority requirements for mergers (iv) supermajority requirements for charter amendments (v) poison pill (vi) golden parachute.

One may argue that most of components in the Entrenchment Index are about anti-takeover provisions, and the role of the takeover market as an external disciplining mechanism outside the US may not be as significant as that in the US. However, we argue that although most of the provisions in the original E-Index are closely related to anti-takeover, they are also associated with the entrenchment of corporate insiders in general, even without a takeover market. In addition, many other studies have found that the international market for corporate control can be an effective governance tool as in the US market (e.g., Kang, 1993; Albuquerque, Ferreira, Marques, Matos, 2018). Moreover, our goal is to test whether what are effective for U.S. firms (e.g., rules facilitating/reducing insider entrenchment through takeover defenses) are universally applicable to companies in other countries. Therefore, we stick to the Global E-Index as a proxy for rule-based corporate governance for our analysis on the global sample.

2.2.2 Institutional Ownership

Corporate ownership can have varying effects on the way corporations are run. Alchian and Demsetz (1972) argue that managerial agency problems are controlled in part by dynamic changes in ownership, and Bebchuk & Hamdani (2009) further argue that the functioning of corporate rules crucially depends on a firm's ownership structure. Different interest groups, whether they be family owners or speculators, can have substantial influence on corporate processes due to differing inherent interests. In addition, large owners can also be a substitute of board in terms of monitor, advise, and mediate among shareholders (Burkart, Miglietta, Ostergaard, 2017).

While the effects of ownership concentration are still being fiercely debated, the literature has rapidly moved to the role of a particular type of blockholding as an important governance mechanism, namely the presence and concentration of institutional ownership. Institutional owners are regarded as relatively independent stakeholders—since they often have fewer business ties to firms and are also more involved in monitoring corporations—and as informed "smart money" since they are sophisticated investors (Borochin & Yang, 2017). In most firms, especially the largest ones in the US, institutional investors collectively hold a dominant position. Such presence in terms of ownership concentration reduces coordination costs and providers greater monitoring incentives. Institutional investors have higher competence as shareholders (vis-à-vis retail investors) because they employ teams of professional investment managers who are knowledgeable and experienced in business and finance (Goshen & Hannes, 2018). As shown by Ferreira and Matos (2008) and Bena et al. (2017), institutional ownership, especially the foreign one, is often associated with higher firm valuations. The positive value effect is mostly driven by institutional investors' activism, though its magnitude varies over time (Denes, Karpoff, McWilliams, 2017). Some recent studies have argued that the incentives and transparency concerns of mutual fund managers can lead to corporate myopia (e.g., Agarwal,

Vashishtha, Venkatachalam, 2017; Bebchuk, Cohen, Hirst, 2017). However, as argued by Fisch, Hamdani and Solomon (2018), even "passive" institutional investors such as index funds and ETFs are still incentivized to increasingly engage with portfolio firms and increase resources to that engagement by focusing on improving corporate governance,[23] which is supported by empirical evidence.

We collect firm level institutional ownership information from the *FactSet* database. This database allows us to exploit a range of information specific to the characteristics of institutional ownership. We have information on total institutional ownership in percentage of market capitalization (IO), total independent ownership ratio (IO_INDEP)[24] and foreign institutional ownership in percentage of market capitalization (IO_FOR). Furthermore, we have information on the concentration of institutional ownership. We have Herfindahl-Hirschman Index for ownership concentration, institutional blockholders (>=5%) in percentage of market capitalization and the total number of institutional owners (NBR_FIRMS). These variables were originally compiled by Ferreira and Matos (2008) and have been updated over time for international public firms in the Factset universe. In our latter section, we conduct analyses both on the whole Factset universe to utilize the ownership information to the fullest, but also on the subsample of firms which have MSCI *GOVSCORE* ratings to make our analyses more comparable.

2.2.3 Country-level Governance Rules

Country-level laws and regulations protecting investor rights and maintaining societal orders are another important aspect of governance (La Porta et al., 2008), and can affect firm-level governance rules. Aggarwal et al. (2009) argue that firm-level and country-level governance rules can be either substitutes or complements, and find more supporting evidence for the latter. Iliev, Lins, Miller, Roth (2015) also argue that without the ability to cast a mandatory and binding vote by laws and regulations, the use of voting to engage in activism will be less effective.[25] Nuances also exist with regard to what country-level rules actually matter, such as investor protection, rule of law and regulatory quality, and

[23] The argument offered by Fisch, Hamdani, and Solomon (2018) is that although passives institutional investors such as index funds are locked into their investments, the shareholders who invest in these funds are not and can exit at any time by selling their shares. As a result, mutual funds compete for investors. If active managers can generate substantial alpha on a cost-adjusted basis, fund investors will exit index funds in favor of actively-managed alternatives. Passive investors can reduce the comparative advantage of active funds by monitoring their portfolio companies and exercising their governance rights to promote firm value by replying on "voice" (monitoring on decision-making) instead of exit.

[24] Ferreira and Matos (2008) classify institutions according to the potential for business ties to a corporation as independent or "grey" institutions. Independent institutional ownership is the percentage of shares held by mutual fund managers and investment advisers. These institutions are more likely to collect information, are subject to fewer regulatory restrictions, and have fewer potential business relationships with the corporations in which they invest.

[25] More specifically, They find significantly lower levels of shareholder support for company recommendations in countries with weak investor protection laws, weak legal enforcement, and low levels of corporate disclosure, and conclude that investors engage in greater activism through dissent voting when they expect greater expropriation as a result of poor country-level institutions.

political institutions (Perotti, 2014). We take these views in the existing studies as the base for our empirical investigation and test whether and how the rule-based firm-level governance (i.e., the Global E-Index) is contingent on the country-level institutional environment. To confront these nuances, we have collected a range of data on institutional characteristics that measure country-level governance rules.

Investor Protection: We measure the level of investor protection, by incorporating a variety of proxies. First, we collected data on legal origins and investor protection laws such as the Anti-Director Rights Index (ADRI) and the Anti-Self-Dealing Index (ASDI) from the author's website (La Porta et al., 2008; (Djankov, La Porta, Lopez-de-Silanes, & Shleifer, 2008). In the literature, common law is traditionally regarded as a legal family that provides stronger investor protection. ADRI is a well-established measure quantifying country level shareholder protection and incorporates a range on information including whether proxy voting by mail is allowed, ability to call extraordinary meetings, board representation and more. ASDI is a measure of legal protection of minority shareholders against expropriation by corporate insiders, and incorporates a range of information (including ease of taking legal action, disclosure requirements, etc.) and establishes ex ante and ex post indices of private control of self-dealing. Here as well, the higher the index value, the better minority shareholders are protected.

Institutional Quality: Besides the above indices measuring investor protection by laws, we also measure country-level governance rules using the Institutional Quality data from the World Bank's World Governance Indicators database, one of the most widely used country-level governance measures in the literature. There are a total of six variables that measure institutional quality: (1) Rule of Law, which measures the citizens' confidence on the rules of society); (2) Government Effectiveness, which measures the quality of public services, (3) Control of Corruption, which measures the extent to which public power is exercised for private gains; (4) Regulatory Quality, which measures the ability of the government to formulate and implement sounds policies; (5) Political Stability & Absence of Violence, which measures the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means; and (6) Voice and Accountability, which measures the extent to which a country's citizens are able to participate in selecting their government. We use all these six measures to explore the contingent effect of institutional quality in our analysis.

Political Institutions: Scholars have also argued that the roles of corporate governance are crucially determined by political institutions of a company's country (e.g., Roe, 2003; Pagano & Volpin, 2005; Bebchuk & Neeman, 2010). Political institutions influence policymakers' choice of the degree of investor and stakeholder (such as labor) protections via policy leaning, electoral system, and lobbying by corporate insiders. For example, Pagano & Volpin (2005) find that proportional electoral systems are conducive to weaker investor protection and stronger employment protection than majoritarian

systems, and Döring and Manow (2017) find that countries with majoritarian rules more often elect conservative governments than those with proportional representation electoral systems.

We collect information from the World Bank's Database of Political Institutions. The database provides a comprehensive dataset of a country's political systems, including stability of the government, fragmentation of opposition, government parties in the legislature, the political orientation of the ruling governments and parties, and the electoral system, etc. In our analysis, the variable Executive Political Orientation measures chief executive party's political orientation with respect to economic policy, which is coded as 1 for right-leaning (parties that are defined as conservative, Christian democratic, or right-wing), 2 for center-leaning (parties that are defined as centrist or when party position can best be described as centrist, e.g. party advocates strengthening private enterprise in a social-liberal context; a party is not described as centrist if competing factions "average out" to a centrist position, and 3 for left-leaning (parties that are defined as communist, socialist, social democratic, or left-wing). Furthermore, we also constructed an indicator for the proportionality of the voting system similar to that used by Pagano and Volpin (2005). We construct a single variable, *Proportionality*, combining three indicators that describe the electoral system. These include *Proportional Rules* ("PR", which equals one if at least some cadidates are elected via proportional rule), *Plurality* (PLURALITY, which equals one if at least some legislators are elected via a majoritarian rule) and *House System* (HOUSESYS, which equals one if most House seats are allocated via a majoritarian rule). The indicator is defined as $PR - PLURALITY - HOUSESYS + 2$. The variable equals to three for a purely proportional system and 0 for a pure majoritarian system.

2.2.4 Other Variables

Our key dependent variable is Tobin's *q*, measured as the market-to-book ratio of assets.[26] To take into account of the potential "joint hypothesis" issues, we also include an accounting measure for operational performance, namely, Return on Assets (ROA). Data for Tobin's q and ROA are collected from the Thomson Reuter's Worldscope. In our analysis, we control for a range of standard firm- and country-level covariates. At the firm-level, we control for firm size (the logarithm of total assets), leverage (total debt to total assets ratio), cash holdings (scaled by total assets), capital expenditures (scaled by total assets), and sales growth rate. We collect information on these variables from the

[26] We are aware of some recent critiques on using Tobin's q (or market-to-book ratio) as a proxy for firm valuation in corporate governance, corporate law, and finance studies (Dybvig and Warachka, 2015; Bartlett and Partnoy, 2018). In particular, there are potential measurement errors, namely market values for preferred stock and debt are usually not available and simply replaced by their book values. The measure does not perfectly capture the replacement costs of both tangible assets and intangible assets, with the later including monopoly power, customer goodwill, patents and other technical knowledge, quality of management, growth opportunities, etc. Also, while Bebchuk et al. (2009) use industry-adjusted *q* as the dependent variable, we control for industry fixed effects in all of our analysis and therefore do not use industry-adjusted *q* following the critique by Gomley and Matsa (2014).

Thomson Reuter's Worldscope database. In addition, we include the logarithm of GDP per capita from the World Bank.

2.2.5 Summary Statistics

We present a series of summary statistics in Tables 1 and 2. In Table 1, we report the number of observations as well as the basic characteristics of our variables. Due to the wide extent of information that we are incorporating, it is important to understand the overlap in our data with respect to country-year coverage. Therefore, in Table 2, we report the number of countries and years that we have available for each variable of interest, conditional that we observe basic firm characteristics as well.

Because the Global E-Index in our main analyses is constructed using the MSCI Governance Metrics data, it is constrained to the 7,000+ companies covered by the Governance Metrics sample. In contrast, variables related to institutional ownership apply to the whole Factset universe covering 27,000+ companies across the world.

Table 2-1: Summary Statistics

Rule-based governance	(1) Obs.	(2) Mean	(3) Std. Dev.	(4) Min.	(5) Max.	(6) 25th percentile	(7) Median	(8) 75th percentile
MSCI Global E-Index	25,113	1.484	1.184	0	5	1	1	2
Thomson Reuters Global E-Index	9,357	1.729	1.398	0	5	1	1	3
Bebchuk et al. (2009) E-Index	4,890	2.386	1.288	0	6	1	2	3
ISS E-Index	6,553	3.494	1.127	0	6	3	3	4
MSCI Governance Score	25,113	5.717	1.666	0	10	4.600	5.700	6.900
Institutional ownership	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**25th percentile**	**Median**	**75th percentile**
Ln(Num IO)	187,812	2.888	1.668	0.693	7.948	1.386	2.708	4.205
Total IO	187,812	0.201	0.270	0	1	0.0159	0.0805	0.261
Foreign IO	187,812	0.0457	0.0884	0	1	0.000883	0.0117	0.0531
Independent IO	187,812	0.190	0.257	0	1	0.0149	0.0758	0.244
IO Block Holders (5%)	187,812	0.0581	0.110	0	1	0	0	0.0748
IO Herf	187,797	0.381	0.344	0.00728	1	0.0886	0.244	0.614
Institutional quality	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**25th percentile**	**Median**	**75th percentile**
Rule of Law	259,780	1.117	0.756	-1.126	2.120	0.827	1.448	1.615
Government Effectiveness	259,780	1.260	0.669	-0.889	2.431	1.050	1.514	1.721
Control of Corruption	259,780	1.111	0.900	-1.134	2.586	0.461	1.357	1.766
Regulatory Quality	259,780	1.091	0.694	-0.796	2.263	0.803	1.263	1.617
Political Stability	259,780	0.405	0.711	-2.386	1.663	0.0486	0.575	0.973
Voice and Accountability	259,780	0.771	0.867	-1.687	1.826	0.614	1.079	1.328
Anti-Self-Dealing Index	300,200	0.610	0.197	0.172	1	0.499	0.654	0.757
Anti-Directors Rights Index (ADRI)	273,188	3.955	1.254	0	5	3	4	5
Common Law	300,200	0.513	0.500	0	1	0	1	1
Right-leaning Executive Party	251,460	0.485	0.500	0	1	0	0	1
Proportionality	271,553	0.849	1.010	0	3	0	1	1
Other variables	**Obs.**	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**25th percentile**	**Median**	**75th percentile**
Ln(Assets)	300,200	12.37	1.911	8.272	16.16	11.09	12.32	13.66
Leverage	300,200	0.227	0.194	0	0.651	0.0444	0.198	0.360
Sales Growth	300,200	0.343	0.660	-0.504	2.268	-0.0576	0.174	0.519
Cash / Total Assets	300,200	0.166	0.169	0.00469	0.654	0.0403	0.108	0.231
CAPEX / Total Assets	300,200	0.0516	0.0513	0.000333	0.200	0.0140	0.0348	0.0707
Tobin's *q*	300,200	1.640	1.115	0.662	5.329	0.948	1.233	1.853
Return on Assets (ROA)	300,136	-0.00835	0.156	-0.587	0.159	-0.00893	0.0284	0.0665

Note: All firm level continuous controls (non-dummy variables) are winsorized at the 5th and 95th percentiles.

Table 2-2: Data Coverage of Governance Variables

Variables	Countries	Years
MSCI Entrenchment Index	45	2010 - 2015
Thomson Reuters Entrenchment Index	32	2003 - 2015
MSCI Governance Score	45	2010 - 2015
Ln (Num IO)	45	2001 - 2014
IO Herf	45	2001 - 2014
IO Block Holders (5%)	45	2001 - 2014
Total IO	45	2001 - 2014
Independent IO	45	2001 - 2014
Foreign IO	45	2001 - 2014
Control of Corruption	45	1996 - 2015
Government Effectiveness	45	1996 - 2015
Political Stability	45	1996 - 2015
Voice and Accountability	45	1996 - 2015
Regulatory Quality	45	1996 - 2015
Rule of Law	45	1996 - 2015
Common Law	45	As of 1993
Anti-director Rights Index (ADRI)	37	As of 1993
Anti-Self-Dealing Index (ASDI)	45	As of 1993

2.3 Main Results

We begin our empirical analysis by validating our self-constructed E-Index on a subsample of US firms and comparing it to the results using the original Bebchuk et al. (2009) E-Index and other governance indices. We then test these self-constructed and aggregate governance indices on the global sample of firms, which allows us to take a first glimpse into the effects of governance practices and whether they are in line with prior expectations. After analyzing the global sample, we turn our focus into an extended sub-sample analysis, whereby we run the same regressions, but on various sub-samples classified in the previous literature, such as common law countries, French civil law countries, Western Europe, Scandinavian countries, East Asian countries, developed or OECD countries, etc. We use these extensive tests to determine whether we are able to find any universal corporate practices. Once we identify those deemed as non-universal, we explore their contingencies, that is, under what ownership and institutional environment do firm-level rule-based governance become effective.

2.3.1 Baseline Results: Rule-Based Governance in the U.S. Sample

We first perform the baseline tests by replicating the results in the original Bebchuk et al. (2009) paper, using various measures of E-Index to verify that our sample are indeed representative and our self-constructed indices are comparable to those in the literature. In particular, these measures are

constructed using data from MSCI Governance Metrics (MSCI E-Index), the "G" part of Thomson Reuters ASSET4 ESG (TR E-Index), the ISS Governance (ISS E-Index), as well as the original E-Index from Bebchuk et al. (2009). These serve as a sanity check that our self-constructed E-index capture similar aspects as the original E-Index. To investigate that whether the governance effects show up in both short-term market valuations and operating profitability, we use both Tobin's *q* and ROA as our dependent variables. Columns (1), (3), (5), (7) report the results on Tobin's *q*, whereas Columns (2), (4), (6), (8) report the results on ROA. All independent variables are lagged by one-year. Some interesting observations can be made. First, the coefficients of various E-indices are negative and statistically significant. This is consistent with the key finding in the original thesis of Bebchuk et al. (2009). The coefficient is –0.04 when the dependent variable is Tobin's *q*, which is also comparable to –0.02 in Bebchuk et al. (2009) with industry-adjusted *q* as the dependent variable. Given that all of four E-indices have the "correct" sign of coefficient with Tobin's *q*, and that the original E-Index and the ISS E-Index only cover US firms, we consider our self-constructed MSCI E-Index and TR E-Index as valid measures of corporate rules that can be expanded to the global sample and represent the original E-Index by Bebchuk et al. (2009). Therefore, in the next section we extend the MSCI E-Index and the TR E-Index to the global sample using the same construction (i.e., Global E-Index for MSCI and TR, respectively).

Table 2-3: US Results on Corporate Rules

The dependent variables are Tobin's *q* (Q) and Return on Assets (ROA). Q, ROA and all firm level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DV = Q	*DV = ROA*	*DV = Q*	*DV = ROA*	*DV = Q*	*DV = ROA*	*DV = Q*	*DV = ROA*
MSCI E-Index t-1	-0.032***	-0.037***						
	(0.012)	(0.012)						
Thomson Reuters E-Index t-1			-0.067***	-0.072***				
			(0.013)	(0.014)				
Bebchuk et al. (2009) E-Index t-1					-0.040***	-0.035***		
					(0.011)	(0.011)		
ISS E-Index t-1							-0.052***	-0.054***
							(0.010)	(0.011)
Ln(Assets) $_{t-1}$	-0.018*	-0.012	-0.295***	-0.285***	0.061***	0.064***	0.019	0.020*
	(0.009)	(0.010)	(0.025)	(0.026)	(0.014)	(0.015)	(0.012)	(0.012)
Leverage $_{t-1}$	-0.098	-0.098	-0.293***	-0.333**	-0.667***	-0.664***	-0.572***	-0.580***
	(0.074)	(0.076)	(0.112)	(0.132)	(0.111)	(0.114)	(0.093)	(0.097)
Cash / Total Assets $_{t-1}$	2.200***	2.166***	2.570***	2.552***	1.767***	1.770***	1.963***	1.986***
	(0.095)	(0.097)	(0.163)	(0.177)	(0.145)	(0.150)	(0.120)	(0.122)
CAPEX / Total Assets $_{t-1}$	2.941***	3.177***	3.095***	3.454***	3.702***	3.774***	3.511***	3.907***
	(0.315)	(0.319)	(0.466)	(0.557)	(0.447)	(0.472)	(0.363)	(0.380)
Sales Growth $_{t-1}$	0.300***	0.347***	0.232***	0.253***	0.241***	0.263***	0.301***	0.371***
	(0.028)	(0.030)	(0.040)	(0.051)	(0.039)	(0.043)	(0.037)	(0.042)
Observations	9,159	9,111	4,547	4,293	4,595	4,498	6,298	6,213
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	No	No	No	No	No	No	No
Countries	USA	USA	USA	USA	USA	USA	USA	USA

2.3.2 Baseline Results: Rule-Based Governance in the Global Sample

We next move to the global sample by using our self-constructed MSCI Global E-Index and the TR Global E-Index, together with the aggregated Governance Score assembled by MSCI as a comparison which also covers global firms. The base results for a global sample of firms are reported in Table 4. The coefficient for the MSCI Global E-Index remains negative and significant, but with much smaller economic magnitude. The coefficient of the TR Global E-Index becomes insignificant, which may be due to the missing values for some firms resulting in lack of power with a smaller sample. The coefficient for the MSCI Governance Score, which can be considered as the inverse of "entrenchment" and is constructed using a "kitchen sink" approach of aggregating all possible governance dimensions, is positive and marginally significant, indicating that it measures similar aspects of governance (at least in terms of the direction of the effect) as our self-constructed E-index. The economic significance of the MSCI Global E-Index is comparable to that in our US sample and to the one in Bebchuk et al. (2009). The results are not that clear for ROA, as all the coefficients of the Global E-Index and the Governance Score are insignificant and virtually zero. This is unsurprising given that corporate rules and governance provisions within a firm are stable over time, whereas ROA reflects the firm's short-term profitability which can vary substantially over time. It is also indicative that the effects of rule-based governance on firm valuation and operational performance may not function in the same way, and market reactions capture things other than operational performance.

Table 2-4: Main Results on Corporate Rules

The dependent variables are Tobin's *q* (Q) and Return on Assets (ROA). Q, ROA and all firm-level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	DV = Q	*DV = ROA*	*DV = Q*	*DV = ROA*	*DV = Q*	*DV = ROA*
MSCI Global E-Index t-1	-0.027***	-0.001				
	(0.009)	(0.001)				
Thomson Reuters Global E-Index t-1			-0.010	-0.001		
			(0.013)	(0.001)		
MSCI Governance Score t-1					0.007**	0.000
					(0.004)	(0.001)
Ln(Assets) $_{t-1}$	-0.203***	0.012***	-0.400***	-0.007***	-0.201***	0.012***
	(0.014)	(0.002)	(0.026)	(0.002)	(0.014)	(0.002)
Leverage $_{t-1}$	-0.282***	-0.079***	-0.253**	-0.050***	-0.283***	-0.080***
	(0.070)	(0.008)	(0.120)	(0.011)	(0.070)	(0.008)
Cash / Total Assets $_{t-1}$	1.007***	-0.009	1.366***	0.035**	1.009***	-0.009
	(0.090)	(0.010)	(0.169)	(0.017)	(0.090)	(0.010)
CAPEX / Total Assets $_{t-1}$	0.637***	0.066**	0.885***	0.074**	0.643***	0.066**
	(0.198)	(0.028)	(0.322)	(0.035)	(0.198)	(0.028)
Sales Growth $_{t-1}$	0.112***	0.009***	0.061**	0.010***	0.112***	0.009***
	(0.015)	(0.002)	(0.024)	(0.003)	(0.015)	(0.002)
Ln(GDP per capita) $_{t-1}$	-0.092	-0.040***	0.414***	0.042***	-0.078	-0.039***
	(0.058)	(0.007)	(0.143)	(0.016)	(0.059)	(0.007)
Observations	19,932	20,020	9,356	9,383	19,932	20,020
Number of company	4,825	4,838	1,852	1,852	4,825	4,838
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	All	All	All	All	All

Among the three governance measures, we think MSCI Global E-Index is a better proxy for rule-based corporate governance (or "poor governance") for the global sample for several reasons. First, it has much fewer missing observations and covers much more global firms than the Thomson Reuters Global E-Index. Second, although the MSCI Governance Score also covers a significant number of firms with non-missing observations, as mentioned earlier and similar to the critique by Bebchuk et al. (2009), a measure that aggregates many aspects of governance is usually not informative and may create substantial noises in the measurement of governance. The central debate in the universal corporate governance literature is that what matters for corporate governance in the U.S.—i.e., insider entrenchment as measured by Bebchuk et al. (2009)—can be universally applied to the rest of the world. An aggregate governance rating or score constructed using the "kitchen sink" approach may obscure the distinction between what governance issues matter for the U.S. and for other countries; some provisions in the aggregate score may not even matter for U.S. firms. Third, the policy implications are clearer if we focus on the most important governance practices, which policymakers and shareholders can enact upon. For these reasons, we will use our self-constructed MSCI Global E-Index as a proxy for firm-level corporate rules in the subsequent analyses.

2.3.3 The Role of Institutional Ownership

We continue the analysis by examining the effects of institutional ownership, another important governance mechanism that has been broadly discussed. In particular, the extant literature considers the governance effects of ownership being mostly from the stakes held by institutional investors, which translate into stronger monitoring and higher firm valuation, not only within but also across countries. As shown in Table 5, under a global sample, the number of institutional investors (*Ln(Number of IO firms)*), total institutional ownership (*Total IO*), ownership by independent institutional investors (*Independent IO*) and by foreign investors (*Foreign IO*) are all loaded with positive coefficients, whereas the coefficient of the concentration of institutional ownership based on the Herfindahl measure (*IO Herf*) has a negative sign. Institutional investors holding more than 5% ownership (*IO Block Holders*) is also negatively correlated with firm valuation. In addition, the sign and significance of the coefficients in Tobin's *q* regressions and ROA regressions are mostly consistent across different measures of ownership. When we conduct the same test on a subsample of firms with MSCI Governance Metrics coverage, we obtain very similar results.

These results are largely consistent with the extant literature on the non-monotonic effect of institutional ownership. In general, some degree of concentration in ownership—especially that held by independent institutional investors (vis-à-vis the "captive" institutions which may have different incentive structures) and foreign ones—help promote governance practices (Aggarwal et al., 2011; Ferreira & Matos, 2008; Bena et al., 2017). However, a high degree of ownership concentration by

blockholders, even if it's held by institutional investors, may be detrimental to firm value. Overall, our results on global sample are largely consistent with the findings in the literature based on the U.S. sample.

Table 2-5: Main Results on Corporate Ownership

The dependent variables are Tobin's *q* (Q) and Return on Assets (ROA). Q, ROA and all firm level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable	*Q*	*ROA*	*Q*	*ROA*	*Q*	*ROA*	*Q*	*ROA*	*Q*	*ROA*	*Q*	*ROA*
Ln(Number of IO firms) $_{t-1}$	0.199***	0.011***										
	(0.004)	(0.001)										
IO Herf $_{t-1}$			-0.282***	-0.017***								
			(0.011)	(0.002)								
IO Block Holders (5%) $_{t-1}$					-0.393***	-0.007						
					(0.036)	(0.006)						
Total IO $_{t-1}$							0.224***	0.072***				
							(0.026)	(0.004)				
Independent IO $_{t-1}$									0.222***	0.077***		
									(0.027)	(0.004)		
Foreign IO $_{t-1}$											0.657***	0.002
											(0.056)	(0.007)
Ln(Assets) $_{t-1}$	-0.339***	0.007***	-0.252***	0.012***	-0.216***	0.014***	-0.233***	0.010***	-0.232***	0.010***	-0.230***	0.014***
	(0.005)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)
Leverage $_{t-1}$	0.365***	-0.067***	0.230***	-0.074***	0.171***	-0.077***	0.194***	-0.071***	0.193***	-0.071***	0.189***	-0.077***
	(0.025)	(0.004)	(0.025)	(0.004)	(0.025)	(0.004)	(0.025)	(0.004)	(0.025)	(0.004)	(0.025)	(0.004)
Cash / Total Assets $_{t-1}$	0.590***	0.004	0.654***	0.008*	0.688***	0.010**	0.674***	0.005	0.674***	0.005	0.674***	0.010**
	(0.028)	(0.004)	(0.029)	(0.004)	(0.029)	(0.004)	(0.029)	(0.004)	(0.029)	(0.004)	(0.029)	(0.004)
CAPEX / Total Assets $_{t-1}$	0.274***	0.008	0.468***	0.018**	0.583***	0.025***	0.551***	0.017*	0.552***	0.016*	0.553***	0.025***
	(0.058)	(0.009)	(0.059)	(0.009)	(0.059)	(0.009)	(0.059)	(0.009)	(0.059)	(0.009)	(0.059)	(0.009)
Sales Growth $_{t-1}$	0.072***	0.005***	0.075***	0.006***	0.079***	0.006***	0.081***	0.006***	0.081***	0.006***	0.080***	0.006***
	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)
Ln(GDP per capita) $_{t-1}$	0.130***	-0.023***	0.111***	-0.024***	0.104***	-0.025***	0.127***	-0.019***	0.128***	-0.019***	0.116***	-0.024***
	(0.019)	(0.002)	(0.019)	(0.002)	(0.019)	(0.002)	(0.019)	(0.002)	(0.019)	(0.002)	(0.020)	(0.002)
Observations	187,812	188,628	187,797	188,612	187,812	188,628	187,812	188,628	187,812	188,628	187,812	188,628
Number of company	27,890	27,993	27,887	27,990	27,890	27,993	27,890	27,993	27,890	27,993	27,890	27,993
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	All	All	All	All	All	All	All	All	All	All	All

2.3.4 The Role of Country-level Investor Protections

We further test the roles of country-level investor protections by law as another mechanism of corporate governance. Given that all these country-level indices are time-invariant, whereas ROA as an operational measure can vary significantly across time, we only test the effects on market valuations (Tobin's q) on our global sample and without controlling for country fixed effects (due to the multicollinearity concern). Investor protections by law are proxied by the widely-cited Antidirector Rights Index (ADRI), Anti-self-dealing Index (ASDI), and a common law dummy. The results are reported in Table 6. Again, most country-level investor protection indices have the "correct" signs: firms operating in common law countries and countries with higher index values for ADRI and ASDI (i.e., stronger investor protections by law) have higher valuations. This is in line with the prior literature and serves as another "sanity check" of our empirical investigation: our sample firms are comparable to those in the extant studies investigating country-level governance (e.g., La Porta et al., 2002; Djankov et al., 2008).

Table 2-6: Main Results on Country Variables:

The dependent variables are Tobin's *q* in all regressions. Tobin's *q* and all firm level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Dependent Variable = Tobin's q	(1)	(2)	(3)
Anti-director Rights Index (ADRI)	0.083***		
	(0.004)		
Anti-Self-Dealing Index		0.365***	
		(0.023)	
Common Law			0.233***
			(0.010)
Ln(Assets) $_{t-1}$	-0.087***	-0.086***	-0.081***
	(0.003)	(0.003)	(0.003)
Leverage $_{t-1}$	0.352***	0.316***	0.315***
	(0.028)	(0.027)	(0.027)
Cash / Total Assets $_{t-1}$	1.313***	1.356***	1.404***
	(0.034)	(0.032)	(0.032)
CAPEX / Total Assets $_{t-1}$	1.613***	1.496***	1.441***
	(0.082)	(0.077)	(0.077)
Sales Growth $_{t-1}$	0.141***	0.145***	0.143***
	(0.005)	(0.005)	(0.005)
Ln(GDP per capita) $_{t-1}$	0.025***	-0.019***	-0.036***
	(0.006)	(0.005)	(0.005)
Observations	273,116	300,129	300,129
Country FE	No	No	No
Industry x Year FE	Yes	Yes	Yes
Countries	All	All	All

Overall, our results on corporate rules, institutional ownership, and country-level investor protections based on both the U.S. sample and the global sample suggest that insider entrenchment is associated with lower firm valuation, whereas more (dispersed) institutional ownership and stronger legal investor protections against corporate insiders are associated with higher firm valuation. Of course, these results need to be interpreted with caution given the lack of a proper identification strategy. Other time-varying firm and country characteristics may simultaneously drive the governance structures and firm value, and institutional investors may selectively invest in firms with certain characteristics that are more likely to be overvalued. Nevertheless, they are mostly consistent with the arguments and findings in the prior literature, lending credibility to the representativeness of our sample and the validity of our empirical proxies, especially the Global E-Index.

2.3.5 Sub-Sample Analyses

Having established the validity of our sample and measurements, we next ask: do our results based on the global full sample carry over to country and regional subsamples? In other words, are the corporate governance practices we identified in the previous section universally applicable in terms of enhancing firm value? This is the central question to the universal corporate governance debates. While country-level investor protection laws are usually very sticky over time (La Porta et al., 2002), there have also been dramatic changes in firm-level governance rules in many countries in recent years.[27] Therefore, exploring whether and how the effectiveness of firm-level governance varies across countries can have significant policy implications and shed light on policy reforms in different countries.

Obviously, the governance effects of country-level investor protection indices cannot be tested in single or a few countries due to its lack of variations within and across countries. Therefore, we only focus on the applicability of the firm-level rule-based governance (i.e., Global E-Index that we identified earlier), and conduct the same tests as in our U.S. and global samples on different country-subsamples. The subsamples in which we group countries or single out certain countries mostly follow those in the extant studies: (1) common law countries; (2) French civil law countries; (3) German civil law countries, which commonly apply the two-tier board structure and other German-style governance arrangements; (4) Western Europe, following the classification by Faccio & Lang (2002); (5) Western Europe excluding United Kingdom, to tease out the "common law" effect; (6) Scandinavian countries, which are classified as a unique legal origin by La Porta et al. (1998, 2008); (7) East Asia, following the classification by Clasessens et al. (2000); (8) Non-US countries; (9) Developed countries, based on the IMF classifications; (10) Developing countries, also based on the IMF classifications; (11) OECD countries; (12) non-OECD countries. We also include in our subsample analysis a few single countries which are deemed in comparative corporate governance literature: (13) Japan, the country with the largest capital market in Asia and most firms in our sample of Asia-Pacific countries; (14) United Kingdom, the origin of common law; (15) China, the largest emerging economy; (16) Canada, a common law country with similar level of investor protection as the U.S. (La Porta et al., 1998) but also significant presence of business groups (La Porta et al., 1999; Morck and Tian, 2015); (17) Germany, where a two-tier board structure is mandated by company law; (18) Singapore and Hong Kong, the two financial centers and common law economies in Asia. Since we have already shown the results of our E-Index in the whole sample and in the U.S. subsample, we do not repeat the analysis here. In addition, given the space limitation as well as the results from previous tables on ROA and related concerns on

[27] For example, Miyajima, Ogawa & Saito (2017) document that, in Japan, the presence of a main bank has been weakened, the ownership of institutional investors has dramatically increased, and independent outside directors have been widely introduced. Also see Kim & Lu (2013) and Fauver, Hung, Li & Taboada (2017) for more systematic evidence on firm-level rule-based corporate governance reforms around the world.

the accounting-based operational performance measure, from this point onward we will only report results using Tobin's *q* as the dependent variable. Results for the subsample analysis are reported in Table 7.

Some salient findings emerge from these results. First, for most subsamples, the coefficient of the Global E-Index is statistically insignificant, and the sign is not always negative, despite the sizable number of observations in each subsample. Even across common law countries—including the subsample of firms in the UK, the origin of common law—which are widely regarded as offering stronger investor protections, the effects of the Global E-Index are not consistent. Only in the subsample of Japan, China, Canada, Non-U.S., developed countries and the OECD countries, we observe a negative and significant coefficient. In other words, while it is established that managerial entrenchment is negatively correlated with firm valuation in U.S. firms and in our global sample, such negative correlation in the global sample seems to be driven by only a few countries and regions.

Table 2-7: Sub-Sample Results on Corporate Rules Variables

The dependent variables are Tobin's *q* (Q) in all regressions. Q and all firm level controls are winsorized at the 5th and 95th percentiles. The Global E-Index is constructed using the MSCI Governance Metrics data. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Dependent Variable = Q	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Global E-Index t-1	-0.014	-0.007	0.013	-0.011	0.015	0.058	0.003	-0.056***	-0.015*
	(0.010)	(0.023)	(0.020)	(0.018)	(0.018)	(0.062)	(0.026)	(0.012)	(0.009)
Observations	13,674	2,058	3,515	3,503	2,039	554	3,519	10,715	17,014
Samples	Common Law	French Civil Law	German Civil Law	Western Europe	Western Europe ex-UK	Scandinavian countries	East Asia	Non-US	Developed (IMF)

Dependent Variable = Q	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Global E-Index t-1	-0.002	-0.023***	0.019	-0.050*	-0.045	-0.299***	-0.102**	-0.024	0.170***
	(0.026)	(0.009)	(0.030)	(0.027)	(0.046)	(0.091)	(0.046)	(0.032)	(0.061)
Observations	2,738	17,247	2,554	2,006	1,349	325	661	365	376
Sample	Developing (IMF)	OECD	Non-OECD	Japan	United Kingdom	China	Canada	Germany	Singapore & Hong Kong

Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

We conduct similar analysis for our institutional ownership variables from Factset with the same classifications of subsamples and report the results in Table 8, as well as the overall sample and the U.S. sample. Interestingly, we find that most institutional ownership variables (e.g., *Total IO, Independent IO, Foreign IO, Ln(number of IO firms)*) are consistently and positively correlated with Tobin's *q* across different subsamples. Similarly, the Herfindahl index of institutional ownership (*IO Herf*) and blockholdings of institutional investors (*IO Block Holders*)—two variables that we interpret as capturing ownership concentration—are consistently and negatively correlated with Tobin's Q across the board. The numbers of observations are much larger in these tests because we are not bounded by the Governance Metrics ratings but are able to utilize the whole Factset universe. In Panel B of Table 8, we conduct the similar analysis on the same country subsamples, and only on those firms that are covered by the MSCI Governance Metrics data. This results in much smaller sample sizes, and we lose statistical significance in some subsamples—especially for Total IO and Independent IO in French civil law and German civil law countries, as well as in single-country subsamples—likely due to the issue of weaker predictive power with smaller sample. Nevertheless, none of the coefficients flips the sign. For the total number of institutional investors (i.e., *Ln(Num IO)*) and the concentration of institutional ownership (e.g., *IO Herf*), the previous results are mostly upheld.

The consistent significance of the coefficients on institutional ownership variables in more subsamples echoes the recent wave of studies on the role of professional institutional investors in shaping corporate governance practice around the world. They are regarded as increasingly sophisticated shareholders who are capable of achieving governance aims via activism without judicial assistance (Goshen & Hannes, 2018). In particular, most prior studies have shown that institutional owners act as effective monitors of firms and they are not concentrating solely on short term gains (Ferreira & Matos, 2008; Aggarwal et al., 2011; Bebchuk, Brav & Jiang, 2015; Cremers et al., 2016; Bena et al., 2017). Although Bebchuk, Cohen, Hirst (2017) caution that agency problems within institutional investors may reduce their incentive to exert activism to their portfolio companies, Fisch, Hamdani, Solomon (2018) argue and find supporting evidence that even passive investors are incentivized to actively engage with their portfolio firms on improving their corporate governance and reducing underperformance. Our results are consistent with the latter argument, as well as with recent literature on "governance traveling around the world" enabled by institutional investors (e.g., Ferreira & Matos, 2008; Aggarwal, Erel, Ferreira, Matos, 2011).

Table 2-8: Sub-Sample Results on Ownership Variables

The dependent variables are Tobin's *q* in all regressions. Tobin's *q* and all firm level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Panel A. Results of Ownership Variables based on Full Factset Sample

Dependent Variable = Tobin's q	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln(Num IO) $_{t-1}$	0.199***	0.280***	0.146***	0.131***	0.214***	0.177***	0.294***	0.199***	0.290***	0.181***
	(0.004)	(0.008)	(0.010)	(0.007)	(0.010)	(0.011)	(0.026)	(0.007)	(0.013)	(0.005)
Observations	187,812	96,579	23,765	60,617	38,304	25,448	6,851	53,226	46,498	141,314
IO Herf $_{t-1}$	-0.282***	-0.340***	-0.238***	-0.226***	-0.247***	-0.216***	-0.399***	-0.347***	-0.174***	-0.288***
	(0.011)	(0.020)	(0.026)	(0.015)	(0.024)	(0.026)	(0.061)	(0.016)	(0.042)	(0.011)
Observations	187,797	96,565	23,764	60,617	38,304	25,448	6,851	53,225	46,486	141,311
IO Block Holders (5%) $_{t-1}$	-0.393***	-0.397***	-0.053	-0.390***	-0.368***	-0.399***	-0.450***	-0.731***	-0.349***	-0.323***
	(0.036)	(0.040)	(0.151)	(0.116)	(0.085)	(0.145)	(0.168)	(0.143)	(0.048)	(0.056)
Observations	187,812	96,579	23,765	60,617	38,304	25,448	6,851	53,226	46,498	141,314
Total IO $_{t-1}$	0.224***	0.253***	0.269***	0.497***	0.237***	0.251***	0.401***	0.931***	0.355***	0.416***
	(0.026)	(0.031)	(0.092)	(0.070)	(0.059)	(0.082)	(0.152)	(0.100)	(0.039)	(0.039)
Observations	187,812	96,579	23,765	60,617	38,304	25,448	6,851	53,226	46,498	141,314
Independent IO $_{t-1}$	0.222***	0.250***	0.293***	0.538***	0.238***	0.293***	0.495***	0.947***	0.353***	0.435***
	(0.027)	(0.032)	(0.101)	(0.076)	(0.062)	(0.090)	(0.164)	(0.103)	(0.040)	(0.041)
Observations	187,812	96,579	23,765	60,617	38,304	25,448	6,851	53,226	46,498	141,314
Foreign IO $_{t-1}$	0.657***	0.890***	0.373***	0.186*	0.791***	0.654***	1.416***	0.529***	0.177	0.566***
	(0.056)	(0.084)	(0.113)	(0.098)	(0.094)	(0.114)	(0.238)	(0.112)	(0.210)	(0.058)
Observations	187,812	96,579	23,765	60,617	38,304	25,448	6,851	53,226	46,498	141,314
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	Common Law	French Civil Law	German Law	Western Europe	Western Europe ex-UK	Scandinavian countries	East Asia	U.S.	Non-U.S.

Table 8 (Continued)

Dependent Variable = Tobin's q	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Ln(Num IO) $_{t-1}$	0.213***	0.174***	0.209***	0.182***	0.213***	0.315***	0.073***	0.308***	0.092***	0.245***
	(0.005)	(0.009)	(0.005)	(0.009)	(0.009)	(0.025)	(0.014)	(0.023)	(0.020)	(0.030)
Observations	145,842	41,970	149,338	38,474	31,657	12,172	11,382	7,882	5,624	4,788
IO Herf $_{t-1}$	-0.276***	-0.299***	-0.269***	-0.326***	-0.339***	-0.301***	-0.159***	-0.317***	-0.087*	-0.353***
	(0.013)	(0.019)	(0.013)	(0.020)	(0.019)	(0.051)	(0.031)	(0.064)	(0.047)	(0.069)
Observations	145,828	41,969	149,324	38,473	31,657	12,172	11,382	7,880	5,624	4,788
IO Block Holders (5%) $_{t-1}$	-0.404***	-0.228*	-0.385***	-0.350***	-0.670***	-0.422***	-0.349	-0.392***	-0.359	-0.273
	(0.038)	(0.120)	(0.038)	(0.120)	(0.209)	(0.107)	(0.266)	(0.131)	(0.262)	(0.299)
Observations	145,842	41,970	149,338	38,474	31,657	12,172	11,382	7,882	5,624	4,788
Total IO $_{t-1}$	0.222***	0.472***	0.214***	0.637***	1.351***	0.158*	0.375***	0.579***	0.048	0.907***
	(0.028)	(0.078)	(0.028)	(0.096)	(0.148)	(0.090)	(0.144)	(0.115)	(0.156)	(0.286)
Observations	145,842	41,970	149,338	38,474	31,657	12,172	11,382	7,882	5,624	4,788
Independent IO $_{t-1}$	0.215***	0.549***	0.209***	0.661***	1.388***	0.134	0.408***	0.608***	0.067	0.966***
	(0.029)	(0.085)	(0.029)	(0.098)	(0.155)	(0.091)	(0.144)	(0.122)	(0.162)	(0.292)
Observations	145,842	41,970	149,338	38,474	31,657	12,172	11,382	7,882	5,624	4,788
Foreign IO $_{t-1}$	0.762***	0.359***	0.741***	0.319***	0.769***	1.235***	-0.812***	0.873***	0.235	0.786**
	(0.063)	(0.109)	(0.062)	(0.120)	(0.200)	(0.210)	(0.204)	(0.170)	(0.196)	(0.310)
Observations	145,842	41,970	149,338	38,474	31,657	12,172	11,382	7,882	5,624	4,788
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	Developed (IMF)	Developing (IMF)	OECD	Non-OECD	Japan	UK	China	Canada	Germany	Singapore and Hong Kong

Table 8 (Continued)

Panel B: Results of Ownership Variables Based on MSCI Governance Metrics Sample										
Dependent Variable = Tobin's q	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln(Num IO) $_{t-1}$	0.459***	0.599***	0.167***	0.296***	0.360***	0.230***	0.243**	0.284***	0.673***	0.335***
	(0.028)	(0.039)	(0.049)	(0.055)	(0.050)	(0.055)	(0.115)	(0.058)	(0.053)	(0.031)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
IO Herf $_{t-1}$	-0.799***	-1.044***	-0.097	-0.925***	-1.409***	-1.013***	-0.455	-0.878***	-0.824***	-0.826***
	(0.127)	(0.187)	(0.194)	(0.250)	(0.264)	(0.280)	(0.531)	(0.270)	(0.241)	(0.152)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
IO Block Holders (5%) $_{t-1}$	-0.223***	-0.246***	-0.356	-0.857***	-0.542***	-0.702***	-0.097	-1.275***	-0.341***	-0.413***
	(0.077)	(0.082)	(0.293)	(0.288)	(0.177)	(0.267)	(0.458)	(0.482)	(0.090)	(0.145)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
Total IO $_{t-1}$	0.304***	0.287***	0.382	0.083	0.149	-0.026	0.490	0.460*	0.097	0.446***
	(0.067)	(0.077)	(0.241)	(0.176)	(0.147)	(0.180)	(0.452)	(0.238)	(0.088)	(0.102)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
Independent IO $_{t-1}$	0.325***	0.292***	0.426*	0.130	0.238	0.071	0.798	0.484**	0.095	0.490***
	(0.069)	(0.078)	(0.257)	(0.196)	(0.159)	(0.196)	(0.534)	(0.243)	(0.090)	(0.110)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
Foreign IO $_{t-1}$	0.664***	0.798***	0.459	0.123	0.429**	0.160	0.915*	0.418	0.234	0.643***
	(0.113)	(0.167)	(0.288)	(0.196)	(0.174)	(0.192)	(0.476)	(0.273)	(0.309)	(0.120)
Observations	16,365	11,095	1,792	3,012	3,011	1,777	466	2,832	7,452	8,913
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	Common Law	French Civil Law	German Civil Law	Western Europe	Western Europe ex-UK	Scandinavian countries	East Asia	U.S.	Non-U.S.

Table 8 (Continued)

Dependent Variable = Tobin's q	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Ln(Num IO) $_{t-1}$	0.335***	0.341***	0.474***	0.323***	0.446***	0.648***	0.224**	0.215	0.427**	0.434***
	(0.031)	(0.061)	(0.032)	(0.062)	(0.058)	(0.090)	(0.094)	(0.143)	(0.199)	(0.129)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
IO Herf $_{t-1}$	-0.826***	-0.492**	-0.789***	-0.722***	-0.837***	-1.801***	-1.130**	-0.571	-4.259**	-0.721
	(0.152)	(0.245)	(0.149)	(0.265)	(0.308)	(0.477)	(0.505)	(0.406)	(1.859)	(0.935)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
IO Block Holders (5%) $_{t-1}$	-0.413***	-0.296	-0.273***	-0.515	-0.300	-0.515*	-0.019	0.249	-0.922*	-0.129
	(0.145)	(0.315)	(0.079)	(0.372)	(0.489)	(0.263)	(0.600)	(0.381)	(0.480)	(1.850)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
Total IO $_{t-1}$	0.446***	0.445**	0.236***	0.364*	1.034***	0.120	0.258	0.507	0.124	0.869
	(0.102)	(0.221)	(0.070)	(0.218)	(0.271)	(0.273)	(0.528)	(0.315)	(0.445)	(0.821)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
Independent IO $_{t-1}$	0.490***	0.536**	0.250***	0.411*	1.054***	0.216	0.308	0.515	0.285	0.982
	(0.110)	(0.241)	(0.072)	(0.226)	(0.291)	(0.288)	(0.541)	(0.321)	(0.489)	(0.818)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
Foreign IO $_{t-1}$	0.643***	0.532**	0.612***	0.347	1.099***	0.628	0.573	1.424***	0.080	1.223
	(0.120)	(0.263)	(0.125)	(0.242)	(0.315)	(0.403)	(0.436)	(0.358)	(0.551)	(1.010)
Observations	8,913	2,248	14,285	2,080	1,631	1,140	321	505	330	334
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	Developed (IMF)	Developing (IMF)	OECD	Non-OECD	Japan	United Kingdom	China	Canada	Germany	Singapore and Hong Kong

As suggested by Black, de Carvalho, and Gorga (2012), if there is sufficient commonality in some aspects of corporate governance practices, such as our findings on institutional ownership, it could make sense to adopt "across the board" rules, both within and across countries. However, if some standard sets of corporate governance practice do not universally apply across countries, such as our findings on insider entrenchment, a natural and important question is when such governance practice will work, which we will address in the next section.

2.3.6 What Matters in Corporate Governance…. and When?

Having showed that the standard set of governance practices, or the lack of such governance mechanisms, do not universally apply to different countries and regions, it is important to understand why this is the case and under what conditions they can function effectively as observed in the US. Our investigation of the conditional (or contingent) effects of corporate governance is largely motivated by the literature focusing on what institutions are critical for finance: contracting, protection of investor rights, and prudential regulation (see a review by Perotti (2014)). Similarly, research on comparative corporate governance from the perspectives of economics, management, and law has offered primarily three main explanations on the effectiveness of corporate governance. First, the effectiveness of firm-level governance depends considerably on companies' ownership structure, and measures that protect outside investors in a company without a controlling shareholder are often irrelevant or even harmful when it comes to investor protection in companies with a controlling shareholder, and vice versa (e.g., Bebchuk and Hamdani, 2009). Second, the effectiveness of firm-level governance depends on the strength of country-level governance and institutional environment, which include a country's laws and the institutions that enforce the laws. Aggarwal, Erel, Stulz, and Williamson (2009) argue that firm-level governance and country-level investor protection can be substitutes or complements, and find empirical supports for the complementary effects. That is, rule-based governance at the firm-level is enacted by strong institutional infrastructure. Third, some researchers favor a political theory of corporate governance, arguing that the form and effectiveness of firm-level governance are shaped by political forces, such as electoral system and the political orientation (e.g. left versus right) of the ruling parties, the government, or the nation as a whole (e.g., Pagano & Volpin, 2005; Roe, 2003).

All of the above theories suggest that the effect of corporate governance mechanisms should vary across countries and some of these conditions (e.g., ownership structure, country-level governance, and politics) may account for our cross-country results. Aguilera, Judge and Terjesen (2018) summarize these different firm- and country-level factors as *contingencies* for the effectiveness of corporate governance. To further understand the reasons that hinder or foster the effectiveness of corporate governance practices, we test the above theories on corporate governance contingencies. Our primary focus in this section is on the contingent effects of corporate rules (i.e., the Global E-Index), because they are more implementable at the

firm-level and their effects are shown to be varying across different subsamples in Table 7. We test the contingencies of the firm-level governance rules' effects on firm valuation by conducting analyses on its interaction with other external governance mechanisms as well as with institutional characteristics.

2.3.7.1 The *Contingent* Effect of Institutional Ownership

One argument on the universality of corporate governance practices is that the functioning of firm-level corporate rules crucially depends on the ownership structure of the firm (Bebchuk & Hamdani, 2009; Bebchuk & Weisbach. 2010). In particular, Bebchuk & Hamdani (2009) argue that the E-index principally measure take-over defenses, which are of limited relevance in countries in which most firms have controlling shareholders who can block any major decision. Ownership concentration and the presence of controlling shareholders may be endogenous (e.g., Demsetz & Lehn, 1985; Demsetz & Villalonga, 2001), and can have different and direct effects on firm value which have been well documented in the literature (e.g., Claessens, Djankov, Fan and Lang, 2002). However, our focus is on the governance role of ownership. While blockholdings by family or state are prevalent and may affect managerial incentives (La Porta, Lopez-de-Silanes, Shleifer, Vishny, 1999), the nature of agency problems and governance are very different in these firms Therefore, we focus on the role of institutional ownership which is usually considered as an external governance mechanism (e.g., Gillan & Starks, 2000; Aggarwal et al., 2011). To test the association between insider entrenchment and firm valuation conditional on institutional ownership as an external governance mechanism, we interact Global E-Index with various institutional ownership variables to see if its effect becomes stronger or weaker with more institutional shareholdings. We report the results for both Tobin's *q* as dependent variables in Table 9.

First, we find that the interactions between Global E-Index and various measures of institutional ownership are mostly negative and significant. Perhaps the most interesting results are those in Columns (1)-(2), in which the interaction of Global E-Index with Total IO and with Independent IO are negative and significant. As Total IO and Independent IO measure the aggregate ownership in a company held by professional institutional investors, these results speak to the overall contingency effect of institutional shareholdings. In unreported tests when we "inverse" the Global E-Index by using greater value to denote better governance, the coefficient of the interaction term is positive and significant. The results potentially imply that the costs of "poor" governance (i.e., strong entrenchment) are greater in firms where there are higher levels of monitoring by institutional investors. A plausible explanation is that stronger monitoring by institutional investors makes rule-based governance at the firm-level more important, as managerial entrenchment can be more harmful in firms that heavily reply on external monitoring. This finding potentially highlight the role of entrenchment is conditional on external governance mechanisms such as institutional holdings. These results are similar in spirit to the results of Aggarwal et al. (2009), which highlight the additional benefits of good governance in environments with more independent (i.e. institutional) ownership,

and potentially suggest that it may be costlier to enact value enhancing governance rules in firms with less institutional owners.

An alternative interpretation is that institutional investors may target badly governed firms (in which managers are more entrenched) so as to benefit more from the improvement in valuation under their monitoring. However, this interpretation is inconsistent with the findings by Ferreira & Matos (2008) that institutional investors usually target better governed firms to invest in the first place. Nevertheless, we acknowledge the plausibility of such explanation. However, along this line, a more plausible interpretation is that given that entrenchment has different effects on firm value, institutional investors usually target firms in which managerial entrenchment signifies agency problem, rather than firms in which entrenched managers act in the interest of the firm. In other words, firms with a high E-index value and no institutional investors may be inherently different from firms scoring high in the E-index and institutional owners. This interpretation can also explain why the effect of the Global E-Index is positive when Institutional Ownership takes the value of zero. Similar can be said for Column (3) and Column (4) when we interact the Global E-Index with the number of institutional investors and ownership by institutional blockholders (above 5% holdings), although the coefficient of the interaction term is insignificant in Column (3), and the coefficients of *IO Block Holders (5%)* are mostly negative in Table 5 and Table 7. The latter (i.e., results for *IO Block Holders (5%)*) may potentially suggest that while blockholding itself may negatively affect firm value in general, it can also serve to balance managerial power by reducing managerial entrenchment.

Table 9 also reports positive coefficients of the interaction terms *Global E-Index* × *IO Herf* (Column (5)) and *Global E-Index* × *Foreign IO* (Column (6)), suggesting that in firms with greater ownership concentration (by institutional investors) and foreign ownership, the negative effect of managerial entrenchment is weaker. The result in Column (5) is consistent with the argument by Bebchuk and Hamdani (2009) that managerial entrenchment becomes less of a governance issue in firms with concentrated ownership, as the nature of agency problem is fundamentally different in this type of firms. The result in Column (6) is consistent with the argument by Bena et al. (2017) that foreign institutional investors can exert a disciplinary role on entrenched corporate insiders worldwide. While the exact mechanisms for how corporate rules and institutional ownership interact may still need further investigation with more detailed data, these results point to the fact that managerial entrenchment does not universally affect firm value across firms with different ownership structures in the same way.

Table 2-9 The Role of Entrenchment Conditional on Institutional Ownership:

The dependent variables are Tobin's *q*. All firm level controls are winsorized at the 5th and 95th percentiles. The Global E-Index is constructed using the MSCI Governance Metrics data. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
Total IO $_{t-1}$ × Global E-Index $_{t-1}$	-0.179***					
	(0.014)					
Total IO $_{t-1}$	0.601***					
	(0.071)					
Independent IO $_{t-1}$ × Global E-Index$_{t-1}$		-0.184***				
		(0.015)				
Independent IO $_{t-1}$		0.624***				
		(0.074)				
Ln(Num IO) $_{t-1}$ × Global E-Index $_{t-1}$			-0.001			
			(0.006)			
Ln(Num IO) $_{t-1}$			0.462***			
			(0.029)			
IO Block Holders$_{t-1}$ × Global E-Index $_{t-1}$				-0.321***		
				(0.034)		
IO Block Holders $_{t-1}$				0.349***		
				(0.098)		
IO Herf $_{t-1}$ × Global E-Index $_{t-1}$					0.194**	
					(0.083)	
IO Herf $_{t-1}$					-1.044***	
					(0.173)	
Foreign IO $_{t-1}$ × Global E-Index $_{t-1}$						0.139***
						(0.038)
Foreign IO $_{t-1}$						0.437***
						(0.125)
Global E-Index $_{t-1}$	0.075***	0.072***	-0.022	0.021**	-0.034***	-0.038***
	(0.012)	(0.012)	(0.031)	(0.010)	(0.010)	(0.010)
Observations	16,365	16,365	16,365	16,365	16,365	16,365
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	All	All	All	All	All

2.3.7.2 The *Contingent* Effect of Country-level Governance

Another view considers legal rules and other country-level governance institutions as central and that good governance is achieved through rules that protect minority investors. This approach can be effective if a common set of rules can be applied to a broad spectrum of countries and a broad spectrum of firms within a country (Black, de Carvalho, Gorga, 2012; Aggarwal et al., 2009). Our investigation of how country-level governance interacts with firm-level rule-based governance is in line with Aggarwal et al. (2009), especially

with regard to whether country-level governance and firm-level governance are substitutive or complementary.[28] More specifically, we interact the Global E-Index with the aforementioned investor protection indices (ADRI, ASDI, and the common law dummy) as in Table 10, as well as with the institutional quality indices (using the World Bank's World Governance Indicators) as in Table 11. For simplicity, we call both investor protection indices and World Bank institutional quality indices "country-level governance".

Again, some interesting observations can be made. First, in both Table 10 and Table 11, we find that the effect of the E-index crucially depends on country-level institutional quality, and their interactions are mostly negative, especially with World Governance Indicators. The negative association between insider entrenchment and firm value is stronger in countries with stronger governance rules. Again, when we inverse the value of the E-index, the coefficient of the interaction term becomes positive. That is, when country-level governance rules are strong, the role of firm-level corporate governance is also greater. This is consistent with the "complement" view of corporate governance and we show that similar effects persist with regards to institutional quality—not just investor protection—as well on a on a global scale (not only from a US vs. non-US angle). Similar to the earlier results conditional on institutional ownership, the results imply that in an environment with strong institutional quality, entrenchment is related to lower firm valuation. Since firms usually do not choose their institutional environments, a more plausible interpretation of these results may be that it is costlier to enact good rule-based corporate governance (i.e. lower E-index values) in countries with lower levels of institutional development. These results further cast doubt on the feasibility of setting a global governance standard in terms of corporate rules at the firm-level. Indexes on corporate level investor protection such as the one used by Aggarwal, Erel, Stulz, & Williamson (2009) may be misleading if they are designed from the perspective of protecting shareholders in the U.S. situation, namely, when ownership structure is relatively dispersed, institutional shareholdings are relatively prevalent, and investor protection by laws are relatively strong.

[28] Aggarwal et al. (2008) examined whether comparable non-U.S. firms chose higher or lower levels of investor protection compared to similar U.S. firms. They found that compared to matching firms, only a small proportion of foreign firms have a higher index value (for investor protection), yet the value of foreign firms fell as the index decreases relative to the index of matching U.S firms.

Table 2-10: The Role of Entrenchment Conditional on Investor Protection Laws

The dependent variables are Tobin's *q*. All firm level controls are winsorized at the 5th and 95th percentiles. The Global E-Index is constructed using the MSCI Governance Metrics data. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Dependent variable = Tobin's q	(1)	(2)	(3)
Common Law × Global E-Index $_{t-1}$	-0.022***		
	(0.004)		
Common Law	0.034**		
	(0.014)		
Anti-Self-Dealing Index × Global E-Index $_{t-1}$		-0.014	
		(0.023)	
Anti-Self-Dealing Index		-0.052	
		(0.078)	
Anti-director Rights Index (ADRI) × Global E-Index $_{t-1}$			-0.048***
			(0.009)
Anti-director Rights Index (ADRI)			0.186***
			(0.034)
Global E-Index $_{t-1}$	0.076***	-0.015	0.010
	(0.022)	(0.017)	(0.011)
Observations	19,353	19,932	19,932
Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Country FE	No	No	No
Countries	All	All	All

Table 2-11: The Role of Entrenchment Index Conditional on Institutional Quality

The dependent variables are Tobin's *q*. All firm level controls are winsorized at the 5th and 95th percentiles. The Global E-Index is constructed using the MSCI Governance Metrics data. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Dependent variable = Tobin's q	(1)	(2)	(3)	(4)	(5)	(6)
Rule of Law $_{t-1}$ × Global E-Index $_{t-1}$	-0.080***					
	(0.009)					
Rule of Law $_{t-1}$	-0.012					
	(0.033)					
Regulatory Quality $_{t-1}$ × Global E-Index $_{t-1}$		-0.065***				
		(0.010)				
Regulatory Quality $_{t-1}$		-0.064*				
		(0.036)				
Voice and Accountability $_{t-1}$ × Global E-Index $_{t-1}$			-0.065***			
			(0.008)			
Voice and Accountability $_{t-1}$			0.129***			
			(0.030)			
Political Stability $_{t-1}$ × Global E-Index $_{t-1}$				-0.037***		
				(0.010)		
Political Stability $_{t-1}$				-0.037		
				(0.031)		
Government Effectiveness $_{t-1}$ × Global E-Index $_{t-1}$					-0.077***	
					(0.011)	
Government Effectiveness $_{t-1}$					0.094***	
					(0.030)	
Control of Corruption $_{t-1}$ × Global E-Index $_{t-1}$						-0.037***
						(0.008)
Control of Corruption $_{t-1}$						0.147***
						(0.030)
Global E-Index $_{t-1}$	0.097***	0.061***	0.045***	-0.006	0.087***	0.029**
	(0.016)	(0.015)	(0.012)	(0.010)	(0.018)	(0.013)
Observations	19,932	19,932	19,932	19,932	19,932	19,932
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Combining the results of the conditional effects of institutional quality—which is arguably more exogenously determined—with that on institutional ownership, they seem to suggest that strong external governance mechanisms (institutional investors and regulations) make internal governance mechanisms (i.e., curbing managerial entrenchment) more important. External governance and internal governance are more complementary than substitutive, consistent with the argument by Aggarwal et al. (2009).

2.3.7.3. The *Contingent* Effect of Political Institutions

A third view emphasizes the importance of the political economy in influencing the effectiveness of corporate governance practices. Political institutions matter for how some polities settle conflict, or the ways in which corporate players team up to work together, can affect insider-shareholder relationship (Roe, 2003). They also matter as political orientation and electoral systems can fundamentally determine how those in power can influence protections towards shareholders vis-à-vis other stakeholders such as employees (Pagano & Volpin, 2005). For example, lobbying by interest groups such as powerful corporate insiders and incumbent firms seeking to retain market power can lead to weaker protections for outside insiders and new entrants (Rajan and Zingales, 2003, 2004;; Bebchuk and Neeman, 2009). Similarly, Pagano & Volpin (2005) argue that proportional electoral systems (vis-à-vis majoritarian systems) are conductive to weaker investor protection and in favor of stronger employment protection. Despite their importance, there has been limited research on the political determinants of firm-level corporate governance practices and their effectiveness, especially from a systematic and global perspective. Our empirical investigation here, albeit mostly exploratory, aims to provide some systematic evidence of such political contingency by focusing on the ruling government's political orientation and the country's electoral system. Specifically, we examine the country-level political determinants of firm-level corporate governance by first interacting our Global E-Index with a dummy indicating right-leaning governments which are those defined as conservative, Christian democratic or right-wing, which are often regarded as more pro-business, and favorable towards capital markets, investors and management (vis-à-vis other stakeholders) (Potrafke, 2010). Similarly, we also interact the country's electoral system—proportional vs. majoritarian—with the Global E-Index to explore its political contingency. We report the interaction results in Table 12.

We find that firms operating in countries with right-leaning governments and with proportional electoral systems have higher Tobin's *q* when they score higher on the Global E-Index (i.e. the interaction is positive). The result in Column (1) indicates that the negative effect of entrenchment on firm value is weaker when the political regime is more right-leaning which is arguably more pro-business. The result in Column (2) suggests that a proportional (vis-à-vis majoritarian) electoral system also attenuates the negative association between entrenchment and firm value. At the first sight, the

latter result seems to be inconsistent with the argument made by Pagano & Volpin (2005) that the proportionality of the electoral system is negatively correlated with shareholder protection (vis-à-vis employment protection). The argument by Pagano & Volpin (2005) is in line with what has been argued in the political science literature that proportional representation is more favorable for the election of left-wing parties into power (e.g., Doring & Manow, 2017), which are less likely to support pro-shareholder policies. A key distinction of our empirical investigation is that we focus on the *conditional* (instead of *direct*) effect of electoral system on insider entrenchment and valuation at the firm-level. In this regard, our results may suggest that while electoral rules and the executive party's political leaning may be interrelated, they may also function differently on influencing the governance effects at the firm level. Both pro-business polities and broader participation tend to lead to stronger investor protection (Perotti, 2014). While the exact mechanisms require further in-depth analysis, one might argue that firms with greater entrenchment of their insiders are better at capturing value in such environments by focusing more on the long-term. Of course, this analysis is exploratory in nature as a first step towards understanding the political effects of corporate governance efficiency, and we show that certain types of political movements (e.g. right wing dominated governments) affect corporate governance outcomes.

Table 2-12: The Role of Entrenchment Conditional on Political Institutions

The dependent variables are Tobin's *q*. All firm level controls are winsorized at the 5th and 95th percentiles. All regressions control for firm characteristics and fixed effects are reported at the bottom of the table. Precise definitions for each variable can be found in the Appendix. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the firm level and reported in parentheses.

Dependent variable = Tobin's Q	(1)	(2)
Right-leaning Executive Party $_{t-1}$ × Global E-Index $_{t-1}$	0.024**	
	(0.010)	
Right-leaning Executive Party $_{t-1}$	-0.014	
	(0.025)	
Proportionality × Global E-Index $_{t-1}$		0.031***
		(0.005)
Proportionality		-0.019
		(0.017)
Global E-Index $_{t-1}$	-0.033***	-0.039***
	(0.009)	(0.009)
Observations	16,311	19,220
Controls	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
Countries	All	All

In summary, our empirical evidences suggest that the role of entrenchment or corporate-rule-based governance mechanisms cannot universally apply across countries, and are contingent on the firm's ownership structure (especially with regard to institutional ownership), country-level

governance, and the political environments. Of course, there may be other contingencies to corporate governance, such as cultures. However, cultural traits are notoriously difficult to measure, and the main purpose of our research is to provide relevant policy and managerial implications with respect to corporate governance reforms at both the country-level and the firm-level, leaving limited room for considering the roles of culture and social norms. In addition, culture and norms are very time-invariant, making them poor candidates to explain phases of corporate governance change and financial evolution (Perotti, 2014). Our ultimate goal is to shed light on which aspects of governance matter for firm valuation and under what conditions. Therefore, we only focus on a few important firm- and country-level governance mechanisms discussed above.

2.4 Evidence from Shareholder Proposals on Corporate Governance Around the World

So far, our results indicate that while the effects of corporate-rule-based governance mechanisms (proxied by the global E-index) are conditional on external governance and political environments, the effects of institutional shareholdings seem to be universally associated with higher firm valuation. We try to provide further evidence on whether corporate governance enacted by institutional investors is correlated to higher firm valuation across the world. A widely accepted argument in the literature is that institutional investors can act as monitors and use their activism to influence the decision-making process within their portfolio companies during board meetings and with proxy fights, and help spill over similar governance practices across national borders (e.g., Aggarwal et al., 2011). Even for passive investors, their sponsors have an incentive to use "voice" to compete with active fund managers and force their funds to actively engage with their portfolio firms on improving their corporate governance and reducing underperformance (Fisch, Hamdami, Solomon, 2018).

Of course, there are many ways institutional investors can monitor the portfolio company. For identification purpose, we focus on one particular but also commonly used way, namely, through sponsoring governance proposals during portfolio companies' shareholder meetings. To do so, we collect data from the ISS Voting Analytics database for both US and Global Voting Outcomes datasets. This is the most comprehensive dataset available on voting data for U.S. and global companies. The detailed voting outcomes data are available from 2003 (for global companies, most data are available from 2013 onwards). The dataset provides over 700,000 historical votes, sponsored by management or shareholders, as well as information on the threshold of passage, as some proposals are not necessarily passed by more than 50% of support. For example, they may only be considered as being passed if they receive supporting votes that are over 66.7% or 75% of the cumulative votes. Our analysis uses actual thresholds of passing instead of the conventional 50% threshold. We then apply a regression

discontinuity design (RDD) on both the U.S. sample and the global sample by focusing on the stock market reaction to the passage of governance-related close-call proposals. The identification is based on the assumption that the passage of such close-call proposals is akin to a randomly assigning shareholder-initiated (i.e., mostly by institutional investors) governance to companies and hence is less likely to be correlated with firm characteristics. This is similar to Cunat, Gine and Guadalupe (2012) who find positive abnormal returns around the passage of close-call governance proposals for U.S. companies. They interpret the result as being suggestive that corporate governance enacted by shareholder proposals is value-enhancing thus induces favorable stock market reaction.

An example of such close-call proposal is CA Immobilien Anlagen AG, whereby the shareholder proposal won by a small margin. By looking at the voting data, one can infer that shareholders wanted to increase the size of the board, whereas management wanted to decrease the size of the board—they had their own proposal opposing that of the shareholders. The management proposal consequently failed by a small margin. Overall, shareholders won, and analysts seem to have agreed with their case as the cumulative abnormal return around the voting day was positive. In addition, to rule out potential confounding effect, we drop the observations for which there were several proposals being voted on the same day.[29]

We limit our analysis to shareholder proposals (vis-à-vis management proposals), similar to Cunat et al. (2012), because we are mostly interested in the effects of votes cast by institutional investors and it is commonly understood that the vast majority of shareholder proposals are cast by these investors. We then employ the RDD analysis as in Cunat et al. (2012) by comparing the stock market reaction, i.e., the cumulative abnormal returns (CARs) by the voting company, to shareholder proposals which passed or failed by a small margin.[30] We visually show the CARs around the required threshold for both the U.S. sample and the non-U.S. sample in Figure 1. Due to the nature of RDD which relies on a narrow margin around the passing threshold, we are not able to split our sample into more granular subsamples as what we did in Table 7, otherwise for most subsamples we won't have sufficient observations. Nevertheless, we still find that investors in general react positively to the passage of shareholder proposals by institutional shareholders for both the U.S. subsample and for the non-U.S. subsample. In other words, this may potentially indicate that the external governance by institutional investors is not only a value adding mechanism in the U.S. as shown by Cunat et al. (2012) but also

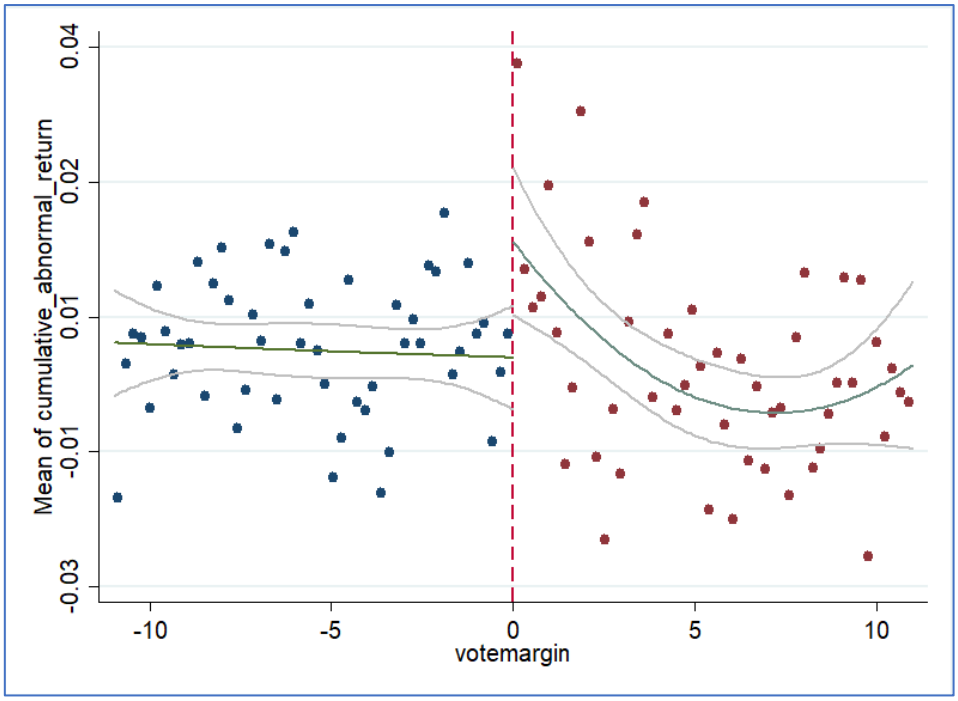
[29] An example of a confounding event is the shareholder sponsored vote by Bellamy's Australia Limited, which failed by a margin of about 7%. Interestingly, in this case, the cumulative abnormal return was positive, however, there was a clear reason for this. On that same day, there were two other votes taking place, and in the end, all three votes were surrounding the election of specific directors. Shareholders ended up winning the other two proposals by a margin of 11% and 15%, which explains why the cumulative abnormal return was positive.

[30] The cumulative abnormal returns are computed in the three days surrounding the shareholder vote and the expected returns are obtained from the market model estimated over a period starting 280 days before the vote until 30 days before the vote.

globally. In Table 13, we formally report the RDD estimates of the difference in abnormal returns between proposals that pass and proposals that do not pass for varying intervals along the threshold of the day of the vote. As the votes are restricted to samples close to the threshold, the results become more and more positive. To be more precise, for the votes within two percentage points of the threshold, the abnormal returns are positive and significant for the proposals that passed for both the U.S. and non-U.S. samples. The average difference in one-day abnormal return between firms that marginally pass or fail a governance proposal is about 80 basis points for U.S. firms, and 4% for non-U.S. firms. The magnitude of results for the U.S. sample is slightly smaller but still quite comparable to that of Cunat et al. (2012) results (+0.013), and the results for non-U.S. firms are much larger, probably due to the fact that the sample sizes are much smaller for non-U.S. firms. Again, despite our identification using the RDD setting, our results should still be interpreted with caution, especially in light of the caveat by Bach & Metzger (2018) that one cannot routinely use an RDD to identify the causal effects of changes in corporate governance generated by shareholder votes. Nevertheless, these results provide further supporting evidence for the universality of institutional ownership on firm value.

Figure 2-1: Evidence from Close-Call Shareholder Proposals on Corporate Governance

(a) US Sample



(b) non-US Sample

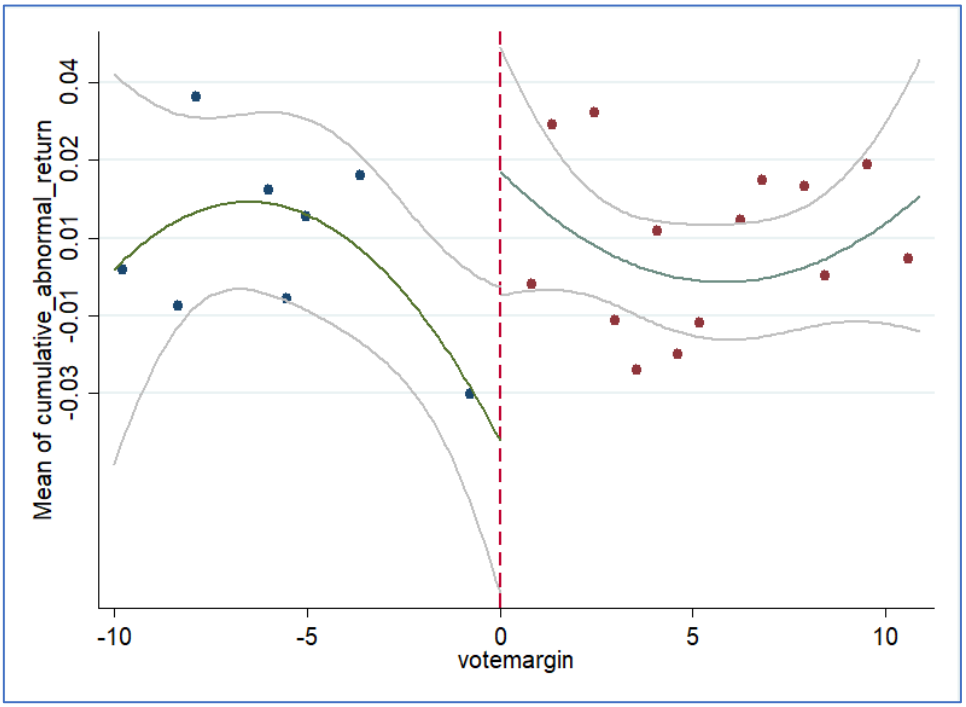


Table 2-13: Close-Call Shareholder Proposals and Abnormal Returns

This table presents regressions of the abnormal returns on the day of the vote on whether the shareholder proposal passed. Cumulative abnormal returns are computed in the three days surrounding the shareholder vote. Expected returns are obtained from the market model estimated over a period starting 280 days before the vote until 30 days before the vote. Each column restricts the sample to observations within certain points of the threshold indicated at the bottom of the table. Columns 1-3 restricts the sample to votes for US firms and columns 4-6 to non-US firms. *, **, *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	US Sample			Non-US Sample		
	(1)	(2)	(3)	(5)	(6)	(7)
Pass	-0.002	0.003	0.008*	0.005	0.007	0.040**
	(0.002)	(0.003)	(0.005)	(0.009)	(0.011)	(0.017)
Observations	555	292	112	46	32	11
R-squared	0.002	0.004	0.026	0.006	0.015	0.395
Vote Margin (%)	-12;+12	-6;-6	-2;-2	-12;-12	-6;-6	-2;-2

2.5 Conclusion

By now, there has been substantial evidence that one size does not always fit all firms in all countries. Optimal governance likely differs across different countries. Within a given country, optimal governance may depend on firm characteristics such as the ownership structure, especially with the involvement of institutional shareholders. To date, we still know relatively little about the extent to which broad corporate governance principles can be applied across countries. More importantly, if one size doesn't fit all, under what conditions do the commonly well-regarded "good" governance practices can enhance firm value?

With increasing globalization, the emergence of institutional investors, and changes in governance regulation[31] around the world, the landscape of corporate governance has been rapidly changing as well. It is therefore important to understand which corporate governance mechanisms are effective and under what conditions. Legal rules can be effective if many corporate governance practices are universal, so that a common set of rules can be applied to a broad spectrum of countries, and a broad spectrum of firms within each country. In contrast, if good corporate governance is often "local", i.e., varying across countries, and across firms within a country, a more flexible approach will often be appropriate, such as comply or explain rules and multiple governance stock exchange listing tiers (Black, de Carvalho, Gorga, 2012). Similar debates exist regarding whether there are "best management practices" or whether every management practice is contingent (e.g., Bloom, Genakos, Sadun, Reenen, 2012).

[31] For example, the securities regulators in Hong Kong and Singapore recently removed the "one-share-one-vote" requirement for stock exchange listing. See the Economist article: www.economist.com/finance-and-economics/2018/03/01/hong-kong-and-singapore-succumb-to-the-lure-of-dual-class-shares.

Our approach combines firm-level agency and institutional context, which is a useful framework to understand corporate governance deviance around the world (Aguilera, Judge, Terjesen, 2018). Our findings shed light on the recent literature of how governance can travel around the world due to the role of institutional investors, but also cast doubt on the effectiveness of such global governance transmission. They also echo the argument by Goshen & Hannes (2018) on the rise of institutional ownership and the death of corporate law in shaping governance in the U.S. As they argue, the more competent shareholders become, the less important corporate law will be. Our results show that, in an increasingly globalized era, such a shift in the composition of the capital markets and the importance of different governance mechanisms is not only happening in the U.S. but also around the world.

The analysis we have conducted here is in many ways exploratory, and many open questions remain. We have mainly analyzed firm-level governance rules that are strongly related to anti-takeover mechanisms—a typical way of obtaining corporate control in the U.S.—and their institutional contexts across other countries. There are many other governance arrangements outside the U.S., such as board structure (e.g., one-tier vs. two-tier board and different committees on the board) and CEO compensation contract, which we remain silent on. Also, given the lack of systematic governance reforms across the world, it is very difficult to pin down causality and the exact mechanisms through which governance affects firm value. More studies across various fields (e.g., finance, economics, accounting, management, law) can be done to further explore the other aspects of governance and the channels through which they affect firm value. Nevertheless, taking our findings at the face value, an important policy implication is that not all governance practices can be universally applied, and investors and policymakers need to pay attention to the institutional contexts of corporate governance in a more granular fashion.

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2.7 Appendix

Table 2-14: A1 Summary of the Data Sources

Variables	Source	Definition
Total IO	FactSet/LionShares	Total institutional ownership ratio in percentage of market capitalization
Independent IO	FactSet/LionShares	Ownership by independent institutions in percentage of market capitalization
Ln(Num IO)	FactSet/LionShares	The natural logarithm of the number of institutional owners
IO Herf	FactSet/LionShares	The Herfindahl-Hirschman index of institutional ownership of a company (a measure of concentration of institutional ownership)
IO Block Holder	FactSet/LionShares	Institutional ownership by institutional block holders (>=5%) in percentage of market capitalization
Foreign IO	FactSet/LionShares	Foreign institutional ownership ratio in percentage of market capitalization
(1) Staggered Board	Asset4 ESG	Dummy for whether the company has a staggered board structure (CGSRDP053)
(2) Poison Pill	Asset4 ESG	Dummy for whether the company have a poison pill (CGSRDP050)
(3) Golden Parachute	Asset4 ESG	Dummy for whether the company has a golden parachute or other restrictive clauses related to changes of control (compensation plan for accelerated pay-out) (CGSRDP055)
(4) Super-majority Requirement	Asset4 ESG	Dummy for whether the company has a supermajority vote requirement or qualified majority (for amendments of charters and bylaws or lock-in provisions) (CGSRDP054)
(5) Other Anti-Takeover Devices	Asset4 ESG	Dummy for whether the company has some other form of anti-takeover device (limitation of director liability, people pill, customer refund programme, etc.) (CGSRDP063)
(6) Election Super-Majority Requirement	Asset4 ESG	Dummy for whether the company's board members are generally elected with a majority vote (CGSRDP033)
Thomson Reuters Entrenchment Index	Asset4 ESG	(1) + (2) + (3) + (4) + (5) + (6)
(i) Classified Board	MSCI	Do all directors stand for annual re-election? Flagged if no.
(ii) Poison Pill	MSCI	Has the company adopted a shareholder rights plan (“poison pill”)? Flagged if yes.
(iii) Golden Parachute	MSCI	Does the CEO’s potential cash severance pay exceed five times their annual pay? Flagged if yes.
(iv) Charter Amendments	MSCI	Does the company have the unilateral right to amend the company’s articles/ constitution without shareholder approval? Flagged if yes.
(v) Bylaws Amendments	MSCI	Does the company have the unilateral right to amend the company’s bylaws without shareholder approval? Flagged if yes.
MSCI Entrenchment Index	MSCI	(i) + (ii) + (iii) + (iv) + (v)
Bebchuk et al. Entrenchment Index	Website	Their file contains the original Entrenchment Index for the period 1990-2006 for all the firms followed by the Investor Responsibility Research Center. For details on the construction of the entrenchment index, see Bebchuk, Lucian, Alma Cohen, and Allen Ferrel, "What Matters in Corporate Governance?" Review of Financial Studies

		(2009), available at http://papers.ssrn.com/abstract=593423. The original E-index provisions included i) Staggered board: a board in which directors are divided into separate classes (typically three) with each class being elected to overlapping terms ii) Limitation on amending bylaws: a provision limiting shareholders' ability through majority vote to amend the corporate bylaws iii) Limitation on amending the charter: a provision limiting shareholders' ability through majority iv) vote to amend the corporate charter v) supermajority to approve a merger: a requirement that requires more than a majority of shareholders to approve a merger vi) Golden parachute: a severance agreement that provides benefits to management/board members in the event of firing, demotion, or resignation following a change in control vii) poison pill: a shareholder right that is triggered in the event of an unauthorized change in control that typically renders the target company financially unattractive or dilutes the voting power of the acquirer.
a) Classified Board	ISS RiskMetrics	Classified Board
b) Poison Pill	ISS RiskMetrics	Poison Pill
c) Golden Parachutes	ISS RiskMetrics	Golden Parachutes
d) Limit Ability to Amend e) Charter	ISS RiskMetrics	Limit Ability to Amend Charter
f) Limit Ability to Amend ByLaws	ISS RiskMetrics	Limit Ability to Amend ByLaws
g) Supermajority in mergers dummy	ISS RiskMetrics	Supermajority in mergers dummy equal to one if supermajority requirement exceeded 66%.
ISS Entrenchment Index	ISS RiskMetrics	(a) + (b) + (c) + (d) + (e) + (f) + (g)
Supervisory Board	Orbis	Dummy equal to one if Orbis identifies the firm as having supervisory board or an advisory board.
GDP per capita	World Bank Indicators	GDP per capita (current US\$)
Control of Corruption	World Bank Indicators	capturing perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests
Government Effectiveness	World Bank Indicators	capturing perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government's commitment to such policies
Rule of Law	World Bank Indicators	capturing perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence.
Regulatory Quality	World Bank Indicators	capturing perceptions of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development.
Political Stability & Absence of Violence	World Bank Indicators	capturing perceptions of the likelihood that the government will be destabilized or overthrown by unconstitutional or violent means, including politically-motivated violence and terrorism.
Voice and Accountability	World Bank Indicators	capturing perceptions of the extent to which a country's citizens are able to participate in selecting their government, as well as freedom of expression, freedom of association, and a free media.

Right-leaning Executive Party	World Bank Database of Political Institutions	Dummy variable equal to one if chief executive party is right orientated
Proportionality	World Bank Database of Political Institutions	Indicator for the proportionality of the voting system as used by Pagano and Volpin (2005), combining a single indicator of three variables that describe the electoral system. These include PR (equals one if at least some cadidates are elected via proportional rule), PLURALITY (if at least some legislators are elected via a majoritarian rule) and HOUSESYS (if most seats are allocated via a majoritarian rule. The indicator is defined as PR – PLURALITY – HOUSESYS + 2. If the variable equals 3, all the seats are assigned via a proportional rule (pure proportionality) and 0 if no seats are assigned this way (pure majoritarian)
Legal Origin	Website	Indicator for whether a country is from a Common (1), French Civil (2), German Civil (4) or Scandinavian Civil (5) law origin (LaPorta, Lopez-de-Silanes, and Shleifer, 2008) - http://scholar.harvard.edu/shleifer/publications
Anti-Self-Dealing Index (ASDI)	Website	Measures legal protection of minority shareholders against expropriation by corporate insiders (Shleifer, Djankov, LaPorta, and Lopez-de-Silanes, 2008) - http://scholar.harvard.edu/shleifer/publications
Anti-Director Rights Index (ADRI)	Website	Measures how shareholders and creditors are protected by law from expropriation by the managers and controlling shareholders of firms, compiled by LaPorta, Lopez-de-Silanes, Shleifer, and Vishny (1998) and updated by La Porta, Lopez-de-Silanes, and Shleifer (2008) - http://scholar.harvard.edu/shleifer/publications
Shareholder Proposals	ISS Voting Analytics	Institutional Shareholder Services (ISS) Voting Analytics provides corporate voting results at the proposal level. The data is available for both management and shareholder proposals. The coverage includes the S&P500 from 1997-2006 and the Russell 3000 from 2003 onwards.
Ln(Assets)	WorldScope	The natural logarithm of total assets in US dollars (item 02999)
Leverage	WorldScope	Total debt (item 03255) divided by total assets (item 02999)
Sales Growth	WorldScope	Two-year average growth rate in net sales (item 01001)
Cash/Total Assets	WorldScope	Cash and short-term investments (item 02001) divided by total assets (item 02999)
CAPEX/Total Assets	WorldScope	Capital expenditures (item 04601) divided by total assets (item 02999)
Tobin's *q*	WorldScope	Market Capitalization (WC08001) + Total Liabilities (WC03351) / Common Stock (WC03501) + Total Liabilities (WC03351)

3. FINANCE AND DEMAND FOR SKILL: EVIDENCE FROM UGANDA[32]

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Abstract

We explore the empirical interaction between firm growth, financing constraints and job creation. Using a novel small-business survey from Uganda, we find that the extent to which small businesses expand skilled employment as their sales and profits increase is significantly related to access to external funding, while the hiring of casual and family workers is not. The results are robust to the inclusion of various firm level controls, region and sector fixed effects. We support our findings by providing empirical evidence using planned hiring regression specifications.

Keywords: Financial Access, Trained and Experienced Employment, Uganda.

JEL Classification Codes: O15, 016, 055.

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3.1 Introduction

Creating stable employment opportunities is a primary concern for policymakers in many developing economies. Most developing countries in Africa have grown rapidly over the past decade, even as the Global Recession has gripped many European countries and the United States. Since most of its countries have become independent around 1960, Sub-Saharan Africa has experienced the best period of growth between 2000-2012, where GDP grew more than 4.5% annually on average. However, this growth has not translated into high growth rates in job creation, as in many African countries, growth has primarily relied upon oil, gas and mineral extraction. Even though the number of industrial sector jobs is projected to increase by 55 percent over the next 10 years, the growth comes from a small base and will not come close to absorbing the millions of new workers entering the labor force each year (Brooks et al., 2014). What is even more challenging is that many educated workers also fail to be absorbed into the labor market. The lack of employment opportunities for highly skilled and trained workers raises the question of whether there is a shortage of firm-level demand for skill in African economies or there are other constraints holding back their inclusion in the labor market.

The vast literature on finance and growth has shown the importance of access to external funding for firm-level investment decisions, economic development and growth (see Levine, 2005, for an overview). Access to credit remains difficult for firms in Sub-Saharan Africa and continues to top the policy agendas concerning African economic development. In this context, while several papers have documented the relationship between financing constraints and capital investment and growth, there are relatively few papers gauging the importance of financing constraints for hiring decisions, especially of skilled workers with experience and training associated with a particular occupation. The aim of this paper is to close this gap and investigate the role of financial constraints in firms' skilled labor demand in developing countries. Specifically, using a unique small-business survey from Uganda, we test whether the likelihood of skilled job creation at profitable businesses rises with access to external finance.

Economic theory predicts a critical role of financing constraints in hiring decisions, similar to the role that they have for capital investment (Benmelech et al., 2011). First, timing differences between when wages are paid and when revenues are received requires working capital, and the lack of it can constrain labor demand. This results in a stronger financing constraint in the case of hiring skilled workers, because skilled employees often get paid a skill-premium and hence receive higher wages. In addition, the expected contribution of skilled workers for future revenues might be relatively more intangible than that of unskilled labor - raising firm-level uncertainty and preventing the efficient allocation of external finance to productive

users. Second, if hiring of permanent staff implies fixed costs (including training or firing costs), this might require additional funding especially for smaller firms, again an effect which is expected to be stronger for trained and experienced workers (Oi, 1962). Third, job creation might be restricted by financing constraints if capital and labor are complementary factors in the production process: access to finance can yield access to "new machinery" and stimulate the demand for "skill" to operate such newly installed machinery. Finally, the decision whether to hire permanent or fixed-term employees (Caggese and Cuñat, 2008) is likely to be more pronounced for hiring of trained and experienced workers, for whom the outside options are larger. We expect all of these channels to be stronger at profitable firms on growth paths, where demand for skilled and experienced workers should be higher. We evaluate the empirical implications of financial access for skill-growth among small businesses, where development of in-house human capital is limited and access to skilled-employees is likely to be possible only through the labor market.

We empirically explore the interactions between financing constraints, firm growth - measured in sales (as well as in profits) - and job creation. Using comprehensive small-business survey data collected in 2013 from a nationwide representative sample of 1,839 Ugandan companies, we test the extent to which access to external bank funding conditions the relationship between firm growth and employment creation. In our empirical analysis, first, following Popov and Udell (2012), Brown, Ongena, Popov and Yein (2011), Cox and Jappelli (1993), and Duca and Rosenthal (1993), we isolate demand from supply-side financial constraints by distinguishing between firms that (i) applied for and received a loan, (ii) were rejected or discouraged from applying for a loan, and (iii) state that they did not need a loan. Second, our survey also allows us to distinguish between different types of employee categories at a given establishment such as trained and experienced, as well as permanent, casual and family/friends. While theory predicts a constraining effect of lack of access to finance on the hiring of skilled and trained workers, it does not do so for casual workers or family workers. As a result, we can exploit this difference in our hypothesis testing. Third, we differentiate between growing and non-growing firms, which will determine the demand for labor. Finally, as a key robustness test we relate today's financing constraints to future hiring plans.

While we use data for one specific Sub-Saharan African country, Uganda resembles in its economic and demographic structure many other developing economies. Uganda is a landlocked low-income country in Africa, which has experienced high GDP growth over the recent years, reaching 3.2% in 2013 (during our sample period) and projected rates of 5.5% for 2017. However, the vast majority of Uganda's labor force remains employed in relatively low productivity sectors, such as agriculture (World Bank, 2013, 2016). Ugandans with higher education are more likely to be unemployed and to under-utilize their skill sets; many educated workers are employed in a job ill-suited to their skills or emigrate to find appropriate

employment (EDPRD Uganda, 2014). Uganda has the world's youngest population with over 78% of its population below the age of 30; as the population continues to grow at a rate of 3.2% annually, the country has one of the highest youth unemployment rates in Sub-Saharan Africa (The State of Uganda Population Report, 2013). Many of the new entrants are highly educated as a result of past policies to encourage school enrolment and completing further education. The challenge for Ugandan policy makers is therefore to oversee the labor force's effective transition from a predominantly low productivity and agriculture based economy to a high-human capital intensive - manufacturing and service sector based - economy.

Our empirical analysis shows that growing and profitable firms only hire experienced and trained employees if they are not rejected or discouraged from applying for external finance. In our regression analysis we do not find such a significant relationship in the case of hiring casual employees or family and friends, suggesting that - in line with theory - financing constraints are more likely to bind in the context of employment contracts associated with experienced and trained employees with high human capital intensity. This empirical evidence is robust to the inclusion of a large set of firm level controls as well as region and sector fixed effects. Our results are in line with prior and well established conclusions emphasized by Kaplan and Zingales (1997), who document that firms on a growth path are more likely to invest in physical capital when provided with access to finance and Almeida, Campello and Weisbach (2004) who show that constrained firms have a positive cash flow sensitivity of investment. Our findings are also robust to alternative classifications of financial access. Finally, the key empirical results also hold when we regress firms' future hiring intentions (i.e. planned hiring) on firm performance and financial access - after controlling for recent hiring trends at the firm.

The findings from our research have important policy implications as they underline the importance of well developed financial systems for job creation. As policy makers grapple with the challenge of creating formal and permanent jobs in a still growing society, financial sector policies can be critical. The inability of constrained firms to hire skilled labor even though they are performing well is an issue for economic development and highlights the misallocation of high quality labor. Our results also show that beyond helping firms grow faster, more efficient financial systems can also have an impact on poverty alleviation by creating urgently needed jobs.

Our paper contributes to this literature by gauging the role of financing constraints in the relationship between firm growth and skilled job creation in a low-income country. In this respect, our paper is related to several strands of literature. First, we add to the broad literature on financing constraints in developing economies. This literature includes a series of papers that investigate the effects of firm

financing constraints on firm growth (such as Ayyagari et al., 2008; Beck et al., 2005; Beck et al., 2008) as well as firm sales and performance (Banerjee and Duflo, 2014; Zia, 2008; De Mel et al., 2008).

Second, and more specifically, we add to a smaller and more recent literature on the relationship between financing constraints and job creation. In this research frontier, on the aggregate level, Pagano and Pica (2012) show a positive and significant relationship between financial development and job creation in developing countries, while also uncovering that employment grows slower during down-turns in industries with high external-finance dependence. Using a general equilibrium model of occupation choice Giné and Townsend (2004) argue that for Thailand financial liberalization has contributed to migration of subsistence agricultural workers into urban salaried jobs. Another strand of this literature concentrated on the effects of bank finance on employment patterns. For instance, for the case of U.S., Beck, Levine and Levkov (2010) and Benmelech, Bergman and Seru (2011) show that branch deregulation and consequent financial liberalization led to decreases in unemployment and increased labor market participation especially among low-skilled workers. Chodorow-Reich (2014) shows that firms with a pre-crisis relationships with less healthy lenders had higher reductions in employment following the Lehman bankruptcy compared to pre-crisis clients of healthier lenders. In a similar study Cingano et al. (2016) show that bank exposure to the credit shock predicts firm investment rates, resulting in lower levels of firm employment. Finally, Popov and Rocholl (2017) study the impact of exogenous funding shocks to German savings banks on firms' labor market behavior and find that firms which have credit relationships with banks exposed to the mortgage crises experienced a significant decline in labor demand during the Global Financial Crisis.

Several papers gauge the differential effect of financing constraints on different categories of employment. Benito and Hernando (2007) show for a sample of Spanish firms from 1985-2000 that higher cash flow is associated with relatively more temporary employment while higher levels of financial pressure is associated with relatively lower levels of temporary employment. Caggese and Cuñat (2008) show for a sample of Italian firms that financing constraints bias enterprises towards fixed-term rather than permanent contracts. Finally, Popov (2015) investigates the determinants of firm-on-the-job training and finds that credit constrained firms have a 38% lower probability of investing in on-the-job training. Our paper contributes to these different strands of the literature focusing on a low-income country, where access to finance is substantially more scarce than in most advanced and middle-income countries, studied in the literature so far.

While this is one of the first papers gauging the relationship between access to finance and firms' hiring decisions in a low-income country, our analysis is subject to several caveats, including measurement

concerns stemming from the survey nature of our data. Most importantly, the cross-sectional nature of our data allows us to only imperfectly control for endogeneity concerns. While we offer an array of robustness test to address concerns of reverse causation, omitted variable bias and heterogeneity, we like to stress that the nature of our data does not allow for causal inference. Given the unique and novel nature of the data, we still think that our findings are of interest.

The remainder of the paper is structured as follows. Section 2 elaborates on the composition of our data and section 3 on the benchmark econometric model. Section 4 presents the main empirical findings. Section 5 presents a series of robustness checks and tackles the endogeneity concerns with respect to financial access and firm performance. Lastly, section 6 concludes.

3.2 Data

Our data come from a small-business survey conducted in 2013 in Uganda. Together with Financial Sector Deepening Trust Uganda (FSD-U), we designed the survey instrument that allowed us to collect this business-level data. The project was funded by the Department for International Development (DFID) and Financial Sector Deepening Trust Uganda (FSD-U). The survey was administered by an independent consulting company and in total data from 1839 small and medium sized businesses were collected. The majority of the survey respondents were either owners of the firm or higher level managers that had adequate access to firm financial and operational information.[36] The survey data provide information on firm financials, operations and most importantly, detailed answers on employment characteristics. For the purpose of this study, we exclude all financial firms from our analysis. After this refinement, we are left with 1702 firms. They come from all five regions of Uganda, 79 districts as well as 16 sectors. The businesses are sampled from sectors such as manufacturing, construction, agricultural, forestry, and utilities. Firms were randomly selected to take part in the survey and compliance was optional. Table A1 describes the composition of our data-set with respect to location, region and sector classifications. In unreported tabulations, we find very little sectoral level differences when it comes to financial constraints and employment categories and we do not observe any pattern for dominant financial constraint status or employment behavior specific to a particular industry.

[36] To be exact, 1,256 of the respondents were owners and over 450 were identified as managers.

3.2.1 Labor and Employment Variables

The main focus of this study is to understand the determinants of firms' employment composition and especially the demand for skilled labor. To serve this purpose, our survey categorizes five different types of employment. Specifically, the categories that we identify are (i) *Trained*, (ii) *Experienced*, (iii) *Permanent*, (iv) *Casual* and (v) *Family/Friends*. *Trained* employees are those who have a formal training appropriate for their particular occupation, while *Experienced* employees are those who have work experience for at least two consecutive years in a particular occupation; we refer to these two categories as skilled employees. *Permanent* employees are those who have worked at the interviewed firm on a daily basis for at least 3 consecutive months, while *Casual* employees are part time workers and *Family* employees are either family, relatives or friends. Skilled employees are expected to be more costly for the firm. In order to draw a clear picture to this end, the survey asks respondents to provide the average monthly salary that they pay to each category of employees. The survey responses show that the average salary for skilled employees is almost double that of other employees; 225,700 Ugandan Shilling for trained and experienced (approximately 88 US Dollars in 2013) compared to 135,400 Ugandan shillings for casual and family employees (53 US Dollars). When comparing within the same firm, we find an even starker difference, with the average wages for trained and experienced workers being 312% higher than for casual and family workers. Therefore, we conjecture that the hiring rates and the demand for skilled workers is adversely affected by firms' limited financial access compared to the overall demand for employees.

The survey also asks the respondents if the labor demand for the categories (i)-(v) above increased, decreased or stayed the same over the last 12 months. This means we have the information whether the firm hired or fired employees the past one year and if so, how many. This allows us to investigate the dynamics governing the composition of employment both along extensive and intensive margins. We would like to highlight that the employee categories (i)-(v) are not fully distinct from one another and there are clear overlaps among certain variables. For example, there might be certain cases where a particular worker is a permanent employee as well as a family member.

We present summary statistics of the key variables in Table 1. We observe that only a small proportion of our firms hired over the previous 12 months. Specifically, over this period of time we observe that 149 firms hired permanent employees, 65 that hired casual employees, 35 that hired family employees, 86 hired trained employees and 87 hired experienced employees.

3.2.2 Firm Performance, Financing Constraints and Control Variables

In our regression analysis, firm performance is proxied by either sales or profits. The survey asks each firm its sales and profits from the previous month, where profits refer to the total income after paying all expenses including wages for employees, taxes, rents, interest expenditures etc., but not including any income paid to the owner. In addition, the survey asks whether sales and profits had increased or decreased when compared to the same month of the last year. We use this information to construct variables $\Delta Sales$ and $\Delta Profits$, which take the value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago.[37]

In order to measure the degree of financing constraints, we utilize information on whether a firm has outstanding loans and whether the firm has applied for a loan over the last year as well as the reasons for not having applied for a loan. This information allows us to identify firms who were rejected or discouraged from applying for loans. It also allows us to distinguish between firms that have not applied for a loan because they were discouraged and firms that have not applied because they have no demand. There are many firms in our sample who never applied for a loan, however, would need a commercial loan for their operations. In line with earlier and established research, we split the sample based on categories of financial access as (i) *Applied and Got a Loan* (ii) *Cannot Get a Loan* and (iii) *Do not Need a Loan*. Group (i) consists of firms who applied for a loan and got accepted to receive one.[38] Group (ii) include firms that applied for a loan, but got rejected as well as firms that did not apply for a loan, but state in the survey that they need a loan for their operations. Group (iii) includes all the firms who have not applied for a loan, because they do not need a loan for their business. Including this third group allows us to disentangle between demand and supply-side constraints. The data in Table 2 show that 62.5% of firms were either rejected or discouraged from applying, while 10.5% received a loan.

We include a set of additional control variables in our econometric analysis. As a standard control variable to proxy size, we include the *Total Employees* of the firm. Total employees include the total of both permanent and casual employees. We also include the log of invested capital. *InvestedCapital*

[37] While the survey also asked respondents to quantify the realized profits and sales, very few of them were able to provide this information. Survey questions in developing countries are typically phrased in such a manner so that it is most conceivable for the respondents. For example, in many cases firms are only able report their monthly revenues. This is mainly because they do not have any systematic book keeping or reliable estimates for annual performance. Therefore, our questions are also phrased in monthly terms.

[38] For consistency, we drop 11 observations from the sample where firms admitted to having a loan, but have never applied for a loan.

measures the investments made by the firm over the past 12 months. These purchases include machinery, equipment (including computers and software), buildings, land, training/human capital for the employees and other investment.[39] We include *BusinessAge* to measure firm experience. As firms with higher R&D expenses require more formal and high skilled employees, we add a dummy variable to our benchmark regression equation, which will be equal to one if the firm introduced any innovative product, service or process over the past two years.[40]. Lastly, we include the education of the owner as a control variable. It might be that owners who have a higher education truly understand the benefits derived from skilled employment. Because of this, they might have a large influence on the employment composition of the firm. Therefore, we include three dummy variables. The owner is regarded as having a *LowEducation* if he has at most primary school education. We classify *MediumEducation* if the owner has a secondary degree education and *HighEducation* if they have a tertiary, university (undergraduate), postgraduate (Masters, Doctors or PhD) degree education. We maintain the education groups *Other* as the base category in our analysis.[41] The Appendix provides more information on the additional control variables included in our analysis.

[39] Other investments firms made included purchases of furniture, agriculture related investments such as livestock, rental expenditures, nets, watering pipes, boats and all other kinds of miscellaneous items. While we have information on the total *CapitalStock* of firms, only a small proportion of the firms in the sample disclosed this information.

[40] Respondents were asked to provide yes or no answers to this question. If they answered yes, we asked them to describe in detail the main innovative product, service or process that the establishment introduced from fiscal year 2010 through 2012. There were a variety of responses. For example, the "introduction of tomato and chilli sauces" and the "introduction of self-produced fertilizer".

[41] 42 respondents were unable to categorize the education of the owner. *Other* includes a variety of answers, such as "do not know", "different education", "vocational training", "diploma" and more.

Table 3-1: Summary Statistics

(a) Employment Variables

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max	(6) p25	(7) p50	(8) p75
DHire Trained	1,580	0.054	0.227	0	1	0	0	0
DHire Experienced	1,569	0.055	0.229	0	1	0	0	0
DHire Permanent	1,603	0.093	0.290	0	1	0	0	0
DHire Casual	1,575	0.041	0.199	0	1	0	0	0
DHire Family	1,572	0.022	0.148	0	1	0	0	0
Hire Trained	1,580	0.258	1.954	0	54	0	0	0
Hire Experienced	1,569	0.231	1.410	0	25	0	0	0
Hire Permanent	1,603	0.437	2.548	0	54	0	0	0
Hire Casual	1,575	0.168	1.123	0	20	0	0	0
Hire Family	1,572	0.052	0.558	0	16	0	0	0
DPlanned Trained	1,601	0.094	0.292	0	1	0	0	0
DPlanned Experienced	1,592	0.109	0.312	0	1	0	0	0
Planned Train	275	1.284	1.866	0	20	0	1	2
Planned Exper	266	1.470	2.242	0	30	0	1	2
Trained Employees	1,567	2.206	5.137	0	65	0	0	2
Experienced Employees	1,622	3.566	5.985	0	100	1	2	4
Permanent Employees	1,687	4.871	7.197	0	90	2	3	5
Casual Employees	1,606	2.295	7.095	0	150	0	0	2
Family Employees	1,544	0.610	1.844	0	35	0	0	1
Total Employees	1,597	7.064	10.71	0	200	2	4	8

(b) Performance, Financial and Control Variables

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max	(6) p25	(7) p50	(8) p75
Δ Profit	1,702	-0.0517	0.794	-1	1	-1	0	1
Δ Sales	1,490	-0.0389	0.880	-1	1	-1	0	1
Applied for Loan	1,702	0.417	0.493	0	1	0	0	1
Cannot Get Loan	1,702	0.633	0.482	0	1	0	1	1
Do Not Need a Loan	1,702	0.261	0.440	0	1	0	0	1
Invested Capital	1,487	9.10e6	5.92e7	0	1.01e9	0	0	1.50e6
Business Age	1,677	10.090	7.673	1	70	5	8	12
New Innovative Product	1,702	0.259	0.438	0	1	0	0	1
Low Education	1,702	0.193	0.395	0	1	0	0	0
Medium Education	1,702	0.287	0.453	0	1	0	0	1
High Education	1,702	0.499	0.500	0	1	0	0	1

(c) Two-Sample *t*-Test for Equal Means

VARIABLES	(1) N	(2) Mean Applied and got a Loan	(3) Mean Cannot Get Loan	(4) *t*-test
Δ Profit	1257	-0.05	-0.16	*
Δ Sales	1121	-0.04	-0.15	
Invested Capital	1118	9.6e6	1.3e7	
Total Employees	1117	7.06	7.69	
Business Age	1236	10.00	10.64	
New Innovative Product	1257	0.25	0.41	***
Low Education	1257	0.21	0.21	
Medium Education	1257	0.29	0.30	
High Education	1257	0.48	0.48	

Notes: The Hiring variables with the letter "D" indicate that the variable is a dummy variable. The performance variables $\Delta Profit$ and $\Delta Sales$ take a value of 1 if performance increased, 0 if there was no change and -1 for a

decrease. *DPlanned* variables with a missing values have been converted to 0.

Panel C in Table 1 presents two-sample t-tests for equal means. The population of firms is split between firms who *Applied and Got a Loan* and firms who *Cannot Get a Loan*. The results show that the firms are qualitatively similar when it comes to performance, invested capital, age and education. However, firms who are not financially constrained were significantly more likely to introduce a new innovative product, service or process. Results remain similar when we compare firms who *Applied and Got a Loan* against all others. In addition, since we observe low amounts of overall hiring in our sample, one might be concerned that our findings hinge on a very small subset of firms that would therefore drive our results. Two sample t-tests among firms who hire skilled employees remain the same. Unconstrained firms who hire skilled employees and constrained firms who hire skilled employees are qualitatively similar when it comes to performance, invested capital, age and education. We do not report these results for brevity.

Table A2 presents the correlations between key variables to be included in the regression analysis. The measures of firm-performance, "changes in profits" and "changes in sales" relative to the year before (Δ *Profit* and Δ *Sales*), are positively correlated with hiring trained, experienced and permanent employees, but not with hiring casual and family employees. As expected, higher firm size and financial access is correlated with all five categories of employee types.

3.3 Model and Methodology

To explore the interactions between performance, financial access and dynamics of employment composition, we will estimate the following empirical specification:

$$ChangeinEmployee_{ik} = \alpha_0 + \beta_0 PerformanceGroup_i + \beta_i PerformanceGroup_i * AppliedandGotLoan_i + \beta_2 PerformanceGroup_i * CannotGetaLoan_i + \beta_3 PerformanceGroup_i + \beta_4 AppliedandGotLoan + \beta_5 CannotGetaLoan + \lambda X_i + \varepsilon_i \quad (1)$$

The dependent variable, *ChangeinEmployee*, is based on the five different categories of employment that we consider; namely, trained, experienced, permanent, casual and family. The firm is indexed by i and the type of employment is denoted via the subscript k. We vary these variables by examining the hiring decisions at employee type k. We use both extensive and intensive margins of hiring decisions. First, we use a dummy variable equalling to 1 if the firm had hired (variable *DHire*) at least one employee from a particular category of k in the past 12 months, thus the *extensive* margin demand for employment. In a second step, we use the actual number of employees hired, which measures the *intensive* hiring margin.

On the right hand side of our regressions, we proxy the performance of the firm based on either of two measures: *Changes in Sales* and *Changes in Profits*. *PerformanceGroup* variables are categorical variables that indicate whether sales or profits increased or decreased, as already discussed above. Specifically, variables $\Delta Sales$ and $\Delta Profits$take the value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits relative to the performance of the firm a year ago. We include a vector of firm-specific control variables - captured by X_i in the regression specification - as additional right-hand-side variables.

The baseline regressions are estimated via standard OLS (Ordinary Least Squares) for the extensive margin and via Tobit model for the intensive margin. We use OLS instead of probit or logit models for the extensive margin regressions, as we would otherwise lose sectoral and locational cells where either all firms hire or (more likely) no firm hires employees of a specific type. We use Tobit estimations for the regressions with actual hiring as the dependent variable, given their left-censored nature, as we need to account for both the probability of being above the limit (in this case zero), as well as the continuous values of hiring being above zero.

The main interest of all our regressions is to evaluate the significance of the interaction terms between financial access and performance. To test formally whether access to finance interacts with measures of performance in determining the changes in employment composition, we will conduct Wald tests as follows.

$$H_0 = \beta_1 PerformanceGroup_i * AppliedandGotLoan = \beta_2 PerformanceGroup_i * CannotGetaLoan_i = 0$$

3.4 Empirical Results

The results in Table 2 show that only growing firms with access to external funding experience a rise in hiring of skilled employees. In our regressions, the main coefficients of interest are the interaction terms, which we present at the top rows of each table. Firms who experience increases in performance (via profit or sales) and at the same time also have a bank loan, hire more trained and experienced employees when compared to their well-performing but constrained counterparts, who don't have access to external finance. The Wald tests of the differences of the coefficients show that this difference is significant. Also, both constrained and unconstrained firms are more likely to hire skilled employees than firms who do not demand external funding. The economic effects of our regressions are also significant and sizable. The first column in table 2 shows that among firms with rising profits, firms with access to loans are 6.5 percentage

points more likely to hire trained and 7 percentage points more likely to hire experienced employees than financially constrained firms. Given that the average likelihood of hiring trained or experienced workers is 5%, this is a large economic effect. We find similar large skilled hiring effects among firms with rising sales, who have access to finance.

Turning to the control variables in our regressions, we find that larger firms, measured by invested capital and total employees, are more likely to hire trained and experienced employees. In addition, as an expected pattern, firms that introduced a new innovative product, are also more likely to hire skilled labor, because new equipment and sale of new products could require a new set of skills. Our results do not find any evidence for the effects of firm age or education of the business owner on the propensity to hire more skilled employees. Furthermore, there is a potential concern that our performance variables $\Delta Sales$ and $\Delta Profits$ impose linearity as they take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits relative to the performance of the firm a year ago. Therefore, we provide a robustness test in Appendix Table A11 where we replace these variables with dummies *PerformanceIncreased* and *PerformanceDecreased* and we find that our main results remain unchanged.

Table 3-2: Extensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.139**		0.163**	
	(0.063)		(0.066)	
Profit Increased* Cannot Get Loan	0.053**		0.077***	
	(0.022)		(0.023)	
Sales Increased* Applied and got a Loan		0.112*		0.136**
		(0.058)		(0.062)
Sales Increased* Cannot Get Loan		0.050**		0.072***
		(0.022)		(0.023)
Δ Profit	0.001		-0.009	
	(0.008)		(0.010)	
Δ Sales		0.003		-0.009
		(0.009)		(0.010)
Applied and got a Loan	-0.005	-0.004	-0.000	-0.002
	(0.018)	(0.021)	(0.023)	(0.027)
Cannot Get Loan	0.018	0.017	0.000	-0.004
	(0.011)	(0.011)	(0.012)	(0.014)
Total Employees	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.056***	0.059***	0.042**	0.044**
	(0.017)	(0.019)	(0.018)	(0.019)
High Education	0.030**	0.042***	0.029*	0.041**
	(0.014)	(0.015)	(0.017)	(0.017)
Medium Education	0.016	0.021	-0.000	0.004
	(0.013)	(0.014)	(0.015)	(0.017)
Observations	1,307	1,181	1,302	1,176
R-squared	0.103	0.110	0.098	0.100
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	4.795	3.853	7.840	6.487
Prob > F-val	0.00842	0.0215	0.000413	0.00158

Notes: This table shows our baseline estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

The results in Table 3 show that financing constraints matter not only for the extensive margin but also for the intensive margin. Specifically, in Table 3 we run the same regression structure as in Table 2, except we use realized hiring numbers and we estimate the model via the Tobit censored regression model. The results in Table 3 are consistent with our previous findings on the extensive margin. For experienced employees, our variables of interest remain significant and the null hypothesis (interactions are equal to one another) is rejected under both specifications. The results for trained employees are less significant, however, the signs and magnitudes of the coefficients are in line with our earlier results. In terms of economic effects, firms with a high performance and access to external funding employ between 0.3-3.5 more skilled employees than firms with high performance and financing constraints.

Table 3-3: Intensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) Hire Trained	(2) Hire Trained	(3) Hire Experienced	(4) Hire Experienced
Profit Increased* Applied and got a Loan	7.100**		8.463**	
	(3.591)		(3.434)	
Profit Increased* Cannot Get Loan	4.967**		7.497***	
	(2.522)		(2.723)	
Sales Increased* Applied and got a Loan		5.947		8.240**
		(3.675)		(3.566)
Sales Increased* Cannot Get Loan		4.892*		7.899***
		(2.592)		(2.816)
Δ Profit	-0.118		-1.259	
	(1.169)		(1.287)	
Δ Sales		0.066		-1.686
		(1.199)		(1.254)
Applied and got a Loan	0.433	0.327	-0.255	-0.794
	(2.655)	(2.712)	(2.291)	(2.402)
Cannot Get Loan	2.311	1.613	-0.572	-1.503
	(1.833)	(1.828)	(1.619)	(1.686)
Total Employees	0.129**	0.116**	0.171***	0.156***
	(0.051)	(0.049)	(0.050)	(0.049)
ln(1+Invested Capital)	0.241***	0.248***	0.212**	0.210**
	(0.085)	(0.088)	(0.086)	(0.089)
Business Age	-0.103	-0.097	-0.101	-0.100
	(0.075)	(0.078)	(0.071)	(0.074)
New Innovative Product	5.086***	5.180***	3.564***	3.744***
	(1.439)	(1.483)	(1.340)	(1.371)
High Education	3.469*	5.466***	1.877	3.042*
	(1.963)	(2.045)	(1.654)	(1.634)
Medium Education	1.874	3.462*	-1.001	-0.168
	(1.938)	(2.046)	(1.684)	(1.705)
Observations	1,307	1,181	1,302	1,176
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	2.522	2.002	4.317	4.222
Prob > F-val	0.0807	0.135	0.0135	0.0149

Notes: This table shows the estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is realized hiring variable measuring the amount of trained or experienced employees the firm hired in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using a Tobit regression where we account for left censoring at zero. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

In unreported regressions, we find that our results are neither driven by sectoral level differences nor by outliers. To this end, we ran the extensive margin regressions, while dropping one sector at a time from our sample. Under each reduced sample specification, our main results remained consistent. In addition, we winsorized our dependent variables at the 99% level for our intensive margin regressions in case our results were driven by outliers. This refinement did not have an affect on the main results either.

The results in Table 4 show that access to external funding is not relevant for hiring decisions of less skilled workers, including casual and family workers. Here, we test whether financing constraints could also be important for hiring decisions of permanent or casual employees or family members. While theory does not make a clear prediction on the hiring of permanent employees, as they could be either skilled or not-skilled, theory predicts a non-positive impact of access to external funding for the hiring decisions of casual workers or family members - as these employment categories are potential substitutes for access to credit as evidenced by Bos, De Haas and Millone (2016). Nearly all our coefficients of interest in Table 4 are insignificant, suggesting that growing firms with financial access do not employ more casual employees or family members. These results remain the same for various econometric specifications. We note that in some cases financially constrained firms are more likely to hire *other* forms of labor. This might imply a substitution effect on employment. These results are intuitive, but we cannot establish this relationship for all our econometric specifications. Furthermore, in our current specification, we see that unconstrained firms with higher profits employ more permanent employees. This too is an intuitive result, however, we are also unable to establish this relationship for all our econometric specifications.

To further test any potential substitution effects, we exploit a question that asks the respondents whether they had made any "training or human capital investments" for their employees in the past year. With this information, we run the same regressions as before, but replace the dependent variable with a dummy variable *DEmployee Training*. We present the results in columns 7 and 8. Our results are similar to earlier findings. Well-performing firms with access to finance are more likely to train their employees or invest in human capital. These results are in line with findings by Popov (2015) and demonstrate that the firms in our sample do not substitute the lack of skilled employment with employee training or similar forms of human capital investment. The results from all these empirical tests indicate that alleviating financial constraints has a clear effect on relatively high-skill labor as opposed to the lower skilled and casual employment.

Table 3-4: Extensive Margin Effects: Hiring Other Employees

VARIABLES	(1) DHire Permanent	(2) DHire Permanent	(3) DHire Casual	(4) DHire Casual	(5) DHire Family	(6) DHire Family	(7) DEmployee Training	(8) DEmployee Training
Profit Increased* Applied and got a Loan	0.134**		0.019		0.008		0.139*	
	(0.066)		(0.046)		(0.042)		(0.072)	
Profit Increased* Cannot Get Loan	0.104***		0.042*		0.016		0.108***	
	(0.030)		(0.022)		(0.014)		(0.026)	
Sales Increased* Applied and got a Loan		0.103		0.004		-0.001		0.150**
		(0.063)		(0.046)		(0.041)		(0.073)
Sales Increased* Cannot Get Loan		0.117***		0.030		0.014		0.085***
		(0.030)		(0.022)		(0.014)		(0.027)
Δ Profit	-0.001		-0.006		0.000		-0.049***	
	(0.013)		(0.010)		(0.006)		(0.013)	
Δ Sales		-0.002		-0.005		-0.003		-0.041***
		(0.013)		(0.011)		(0.006)		(0.014)
Applied and got a Loan	-0.009	-0.003	0.001	0.003	0.023	0.027	0.017	0.018
	(0.027)	(0.031)	(0.022)	(0.026)	(0.018)	(0.022)	(0.038)	(0.042)
Cannot Get Loan	0.007	0.001	-0.004	-0.005	0.005	0.006	-0.056***	-0.052**
	(0.017)	(0.017)	(0.012)	(0.013)	(0.005)	(0.008)	(0.018)	(0.020)
Total Employees	0.004***	0.004***	0.002**	0.002**	0.001	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
ln(1+Invested Capital)	0.003***	0.004***	0.002**	0.002**	0.001**	0.001**	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.000	-0.000	-0.000	-0.000	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
New Innovative Product	0.066***	0.064***	0.018	0.019	0.021*	0.022*	0.077***	0.080***
	(0.021)	(0.022)	(0.016)	(0.017)	(0.011)	(0.012)	(0.022)	(0.024)
High Education	0.033	0.042**	0.020	0.028**	0.018**	0.021**	0.024	0.027
	(0.020)	(0.020)	(0.014)	(0.014)	(0.009)	(0.010)	(0.020)	(0.022)
Medium Education	0.010	0.016	0.021	0.028*	0.013	0.015	-0.030*	-0.035*
	(0.019)	(0.020)	(0.014)	(0.015)	(0.010)	(0.011)	(0.018)	(0.020)
Observations	1,325	1,198	1,308	1,181	1,304	1,177	1,392	1,244
R-squared	0.122	0.136	0.046	0.047	0.043	0.045	0.130	0.136
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wald Test	6.819	8.019	1.794	1.057	0.652	0.509	9.373	6.033
Prob > F-val	0.00113	0.000347	0.167	0.348	0.521	0.601	9.06e-05	0.00247

Notes: This table shows our baseline estimation results for the relationship between hiring other labor (permanent, casual and family), employee training, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired permanent, casual or family employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 8 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

In summary, growing firms with demand for external finance hire skilled and trained employees if their financing needs are satisfied, while there appears to be no such constraints for the hiring of casual workers and family members. This result is in line with Kaplan and Zingales (1997) analysis on "the positive association between cash-flow sensitivity of investment and financial access" - providing indicative evidence that in developing countries human capital investment's cash-flow sensitivity behaves in a way similar to physical capital investment's cash-flow sensitivity.

3.5 Robustness Tests

In this section, we present a series of robustness checks for our main findings. First, we provide indicative evidence for why the relationship between financial access and hiring of skilled employees might not be caused from the direction of performance. In addition, we conduct a robustness test where we alter our financial access sub-groups. Finally, we use alternative econometric specifications to test the sensitivity of our results to the model specification. All tables associated with the robustness analysis are presented in the Online Appendix of the paper – as Supplementary Material.

3.5.1 Addressing Reverse Causality

Reverse causality is a concern for our model, as it is likely that changes in employment quality could have an effect on firms' probability of financing as well as firm performance. It may be the case that changes in employee composition drive access to finance. The higher the quantity of skilled employees, the larger the firm, the greater legitimacy in the loan application process. Because of this reverse causality concern, our model might be improperly identified. To potentially address this possible bias, we analyze the effects of firm performance and financial access on *Planned Hiring*. For this, we utilize a survey question on how many trained, experienced, permanent and casual employees firms plan to hire over the upcoming 12 months. We run the same regressions as before, except that our dependent variable is now *PlannedHiring*. Unlike contemporaneous hiring, which might affect access to external funding (by, e.g., sending positive signals to lender), it is relatively less likely that future hiring plans influence today's access to external finance.[42]

[42] We acknowledge that we cannot fully eliminate this concern. It could be argued that because firms were planning to steadily increase employment over the next few years, this is the reason they applied for the loan in the first place. This might happen in order to cover for the gradual increases in costs.

We run the same models as before with planned hiring of firms as a dependent variable. The *DPlanned Hiring* variable measures the hiring intentions of the firm. The variable takes the value of 1 if the firm intends to hire one or more employees in the future. In the survey approximately 300 firms gave actual values to these questions of which only around a 100 admitted to not planning to hire anyone in the future. All other 1500 observations were labeled as missing values. In order to have enough observations to conduct this analysis, we convert all missing values to zero. We understand that this is a strong assumption; and therefore, we also perform alternative conversions to assure that our findings are not biased by this conversion. Specifically, we only consider missing values that were labeled as "do not know" as zero, thus only converting approximately 60 missing observations. This is a more conservative conversion as the answer "do not know" can reasonably be interpreted as a zero value. With this specification, our results are very much in line with our prior findings.[43]

Table A3 shows a positive and significant relationship between the interaction of profit/sales growth and access to external funding, on the one hand, and future hiring of trained or experienced employees, on the other hand. Columns (1)-(4) report the result that converts all the missing values to zero and columns (5)-(8) with the more conservative conversion. Overall, the results are very similar to our prior findings. Firms with greater financial access and rising performance *plan* to hire more skilled employees. Our Wald tests are significant across all specifications and the magnitude of all coefficients are very similar to those found in earlier regressions. Most notably, our first interaction $Performance Increase * Applied and Got a Loan$ remains strongly significant across nearly all specifications. In economic terms, growing firms with access to external funding are between 8 and 42 percentage points more likely to plan to hire skilled employees than growing firms with financing constraints.

Additionally, we also test whether our results hold with intensive margin values for planned hiring. Firms reported that they were planning to hire on average between 0-30 employees depending on the employee type. In this setting, we run the same Tobit regressions as before, with actual values for planned hiring and converting only the "do not know" replies to zero. We present the Tobit regression results in Table A4. Under this specification, the majority of our results remain consistent with the previous findings. Well-performing firms with access to finance plan to employ between 1.3-4.5 more skilled employees than firms with a high performance but with no access to credit.

[43] For clarity, responses are not driven by the position of the responder in the firm. As we mentioned before, 1,248 out of the total 1,839 respondents were owners of the firm and therefore "do not know" responses are not driven by the answers given by managers or other employee statuses.

Finally, to control for the fact that firms might exhibit some persistence over time with respect to hiring behaviour, we include actual current hiring in our planned hiring regressions. We conduct separate regressions where we include a dummy variable as well as the real hiring variable on whether the firm had previously hired a particular skilled employee type. We present the results in Table A5. In columns 1-4, we present the regression results for the extensive margin and columns 5-8 for the intensive margin while controlling for realized hiring behavior. Our results remain consistent with our earlier findings, firms with financial access and high firm performance hire more skilled employees than constrained firms.

3.5.2 Alternative Access to Finance Classification

In this section, we take a closer look into the division of our financial access sub-samples. Specifically, we distinguish among firms with financing constraints between those that were rejected and those that were discouraged, though we expect the differences between these two groups to be small. The purpose of this exercise is to separate these groups and to test whether there are further differences in employment composition. In addition, we use this to test the consistency and robustness of our results while altering our financial access sub-samples.

The results in Table A6 show a significant association between external funding and hiring decisions of skilled employees and all of our previous findings remain consistent across all specifications. Firms with greater financial access (i.e. *Applied for a Loan and Got a Loan*) exhibit a rise in skilled employment relatively more than their constrained counterparts as performance increases. The results from the Wald tests are in line with these results. We also conduct Wald tests separately between each pair of interactions for all possible combinations. For example, in the lower rows of table A6, "Test12 Chi" tests for the equality between the first and the second interaction and "Test 23 Chi" between the second and the third interaction respectively. By doing so, we formally test for the significant differences among all financial access sub-samples. The results from these alternative specifications are also in line with the prior findings. Based on the Wald tests, nearly all our interactions are significantly distinct from one another. Coefficients are broadly significant and are in line with prior results claiming that greater financial access and positive performance increase the hiring rates of skilled employment. We do not find a consistent ranking in terms of whether rejected or discouraged are more likely to hire trained and experienced workers as their performance goes up.

To further test the legitimacy of our discouraged group specification, we change our interpretation for the discouraged firms. In our survey, we asked whether firms "would like to take out new debt in the

next 12 months". With this question, we create a new variable *Cannot Get a Loan2*. This includes firms who (i) applied for a loan, but got rejected and (ii) firms who did not apply for a loan but admit needing loan application and other financial services as well as would like to take out new debt in the next 12 months. Overall, this is a more conservative approach for classifying our constrained firms. The results in Table A7 show that our findings remain unchanged. Financial access and rising performance interaction are positively linked with the hiring of skilled employment.

3.5.3 Accounting for Total Number of Skilled Employees

Our analysis is unable to fully account for the average firm level skilled employment requirements and as such, firm size proxies might be inadequate to address this concern. In order to address this, we include the total amount of skilled employees as a control variable in our regressions. In the survey, firms were asked "how many of their their total employees were trained or experienced". By incorporating this information, we further control for firm size and average firm level skilled employee requirements. The results in Table A8 show that our main findings remain unchanged. As one would expect, we see that firms with more skilled employees hire more skilled employees. In addition and in line with our earlier results, greater financial access and positive performance continue to exhibit its strong postive assocation with the hiring rates of skilled employees.

3.5.4 Other Robustness Tests

Estimating regressions where the dependent variable is a dichotomous variable is more often better implemented by using other limited dependent variable estimators such as Probit and Logit. When using OLS regressions with dichotomous dependent variables, predicted probabilities are not necessarily bounded by values 0 and 1. OLS also assumes that there is a linear and additive relationship between the dependent and independent variables and this is not always necessarily the case. Due to these inherent difficulties and more, we perform alternative tests by incorporating a Probit model. We present the results from this estimation strategy in Table A9. Results from these tests are in line with our prior results.

Finally, to further address potential concerns regarding omitted variables, we interact all the firm level controls with the financial access variables. The results from these regressions are reported in Table A10. Our main findings remain unchanged and we continue to observe that firms with financial access and high performance have higher probability of employing skilled labor, with significance levels and coefficient sizes similar to our baseline results. These results thus show that the interaction of firm growth with access to external funding does not proxy for the interaction of access to external finance with other

firm characteristics.

The paper so far has been incorporating the number of new hires as a dependent variable and one might argue that it would be best to incorporate net employment changes to take into account the fact that employee turnover may differ across firms. Data limitations prevent us from running such a regression, however, as our data set has very few firms that hire skilled employees to begin with and from that subsample alone, we find much fewer firms who fire skilled employees as well. Therefore, to address all these concerns and limitations, table A12 shows the results from our main empirical analysis, while controlling for whether the firm had fired a skilled employee in the past year. We find that turnover differences, as expected, matter for hiring skilled employment and that our main results remain unchanged.

Lastly, based on the results of the main empirical analysis, one might argue that financially unconstrained firms (whose profits grow) expand employment significantly more not only than firms that were refused a loan but also than firms not needing a loan. If financially unconstrained firms with good performance expand employment significantly more than firms not needing a loan, this might suggest that firms that get a loan are enjoying an unobserved growth opportunity and therefore for this reason they need (and get) a loan. As such, they are more likely to expand profits and employment, while firms not needing a loan do not need it because they do not have a sizable growth opportunity. Even though our regressions control for managerial capacity (by including education variables) we run an additional analysis to address this concern, whereby we include the interaction terms *PerformanceIncreased*AppliedandGotaLoan* and *PerformanceIncreased*DoNotNeedaLoan* and run the Wald-tests between these two interaction terms. The results from this analysis - that we report in table A13 - are unable to reject the null hypothesis indicating that we are unable to find support for the previous concern.

3.6 Conclusion

We use a novel small-business survey from Uganda and show that the interaction between performance and financial access is positively correlated with high-skill labor demand. We then further investigate this relationship and show that performance and financial access are not related to the hiring rates of other types of employees, which include casual and family labor.

The results from our study underline the potential importance of financial sector development for employment. As firms grow and become profitable, with access to finance employment opportunities could increase for those who are formally trained, educated and more experienced - with complementary “skills” to business needs. This is an important issue for many nations and especially for developing countries. In

this respect, our paper points out another reason why policymakers might need to tackle small firms' financing constraints.

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3.8 Appendix

Table 3-5: A1 Sector and Region Composition

SECTOR	REGION											
	NORTHERN		EASTERN		WESTERN		CENTRAL		KAMPALA		Total	
	No.	Col%	No.	Col%	No.	Col%	No.	Col%	No.	Col%	No.	Col%
ACCOMODATION	25	16.2	22	9.0	32	9.2	43	6.8	17	5.2	139	8.2
AGRICULTURE	3	1.9	46	18.9	50	14.4	178	28.3	22	6.7	299	17.6
CONSTRUCTION	4	2.6	6	2.5	12	3.5	7	1.1	9	2.7	38	2.2
EDUCATION & HEALTH	31	20.1	32	13.1	49	14.1	95	15.1	29	8.8	236	13.9
FOOD PROCESSING	18	11.7	39	16.0	27	7.8	43	6.8	21	6.4	148	8.7
INFORMATION & COMMUNICATION	10	6.5	16	6.6	42	12.1	21	3.3	26	7.9	115	6.8
MINING	1	0.6	5	2.0	5	1.4	17	2.7	7	2.1	35	2.1
OTHER MANUFACTURING	27	17.5	11	4.5	47	13.5	58	9.2	24	7.3	167	9.8
REAL ESTATE	1	0.6	0	0.0	6	1.7	34	5.4	39	11.9	80	4.7
RECREATION & PERSONAL	13	8.4	24	9.8	40	11.5	71	11.3	42	12.8	190	11.2
TRADING	14	9.1	9	3.7	24	6.9	37	5.9	65	19.8	149	8.8
TRANSPORT, UTILITIES & STORAGE	7	4.5	34	13.9	13	3.7	24	3.8	28	8.5	106	6.2
Total	154	100.0	244	100.0	347	100.0	628	100.0	329	100.0	1702	100.0

Table 3-6: A2 Correlation Table

	DHire Permanent	DHire Casual	DHire Family	DHire Trained	DHire Experienced	Δ Profit	Δ Sales	Total Employees	Invested Capital	Business Age	New Innovative Product	Applied for a Loan	Applied and Got a Loan
DHire Casual	0.46***	1											
DHire Family	0.44***	0.47***	1										
DHire Trained	0.73***	0.38***	0.41***	1									
DHire Experienced	0.73***	0.54***	0.54***	0.76***	1								
Δ Profit	0.13***	0.03	0.05*	0.12***	0.11***	1							
Δ Sales	0.15***	0.03	0.05*	0.13***	0.12***	0.80***	1						
Total Employees	0.18***	0.12***	0.07*	0.14***	0.16***	0.01	0.04	1					
Invested Capital	0.16***	0.08**	0.13***	0.18***	0.18***	-0.03	-0.02	0.21***	1				
Business Age	-0.03	-0.00	-0.01	-0.03	-0.03	-0.06*	-0.09***	0.13***	0.04	1			
New Innovative Product	0.20***	0.11***	0.13***	0.22***	0.19***	0.06*	0.11***	0.13***	0.13**	0.00	1		
Applied for a Loan	0.04	0.01	0.01	0.04	0.04	-0.03	-0.03	0.09***	0.05*	0.03	0.06*	1	
Applied and Got a Loan	0.04	0.03	0.07**	0.07**	0.07**	-0.05	-0.04	0.02	0.02	0.02	0.12***	0.41***	1
Cannot Get a Loan	0.02	-0.00	0.02	0.02	0.00	0.00	0.00	0.00	0.01	-0.02	-0.01	0.20***	-0.45***

$^{*}\ p < 0.05\ ^{**}\ p < 0.01\ ^{***}\ p < 0.001$

Table 3-7: A3 Extensive Margin Effects: Planned Skilled Hiring

VARIABLES	(1) DPlanned Trained	(2) DPlanned Trained	(3) DPlanned Experienced	(4) DPlanned Experienced	(5) DPlanned Trained	(6) DPlanned Trained	(7) DPlanned Experienced	(8) DPlanned Experienced
Profit Increased* Applied and got a Loan	0.223***		0.311***		0.185		0.511***	
	(0.080)		(0.082)		(0.163)		(0.171)	
Profit Increased* Cannot Get Loan	0.049		0.079**		0.046		0.208**	
	(0.030)		(0.032)		(0.096)		(0.102)	
Sales Increased* Applied and got a Loan		0.259***		0.348***		0.341**		0.580***
		(0.080)		(0.080)		(0.171)		(0.171)
Sales Increased* Cannot Get Loan		0.016		0.035		0.051		0.096
		(0.033)		(0.035)		(0.099)		(0.106)
Δ Profit	-0.015		-0.023*		-0.047		-0.148**	
	(0.013)		(0.013)		(0.054)		(0.058)	
Δ Sales		-0.009		-0.011		-0.095*		-0.134**
		(0.014)		(0.014)		(0.057)		(0.060)
Applied and got a Loan	0.058*	0.052	0.025	0.007	0.019	-0.057	-0.175	-0.263**
	(0.033)	(0.036)	(0.031)	(0.033)	(0.127)	(0.136)	(0.127)	(0.126)
Cannot Get Loan	0.025	0.040*	0.048**	0.063***	-0.120	-0.116	-0.058	-0.041
	(0.019)	(0.023)	(0.019)	(0.024)	(0.084)	(0.084)	(0.085)	(0.084)
Total Employees	-0.001**	-0.002***	-0.001	-0.001*	-0.005**	-0.003	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)
ln(1+Invested Capital)	0.005***	0.005***	0.002	0.002	0.015***	0.015***	-0.005	-0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)	(0.004)
Business Age	-0.001	-0.001	-0.001	-0.001	-0.002	-0.003	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.004)
New Innovative Product	0.016	0.011	0.073***	0.073***	-0.145***	-0.138**	0.138**	0.139**
	(0.021)	(0.022)	(0.023)	(0.024)	(0.055)	(0.054)	(0.061)	(0.061)
High Education	0.026	0.033	0.075***	0.086***	0.091	0.104	0.322***	0.345***
	(0.021)	(0.023)	(0.023)	(0.024)	(0.069)	(0.072)	(0.076)	(0.078)
Medium Education	-0.020	-0.019	0.015	0.017	-0.050	-0.033	0.130	0.157**
	(0.017)	(0.018)	(0.019)	(0.020)	(0.070)	(0.070)	(0.079)	(0.079)
Observations	1,338	1,193	1,326	1,182	297	287	298	288
R-squared	0.178	0.188	0.173	0.182	0.404	0.411	0.300	0.315
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wald Test	4.629	5.363	9.007	9.417	0.640	2.083	4.809	6.146
Prob > F-val	0.00992	0.00480	0.000130	8.77e-05	0.528	0.127	0.00887	0.00246

Notes: This table shows the estimation results for the relationship between the planned hiring of skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm planned hiring trained or experienced employees in the next 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 8 using an OLS regression. In columns 1-4, all missing values of the dependent variables were converted to a value of zero. In columns 5-8, only missing values that were labeled as "do not know" were converted to a value of zero. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-8: A4 Intensive Margin Effects: Planned Skilled Hiring

VARIABLES	(1) Planned Trained	(2) Planned Trained	(3) Planned Experienced	(4) Planned Experienced	(5) Planned Trained	(6) Planned Trained	(7) Planned Experienced	(8) Planned Experienced
Profit Increased* Applied and got a Loan	2.726***		4.731***		1.052		3.164***	
	(1.039)		(1.443)		(0.821)		(1.154)	
Profit Increased* Cannot Get Loan	1.533*		1.989**		0.811		1.298*	
	(0.811)		(0.908)		(0.742)		(0.747)	
Sales Increased* Applied and got a Loan		3.311***		5.328***		2.029**		3.693***
		(1.180)		(1.662)		(0.966)		(1.361)
Sales Increased* Cannot Get Loan		0.699		0.783		0.418		0.225
		(0.830)		(0.853)		(0.728)		(0.687)
Δ Profit	-0.534		-0.700*		-0.458		-0.773**	
	(0.391)		(0.415)		(0.351)		(0.385)	
Δ Sales		-0.326		-0.299		-0.524		-0.530
		(0.399)		(0.403)		(0.387)		(0.396)
Applied and got a Loan	1.680**	1.350	0.878	0.055	0.455	-0.081	-0.882	-1.724*
	(0.829)	(0.912)	(0.957)	(1.086)	(0.635)	(0.669)	(0.824)	(0.997)
Cannot Get Loan	0.607	0.929	1.427**	1.685**	-0.565	-0.417	0.100	0.275
	(0.555)	(0.613)	(0.625)	(0.688)	(0.545)	(0.529)	(0.511)	(0.490)
Total Employees	-0.034	-0.035	-0.002	-0.008	-0.015	-0.003	0.030	0.035
	(0.022)	(0.023)	(0.015)	(0.015)	(0.021)	(0.021)	(0.033)	(0.035)
ln(1+Invested Capital)	0.145***	0.151***	0.060**	0.066**	0.125***	0.134***	-0.012	0.007
	(0.034)	(0.036)	(0.030)	(0.030)	(0.038)	(0.042)	(0.027)	(0.025)
Business Age	-0.008	-0.003	-0.011	-0.007	0.021	0.021	0.011	0.007
	(0.038)	(0.035)	(0.036)	(0.034)	(0.028)	(0.026)	(0.029)	(0.027)
New Innovative Product	0.255	0.081	1.483**	1.351**	-0.805**	-0.829**	0.936*	0.936*
	(0.421)	(0.421)	(0.603)	(0.594)	(0.327)	(0.326)	(0.534)	(0.538)
High Education	0.810	0.969	1.871***	1.984***	1.034*	1.169*	2.068***	2.211***
	(0.587)	(0.609)	(0.689)	(0.707)	(0.594)	(0.637)	(0.640)	(0.688)
Medium Education	-0.599	-0.488	0.264	0.319	-0.184	-0.045	0.774	0.965
	(0.622)	(0.629)	(0.640)	(0.649)	(0.548)	(0.559)	(0.591)	(0.603)
Observations	1,338	1,193	1,326	1,182	297	287	298	288
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wald Test	3.771	4.281	5.444	5.844	0.986	2.589	3.775	4.582
Prob > F-val	0.0233	0.0140	0.00442	0.00298	0.374	0.0770	0.0241	0.0111

Notes: This table shows the estimation results for the relationship between the planned hiring of skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable measures the real amount of planned hiring in the next 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 8 using a Tobit regression where we account for left censoring at zero. In columns 1-4, all missing values of the dependent variables were converted to a value of zero. In columns 5-8, only missing values that were labeled as "do not know" were converted to a value of zero. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-9: A5 Extensive and Intensive Margin Effects: Planned Skilled Employees and Controlling for Actual Hiring

VARIABLES	(1) DPlanned Trained	(2) DPlanned Trained	(3) DPlanned Experienced	(4) DPlanned Experienced	(5) Planned Trained	(6) Planned Trained	(7) Planned Experienced	(8) Planned Experienced
Profit Increased* Applied and got a Loan	0.169**		0.252***		2.174**		3.217***	
	(0.077)		(0.081)		(0.984)		(0.897)	
Profit Increased* Cannot Get Loan	0.025		0.043		1.162		1.121*	
	(0.031)		(0.032)		(0.802)		(0.612)	
Sales Increased* Applied and got a Loan		0.218***		0.302***		2.699**		3.562***
		(0.078)		(0.079)		(1.116)		(0.968)
Sales Increased* Cannot Get Loan		-0.004		0.011		0.298		0.201
		(0.034)		(0.035)		(0.807)		(0.614)
Δ Profit	-0.010		-0.013		-0.420		-0.490	
	(0.013)		(0.014)		(0.394)		(0.320)	
Δ Sales		-0.006		-0.003		-0.154		-0.090
		(0.014)		(0.014)		(0.396)		(0.310)
DHire Trained	0.239***	0.227***						
	(0.065)	(0.066)						
DHire Experienced			0.200***	0.183***				
			(0.060)	(0.060)				
Hire Trained					0.391**	0.372**		
					(0.191)	(0.188)		
Hire Experienced							0.213*	0.188
							(0.126)	(0.122)
Applied and got a Loan	0.076**	0.064*	0.041	0.020	2.075**	1.670*	1.012	0.343
	(0.034)	(0.037)	(0.032)	(0.033)	(0.843)	(0.914)	(0.720)	(0.822)
Cannot Get Loan	0.031	0.046*	0.056***	0.070***	0.837	1.111*	1.259***	1.414***
	(0.019)	(0.024)	(0.020)	(0.024)	(0.560)	(0.604)	(0.458)	(0.484)
Total Employees	-0.002***	-0.002***	-0.002**	-0.002**	-0.070**	-0.071*	-0.028	-0.029
	(0.001)	(0.001)	(0.001)	(0.001)	(0.034)	(0.036)	(0.020)	(0.020)
Observations	1,263	1,140	1,242	1,119	1,303	1,161	1,281	1,140
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wald Test	2.513	4.281	5.184	7.428	2.587	3.488	6.525	8.014
Prob > F-val	0.0814	0.0141	0.00573	0.000625	0.0756	0.0309	0.00152	0.000350

Notes: This table shows the results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. For columns 1-4, the dependent variable is a dummy variable for whether the firm planned hiring trained or experienced employees in the next 12 months. For columns 5-8, the dependent variable is measuring the real amount of planned hiring in the next 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1-4 using an OLS regression and columns 5-8 using a Tobit regression where we account for left censoring at zero. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-10: A6 Alternative Financial Access Measures, Extensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.139**		0.163**	
	(0.063)		(0.066)	
Profit Increased* Applied and was Rejected a Loan	0.035		0.068**	
	(0.027)		(0.029)	
Profit Increased* Did not Apply but Needs Loan Service	0.074**		0.086***	
	(0.032)		(0.031)	
Sales Increased* Applied and got a Loan		0.113*		0.136**
		(0.058)		(0.062)
Sales Increased* Applied and was Rejected a Loan		0.040		0.076***
		(0.027)		(0.029)
Sales Increased* Did not Apply but Needs Loan Service		0.062*		0.070**
		(0.032)		(0.031)
Δ Profit	0.001		-0.009	
	(0.008)		(0.010)	
Δ Sales		0.003		-0.009
		(0.009)		(0.010)
Applied and got a Loan	-0.005	-0.005	0.000	-0.002
	(0.018)	(0.021)	(0.023)	(0.027)
Applied and was rejected a Loan	0.016	0.010	0.002	-0.008
	(0.013)	(0.013)	(0.015)	(0.017)
Did not apply for Loan but Needs Loan Services	0.020	0.024*	-0.002	0.000
	(0.013)	(0.014)	(0.013)	(0.016)
Total Employees	0.002**	0.002**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.055***	0.058***	0.041**	0.044**
	(0.017)	(0.019)	(0.018)	(0.019)
High Education	0.031**	0.043***	0.029*	0.041**
	(0.015)	(0.015)	(0.017)	(0.017)
Medium Education	0.016	0.021	-0.000	0.004
	(0.013)	(0.014)	(0.015)	(0.016)
Observations	1,307	1,181	1,302	1,176
R-squared	0.105	0.112	0.098	0.100
Industry FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	3.372	2.650	5.242	4.362
Prob > F-val	0.0179	0.0476	0.00135	0.00460
Test12 F-val	0.0509	0.0727	0.00581	0.00621
Test13 F-val	0.00889	0.0321	0.00179	0.0120
Test23 F-val	0.0453	0.0739	0.00341	0.00641

Notes: This table shows the alternative estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. We also conduct Wald tests separately between two interactions for all possible combinations. "Test12 Chi" tests for the equality between the first and second interaction, "Test 23 Chi" between the second and third interaction and "Test 13 Chi" between the first and third interaction. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-11: A7 Alternative Discouraged Firm Specification, Extensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.131**		0.149**	
	(0.063)		(0.066)	
Profit Increased* Cannot Get Loan2	0.044*		0.062**	
	(0.025)		(0.026)	
Sales Increased* Applied and got a Loan		0.103*		0.124**
		(0.058)		(0.062)
Sales Increased* Cannot Get Loan2		0.039		0.065***
		(0.025)		(0.025)
Δ Profit	0.006		-0.000	
	(0.008)		(0.010)	
Δ Sales		0.009		-0.002
		(0.009)		(0.010)
Applied and got a Loan	-0.010	-0.009	-0.001	-0.003
	(0.017)	(0.020)	(0.022)	(0.026)
Cannot Get Loan2	0.017	0.017	0.000	-0.006
	(0.011)	(0.012)	(0.012)	(0.014)
Total Employees	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.055***	0.058***	0.042**	0.044**
	(0.017)	(0.019)	(0.018)	(0.019)
Medium Education	0.021	0.016	0.015	0.008
	(0.039)	(0.044)	(0.038)	(0.043)
High Education	0.035	0.036	0.044	0.045
	(0.039)	(0.044)	(0.038)	(0.043)
Observations	1,307	1,181	1,302	1,176
R-squared	0.102	0.108	0.095	0.099
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	3.399	2.500	4.761	4.681
Prob > F-val	0.0337	0.0825	0.00871	0.00945

Notes: This table shows the alternative discouraged firm estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. Variable *CannotGetaLoan2* includes firms who (i) applied for a loan, but got rejected and (ii) firms who did not apply for a loan but admit needing loan application and other financial services as well as would like to take out new debt in the next 12 months. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-12: A8 Extensive Margin Effects: Accounting for Total Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(7) DHire Experienced	(8) DHire Experienced
Profit Increased* Applied and got a Loan	0.152**		0.172**	
	(0.065)		(0.068)	
Profit Increased* Cannot Get Loan	0.055**		0.078***	
	(0.022)		(0.023)	
Sales Increased* Applied and got a Loan		0.122**		0.149**
		(0.060)		(0.065)
Sales Increased* Cannot Get Loan		0.054**		0.077***
		(0.022)		(0.023)
Trained Employees	0.010***	0.012***		
	(0.003)	(0.004)		
Experienced Employees			0.006**	0.007**
			(0.003)	(0.003)
Δ Profit	0.003		-0.011	
	(0.008)		(0.010)	
Δ Sales		0.004		-0.012
		(0.009)		(0.010)
Applied and got a Loan	-0.000	-0.001	0.009	0.006
	(0.018)	(0.021)	(0.023)	(0.027)
Cannot Get Loan	0.020*	0.018	0.002	-0.003
	(0.011)	(0.012)	(0.013)	(0.015)
Total Employees	-0.000	-0.001	0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.002**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.053***	0.057***	0.040**	0.042**
	(0.017)	(0.018)	(0.018)	(0.019)
High Education	0.029*	0.042***	0.032*	0.043**
	(0.015)	(0.016)	(0.017)	(0.018)
Medium Education	0.020	0.025*	0.003	0.008
	(0.014)	(0.015)	(0.016)	(0.017)
Observations	1,259	1,138	1,272	1,148
R-squared	0.136	0.148	0.111	0.116
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	5.139	4.363	7.749	7.098
Prob > F-val	0.00599	0.0130	0.000452	0.000865

Notes: Notes: This table shows the relationship between hiring skilled labor, firm performance and financial constraints. *TrainedEmployees* and *ExperienceEmployees* measure the total amount of employees in the firm for each respective employment category. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-13: A9 Probit Regression - Extensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	1.041**		1.154***	
	(0.449)		(0.411)	
Profit Increased* Cannot Get Loan	0.527*		0.874***	
	(0.277)		(0.287)	
Sales Increased* Applied and got a Loan		0.922*		1.083**
		(0.473)		(0.430)
Sales Increased* Cannot Get Loan		0.542*		0.893***
		(0.293)		(0.299)
Δ Profit	-0.010		-0.169	
	(0.143)		(0.152)	
Δ Sales		0.018		-0.191
		(0.154)		(0.151)
Applied and got a Loan	-0.004	-0.011	-0.047	-0.093
	(0.323)	(0.345)	(0.279)	(0.301)
Cannot Get Loan	0.302	0.236	-0.093	-0.185
	(0.224)	(0.234)	(0.197)	(0.211)
Total Employees	0.014**	0.012**	0.019***	0.017***
	(0.006)	(0.006)	(0.005)	(0.005)
ln(1+Invested Capital)	0.029***	0.030***	0.028***	0.028***
	(0.010)	(0.010)	(0.010)	(0.010)
Business Age	-0.012	-0.011	-0.011	-0.011
	(0.009)	(0.010)	(0.009)	(0.009)
New Innovative Product	0.563***	0.587***	0.374**	0.391***
	(0.151)	(0.158)	(0.147)	(0.150)
Medium Education	0.174	0.085	0.149	0.048
	(0.507)	(0.532)	(0.492)	(0.495)
High Education	0.385	0.359	0.532	0.484
	(0.489)	(0.509)	(0.480)	(0.481)
Observations	1,307	1,181	1,302	1,176
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	6.254	4.813	11.48	10.21
Prob > Chi^2	0.0438	0.0902	0.00321	0.00607

Notes: This table shows the results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using a Probit regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-14: A10 Extensive Margin Effects: Hiring Skilled Employees and Interactions

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.124**		0.144**	
	(0.061)		(0.064)	
Profit Increased* Cannot Get Loan	0.052**		0.077***	
	(0.022)		(0.022)	
Sales Increased* Applied and got a Loan		0.103*		0.120**
		(0.057)		(0.061)
Sales Increased* Cannot Get Loan		0.048**		0.070***
		(0.021)		(0.022)
Δ Profit	0.001		-0.009	
	(0.008)		(0.010)	
Δ Sales		0.005		-0.007
		(0.009)		(0.010)
Applied and got a Loan	-0.047	-0.054	-0.076	-0.093
	(0.084)	(0.091)	(0.074)	(0.079)
Cannot Get Loan	0.022	0.043	0.005	0.020
	(0.067)	(0.083)	(0.066)	(0.081)
Total Employees * Applied and got a Loan	-0.001	-0.002	0.000	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Total Employees * Cannot Get Loan	-0.000	-0.001	0.001	-0.000
	(0.002)	(0.003)	(0.002)	(0.003)
ln(1+invested capital)* Applied and got a Loan	-0.000	0.000	0.000	0.001
	(0.003)	(0.003)	(0.004)	(0.004)
ln(1+invested capital)* Cannot Get Loan	0.001	0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.003)
Business Age* Applied and got a Loan	-0.003	-0.002	-0.003	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Business Age* Cannot Get Loan	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product* Applied and got a Loan	0.095*	0.107**	0.100*	0.111*
	(0.050)	(0.053)	(0.057)	(0.062)
New Innovative Product* Cannot Get Loan	0.049	0.053	0.073**	0.076**
	(0.034)	(0.036)	(0.036)	(0.038)
Low Education* Applied and got a Loan	0.047	0.052	0.073	0.087
	(0.086)	(0.094)	(0.081)	(0.088)
Low Education* Cannot Get Loan	-0.040	-0.072	-0.040	-0.070
	(0.067)	(0.082)	(0.068)	(0.082)
Medium Education* Applied and got a Loan	0.026	0.016	0.056	0.057
	(0.082)	(0.091)	(0.074)	(0.083)
Medium Education * Cannot Get Loan	-0.012	-0.038	-0.016	-0.039
	(0.069)	(0.085)	(0.066)	(0.080)
High Education * Applied and got a Loan	0.079	0.090	0.086	0.102
	(0.086)	(0.092)	(0.077)	(0.081)
High Education * Cannot Get Loan	-0.019	-0.037	-0.028	-0.044
	(0.068)	(0.083)	(0.067)	(0.081)
Observations	1,307	1,181	1,302	1,176
R-squared	0.110	0.120	0.105	0.109
Firm Controls	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	4.317	3.602	7.406	6.108
Prob > F-val	0.0135	0.0276	0.000634	0.00230

Notes: This table shows the results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-15: A11 Extensive Margin Effects with Alternative Performance Variables: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.140**		0.143**	
	(0.063)		(0.068)	
Profit Increased* Cannot Get Loan	0.055**		0.057*	
	(0.026)		(0.030)	
Sales Increased* Applied and got a Loan		0.102*		0.120*
		(0.059)		(0.064)
Sales Increased* Cannot Get Loan		0.039		0.055*
		(0.027)		(0.029)
Profit Increased	-0.001		0.016	
	(0.018)		(0.022)	
Profit Decreased	-0.001		0.018	
	(0.011)		(0.011)	
Sales Increased		0.018		0.015
		(0.020)		(0.023)
Sales Decreased		0.004		0.021
		(0.012)		(0.013)
Applied and got a Loan	-0.005	-0.000	0.005	0.004
	(0.018)	(0.020)	(0.023)	(0.027)
Cannot Get Loan	0.018	0.021*	0.005	0.002
	(0.011)	(0.011)	(0.013)	(0.015)
Total Employees	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.003***	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.056***	0.059***	0.041**	0.044**
	(0.017)	(0.018)	(0.018)	(0.019)
High Education	0.031**	0.041***	0.029*	0.041**
	(0.014)	(0.015)	(0.017)	(0.017)
Medium Education	0.016	0.021	-0.000	0.005
	(0.013)	(0.014)	(0.015)	(0.017)
Observations	1,307	1,181	1,302	1,176
R-squared	0.103	0.110	0.099	0.101
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	3.991	2.093	3.246	2.805
Prob > F-val	0.0187	0.124	0.0392	0.0609

Notes: This table shows our baseline estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Performance variables are dummies for whether performance or sales had increased or decreased compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-16: A12 Extensive Margin Effects: Hiring Skilled Employees & Controlling for Firing Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.078		0.100*	
	(0.051)		(0.055)	
Profit Increased* Cannot Get Loan	0.036*		0.055***	
	(0.019)		(0.020)	
Sales Increased* Applied and got a Loan		0.087*		0.117**
		(0.050)		(0.053)
Sales Increased* Cannot Get Loan		0.042**		0.054***
		(0.018)		(0.017)
Δ Profit	0.005		-0.001	
	(0.006)		(0.007)	
Δ Sales		0.003		-0.003
		(0.006)		(0.007)
Applied and got a Loan	0.012	0.002	0.008	-0.015
	(0.016)	(0.017)	(0.018)	(0.018)
Cannot Get Loan	0.030***	0.024***	0.011	-0.000
	(0.008)	(0.008)	(0.008)	(0.010)
Total Employees	0.002**	0.001*	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.001	0.001	0.001*	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Dfire Trained	0.467***		0.661***	
	(0.106)		(0.113)	
Dfire Experienced		0.381***		0.658***
		(0.090)		(0.088)
	(0.000)	(0.000)	(0.000)	(0.001)
New Innovative Product	0.048***	0.041**	0.026*	0.015
	(0.015)	(0.016)	(0.015)	(0.015)
High Education	0.012	0.025*	0.017	0.038***
	(0.013)	(0.013)	(0.013)	(0.014)
Medium Education	0.012	0.023*	0.001	0.017
	(0.012)	(0.013)	(0.012)	(0.013)
Observations	1,288	1,161	1,280	1,154
R-squared	0.183	0.181	0.224	0.309
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	2.761	4.178	4.995	6.548
Prob > F-val	0.0636	0.0156	0.00691	0.00149

Notes: This table shows our baseline estimation results for the relationship between hiring skilled labor, firm performance and financial constraints, while controlling for whether the firm had fired a skilled employee in the past year. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-17: A13 Do Not Need a Loan & Extensive Margin Effects: Hiring Skilled Employees

VARIABLES	(1) DHire Trained	(2) DHire Trained	(3) DHire Experienced	(4) DHire Experienced
Profit Increased* Applied and got a Loan	0.103		0.117*	
	(0.063)		(0.066)	
Profit Increased * Do Not Need Loan	-0.035		-0.022	
	(0.022)		(0.026)	
Sales Increased* Applied and got a Loan		0.078		0.086
		(0.058)		(0.062)
Sales Increased * Do Not Need Loan		-0.023		-0.032
		(0.025)		(0.027)
Δ Profit	0.023**		0.020**	
	(0.009)		(0.010)	
Δ Sales		0.024**		0.020**
		(0.010)		(0.010)
Applied and got a Loan	-0.004	0.000	0.008	0.005
	(0.018)	(0.019)	(0.023)	(0.027)
Cannot Get Loan	0.024**	0.027**	0.017	0.010
	(0.011)	(0.011)	(0.013)	(0.015)
Total Employees	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
ln(1+Invested Capital)	0.002**	0.002**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Business Age	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
New Innovative Product	0.056***	0.059***	0.041**	0.044**
	(0.017)	(0.019)	(0.018)	(0.019)
High Education	0.031**	0.042***	0.031*	0.042**
	(0.014)	(0.015)	(0.017)	(0.017)
Medium Education	0.015	0.020	-0.000	0.003
	(0.013)	(0.014)	(0.015)	(0.017)
Observations	1,307	1,181	1,302	1,176
R-squared	0.100	0.107	0.090	0.095
Sector FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Wald Test	3.110	1.633	2.147	1.957
Prob > F-val	0.0449	0.196	0.117	0.142

Notes: This table shows our baseline estimation results for the relationship between hiring skilled labor, firm performance and financial constraints. The purpose of this estimation is to test whether financially unconstrained firms, whose performance increased, expand employment significantly more than firms not needing a loan. The detailed variable definitions are provided in Section 2 and the Appendix. The dependent variable is a dummy variable for whether the firm hired trained or experienced employees in the past 12 months. Variables $\Delta Sales$ and $\Delta Profit$ take a value of 1 if the performance variable increased, 0 if there was no change and -1 if there was a decrease in sales or profits compared to the performance of the firm a year ago. The Dummy *CannotGetaLoan*, includes firms who applied for a loan, but do not have a loan and also firms who did not apply for a loan, but state in the survey that they need a loan for their operations. We estimate columns 1 to 4 using an OLS regression. To control for unobserved regional and sector level fixed effects, we include sector and region dummies in all our estimations. The result for the Wald Tests are reported in the bottom of the table. The null hypothesis assumes that the interaction terms between performance and financial access are equal to one another. Robust standard errors are reported in parentheses. *** p0.01, ** p0.05, * p0.1

Table 3-18: Table A14: Variable Definitions and Survey Questions

Panel A: Employment Variables	Questions
Hiring Overall	C10: In the past 12 months, has the workforce increased, decreased or stayed the same?
Hiring Permanent	C11: Number of permanent employees hired/laid-off (employees who have worked in the firm on a daily basis for at least 3 consecutive months)
Hiring Casual	C12: Number of casual employees hired/laid-off
Hiring Family	C13: Number of employees from your family, relatives or friends hired/laid-off
Hiring Trained	C14: Number of employees hired/laid-off who have a formal training appropriate for this particular business
Hiring Experienced	C15: Number of employees hired/laid-off who have a work experience for at least of two consecutive years in this particular business occupation
Planned Hiring Overall	C16: In the coming 12 months, do you plan to or anticipate employing more workers?
Planned Hiring Permanent	C18a: How many permanent employees do you anticipate to employ?
Planned Hiring Casual	C18b: How many casual employees do you anticipate to employ?
Planned Hiring Trained	C18c: How many trained employees do you anticipate to employ?
Planned Hiring Experienced	C18d: How many experienced employees do you anticipate to employ?
Employee Training	B12: What Investments have you made in the past year in your business? B12C1: Training/human capital for you or your employees
Panel B: Performance & Financial Access Variables	**Questions**
Performance	B3 & B9: Comparing this last month to the same month a year ago, would you say that your sales/profits have- Increased, Decreased or Stayed the same
Outstanding Loan	E5: Does your business have any outstanding debt/loans?
Applied for a Loan	E4: Has your business ever applied for a loan?
Demand for New Debt	E17: Would you like to take out new debt in the next 12 months?
Demand for Loan Application and Other Financial Services	D9: I am now going to read out a list of services. Please identify which ones you would need - Loan application and other financial services 1) Do not need 2) Need but don't have 3) Would want to develop within the business 4) Would want to outsource 5) Not aware of
Panel C: Control Variables	**Questions**
Invested Capital	B12: What investments have you made in the past year in your business? 1) Machinery and equipment (including computers and software) 2) Buildings/land 3) Trainings/human capital for you or your employees 4) Other (specify)
Business Age	A8: How long has the business been operating (years)?
New Innovative Product	I1: From fiscal year 2010 through 2012, did this establishment introduce any innovative product, service or process? Yes/No
Low Education	D2A: What is the highest level of educational attainment of the business owner? None or Primary
Medium Education	D2A: What is the highest level of educational attainment of the business owner? Secondary
High Education	D2A: What is the highest level of educational attainment of the business owner? Tertiary, University (undergraduate), Postgraduate Masters, Postgraduate Doctors, Postgraduate PHD

Notes: For all the questions above, respondents had nearly always the option to answer either "Don't Know" or "Refuse to Answer"