The Strategic Role Firms’ Political Connections Play in Access to Finance: Coercion of Domestic Banks or Implicit Property Rights Protections?

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Abstract
What role does political influence play in access to finance? Does this role differ across markets? Using a comprehensive cross-country dataset, I characterize how and why domestic political connections affect firms’ propensities to cross-list securities abroad. Connected firms have better access to foreign capital markets; moreover, the effect of connections is magnified for firms located in countries with weak property rights. Hence, domestic political connections, at the firm-level, serve as a strategic substitute for strong national property rights protections. This paper extends the interdisciplinary literature on political influence, institutions, and finance, as its findings require us to update our understanding from existing single-country studies: (i) political connections matter not only in domestic financial markets, but also in foreign financial markets; and, (ii) the primary reason politically connected firms receive better access to finance is not government-ownership or coercion of domestic banks, but rather implicit firm-level property rights protections that reduce privileged firms’ risk premiums vis-à-vis unconnected peers.

Keywords: Cross-listing, Political Connections, Non-Market Strategy, International Finance, Corporate Finance, Corporate Governance, Economic Institutions, Property Rights

JEL Codes: G3, G15, K0, P48, L1, L2, O16, F2, F3, F5, D7

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** An appendix that contains additional figures-and-tables along with an extended literature review is available on the authors’ website as is an electronic copy of the latest version of this paper.

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1 Introduction

Consider two otherwise identical firms—one with political influence and one without. Will these two firms have the same access to finance? Most observers would expect a politically connected firm to have better financing opportunities than an unconnected one. The answers to why and how financing opportunities would differ are less obvious—and are unresolved empirical questions I tackle in this paper.

There are at least two ways to model why maintaining a political connection improves a firm’s access to finance: 1) politicians could extort financiers into offering better terms to favored firms; or, 2) politicians could work within the institutional environment to reduce favored firms’ risk premium. Which strategic political maneuver—manipulating capital markets or providing firm-specific property rights protections—benefits firms more and which is more prevalent around the world?

As for how political connections improve access to finance, firms can finance themselves using a variety of sources: debt or equity; domestic or foreign. Does corporate political influence enable preferential access to just one source of finance (e.g. domestic debt) or does the influence mechanism apply more broadly to enable preferential access to multiple sources of finance?

Existing research into the role of political influence on access to finance tends to focus on domestic debt finance in single country studies (e.g. Khwaja and Mian 2005; Li et al. 2008; Classens et al. 2008). We know little about the role political influence plays in access to other types of finance. We also know little about what country-level factors shape the outcomes documented in these case studies. Nevertheless, a consensus is building to suggest that politically influential firms will have preferential access only to domestic debt because allied
politicians can pressure bank employees into offering larger loans, at below market rates and for longer terms, to favored firms.

If this predominant view about political influence playing a coercive role in finance were to hold, then we would expect politically connected firms to eschew both foreign finance and equity finance at least relative to their unconnected counterparts. Connected firms should favor the cheaper bank debt that affiliated politicians purportedly provide. Following this logic, we should expect that connected firms especially forgo foreign equity financing; however, this is not the case. I show that firms with domestic political connections are more likely than their unconnected counterparts to globalize their financing through cross-listing equity securities on markets outside their home countries.

The purpose of this paper is to improve our understanding of how and why political influence improves firms’ access to finance. It achieves this purpose by empirically investigating the roles that a host of political economy factors play in firms’ choice to globalize their financing through cross-listing. I consider not only domestic firm-level political connections’ role, but also the role of domestic country-level institutions that shape firms’ operating environments and that constrain corporate political activity. Doing so allows me to characterize where the strategic value of political influence in access to finance originates.

Firms’ cross-listing decisions provide a particularly attractive setting in which to identify and characterize the means through which domestic political connections create value in accessing finance. Studying cross-listing, instead of domestic loans, allows me to overcome two weaknesses in existing research designs on political influence and access to finance: (1) I can reduce typical endogeneity concerns; and, (2) I can exploit cross-country data.

Examining firms’ ability to access foreign capital through cross-listing helps free my
study from the identification concerns that typically plague studies of the role political influence plays in improving access to, and the terms on, domestic bank loans. Politicians can readily manipulate local bankers into offering financing arrangements they would not otherwise (La Porta et al. 2002). ¹ Given this reality, statisticians need to worry about firms choosing to become politically connected so that politicians will manipulate domestic bankers’ loan decisions on their behalf. This concern is not salient, however, when we examine firms’ choice to cross-list securities. Equity markets are difficult for politicians to manipulate because of participants dispersed nature (Rajan and Zingales 2003). Foreign securities-investors are also difficult for local politicians to manipulate, particularly when they do not have a local presence where they invest. Domestic bankers have incentives to do as politicians say, but foreign investors and equity market participants do not have the same incentives to bend to local politicians’ whims.

In addition to providing a unique identification strategy, another advantage of looking at cross-listing as my dependent variable is that the data is available across countries. Past studies on political influence and finance leave doubts about their external validity because insufficient cross-country data on firms’ political influence and on bank loan portfolios limited them to single country cases. ² Besides enhancing my study’s external validity, cross-country data allow me to characterize which country-level attributes shape the value of firm-level political connections in obtaining access to finance by exploring interaction terms in my regressions. Single-country studies cannot explore these interactions because there is no variability in country-level characteristics, such as state ownership of banks or property rights institutions. The technique is an innovation in the broader literature on corporate political influence enabled

¹ Governments own banks in many countries and regulate banks in more, incentivizing local bankers to comply with politicians.
² Data on domestic bank loan portfolios are not widely available across countries because they are proprietary. Furthermore, finding a standardized metric of political influence across countries is difficult; Faccio’s (2006) data creates an exception.
by my decision to conduct a cross-country study requiring a unique dataset.

In my analysis, I identify two new factors that improve firms’ access to foreign finance: (1) strong domestic political connections, and (2) strong domestic property rights. Importantly, the effect of political connections is stronger in weak property rights environments, which suggests that political connections play a strategic role as a substitute for strong property rights rather than complementing them.

I can refute the primary alternate mechanism (suggested in the broader literature on the role political influence plays in access to finance) by showing that there is no significant effect on access to foreign finance when firms’ political connections are interacted with governments’ ownership stakes in domestic banks. Hence, we can conclude that the primary role political connections play in access to finance is as a firm-level substitute for weak national property rights rather than as an instrument for capital market manipulation. Presumably, we find this because it is more valuable for politically connected firms to have more secure property rights over the long run than it is for the connected firms to receive preferential loans only during the short run that associated politicians remain in office and are able to coerce bankers.

Consistent with this story, I find that firms associated with politicians who have greater ability and greater personal incentives to help them secure firm-specific property rights protections are the ones that enjoy the best access to foreign finance: Ministers are more valuable than Members of Parliament—as are politician-owners rather than politician-directors. To my surprise, I also find that characteristics of the domestic political institutions (e.g. whether a country is an autocracy or democracy; or, the tenure of the political system) appear to play no systematic role in shaping the value of firms’ domestic political influence across countries.

Italy’s Mediaset S.p.A. provides the prototypical example of a politically connected firm
that receives favorable financing terms (and cross-lists securities) because its politician-owner has used his power to craft measures that protect his enterprise from a weak property rights environment. Italy’s Prime Minister Silvio Berlusconi founded the firm and remains its largest investor (with an ownership stake near 30%). Rather than wielding his power to coerce the largest domestic banks (in which the government maintains a 35% stake) to make him loans on favorable terms while in office, Berlusconi concentrated on passing legislation that protected his television and media enterprises’ content from piracy. The legislation created a more stable long-run operating environment for Mediaset as it provided Berlusconi’s firm enhanced legal recourse against violators of his firms’ intellectual property.\(^3\) Financiers, including ones in foreign capital markets, chose to reward this corporate political strategy. The strategic benefits of focusing on securing property rights specific to Berlusconi’s media enterprises clearly dominated what the benefits would have been if the Prime Minister had coerced government-owned banks to offer him preferential financing only during the periods he remained in office.

The paper is structured as follows. Section 2 integrates a review of closely related literature into the analytic framework I will use.\(^4\) Section 3 describes the data. Section 4 applies the data to empirical tests of my three main hypotheses. Section 5 contains a discussion of how the empirical results require us to update our thinking about the strategic role political connections play in firms receiving preferential access to finance. Section 6 concludes.

2 Analytic Framework and Related Literature

An integrated analysis of the political economy determinants of firms’ access to finance,

\(^3\) For example, the Urbani Decree was among several pieces of legislation aimed at protecting copyrighted media content.

\(^4\) A more comprehensive literature review—on cross-listing, economic institutions, and corporate political influence—is incuded in an Appendix. Visual representations of the three primary hypotheses in the framework section also appear in an Appendix.
including cross-listing, should simultaneously consider the roles of both firm-level and country-level factors. The independent roles of domestic firm-level political connections and domestic country-level institutions are the subject of the first two hypotheses I present. With my third hypothesis, I propose a way to reconcile independent roles of firm- and country-level variables.

2.1 The Role of Domestic Political Connections

Firms’ political connections have a value in their domestic financial markets (Fisman 2001). Political influence enables firms to receive preferential access to domestic debt finance, potentially because politicians meddle with local bankers (Khwaja and Mian 2005; Li et al. 2008; Classens et al. 2008). Domestic debt, however, is only one source of finance used by large firms; foreign and equity finance are alternative sources. Firms frequently finance abroad through cross-listing equity securities (Karolyi 2006). We do not know, however, if the mechanism through which political influence works also wins firms superior access to foreign equity finance. This leads to the first hypothesis, I will test:

**Hypothesis 1:** Cross-listing is more likely if a firm has domestic political connections.

Existing empirical tests of the role domestic political connections play in cross-listing decisions generate mixed evidence: Hung et al. (2008) find a positive effect using data from China, while Leuz and Oberholzer-Gee (2006) and Siegel (2009) find a negative effect using data from Indonesia and Mexico, respectively. No one has yet run the cross-country test of this hypothesis necessary to resolve the debate by showing whether political influence has a positive

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5 In 2004, more than 20% of the firms listed on the New York Stock Exchange were foreign as was over 50% of the value traded on the London Stock Exchange (Karolyi 2006). Firms cross-list when the benefits, including a reduction in the firm’s cost of capital (Hail and Leuz 2009), exceed the costs. For a more in-depth literature review on cross-listing see the Appendix.

6 Hung et al. (2008) argue that the reason they find firms with domestic political connections in China are more likely to cross-list is that the action dramatically improves corporate governance; this result is consistent with the benefits of the regulatory bonding theory of cross-listing promulgated by Coffee (1999) and Stulz (1999).

7 Leuz and Oberholzer-Gee (2006) claim political connections are substitutes for foreign financing in Indonesia because they provide firms a better domestic financing alternative: government coercion of bankers. Siegel (2004) argues that firms with political connections have less need to cross-list because they serve as a reputational bond important only for domestic credit.
or negative effect on cross-listing across countries. I will run that test.

Consistent with mixed empirical evidence from the single-country studies, there are theoretical reasons to expect either result. On one hand, if Khwaja and Mian’s (2005) story that “politically powerful firms obtain rents from government banks by exercising their political influence on bank employees” to obtain preferential access to domestic debt holds for large firms across countries, then we would expect to find a negative effect of firms’ domestic political connections on their propensities to cross-list since these firms should favor cheaper domestic debt to foreign equity.8 On the other hand, if the mechanism through which political influence works applies broadly to obtaining superior access to all sources of capital, then we would expect to find a positive effect of firms’ domestic political connections on their propensities to cross-list. In short: if we find a negative sign on domestic corporate political connections effect on cross-listing, that is consistent with capital market manipulation being the mechanism through which political connections lead to favorable financing; however, if we find a positive sign on domestic corporate political connections, that is consistent with an alternative mechanism.

Several researchers, in the growing literature on the outcomes firms obtain through political influence, document ways firms benefit other than through coercion of local bankers, including: helping firms secure favorable regulation (Agrawal and Knoeber 2001), winning firm-specific tax breaks (Richter et al. 2009), and ensuring that entities win government appropriations (Roberts 1990; de Figueiredo and Silverman 2006). To the extent that these benefits stabilize firms’ operations over the long run, which would reduce the firms’ risk

8 If firms’ weighted average cost of capital is determined in a general equilibrium setting, foreign equity financiers should charge a higher rate to fund politically connected firms that they suspect of receiving some fraction of their domestic debt at below market rates by virtue of politicians’ capital market manipulation. Charging firms that finance themselves using political coercion higher rates would help restore the equilibrium terms and compensate financiers for the additional risk.
premiums, then financiers, both domestic and foreign, should rationally choose to offer politically connected firms better access to finance irrespective of politicians’ meddling.

2.2 The Role of Domestic Economic Institutions

Domestic economic institutions define country-level “rules of the game” (North 1981) and are typically “unbundled” into property rights and contracting varieties for analytic tractability (Acemoglu and Johnson 2005). They shape economic outcomes at the country-level (Acemoglu et al. 2005; La Porta et al. 1998) and at the firm-level (Laeven and Woodruff 2007). Accounting disclosures, as a type of contracting institution, play a prominent role in past inquiry into the determinants of cross-listing (e.g. Coffee 1999, 2002; Stulz 1999; Doidge et al. 2004); however, countries’ economic institutions are multidimensional. Other aspects of countries’ institutions, most notably their property rights, have received no empirical attention in determining firms’ propensities to cross-list. This leads to the second hypothesis I will test:

**Hypothesis 2:** Cross-listing is more likely if firms’ domestic economic institutions are stronger, for both property rights and contracting.

The key difference between the regulatory bonding hypothesis of cross-listing and my Hypothesis 2 is that mine recognizes that several different components of countries’ institutional environment affect whether or not firms cross-list, not just the quality of contracting institutions. Tests of the regulatory bonding hypothesis support part of my Hypothesis 2 by finding that cross-listing is more likely the higher the quality of domestic accounting disclosures (e.g. Doidge

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9 Coffee (1999, 2002) and Stulz (1999) were early proponents of a regulatory bonding hypothesis explaining why some firms cross-list; Doidge et al. (2004) were the first to formalize the regulatory bonding hypothesis, building a mathematical model. The hypothesis states that firms cross-list because they benefit from functionally, and credibly, converging towards having the higher quality contracting institutions (or disclosure requirements) of their cross-listing destination, while maintaining operations in their traditional home; the larger the gap between contracting institutions in a firms’ home country and in its cross-listing destination, the greater the costs and the greater the potential benefits (Doidge et al. 2004).
et al. 2004); however, no one has tested the other part of Hypothesis 2, whether cross-listing is more likely the stronger domestic property rights protections are. I will run that test.

What the cross-listing literature conjectures about the role of property rights, but does not test, supports Hypothesis 2. Regulatory bonding proponents such as Stulz (2005) argue that weak property rights reduce firms’ cross-listing activity because “financial globalization reduces the state’s [and other’s] ability to expropriate.” Regulatory bonding skeptics strike a similar chord: Siegel (2005) suggests that stronger domestic property rights may improve firms’ access to foreign finance since cross-listings alone “are far from a perfect substitute for … preventing fraud, theft, embezzlement, and asset taking.”

Furthermore, foreign financiers operating in major capital markets may not have a strong appetite for investing in firms from weak property rights countries (Stulz 2005). The relative unpredictability of firms’ cash flows in those environments may scare away money-center investors. Domestic financiers may have a comparative advantage in understanding the exact risks weak property rights pose for particular firms—making domestic financiers more willing to extend capital to the average firm in those environments on more favorable terms than foreign financiers, since they can better evaluate the risk premium given local information advantages (Coval and Moskowitz 1999).

2.3 Domestic Political Connections in the Context of Economic Institutions

All firms are embedded in the “rules” that domestic economic institutions dictate. Some firms are (politically) connected to the institutional stewards (or governments) who both set and enforce the rules, while other firms are not. Hence, domestic political connections could
functionally enable firms to operate as though they were in a stronger institutional environment (Xin and Pearce 1996). Even North (1990) acknowledges the importance of political interactions in reducing agents uncertainty when economic institutions cannot, noting that heterogeneity exists in how firms experience institutions.

Depending upon the institutional context, politicians may have greater or less control over the variability between formal protections of property rights and the informal protections which they can offer to favored firms. The institutional stewards (or governments) in the weakest environments are likely to have the greatest ability to manipulate the implicit rules because their explicit rules are further from the ideal (found in strong institutional environments). Hence, in weak institutional environments political actors can create relatively larger opportunities for some firms using their powers to selectively enforce property rights or to otherwise improve outcomes for privileged firms.

Given that political influence can help mitigate institutional weaknesses, it is important to consider a key difference between contracting institutions and property rights institutions: all firms can improve their contracting environment by voluntarily adopting stricter accounting standards (Bradshaw et al. 2004), but only the few firms privileged enough to be politically connected can improve their domestic property rights environment. This leads to the third, and final, hypothesis I will test:

**Hypothesis 3:** Cross-listing is more likely for firms with domestic political connections when domestic property rights institutions are weak.

The possibility that firms with political connections experience national institutions differently than firms without political connections has yet to be explored in any cross-country empirical test. Past inquiry into the political economy determinants of cross-listing has
considered the independent roles of domestic firm-level political connections or of some aspects of country-level institutions, but it has not considered how they interact—that is, whether they work as substitutes or complements. I do.

If political connections create tacit domestic property rights protections for privileged firms—particularly if tacit protections are important to all financiers including foreign ones—I should find support for Hypothesis 3. This is reasonable if we expect domestic governments to protect firms with which they have strong ties from the ills of a weak property rights environment better than they protect firms with which they have no ties. Implicit domestic property rights protections should reduce uncertainty in politically connected firms’ domestic cash flows; these, in turn, should lower their risk premium, making them more attractive to all types of investors. Foreign investors, in particular, may have a difficult time assessing the particular risks weak domestic property rights pose to a specific firm unless the firm maintains political connections as a form of tacit protection.

If, in addition to protecting politically connected firms from weak property rights environments, governments also coerce domestic banks into giving connected firms preferential financing, this would pose a challenge to Hypothesis 3; it would also be inconsistent with politically connected firms being more likely to cross-list as in Hypothesis 1. The reason past research finds that domestic banks extend favorable debt financing terms to politically-connected firms could have little to do with government coercion of bankers. Firms’ preferential access to all sources of finance may instead be a rational market response to the explicit signal political connections send that firms maintaining such ties receive implicit property rights protections.
3 Building a Multi-level, Cross-Sectional Dataset

To test the three hypotheses above—and to achieve this paper’s purpose by investigating the role domestic firm-level political connections and domestic country-level institutions play in firms’ cross-listing choices—I build a multi-level, cross-sectional dataset that includes firms in a variety of industries, across 46 countries. A cross-sectional setting is well suited for testing the hypotheses presented in the prior section; moreover, there is little meaningful time variation in the key independent, political economy, variables that effective panel data tests would require. The firms included in my sample are congruent with those in Faccio’s (2006) work on political connections as well as those in Doidge et al.’s (2004) work on the value of cross-listing. My core sample includes 12,395 firms.

3.1 Dependent Variable: Observed Firm-level Global Financing Decisions

The dependent variable I will use throughout my analysis ($y_i$) will be an indicator of whether or not we observe a firm maintaining a cross-listing in a securities market outside of its home country. It is a dummy variable that will be used in binary choice regressions:

$$y_i = \begin{cases} 
1, & \text{if firm } i \text{ lists equity securities on any market outside its home country} \\
0, & \text{if firm } i \text{ lists equity securities domestically, but nowhere else}
\end{cases}$$

My definition of cross-listing is based on Doidge et al. (2004), but expands on it by incorporating firms that cross-list on any capital market around the world rather than focusing

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11 Acemoglu et al. (2001) assert that country-level economic institutions are highly persistent; data supports this. Likewise, political connections demonstrate little year-to-year variation; when they do it is often the result of a shocks such as unexpected election outcomes (Faccio 2006) or health surprises (Fisman 2001; Fisman et al. 2006). Furthermore, no data is available on how domestic firm-level political connections vary over time for a large cross-section of countries.

12 I limit my sample to firms in both Faccio’s (2006) and Doidge et al.’s (2004) core samples. Both use Worldscope, which covers 96% of the world’s market capitalization; I use Datastream/Worldscope database which fully incorporates all data from earlier Worldscope and Datastream databases. Doidge et al. (2004) exclude firms in the U.S. since they are looking specifically at the value of cross-listing in the U.S. Faccio (2006) excludes firms for which data on ownership control was unavailable on the CD version of Worldscope; Faccio privately shared with me the information necessary to reconstruct her original sample.
exclusively on U.S. cross-listings. I only set $y_{it} = 1$ if a firm actively maintains a cross-listing as of January 2003, to match the timing of the political connections data I use. When $y_{it} = 0$, connected firms may finance themselves through regular domestic channels or by using government coercion of bankers since both are observationally equivalent. Unconnected firms presumably cannot unduly manipulate domestic bankers’ decisions.

### 3.2 Key Independent Variables

#### Domestic Firm-level Political Connections

My data on domestic firm-level political connections comes from Faccio (2006). It is the most comprehensive and credible dataset on firm-level political connections across countries. The dummy variable, $d_{connect,i}$, takes on a value of 1 if a firm is politically connected and a value of 0 if a firm is not connected in the year 2002. The measure of political connections in the Faccio dataset is a strict and objective one, constructed from directly observable criteria for all market participants. A firm is defined as politically connected only if “one of its large shareholders or top officers is a member of parliament, a minister, or is closely related to a top politician.” Since the measure is a conservative one, if anything it may understate the extent of and importance of political connections (Faccio et al. 2006). Another advantage of this strict

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13 Pagano et al. (2002) and Sarkissian and Schill (2004) suggest it is important to consider cross-listings in countries other than the U.S., despite much of the academic literature focusing on the American Depository Receipts (ADRs). As in Doidge et al. (2004), I use the Bank of New York’s ADR dataset and JP Morgan’s ADR.com to check if a firm is cross-listed in the U.S. For cross-listings in other countries, I use Datastream/Worldscope which provides information on all markets.

14 While possible, it is unlikely that when $y_{it} = 1$ a firm may also finance itself through government coercion of banks. It is unlikely because 1) rational foreign financiers would offer a firm suspected of using back-channel financing above market rate financing terms to compensate for their below market financing terms domestically, and 2) the greater disclosure required of cross-listed firms would tend to expose these arrangements and cause them to lose their value or efficacy.

15 The key advantage of the Faccio (2006) dataset is that it spans across countries. It is difficult to gather data on political connections across countries, making such studies rare. One of the problems with collecting data on political connections across countries is that the literature’s definition of just what constitutes a connection changes depending upon context. Sometimes subjective assessments of the strength of connectedness is used as a measure (e.g. Fisman 2001). In other instances, firm’s expenditures on campaign contributions or lobbying are used as a measure (e.g. Claessens et al. 2008). In yet other instances, objective directly observable behavior (such as of whether or not politicians have been or currently are employed by a given firm) are used to construct a dummy variable for political connections (e.g. Faccio 2006, or Ferguson and Voth 2008).

16 It is only accurate in 2002 limits me to cross-sectional analyses. While many firms choose to de-list in the U.S in 2002 following the Sarbanes-Oxley legislation going into effect, this is not a major concern for my analysis because most of those firms re-listed on a European exchanges included in my cross-listing measure.
definition of political connections is that the connections are not the type that firms can readily “buy” or “invest in” since they are both transparent and with individuals who are in positions where they can be held accountable for particularly egregious/abusive rent-seeking behavior.17

**Country-level Economic Institutions**

The data I use for my key country-level independent variable, the quality of economic institutions, is consistent with Acemoglu et al. (2001) and La Porta et al. (1998).18 The variable $PropInst_k$ represents property rights institutions; higher values indicate weaker property rights. $PropInst_k$ is based on two popular measures: the International Country Risk Guide (ICRG) Expropriation Risk Index and the Freedom House Property Rights Index.19 The variable $ContractInst_k$ represents contracting institutions; higher values indicate stronger contracting.

The underlying data was coded by the Center for International Financial Analysis & Research and was featured as the key measure of contracting institutions, in La Porta et al.’s (1998) seminal work on law and finance; the data also features in the cross-listing literature.20 These data sources are not perfect and, hence, not free from criticism; however, they are the most widely used because they the best available measures for quantifying institutional quality.

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17 These properties of the political connections data should relieve some concerns about them being endogenous. If politicians are perceived to be abusing office for personal gain they are likely to be held accountable and removed by election in democratic systems or forcibly in authoritarian ones. For example, consider the reason for Berlusconi’s short first term in 1994 or for the 2006 coup ousting Shinwatra in Thailand. Nevertheless, following the analysis I return to robustness/endogeneity concerns.

18 Both were used by Acemoglu and Johnson (2005); the key difference between their measures of property rights and contracting institutions and mine is that I re-scale the underlying data, making it more readily interpretable in regression analysis. I take the $Exprop_{ICRG}$ data from La Porta et al. (1998); which is “the risk of ‘outright confiscation’ or ‘forced nationalization’.” I also take the $PropRights_{FH}$ data from La Porta et al. (2002); “The score is based, broadly, on the degree of legal protection of private property, the extent to which the government protects and enforces laws that protect private property... and the country’s legal protection to private property.” Rather than using each $Exprop_{ICRG}$ and $PropRights_{FH}$ separately, I rescale and combine the data using principal components to project the $PropRights_{FH}$ measure onto $Exprop_{ICRG}$, creating in essence a weighted average that should maximize the underlying signal from each data series. I first rescale the data such that the higher the score is the lower the value of the index and such that I divide by the weakest possible value of the index, making 1 the maximum value in the data. Principal components leads me to a final measure is 86% $Exprop_{ICRG}$ and 14% $PropRights_{FH}$. I get similar regression results in my analysis, however, if I use either measure separately or an equally weighted average.

19 Alternative measures of property rights institutions such as Henisz’ Political Constraint Index yield similar results (Henisz 2000).

20 The original data is coded on a 0 to 90 scale depending upon the inclusion/omission of 90 items that could appear on accounting statements; 90 represents perfection and 0 no disclosures. Sweden scored the highest (83) and Egypt (24) the lowest; the U.S. scored 71 as a benchmark for generally accepted practices. I rescale the data so that 0 corresponds U.S. levels and so that positive (negative) value indicate higher (lower) disclosures. I take each value, divide by the level in the U.S., and subtract 1.
3.3 Control Variables

Other Firm-level Determinants of Cross-listing

The core firm-level control variables I include in my regressions of the determinants of cross-listing—size, leverage, return on assets, and capital intensity. These are typically the only firm-level controls deemed essential in the broader literature exploring the determinants of cross-listing (Leuz and Oberholzer-Gee 2006; Doidge et al. 2009). I use the standard definitions.\(^{21}\)

Industry-level Indicators

Indicators of firm’s industry come from Datastream/Worldscope; I use these indicators to create dummy variables for each industry: \(d_j\).\(^ {22}\) I also use these indicators to exclude firms in the financial industry from my sample as is standard in the literatures on cross-listing (Doidge et al. 2004) and other types of corporate financing activities.

Country-level Indicators and Other Country-level Variables

Indicators of firm’s home country also come from Datastream/Worldscope; I use these to create dummy variables for each firm’s traditional home country: \(d_k\). To round out my analysis, I also gather country-level data on government ownership of banks (La Porta et al. 2002) and the level of development (as measured by the natural logarithm of GDP). Additionally, I construct country-level incidences of political connections and of cross-listing.\(^ {23}\)

3.4 Summary Statistics

Table 1 provides a country-level correlation matrix. There are several things to notice. First, the correlation coefficient between the incidence of political connections and the incidence

\(^{21}\) Firm size, \(SIZE_i\), is defined as the natural logarithm of Total Assets; Return on Assets, \(ROA_i\), is defined as Operating Income divided by Total Assets; Capital Intensity, \(CAPINT_i\), is defined as Fixed Assets divided by Total Assets; and, Financial Leverage, \(LEV_i\), is defined as Long-term Debt divided by Total Assets.

\(^{22}\) The industries included are broadly defined: Basic Materials, Consumer Goods, Consumer Services, Financials, Health Care, Industrials, Oil & Gas, Technology, Telecommunications, and Utilities.

\(^{23}\) I define these incidences by counting the number of firms in a given country in my sample that meet that criteria and dividing that value by the total number of firms in my sample in that country.
of cross-listings (-0.093) is negative and low at a country-level, suggesting little if any country-level support for Hypothesis 1; this, however, does not preclude firm-level support. Second, the correlation coefficient between the incidence of political connections and the quality of economic institutions (0.029) is relatively low. Third and finally, measures of both types of economic institutions, property rights and contracting, are highly correlated (0.551).

Table 2 slices the data in ways that provides preliminary support for all three hypotheses at a country level. The positive values in row 3 support Hypothesis 1: firms are more likely to cross-list if they are politically connected. The decreasing trend in row 2 values, moving from left to right, supports Hypothesis 2: fewer firms cross-list from weaker property rights countries. The increasing trend in row 4 values supports Hypothesis 3: politically connected firms are more likely to cross-list the weaker property rights are.

Additional summary statistics and views of the data appear in the Appendix.

4 Empirical Determinants of the Political Economy of Cross-listing

I use the three hypotheses presented in the analytic framework section as a guide to structure how I add the domestic firm-level political connections variable and the domestic economic institutions variables to binary choice regressions predicting whether or not a firm cross-lists, using alternative explanatory factors (namely firm size) and core corporate financing characteristics as controls. Ideally, I could accurately estimate a single fully-specified regression model that could be used to simultaneously test all three of my hypotheses, taking the form:

\[
Pr[y_i = 1] = \Lambda(\beta \cdot [Variables of Interest] + \gamma_iX_i + \gamma_kY_k + \gamma_jZ_j + \varepsilon_{ijk})
\]
where *Variables of Interest* represents all of the political economy variables in my hypotheses (including the interacted variables); \( X_i \) represents firm-level controls; \( Y_k \) represents country-level controls; and \( Z_j \) represents industry-level controls.\(^{24}\)

Attempting to estimate a fully-specified version of the statistical model could be problematic, however, since misspecification of the true model can be more costly in binary choice models than in simple linear models and can lead to greater bias/ inconsistent coefficient estimates. Consequently, I will take a more cautious approach and test each hypothesis separately, using dummy variables to hold higher-level explanatory variables in the multi-level structure constant. When I am interested in estimating coefficients on firm-level determinants of cross-listing behavior or on interactions between firm-level determinants and country-level factors, I estimate regressions of the form:

\[
Pr[y_i = 1] = \Lambda\left( \beta \cdot [\text{Variable of Interest}] + \gamma_i X_i + \sum_{j} \gamma_j d_j + \sum_{k} \gamma_k d_k + \varepsilon_{ijk} \right)
\]

where *Variable of Interest* represents the political economy variable in the hypothesis being tested; \( X_i \) represents firm-level controls; \( d_j \), industry dummies; and \( d_k \), country dummies.\(^{25}\)

The advantage of this model, over the fully-specified one, is that we do not need values of \( Y_k \) and \( Z_j \), nor do we need to know what all of the true country-level and industry-level factors are that influence firms’ cross-listing choices—eliminating all concerns about country- and industry- omitted variable bias or misspecification from the inclusion of ‘incorrect’ variables. While my estimate of \( \beta \) will remain consistent in estimating this model, the disadvantage of the

\(^{24}\)Multinomial logit is the most widely-used discrete choice method (Train 2003) and is used by Doidge et al. (2009) in their study of the determinants of cross-listing. I find similar results throughout if the density function inverted is either Gaussian (as in probit) or Extreme Value (as in gompit); likewise, I find similar results using a linear probability model.

\(^{25}\)This approach is useful for testing Hypothesis 1 and 3. When I test Hypothesis 2 (about the role of country-level economic institutions in cross-listing) I can no longer include the country dummy variable \( (d_k) \), since its inclusion effectively holds all country-level factors, including economic institutions, constant; a longer discussion of issues this creates is included with the test.
dummy-variable model vis-à-vis the fully-specified model is that my coefficient estimates will lose econometric efficiency. This efficiency loss biases against finding support for my hypotheses as it is less likely that I find statistically significant values for all coefficients, including those on my variables of interest.

The firm-level controls included in $X_i$ are firm size ($SIZE_i$), return on assets ($ROA_i$), capital intensity ($CAPINT_i$), and financial leverage ($LEV_i$). The logic for each is simple. Size is important to include since it is the best non-political economy factor at predicting if a firm cross-lists; the reasons are varied: larger firms can better bear direct costs of cross-listing (Doidge et al. 2004), larger firms tend to be better appreciated by foreign investors, and larger firms are more likely to exhaust domestic capital markets. Return on assets helps control for firm-profitability to ensure that the results do not simply capture firms that have been more profitable/successful being more likely to cross-list.\(^{26}\) Capital intensity is important to include because it proxies for firms’ financing needs. Finally, leverage is included because it controls for what type of capital firms tend to raise (debt or equity); furthermore, if politically-connected firms receive domestic debt on favorable terms, it helps control for this difference between connected and un-connected firms.\(^{27}\) In principle we may want to consider the inclusion of other firm-level controls; however, the combination of corporate financing characteristics that I include is sufficient.\(^{28}\) Nevertheless, in a quick robustness check, I will appeal to Bertrand et al.

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\(^{26}\) Tobin’s Q is not an appropriate alternative for ROA, or an appropriate control, since Doidge et al. (2004) write and validate empirically a theory showing firms will have a higher Tobin’s Q after they cross-list as a result of reduced agency conflicts.

\(^{27}\) While some argue that political connections make firms more highly leveraged (Faccio et al. 2006), others find that firms with stronger political ties are less likely to be highly leveraged (Myers 2007).

\(^{28}\) For example, Doidge et al. (2009), explore as additional firm-level controls a measure of ‘foreign sales’ and an indicator of ‘government owned’ firms. A measure of ‘foreign sales’ is not readily available in Worldscope for the majority of firms in my sample—a concern Doidge et al. (2009) have with including it in their own work and one that they address by saying that its omission does not alter their results; furthermore, if I include both firm size and industry indicators in my regression I am likely to be capturing some of the effects of foreign sales since the larger firms in an industry are more likely the ones that are exporting. Again, I am not able to reconstruct the variable ‘government owned’ for a large sample of firms in my dataset, but believe that if anything omitting it may bias against my findings since in many ways government ownership is an even more
(2000) to show unobservable firm-level factors are unlikely to drive my results.

4.1 The Role of Domestic Political Connections

The first of several relationships I test is if and how domestic political connections influence firms’ propensities to cross-list in any equity market outside of its home country (Hypothesis 1). To do so, I estimate variations of the following regression, including and omitting combinations of industry and country dummies:

\[ Pr[y_i = 1] = \Lambda \left( \beta \cdot d_{\text{connect}} + \gamma_i X_i + \sum \gamma_j d_j + \sum \gamma_k d_k + \varepsilon_{ijk} \right) \]

where \( d_{\text{connect}} \) represents my dummy variable for politically connected firms; \( X_i \) represents the firm-level controls; \( d_j \), industry dummies; and \( d_k \), country dummies. In these regressions, I treat a firm having a political connection as being exogenous to whether it cross-lists, an assumption I believe to be both reasonable and intuitive, but one that I return to in a discussion of robustness concerns nevertheless.

If the coefficient \( \beta \) on the connected firm dummy variable is positive and significant, it suggests that firms with political connections are more likely to cross-list than firms without them, supporting Hypothesis 1; if the coefficient is negative and significant, it would suggest that firms with political connections are less likely to cross-list, casting doubts on Hypothesis 1.

The results of this estimation appear in Table 3 and provide qualified support for the hypothesis that firms with political connections are more likely than those without them to cross-list. In all cases, the results in Table 3 indicate a positive coefficient of interest \( \beta \). In columns 1 and 2, where I pool my results, across countries, without including any country-level control.
variables, the coefficients on the political connections variable are highly statistically significant (at the 1% level). These results strongly support Hypothesis 1. In columns 3 and 4, I include country dummy variables to run the test within rather than across countries; here the statistical significance falls just outside conventional thresholds. Before rejecting Hypothesis 1, it is important to consider several plausible explanations for why the within country tests of political connections’ effects on cross-listing barely meet conventional thresholds: 1) logit may not be the most efficient functional form; 29) 2) I lose econometric efficiency by estimating a dummy-variable model rather than a fully-specified model; 30) 3) I may miss nuanced differences in political connections by using a binary metric rather than one with variable intensities; 31) or 4) domestic political connections have different effects on firms’ propensities to cross-list depending upon the institutional environment, as Hypothesis 3 suggests.

The marginal effect indicates that the average firm with political connections is approximately 15% more likely to cross-list than an unconnected firm across countries—a number that certainly is economically significant. 32) Furthermore, the magnitude of this estimate and its significance may be understated to the extent that some political connections are unobserved or missing in the dataset such as: those gained through affiliations with business

29 If I run Gompit regressions (inverting an Extreme Value distribution) rather than logit regressions (inverting a Logistic distribution) then the within country results are statistically significant at conventional levels. The gompit approach may in fact be more accurate considering the logistic distribution assumes that the distribution of the outcome variable is approximately symmetric or that the probability of a firm cross-listing is roughly fifty percent whereas the extreme value distribution does not symmetric and more likely to fit the data since closer to thirty percent of firms actually choose to cross-list.

30 This explanation seems to carry weight since when I estimate a model that is fully-specified at the country-level (shown in the Appendix Table on ‘Unpacking’ the Country Dummy), my variable of interest meets the standard threshold for significance while its magnitude remains the exactly the same—consistent with a loss of econometric efficiency in the country-dummy model.

31 Using Faccio’s (2006) data, I must treat the intensity of all political connections as being equally strong when reality is that all connections probably are not created equal. Nothing, other than disaggregating her metric, can be done about this since Faccio’s data is the best available on political connections across countries. I do this before moving on to tests of Hypothesis 2.

32 This estimate comes from regressions run across countries (columns 1 and 2) which do not control for or condition on institutions, the level of development, or any other country-level factors. The marginal effect was calculated by dividing the logit coefficient by 4—an approximation for evaluating marginal effects around the mean of other variables in logit regressions derived by Amemiya—as suggested in the graduate econometrics texts written by both Woolridge and Greene.

<Insert Table 3 Here>
groups such as Korean *chaebol* or Japanese *keiretsu* (Khanna and Rivkin 2001); those sustained through family-owned firms (Bertrand and Schoar 2006; Morck and Yeung 2004); or, those sustained in state-owned enterprises including after privatizations.

Column 5 of Table 3 is included as a robustness check; it omits all firm-level, industry-level, or country-level controls. As in Bertrand et al. (2000), we can surmise that unobservable firm-level characteristics are unlikely to be driving the results since the coefficient ($\beta$) on the connections variable hardly changes between column 1 and 5.\(^{33}\) As another robustness check, I used alternate measures of firm size in the regression; these results are shown in the Appendix. When I use either total assets or total assets squared as a measure of firm size—both of which emphasize the larger firms more than the log of total assets—my results become stronger.\(^{34}\)

**Does the Variety of Political Connection Matter?**

The political connections data can be disaggregated into various types. There were three varieties of political counterparties in the data: members of government (i.e. Executives or Ministers), close relations to key politicians (i.e. members of government’s families), and members of parliament. There were two roles these political counterparties could play at the connected firms: owners or directors. In Table 4, I examine the role different varieties of political connections play, by running regressions of the form:

\[
Pr[y_i = 1] = \Lambda \left( \beta \cdot d_{\text{connect\_variety}_i} + \gamma_i X_i + \sum_{j=1}^{n} \gamma_j d_j + \sum_{k=1}^{m} \gamma_k d_k + \epsilon_{ijk} \right)
\]

where $d_{\text{connect\_variety}_i}$ represents the variety of political connection; $X_i$ represents the

\(^{33}\) Bertrand et al. (2000) argue that if the magnitude of the coefficient of interest remains stable when we increase the effective set of firm-level unobservable variables by dropping variables that are observable, then results are unlikely to be driven by unobservable characteristics. Here column 5 can be compared to column 1 which has no industry-level or country-level controls to check for the magnitude of potential biases caused by variables that are unobservable or on which reliable data does not exist.

\(^{34}\) This finding is inconsistent with a skeptical interpretation of my results suggesting that the only reason that I find a significant relationship between firm-level political connections and cross-listing decisions is because only larger firms are politically connected. Furthermore, boxplots depicting categorical firm level summary statistics (broken out by political status of firms) in the Appendix, show no major differences in the size distribution of connected/unconnected firms across countries.
firm-level controls; \(d_j\), industry dummies; and \(d_k\), country dummies.

In columns 2 through 5 of Table 4, we can see the effect on cross-listing of firms’ various domestic political counterparts. For the average firm, a connection with a Member of Parliament does not affect its propensity to cross-list; however, connections with Ministers do increase the propensity that a firm maintains a cross-listing by about 18% within countries. Firms with connections to key politicians’ close families also see a positive effect on cross-listing, although a smaller one than with the politician himself. If we expect, as in Baron and Ferejohn (1989), that Ministers are more able to set the policy agenda and control enforcement than legislators, then my results are consistent with a story about connected politicians’ ability to assist connected firms by passing favorable legislation or delivering firm-specific property rights.

In columns 6 through 8 of Table 4, we can see how politicians’ ownership or control roles affect firms’ cross-listing propensities. Connections to politicians with large ownership stakes increase the probability that a firm cross-lists by nearly 19% within countries, whereas connections through political directorships (control) have no measurable effect on whether firms cross-list. These results suggest that politicians look out for their own financial interests when making and enforcing legislation. The results also suggest that firms cannot gain all of the benefits of political connections simply by “buying politicians” to put on their boards.

4.2 The Role of Economic Institutions

Hypothesis 2 (that firms are more likely to cross-list if they operate in higher quality domestic economic institutions’ environments) is essentially a country-level prediction, so I first examine the relationship between domestic economic institutions and the country-level incidence of cross-listings. I run country-level bivariate regressions of the institutions measures on the
incidence of cross-listing. The results, shown in Figure 1 for property rights institutions and Figure 2 for contracting institutions, confirm the correlations predicted in Hypothesis 2.\textsuperscript{35}

\begin{align*}
\text{Pr}[y_i = 1] &= \Lambda \left( \beta \cdot Y_k + \gamma_i X_i + \sum_{\nu \gamma} \gamma_j d_j + \epsilon_{ijk} \right)
\end{align*}

where $Y_k$ represents either my measure of property rights ($PropInst_k$) or my measure of contracting ($ContractInst_k$); $X_i$ represents firm-level controls; and, $d_j$, industry dummies.

A negative and significant coefficient ($\beta$) on the variable $PropInst_k$ would support Hypothesis 2 (indicating that weaker domestic property rights institutions make firms less likely to cross-list), while a positive and significant coefficient ($\beta$) on $ContractInst_k$ would also support Hypothesis 2 (indicating that firms with stronger domestic contracting institutions are more likely to cross-list). Results of these firm-level tests appear in Table 5. Each broadly supports Hypothesis 2, showing that firms are more likely to cross-list when they have higher quality domestic economic institutions.

The setting poses two empirical challenges: 1) my measures of contracting institutions and property rights institutions are highly correlated; and, 2) I can no longer include the country dummy-variable ($d_k$) to hold constant all country-level factors that influence cross-listing.\textsuperscript{35}

\textsuperscript{35}I also run country-level multivariate regressions (not shown) that include both varieties of domestic economic institutions and continue to find coefficients with the predicted sign; however, the coefficients are not statistically significant because of limited degrees of freedom (as there are only 37 countries with sufficient data). The signs on the coefficients also remain as predicted by Hypothesis 2 when I add country averages of my firm-level controls to the regressions.
In Table 5, I run tests for contracting institutions and property rights institutions separately; this is a common way to deal with multicollinearity (i.e. that my key independent variables are highly correlated).\footnote{See Woolridge’s graduate-level econometrics text.} The regressions in columns 1 and 2 support the hypothesis that stronger domestic property rights institutions are associated with increased firm propensities to cross-list. The regressions in columns 3 and 4 support the hypothesis that stronger contracting institutions are associated with increased firm propensities to cross-list. At a minimum all four of these regressions show that the partial correlations are those predicted in Hypothesis 2.

Testing the role of a specific country-level political economy variable precludes the use of country dummy variables. If the country-level political economy variable of interest does not sufficiently control for the roles of all country-level factors, these regressions may suffer from a country-level omitted variable bias. Nevertheless, I should still be picking up meaningful partial correlations between country-level economic institutions and firms’ cross-listing decisions—correlations useful in evaluating Hypothesis 3, regarding how the role of political connections depends on the institutional context, which was the primary point of this exercise and in discussing Hypothesis 2.

**Attempting to ‘Unpack’ the Country Dummy Variables**

Ideally, we would know what country-level factors, other than contracting and property rights institutions, are absorbed by the country dummies included in some of my multi-level logit regressions. With that knowledge, I could also estimate a fully-specified regression model that simultaneously tests all three of my hypotheses. Unfortunately, there are an endless number of country-level variables that may influence cross-listing; many of these variables are highly correlated, and the degrees of freedom in the estimation are effectively highly limited (since
there are only 46 countries in my sample). Both the signs and significance levels of coefficients change depending upon the various combinations of conditioning variables included in a specification searches attempting to ‘unpack’ the country dummy variable. Consequently, most country-level variables, except for economic institutions, have difficulty standing up to the scrutiny of an extreme bounds analysis as suggested by Leamer (1985) or Sala-i-Matin (1997). The possibilities I explored nonetheless include measures of: legal origin (La Porta et al. 1998); cultures of rule breaking such as unpaid diplomatic parking tickets (Fisman and Miguel 2007); importance placed on trust, religion, and politics (World Values Surveys); and religious affiliations. Firm-level regressions testing these appear in the Appendix.

4.3 Domestic Political Connections Interacted with Economic Institutions

With a basis for understanding the independent roles domestic political connections and domestic economic institutions play in firms’ cross-listing choices, we can explore their interactions. To test the interaction relationship (Hypothesis 3), I run regressions of the form:

$$Pr[y_i = 1] = A(\beta \cdot d\_connect_i \cdot Y_k + \gamma_i X_i + \sum_{\forall j} \gamma_j d_j + \sum_{\forall k} \gamma_k d_k + \epsilon_{ijk})$$

where $d\_connect_i$ represents my dummy variable for politically connected firms; $Y_k$ represents any country-level variable, but of primary interest is the measure of property rights institutions ($PropInst_k$); $X_i$ represents the firm-level controls; $d_j$, industry dummies; and $d_k$, country dummies. As in my test of Hypothesis 1, I will treat firms’ political connections as being exogenous to firms’ cross-listing decisions. I also will treat the existence of political connections as being independent from the institutional environment. A discussion of why these decisions are reasonable is included in a discussion of robustness concerns.

If Hypothesis 3—that cross-listing is more likely for politically connected firms when
domestic property rights institutions are weak—holds, we should expect the coefficient ($\beta$) to be positive and significant on the interaction between the political connections dummy variable and my measure of property rights institutions (i.e. the coefficient on $d_{\text{connect}}_i \cdot \text{PropInst}_k$).

Results strongly supporting Hypothesis 3 appear in Table 6.

Column 1 shows the baseline specification. The partial marginal effect suggests that ceteris paribus the average politically connected firm in a country with property rights institutions at the level of those in Thailand or Greece is approximately 11 or 12% more likely to cross-list than a politically-connected firm in a country with property rights institutions at the level of those in the United States or the United Kingdom.\footnote{If we instead used regression results in column 3, 5, or 7 of Table 6 our marginal effects might be larger, so these numbers may be understated when considering the total effect. Ai and Norton (2003) show that interpretation of total interaction effects in non-linear models is not as straightforward as it is in linear models. I am, however, interested in the partial marginal effect, not the total marginal effect, so their concerns do not apply. Nonetheless, one simple solution has been to estimate instead linear probability models as a check (e.g. Osili and Paulson (2008); I get approximately the same results when I do.}

In column 2, I add an independent effect for political connections to the baseline specification. Doing so allows me to test whether some fraction of the political influence mechanism’s effect works through an alternate channel besides property rights institutions; if this were the case, contrary to Hypothesis 3, we would expect this variable’s coefficient to be independently statistically significant. It is not.\footnote{We cannot accept the hypothesis that there is an independent effect of political connections as its value is close to zero, suggesting that the empirical model in Column 1 is specified correctly while that in Column 2 is specified incorrectly. Furthermore, Column 2 shows that the coefficient on the interaction term remains stable, despite the potential misspecification.}

I also use the same general framework to test the validity of alternative hypotheses that involve the interaction between firm-level political connections and country-level factors ($d_{\text{connect}}_i \cdot Y_k$) in Table 6. These include interactions with: (1) the fraction of government-owned bank assets; (2) the quality of domestic contracting institutions captured by accounting
disclosures (ContractInst<sub>k</sub>); and, (3) the level of development.\textsuperscript{39} Results of regressions including these interactions appear in columns 3 through 8. These alternative interactions do not substantively alter my core result that the political influence mechanism works primarily through the property rights system—and, if anything, strength the core finding.

The main alternative hypothesis for why politically connected firms receive better access to financing has to do with politicians’ ability to coerce domestic bankers into offering terms they would not otherwise. If this were true, we would expect political connections to be most valuable in countries where governments control banks (and we would not have expected to find that connected firms are more likely to cross-list earlier). Hence, if coercion of bankers were the primary mechanism, we would expect an interaction between firms’ political connections and government ownership of banks to be negative and significant. I test this relationship in columns 3, 4, and 7 using data from La Porta et al. (2002) on the percentage of state-owned bank assets in a country. My results further suggest that coercion of bankers is not the primary mechanism through which politically connected firms receive favorable financing: the coefficients of interest are highly insignificant and the signs flip depending upon conditioning variables.

The results in columns 5, 6, and 7 of Table 6 suggest that domestic political connections do not work through contracting institutions. The logic for testing these relationships is that in weak contracting environments governments may be more likely to coerce banks into making favored firms preferential loans to compensate for institutional weakness.

In column 7, I include several different potential interaction effects simultaneously; again

\textsuperscript{39} All of these alternative country-level factors (Y<sub>k</sub>) are highly correlated with property rights institutions (as seen in Table 1’s country-level correlation matrix). This suggests that we are very likely to find a statistically significant interaction with these alternative variables too, if the logic that political connections help protect firms from weak formal property rights institutions is not the primary reason that politically-connected firms are more-likely to cross-list.
the results reveal that the interaction of political connections with property rights institutions dominates both an independent effect of political connections and any other interaction effect (i.e. those with government ownership of banks or with accounting disclosures). As a final check of partial interactions with country-level economic variables, I show in column 8 that the level of development does not determine whether domestic political connections affect firms’ cross-listing choices. Taken together, the results in Table 6 strongly support Hypothesis 3: ceteris paribus the weaker the property rights institutions environment is, the stronger the effect domestic political connections have on whether a firm cross-lists.

**Partial versus Total Interaction Effects**

To be consistent with Hypothesis 3, the above regressions explore the partial interaction between domestic political connections and domestic property rights institutions. We may, however, also be interested in the total interaction effects. The total relationship between domestic economic institutions and domestic political connections is more complex than the partial one. Given Leuz and Oberholzer-Gee’s (2006) finding that in Indonesia, a country with weak property rights, domestic political connections make firms less likely to cross-list (when we do not control for country-level factors), we should even expect a more complex total relationship. Otherwise, we could not reconcile the Indonesian result with my last findings (in Table 6) from which we would expect the opposite. I can reconcile these two sets of results by exploring total rather than partial interactions. I do so next.

One way to understand how the total effect of political connections varies with property

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40 I replicate Leuz and Oberholzer-Gee’s (2006) findings in an Appendix table. In my regression, while this coefficient is negative, it is not statistically significant; this likely stems from the one difference between my regression and Leuz and Oberholzer-Gee’s (2006): their measure of political connectedness attempts to incorporate the strength of political ties, while my measure from Faccio (2006) is only a dummy variable that captures whether or not a firm is politically connected. Consistent with Hypothesis 1 (as seen in Table 3, column 1) I find the opposite result when expanding the sample beyond Indonesian firms.
rights (without a fully specified econometric model requiring explicit knowledge of all country-level factors) is stratifying the sample into groups of countries by relative levels of property rights institutions and re-running my regressions. Doing so better enables us to explore the total effect of political connections—conditional on firms being located in countries with different levels of property rights institutions—rather than just the partial effects.⁴¹

Results from stratifying the sample, by country-level quartiles of property rights, appear in Table 7.⁴² The regressions indicate that the partial interaction, in Hypothesis 3, does not tell the whole story of the relationship between political connections and cross-listing. If the total relationship mirrored the partial relationship, we would expect the coefficient on the political connections variable to get larger as property rights institutions become weaker (i.e. as we move from the first column to the last). It does not. The coefficient on political connections is not statistically significant in either the countries with the strongest (Q1 in column 1) or weakest (Q4 in column 4) property rights; it is statistically significant only for the middle two quartiles. Hence, the total relationship between firms’ domestic political connections and cross-listing decisions attenuates in environments at both extremes of institutional quality.

<Insert Table 7 Here>

We can explain the attenuation in the total effect political connections have on cross-listing, despite the partial relationship showing that as property rights weaken connected firms are more likely to cross-list. At one extreme, in countries with the strongest property rights, being politically connected may do little to help firms because formal domestic property rights

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⁴¹ This also assuages concerns about the interpretation of interaction coefficients in non-linear regressions (Ai and Norton 2003).
⁴² There may be some concern that the choice of quartiles as a means of stratifying the sample into different groups of countries by institutional quality is a somewhat ad-hoc econometric approach. These concerns are valid; however, the choice of alternate thresholds of institutional quality for grouping countries reveals the same general pattern as using quartiles in Table 7.
protections are sufficient. Firms in these economies may gain little even when politicians help them, making politicians’ impact immeasurably small. At the other extreme, in environments with the weakest property rights, in addition to receiving property rights protections, politically-connected firms may also be recipients of preferential financing from politicians’ coercion of local bankers, which reduces the firms’ attractiveness to foreign capital market participants. These latter results may be consistent with Leuz and Oberholzer-Gee’s (2006) and Siegel’s (2009) stories about why firms with political connections choose not to cross-list, particularly since those researchers found empirical support in Indonesia and Mexico, countries at the bottom end of property rights protections in my sample. Their story might also hold in countries outside my sample that have extremely weak domestic property rights protections, under-developed domestic equity markets, and few cross-listings; Pakistan, which Khwaja and Mian (2004) studied, is among those countries.

 Political connections primary strategic role in improving access to finance is in mitigating the negative consequences of weak property rights; however, there may be some critical threshold of domestic property rights beyond which political connections cannot help or beyond which foreign capital markets no longer respond. Alternatively, in weak property rights environments, political coercion of bankers may still be prevalent, scaring off global financiers.

The Role of Domestic Political Institutions in Creating Value for Politically Connected Firms

Political institutions—such as autocracy or democracy; a right, left, or center government; and, years a government held office—may also shape the value of firms’ political connections in accessing finance; therefore, I tested interactions between domestic firm-level political connections and country-level political institutions variables (using the same framework as for economic institutions). To my surprise, most of the findings were null, with none reaching
statistical significance at the standard levels. The political institutions data I used in my tests came from the World Bank’s Database of Political Institutions (Beck et al. 2001).

4.4 Addressing Robustness Concerns

I believe my findings are robust; however, some readers may question assumptions I made throughout my analysis. I address these concerns here.

Endogeneity of Domestic Political Connections to Firms’ Cross-listing is Unlikely

The type of political connections I observe, where high-level political figures are owners of or directors at firms, seem unlikely to be obtained for the explicit purpose of cross-listing; likewise, cross-listings are unlikely to exist for the explicit purpose of gaining political connections.\footnote{Furthermore, several authors have argued that the types of political connections in my data are a function of chance rather than an explicit calculation on either firms’ or politicians’ part (e.g. Johnson and Mitton 2003; Faccio 2006; Fisman and Miguel 2008).} It is unclear how a large shareholder, officer, or director of a firm advising his firm to maintain a cross-listing has any direct influence as to whether or not he could go on to win an election or secure a political appointment. Furthermore, the data showed that simply installing politicians to boards of directors has little demonstrable effect on firms’ cross-listing behavior. It is also unclear how any domestic political actor would become a top officer or major shareholder of a firm simply because that firm chose to cross-list; in fact, we might expect domestic politicians’ ownership stakes fall since cross-listings typically attract a broader, more diverse shareholder base (Aherane et al. 2004; Edison and Warnock 2004).

Given the existing narratives on how political influence leads to preferential financing terms, if there were any bias in my empirics from an endogenous relationship, the bias would run in the opposite direction of my results, suggesting my magnitudes are under-estimated.\footnote{Recall that politicians can manipulate local bankers into offering preferential terms, which if anything should lead firms to favor domestic bank loans to foreign finance; however, it is unlikely that domestic politicians can manipulate foreign participants in dispersed capital markets who reside outside the institutional setting over which the politicians have dominion.}
Furthermore, politically connected firms have incentives not to reveal information about illicit benefits political influence affords them (Chaney et al. 2008) as is required of firms that cross-list (Leuz and Oberholzer-Gee 2006); this story also runs the opposite direction of my results.

In many ways, the identification strategy I employ (of observing firm behavior in foreign markets) is similar to the one successfully implemented by Osili and Paulson (2008) who look at the personal finance decisions of international migrants to the United States; they isolate holdover home country effects on individuals’ behavior when their US market activity is observed instead of their home country activity. The difference is that I look at firms, not individuals.

**Endogeneity of Domestic Institutions to Firms’ Cross-listing is Unlikely**

Concerns about the endogeneity of domestic institutions to economic outcomes primarily afflict country-level studies. By choosing to study a firm-level outcome concerns about domestic institutions being endogenous to a firm-level dependent variable (cross-listing activity) should be moot. Low-quality domestic institutions really constrain firms that have little or no ability to directly influence their quality—since firms cannot fix institutional flaws, they must instead act strategically to avoid their ill effects. To believe endogeneity of institutions to firms’ cross-listing decisions threatens my results, we would have to think that most firms select their home institutional environment based on access to finance; in that case, we would expect all firms to locate in the countries with the best institutions, which we know does not happen.\(^{45}\)

**Independence of Domestic Political Connections and Domestic Economic Institutions**

Another concern, particularly relevant to my tests of Hypothesis 3, might be about the assumption that the incidence of political connections in a country is independent of its

---

\(^{45}\)If all firms located where the best institutions are we would expect to see every firm in the world located in places like Canada, Finland, Sweden or Switzerland; we know this does not happen. Furthermore, global firms often intentionally locate a portion of their operations in weak institutional environments to arbitrage wage/skill differentials.
institutions. The data verifies this: political connections are equally prevalent in both weak and strong institutional environments.\textsuperscript{46} Hence, political connections are not necessarily more common in places where the data suggests connections might be most valuable. The source of variation in observable domestic political connections across countries has more to do with apparently random (exogenous) regulations in individual countries against firm-politician relationships than it has to do with the quality of countries’ domestic institutions (Faccio 2006).

No Different Treatment of Firm-level Attributes for Politically Connected Firms

Foreign financial markets could respond differently to the core characteristics of connected firms vis-à-vis unconnected firms; if this were true, then we might be concerned about it biasing my core findings. I can test for such biases by estimating:

\[
Pr[y_i = 1] = \Lambda(\gamma_i x_i + \beta \cdot d_{\text{connect}_i} \cdot x_i + \sum_{q_j} \gamma_j d_j + \sum_{q_k} \gamma_k d_k + \epsilon_{ij})
\]

where \(d_{\text{connect}_i}\) represents my dummy variable for politically connected firms; \(x_i\) represents the firm-level controls; \(d_j\), industry dummies; and \(d_k\), country dummies.

If \(\beta\) is measurably different than zero, then we should be concerned that foreign capital markets respond differently to firms’ core characteristics depending upon their political status. If \(\beta\) is not measurably different than zero, concerns about how foreign capital markets may respond differently to firm-level characteristics should not adulterate the prior analysis. The results (in the Appendix) indicate that foreign financial markets do not respond differently to the core observable corporate financing characteristics of firms depending upon their political status.

\textsuperscript{46} The correlation coefficient for the country-level incidence of political connections and property rights institutions is a meager 0.029 as show in the country-level correlation matrix in Table 1. Figure C9 in the Appendix shows a scatterplot that illustrates that political connections are not more prevalent in weak or strong institutional environments. Furthermore the few numerical estimates of the value of political connections at which other researchers have arrived using natural experiments appear to be uncorrelated with the incidence of political connections. For example, connections are very common in the UK, but worthless according to Faccio (2006).
5 Discussion: Role of Political Connections in Access to Finance

My empirical findings require us to update our understanding of how and why political connections improve firms’ access to finance. In most prior characterizations, political influence represents a mechanism that is useful only for domestic capital market manipulation by using politicians to coerce local bankers. Inconsistent with those stories, my findings show political connections improve firms’ access to foreign finance in addition to domestic finance. My findings also support an alternate way to characterize the role political connections play: that role is providing tacit property rights protections that reduce firms’ risk premium.

In most cases, the preferential financing that politically connected firms receive is not likely the result of coercion. Rather, preferential financing is more likely the result of rational decisions by financiers: financiers who observe firms’ explicit political connections choose to offer better financing terms because they realize that politically connected firms receive implicit property rights protections as a result of their relationship with the government. If political connections afford firms the ability to operate as if they were in a superior property rights environment, then connected firms’ cash flows will be more secure than otherwise similar firms’, reducing the connected firms’ relative risk premiums. Since firms with domestic political connections are less risky investments, they should receive relatively better financing terms and relatively better access to capital in well functioning financial markets.

Domestic political connections should matter to foreign investors in addition to domestic investors because “it is necessary to examine both how the institutions are written and how they are implemented” (Siegel 2005). In the absence of political connections, domestic investors are likely to have a comparative advantage over foreign investors in understanding how domestic institutions are implemented; domestic investors are likely to better understand the specific risks
their country’s weak property rights institutions pose to any given firm. Given this reality, domestic political connections may matter more to foreign investors than domestic ones: foreign investors can take the signal political connections provide to mean that privileged firms have implicit property rights protections when otherwise they may find it more difficult to evaluate the specific risks a weak domestic institutional environment poses.

My results suggest that political connections create efficiencies in the markets’ ability to allocate capital by reducing the uncertainty in specific firms’ property rights environment; this contrasts with stories about how political connections create inefficiencies in how the market allocates capital. Political connections serve as a second-best solution for firms that could benefit from stronger property rights protections but that face weak institutions. Of course, as a first-best solution, stronger national property rights protections would bolster the entire economy. Selectively enforced property rights protection make it easier for financiers to identify which firms to fund but do not eliminate aggregate inefficiencies caused by weak national property rights. Weak domestic property rights require firms to dedicate resources to finding strategies that prevent theft and asset taking—using political connections is one such strategy.

Another interesting implication is that (financial) globalization may, in fact, strengthen the position of firms with domestic political connections, since foreign financiers rationally fund firms that can influence their domestic policy environment; this subtly contrasts with stories about how incumbent industrialists should wield political power to oppose all forms of financial development/globalization as the incumbents have the most to lose (Rajan and Zingales 2003).

6 Conclusion

Using a unique multi-level cross-country dataset, my empirical analysis identifies two
new political economy factors that improve firms’ access to foreign finance: 1) domestic firm-
level political connections, and 2) domestic country-level property rights protections. Moreover,
when these two factors are interacted, their sign reverses—suggesting that firm-level political
connections play a strategic role as a substitute for strong national property rights.

These findings require us to update our understanding of how and why political influence
enables firms to obtain better financing. Political connections are useful not only for access to
domestic finance, but also for access to foreign finance. The reason political connections
improve access to all types of finance has more to do with the provision of firm-specific property
rights protections that reduce connected firms’ risk premiums than with domestic capital market
manipulation.

My findings also help reconcile competing political economy stories about the primacy of
either firm- or country-level political economy factors in explaining firms’ cross-listing activity.
Past explanations failed to consider interactions between firm-level political influence measures
and country-level institutions measures as my study does. Furthermore, past studies of firm-
level political connections and cross-listing lacked external validity because they were executed
in single-country environments.

While single-country studies about political influence are and will continue to be
valuable, we need to be cautious about generalizing their results. Furthermore, single-country
studies cannot help us understand where the value of political influence is derived if it is
conditional on the country context, as it is for access to finance. In these cases, applying my
methodological innovation of interacting firm-level political influence measures with country-
level institutional features could lead to promising results in future research on other outcomes.
References


Figures and Tables

Figure 1 – Country-level Relationship between Cross-listing Incidence and Property Rights Institutions  
*The following figure shows the results of a bivariate country-level regression of property rights institutions on the incidence of cross-listings for a particular country in my sample.*

![Country-level Relationship between Cross-listing Incidence and Property Rights Institutions](image1)

Figure 2 – Country-level Relationship between Cross-listing Incidence and Contracting Institutions  
*The following figure shows the results of a bivariate country-level regression of contracting institutions on the incidence of cross-listings for a particular country in my sample.*

![Country-level Relationship between Cross-listing Incidence and Contracting Institutions](image2)
### TABLE 1 - Country Level Correlation Matrix

<table>
<thead>
<tr>
<th></th>
<th>Strong Property Rights</th>
<th>Weak Contracting</th>
<th>Log(GDP per Capita)</th>
<th>Incidence of Political Connections</th>
<th>Incidence of Cross-Listings</th>
<th>Government Ownership of Banks</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Strong Property Rights</strong></td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Weak Contracting</strong></td>
<td>-0.551</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Log(GDP per Capita)</strong></td>
<td>-0.858</td>
<td>0.485</td>
<td>1.000</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Incidence of Political Connections</strong></td>
<td>0.029</td>
<td>0.270</td>
<td>-0.144</td>
<td>1.000</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Incidence of Cross-Listings</strong></td>
<td>-0.327</td>
<td>0.326</td>
<td>0.441</td>
<td>-0.093</td>
<td>1.000</td>
<td></td>
</tr>
<tr>
<td><strong>Government Ownership of Banks</strong></td>
<td>0.379</td>
<td>-0.466</td>
<td>-0.374</td>
<td>-0.160</td>
<td>-0.314</td>
<td>1.000</td>
</tr>
</tbody>
</table>

### TABLE 2 - Country-level Incidence of Firms' Cross-listing, Categorical Means

<table>
<thead>
<tr>
<th>Type of Firm:</th>
<th>All Countries</th>
<th>Q1 (Strongest)</th>
<th>Q2</th>
<th>Q3 (Weakest)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Politically-Connected Firms</td>
<td>46.7%</td>
<td>49.7%</td>
<td>51.6%</td>
<td>50.7%</td>
</tr>
<tr>
<td>Unconnected Firms</td>
<td>38.4%</td>
<td>45.6%</td>
<td>44.4%</td>
<td>38.8%</td>
</tr>
<tr>
<td><strong>Difference (Connected-Unconnected)</strong></td>
<td>8.4%</td>
<td>4.1%</td>
<td>7.2%</td>
<td>12.0%</td>
</tr>
<tr>
<td><strong>Ratio (Connected/Unconnected)</strong></td>
<td>121.8%</td>
<td>109.0%</td>
<td>116.2%</td>
<td>130.9%</td>
</tr>
</tbody>
</table>

The numbers in this table represent categorical means of country level incidences of firms' cross-listing activity by firm type (politically connected in the first row or unconnected in the second row). The first column is meant to be a benchmark. The second through fifth columns represent categorical means broken out by country-level property rights protections. The last two rows calculate the difference between the first two rows and the ratio between the first two rows to illustrate key patterns—politically connected firms are more likely to cross-list, and up to some threshold of property rights institutions political connections appear increasingly valuable. For a country to be included in the sample, for this table, it must have a least one politically-connected firm.
TABLE 3 - Does Being Politically Connected Increase Firms' Probability of Cross-listing?

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Cross List Dummy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Politically Connected</td>
<td>0.613***</td>
</tr>
<tr>
<td></td>
<td>(0.119)</td>
</tr>
</tbody>
</table>

**Firm Level Controls**

<p>| | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Size</td>
<td>-0.007</td>
<td>-0.001</td>
<td>0.687***</td>
<td>0.718***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.029)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Return on Assets</td>
<td>-0.001</td>
<td>0.043</td>
<td>-0.450</td>
<td>-0.265</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.040)</td>
<td>(0.520)</td>
<td>(0.433)</td>
</tr>
<tr>
<td>Capital Intensity</td>
<td>-0.476***</td>
<td>-0.473***</td>
<td>-1.084***</td>
<td>-0.660***</td>
</tr>
<tr>
<td></td>
<td>(0.147)</td>
<td>(0.153)</td>
<td>(0.174)</td>
<td>(0.182)</td>
</tr>
<tr>
<td>Leverage</td>
<td>0.588</td>
<td>0.497</td>
<td>-0.256</td>
<td>-0.208</td>
</tr>
<tr>
<td></td>
<td>(0.369)</td>
<td>(0.380)</td>
<td>(0.245)</td>
<td>(0.252)</td>
</tr>
</tbody>
</table>

**Industry Level Controls**

<table>
<thead>
<tr>
<th>Industry Dummies</th>
<th>No</th>
<th>Yes</th>
<th>No</th>
<th>Yes</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leverage</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

**Country Level Controls**

<table>
<thead>
<tr>
<th>Country Dummies</th>
<th>No</th>
<th>No</th>
<th>Yes</th>
<th>Yes</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leverage</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

**Scenario being Tested:**

<table>
<thead>
<tr>
<th>Wald Tests (F-Statistics)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Joint Insignificance of Industry Dummies</td>
</tr>
<tr>
<td>Joint Insignificance of Country Dummies</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Percentage of Obs. Predicted Correctly</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Observations</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>60.27%</th>
<th>64.15%</th>
<th>77.82%</th>
<th>79.27%</th>
<th>60.33%</th>
</tr>
</thead>
<tbody>
<tr>
<td>7461</td>
<td>7461</td>
<td>7461</td>
<td>7461</td>
<td>7461</td>
<td></td>
</tr>
</tbody>
</table>

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level; † indicates significance at the 15% level for the coefficient on the politically connected dummy variable only. The estimation method for all regressions is logit. In parentheses below the estimated coefficient values are Huber/White robust standard errors. Columns 1 and 5 include a constant that is not reported. Columns 1 and 2 show regressions that pool the data across countries, while Columns 3 and 4 show regressions that are within country tests. For the two Wald tests, the null hypotheses are $H_0: \gamma_j=0$ for all $j$ and $H_0: \gamma_k=0$ for all $k$; the null for each of these are strongly rejected in all regressions, meaning that the including the dummy variables has joint significance at the 1% level. Column 5 is included as a robustness check in the spirit of Bertrand et al. (2000) to show that unobservable firm-level characterististics are unlikely to be driving my results.
**TABLE 4 - Political Connection Varieties: Does the Type of Connection Matter for Cross-listing?**

**Dependent Variable:**

<table>
<thead>
<tr>
<th></th>
<th>Cross List Dummy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Politically Connected (Any Type)</td>
<td>0.233†</td>
</tr>
<tr>
<td>(0.157)</td>
<td></td>
</tr>
</tbody>
</table>

**Political Connection with...**

<table>
<thead>
<tr>
<th>Connection Type</th>
<th>Coefficient 1</th>
<th>Coefficient 2</th>
<th>Coefficient 3</th>
<th>Coefficient 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>...Government (Executive or Minister)</td>
<td>0.712**</td>
<td>0.706**</td>
<td>(0.376)</td>
<td>(0.382)</td>
</tr>
<tr>
<td>...Close Relation of Government Member</td>
<td>0.536**</td>
<td>0.532**</td>
<td>(0.276)</td>
<td>(0.279)</td>
</tr>
<tr>
<td>....Member of Parliament</td>
<td>-0.003</td>
<td>-0.003</td>
<td>(0.202)</td>
<td>(0.202)</td>
</tr>
</tbody>
</table>

**Political Connection through...**

<table>
<thead>
<tr>
<th>Connection Type</th>
<th>Coefficient 1</th>
<th>Coefficient 2</th>
<th>Coefficient 3</th>
<th>Coefficient 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>...Ownership</td>
<td>0.751***</td>
<td>0.740***</td>
<td>(0.225)</td>
<td>(0.223)</td>
</tr>
<tr>
<td>...Directorship</td>
<td></td>
<td>-0.117</td>
<td>(0.194)</td>
<td>(0.194)</td>
</tr>
</tbody>
</table>

**Industry Level Controls**

<table>
<thead>
<tr>
<th>Size</th>
<th>0.718***</th>
<th>0.720***</th>
<th>0.721***</th>
<th>0.720***</th>
<th>0.721***</th>
<th>0.720***</th>
<th>0.719***</th>
<th>0.721***</th>
</tr>
</thead>
<tbody>
<tr>
<td>Return on Assets</td>
<td>-0.265</td>
<td>-0.264</td>
<td>-0.266</td>
<td>-0.265</td>
<td>-0.267</td>
<td>-0.265</td>
<td>-0.265</td>
<td>-0.267</td>
</tr>
<tr>
<td>Capital Intensity</td>
<td>-0.660***</td>
<td>-0.656***</td>
<td>-0.671***</td>
<td>-0.655***</td>
<td>-0.670***</td>
<td>-0.654***</td>
<td>-0.652***</td>
<td>-0.671***</td>
</tr>
<tr>
<td>Leverage</td>
<td>-0.208</td>
<td>-0.224</td>
<td>-0.201</td>
<td>-0.218</td>
<td>-0.195</td>
<td>-0.229</td>
<td>-0.230</td>
<td>-0.194</td>
</tr>
</tbody>
</table>

**Industry Dummies**

| Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Country Dummies**

| Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Scenario being Tested:**

<table>
<thead>
<tr>
<th>Wald Tests (F-Statistics)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Joint Insignificance of Industry Dummies</td>
</tr>
<tr>
<td>Joint Insignificance of Country Dummies</td>
</tr>
</tbody>
</table>

**Percentage of Obs. Predicted Correctly**

| Percentage | 79.27% | 79.29% | 79.33% | 79.24% | 79.32% | 79.32% | 79.25% | 79.29% |

**Number of Observations**

| 7461  | 7461  | 7461  | 7461  | 7461  | 7461  | 7461  | 7461  |

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level; † indicates significance at the 15% level for the coefficient on the politically connected dummy variable in the first column only. The estimation method for all regressions is logit. In parentheses below the estimated coefficient values are Huber/White robust standard errors. For the two Wald tests, the null hypotheses are $H_0: \gamma_j=0$ for all $j$ and $H_0: \gamma_k=0$ for all $k$; the null for each of these are strongly rejected in all regressions, meaning that the including the dummy variables has joint significance at the 1% level.
### TABLE 5 - How Institutions Affect Firms' Probability of Cross-listing

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Cross List Dummy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Weak Domestic Property Rights</td>
<td>-0.627**</td>
</tr>
<tr>
<td></td>
<td>(0.263)</td>
</tr>
<tr>
<td>Strong Domestic Contracting</td>
<td>4.242***</td>
</tr>
<tr>
<td></td>
<td>(0.326)</td>
</tr>
<tr>
<td>Politically Connected</td>
<td>0.594***</td>
</tr>
<tr>
<td></td>
<td>(0.124)</td>
</tr>
<tr>
<td>Size</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>Return on Assets</td>
<td>0.043</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
</tr>
<tr>
<td>Capital Intensity</td>
<td>-0.450***</td>
</tr>
<tr>
<td></td>
<td>(0.158)</td>
</tr>
<tr>
<td>Leverage</td>
<td>0.577</td>
</tr>
<tr>
<td></td>
<td>(0.390)</td>
</tr>
</tbody>
</table>

### Firm Level Controls

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Size</td>
<td>-0.004</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Return</td>
<td>0.043</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>Capital</td>
<td>-0.450***</td>
<td>-0.437**</td>
</tr>
<tr>
<td></td>
<td>(0.158)</td>
<td>(0.158)</td>
</tr>
<tr>
<td>Leverage</td>
<td>0.577</td>
<td>0.543</td>
</tr>
<tr>
<td></td>
<td>(0.390)</td>
<td>(0.390)</td>
</tr>
</tbody>
</table>

### Industry Level Controls

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
</tr>
</thead>
<tbody>
<tr>
<td>% of Obs. Predicted Correctly</td>
<td>64.12%</td>
<td>64.31%</td>
<td>65.94%</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>7355</td>
<td>7355</td>
<td>7191</td>
</tr>
</tbody>
</table>

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level. The estimation method for all regressions is logit. In parentheses below the estimated coefficient values are Huber/White robust standard errors. For the Wald test, the null hypotheses is $H_0: \gamma_j=0$ for all $j$; the null is strongly rejected in all regressions, meaning that the coefficients are jointly significant.
### TABLE 6 - Testing the Interaction between Political Connections and Economic Institutions

<table>
<thead>
<tr>
<th></th>
<th>Dependent Variable:</th>
<th>Cross List Dummy</th>
<th>Firm Level Controls</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Weak Property Rights * Connected</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>1.725*</td>
<td>1.623‡</td>
<td>2.579**</td>
</tr>
<tr>
<td></td>
<td>(0.971)</td>
<td>(1.433)</td>
<td>(1.498)</td>
</tr>
<tr>
<td></td>
<td>-0.785</td>
<td>0.676</td>
<td>-0.243</td>
</tr>
<tr>
<td></td>
<td>(1.151)</td>
<td>(0.718)</td>
<td>(1.384)</td>
</tr>
<tr>
<td></td>
<td>ln_GDP*Connected</td>
<td>0.023</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.229)</td>
<td>(0.240)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Politically Connected</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.718***</td>
<td>0.718***</td>
<td>0.718***</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.028)</td>
<td>(0.028)</td>
</tr>
<tr>
<td></td>
<td>-0.263</td>
<td>-0.263</td>
<td>-0.263</td>
</tr>
<tr>
<td></td>
<td>(0.424)</td>
<td>(0.424)</td>
<td>(0.424)</td>
</tr>
<tr>
<td></td>
<td>-0.635***</td>
<td>-0.635***</td>
<td>-0.635***</td>
</tr>
<tr>
<td></td>
<td>(0.183)</td>
<td>(0.183)</td>
<td>(0.183)</td>
</tr>
<tr>
<td></td>
<td>-0.217</td>
<td>-0.216</td>
<td>-0.217</td>
</tr>
<tr>
<td></td>
<td>(0.254)</td>
<td>(0.254)</td>
<td>(0.254)</td>
</tr>
<tr>
<td></td>
<td>Industry Dummies</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>Country Dummies</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>Scenario being Tested:</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Joint Insig. of Industry Dummies</td>
<td>33.862</td>
<td>33.862</td>
</tr>
<tr>
<td></td>
<td>Joint Insig. of Country Dummies</td>
<td>37.827</td>
<td>37.827</td>
</tr>
<tr>
<td></td>
<td>% of Obs. Predicted Correctly</td>
<td>79.32%</td>
<td>79.29%</td>
</tr>
<tr>
<td></td>
<td>Number of Observations</td>
<td>7355</td>
<td>7355</td>
</tr>
</tbody>
</table>

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level. ‡ represents these coefficients are jointly significant at the 10% level for the second column. The estimation method for all regressions is logit. In parentheses below the estimated coefficient values are Huber/White robust standard errors. For the Wald test, the null hypotheses is $H_0: \gamma_j = 0$ for all $j$ and $H_0: \gamma_k = 0$ for all $k$; the null for each of these is strongly rejected in all regressions, meaning that the including the dummy variables has joint significance at the 1% level.
TABLE 7 - Stratified Sample Approach to Connections and Institutions as Joint Determinants

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Cross List Dummy</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Property Rights Institutions’ Quartiles (Country-Level)</strong></td>
<td></td>
</tr>
<tr>
<td><strong>Sample:</strong></td>
<td>Strongest (0-25%)</td>
</tr>
<tr>
<td>Politically Connected</td>
<td>-0.224</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
</tr>
</tbody>
</table>

**Firm Level Controls**

| | 0.691*** | 0.780*** | 0.866*** | 0.752*** |
| | (0.032) | (0.057) | (0.083) | (0.138) |
| Size | -0.180 | -1.765*** | -0.855 | 0.702 |
| | (0.149) | (0.554) | (0.627) | (2.352) |
| Return on Assets | -0.568** | -0.865* | -0.494 | -0.564 |
| | (0.251) | (0.448) | (0.413) | (0.701) |
| Capital Intensity | -0.192 | -0.119 | -0.743 | 0.237 |
| | (0.392) | (0.474) | (0.527) | (0.685) |

**Industry Level Controls**

| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Country Fixed Effect | Yes | Yes | Yes |

**Scenario being Tested:**

| | Wald Tests (F-Statistics) |
| | 39.309 | 22.549 | 17.025 | 4.590 |
| Joint Insig. of Ind. Dummies | 66.403 | 33.292 | 29.274 | 8.947 |
| Joint Insig. of Country Dummies | 41.81 | 1258 | 1451 | 367 |

% of Obs. Predicted Correctly:

| 77.61% | 79.81% | 85.80% | 78.47% |

Number of Observations:

| 4181 | 1258 | 1451 | 367 |

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level. The estimation method for all regressions is logit. In parentheses below the estimated coefficient values are Huber/White robust standard errors. For the Wald test, the null hypotheses is H₀: γ_j=0 for all j and H₀: γ_k=0 for all k; the null for each of these are strongly rejected in all regressions, meaning that the including the dummy variables has joint significance at the 1% level. The countries that fall into different quartiles are: for Q1 (the strongest property rights institutions), Austria, Canada, Denmark, Finland, Germany, the Netherlands, New Zealand, Norway, Switzerland, and the United Kingdom; for Q2 (with moderately strong property rights institutions), Australia, Belgium, France, Ireland, Italy, Japan, Singapore, Spain, Sweden, and Taiwan; for Q3 (with moderately weak property rights institutions) Brazil, Chile, Greece, Hong Kong, India, Israel, Malaysia, Mexico, South Korea, and Thailand; and, for Q4 (with the weakest property rights institutions), Argentina, Colombia, Indonesia, Peru, the Philippines, South Africa, Sri Lanka, Turkey, Venezuela, and Zimbabwe.