

An Empirical Investigation into the Political Economy of the Firm in a Globalizing World Economy: How Domestic Political Connections Affect Cross-listing Choices

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Abstract

This paper investigates the role domestic firm-level political connections play in determining a firm's propensity to cross-list securities in equity markets outside its traditional home country. It finds that maintaining domestic political connections enables firms to access foreign capital markets *across* countries. It characterizes the role political connections with domestic governments play by showing that the implicit *property rights* protections they provide dominate any potential benefits received through domestic back-channel financial *contracting* arrangements resulting from government coercion or ownership of banks, suggesting the role political connections play in obtaining preferential financing terms has more to do with protections from a weak operating environment than it has to do with capital market manipulation. The weaker the domestic institutional environment is, the less likely the average firm is to cross-list. For connected firms, however, the weaker domestic *property rights* institutions are the more likely they are to cross-list. The paper employs, in its empirical analysis, a multi-level, cross-sectional dataset containing information spanning 46 countries (of varying institutional quality), in which 12,395 firms belong to a wide range of industries and have different political statuses (among other corporate financing characteristics) that influence their global financing behavior.

Keywords: Cross-listing, Political Connections, Economic Institutions, Property Rights, Contracting, Corporate Finance, Corporate Governance

JEL Codes: G3, G15, K1, K4, P48, L1, L2, O16, F2, F3, F5, D7

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1 Introduction

Firms choose not only where to manufacture and sell goods, but also where to finance their operations. Desai (2008) contends that there are three important location decisions corporations make in a globalizing world economy; these are for: (1) their “traditional” home(s) for managerial and day-to-day business operations, (2) their legal home(s), and (3) their financial home(s). Firm-level and country-level political economy considerations play important roles in both the ability and the willingness of firms to “de-center” these homes.

The purpose of this paper is to investigate the role domestic firm-level political connections play in firms’ choices to maintain an alternate financial home through cross-listing securities (in the context the domestic country-level economic institutions that shape firms operating environments). While we know a lot about the political and country risk factors that influence firms’ decisions on where to locate business operations and where to make foreign direct investments,¹ we know much less about the political economy of firms’ decisions to finance outside of their traditional homes. This makes studying the financial location choices of firms in a globalizing economy a worthwhile departure. Furthermore, the setting provides a unique opportunity to identify and characterize the means through which domestic political connections create value for firms in obtaining finance.

While there is some work on the political economy of cross-listing decisions, rarely, if ever, does research into firms’ decisions to globalize the locations in which they finance consider the roles of both firm-level and country-level political economy factors simultaneously as this paper does. Furthermore, the existing research into the role domestic political connections play in cross-listing choices yields mixed results. This paper characterizes the role domestic political connections play in cross-listings as being dependent on the domestic institutional environment in which the firms operate. By highlighting how firm-level connections are embedded within a country-level context, this paper helps to explain why existing research has yet to agree on a single picture of the political economy of cross-listing. A better understanding allows a

¹ On Google Scholar, a recent search for the terms “political economy” and “FDI” returned nearly 40,000 articles, while a search for the term “cross-listing” without also requiring the term “political economy” returned only about 10% as many articles. For some recent work on this topic see Lee and Mansfield (1996); Dunning (1998); Henisz and Williamson (1999); Henisz (2000); Wei (2000); Henisz and Zelner (2001); Jensen (2003); Henisz (2004); Desai and Moel (2008).

regulatory bonding theory of why some firms choose to cross-list to co-exist with an alternative, updated version of the *reputational bonding* theory of why some firms may choose not to cross-list. Furthermore, the result that firms with domestic political connections are more likely to cross-list if they are from weak *property rights* countries, suggests that some portion of the preferential financing terms connected firms receive is derived from protections in the real economy rather than explicit capital market manipulation as most past research suggests.

1.1 Choosing an Alternative Financial Home through Cross-listing

The decision of firms to finance abroad through cross-listing securities on another capital market is a common one.² More than 20% of the securities listed on the New York Stock Exchange (NYSE) in 2004 were for foreign firms. Other popular cross-listing destinations include the largest, deepest, and most efficient capital markets around the world: the NASDAQ in New York, the Deutsche Börse in Frankfurt, the Euronext in Paris, and the London Stock Exchange (LSE).³ On the LSE over 50% of the total value of securities traded in 2004 was derived from foreign firms; on some smaller exchanges, like the one in Switzerland, this number was as high as 93.5%.⁴

The benefits firms can obtain from cross-listing securities can be substantial.⁵ They come in various ways. Each effectively increases a firm's market capitalization (Miller 1999). Some of the ways a firm can benefit from cross-listing include: a reduction in the firm's cost of capital (Hail and Leuz 2008); greater equity research analyst coverage/exposure (Baker, Nofsinger, and Weaver 2002; Lang, Lins, and Miller 2003; Leuz 2003); increased liquidity of shares (Pagano, Röell, and Zechner 2002); a more diverse shareholder base (Aherane, Griever, and Warnock 2004; Edison and Warnock 2004; Ammer, Holland, Smith, and Warnock 2006); and, reduced

² The literature on cross-listings is vast: a simple search for the term "cross-listing" on Google Scholar returns over 4,000 articles. Doidge, Karolyi, Lins, Miller and Stulz (2009) suggest the following comprehensive, and analytic, literature reviews as a starting point: Karolyi (1998, 2006), Claessens, Klingebiel, and Schmukler (2002), and Benos and Weisbach (2004).

³ While more rare, we also observe a few firms cross-listing their securities on exchanges in emerging markets including those in Argentina, Chile, Malaysia, and the Philippines

⁴ All of these facts come from Karolyi (2006); see his Tables 1 and 2 for details on foreign listings on stock exchanges around the world. For a visual representation of the data, see Figures A1 & A2 in this paper's Appendix.

⁵ Since the focus of this paper is on the firm, I omit a discussion of the purported benefits to the domestic capital markets of firms choosing to cross-list. Nevertheless, the literature in this area is also quite rich. For more on home country financial market development resulting from having domestic firms cross-list their securities, see: Hargis (2000), Moel (2001), Claessens, Klingebiel, & Schmukler (2002), Karolyi (2004), and Levine and Schmukler (2006).

agency conflicts between majority and minority shareholders (Coffee 1999, 2002; Stulz 1999; Doidge, Karolyi, and Stulz 2004; Karolyi 2006).

Despite the potentially substantial benefits, not every firm chooses to cross-list because there are associated costs. Aside from one-time, direct financial costs associated with executing the cross-listing transaction, there are a number of ongoing direct costs involved in maintaining the foreign listing. The higher level of disclosure that attractive cross-listing destinations require necessitates that firms employ a greater number of accountants, lawyers, and auditors in order to maintain a cross-listing. These direct costs of compliance are cited as one of the chief deterrents to cross-listing in a survey of firms conducted by Fanto and Karmel (1997). Disclosure costs are significant for many firms, particularly those from countries where the home country disclosure requirements are most lax.⁶ These direct costs are easier for larger firms to bear since a large portion of the costs associated with cross-listing are fixed, making them more difficult for smaller firms to absorb (Pagano, Röell, and Zechner 2002; Doidge, Karolyi, and Stulz 2004).

There may be ongoing indirect costs associated with the stricter disclosure requirements of attractive cross-listing destinations as well. Many firms do not foresee any benefits from disclosing additional information; in other cases, firms do not want to reveal certain information to the public (Licht 2000; Lambert, Leuz, and Verrecchia 2007). Chaney, Faccio, and Parsley (2008) explore a particularly relevant class of firms that often have little to gain from revealing more information: they find cross-country evidence that politically-connected firms prefer not to disclose as much as non-connected firms.

Beyond the indirect costs of cross-listing, there also appear to be pre-conditions for having a favorable reception by foreign investors. These may deter some firms from attempting to cross-list. Notably larger firms fare better with investors located outside of their traditional homes; this fact holds regardless of whether a firm is cross-listed or whether foreign investors choose to invest directly in securities listed on the domestic financial market in a firm's traditional home (Edison and Warnock 2004).

⁶ Following the implementation of the Sarbanes-Oxley Act in July 2002, many firms voluntarily chose to de-list (no maintain their cross-listings) in the United States because the legislation strengthened disclosure requirements. The foreign firms that choose to de-list were either unable or unwilling to respond to this shock to the costs associated with maintaining their foreign financial home in the United States. Many choose to list their securities in London where disclosure requirements were relatively lower following the legislation instead. For more on this see Doidge, Karolyi, and Stulz (2007); Li (2007), Litvak (2007); and, Zingales (2007)

While there has been substantial inquiry into the costs and benefits of cross-listing, there has been less research into the determinants of firms' decision to cross-list (or not). Firm size is one of the best predictors of whether a firm will choose to cross-list its securities. We should expect this given the dual logic that larger firms are both more attractive to foreign investors and more capable of bearing associated costs.

Coffee (1999, 2002) and Stulz (1999) were early proponents of a *regulatory bonding* theory explaining why some firms choose to cross-list securities. It is one of the most common explanations for why a firm chooses to cross-list securities, other than its size. Doidge, Karolyi and Stulz (2004) were the first to formalize the *regulatory bonding* theory building a mathematical model. In essence, the theory 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 potential benefits (Doidge, Karolyi, and Stulz 2004).⁷ There is a large literature attempting to test the *regulatory bonding* theory. While it finds substantial support, there remain skeptics.⁸

Siegel (2004, 2005) introduces the *reputational bonding* theory as an alternative to explain why the *regulatory bonding* theory may fail in some instances. The *reputational bonding* theory is about why some firms choose *not* to cross-list. Siegel suggests that some firms—those that maintain domestic political connections or those that maintain strategic cross-border alliances—have *reputational bonds* that they may use to secure similar benefits, at a lower cost domestically, to the ones they would have gained from cross-listing abroad. Proponents of this *reputational bonding* theory (and its variants) claim that it is a dominant strategy since they expect any associated costs to be lower than those associated with *regulatory bonding*.

Using evidence from Mexico, Siegel (2004) finds that firms employing *reputational bonding* (by maintaining strategic cross-border alliances with multinational firms) are less likely

⁷ Doidge, Karolyi, and Stulz (2004) not only find empirical support for the idea that the larger the gap is between the quality of *contracting* institutions at home and in a cross-listing destination are, the greater the benefits will be, but also they write a formal theoretical model explaining why.

⁸ Support for the regulatory bonding hypothesis is found by Doidge, Karolyi, and Stulz (2004); LeL and Miller (2008), and Doidge, Karolyi, Lins, Miller and Stulz (2009) among others. Doubts about the regulatory bonding hypothesis are cast by Licht (2003); Siegel (2005); Lang, Raedy, and Wilson (2006).

to cross-list securities; he suggests these alliances serve as an alternative value creation mechanism since they also ensure quality corporate governance. Leuz and Oberholzer-Gee (2006) use Indonesian data to argue that firms with domestic political connections are less likely to cross-list than their unconnected counterparts. They speculate that politically connected firms have a reduced need to cross-list because the government to which they are tied can pressure banks into giving the connected firms more favorable loan terms than those found in competitive financial markets. Leuz and Oberholzer-Gee (2006) conclude based upon their Indonesian data that the reason they find politically connected firms are less likely to cross-list is because connected firms are the beneficiaries of domestic capital market manipulation.

One limitation of both Seigel's (2004) and Leuz and Oberholzer-Gee's (2006) tests of *reputational bonding* is that the institutional context is fixed since the tests are run within single countries, rather than across multiple countries; furthermore, economic institutions are relatively weak in both Mexico and Indonesia, where the hypothesis finds its validation. The limitation of testing *reputational bonding* within a single, fixed institutional context is exposed when Hung, Wong, and Zhang (2008) find that stronger domestic political connections make a firm more, not less, likely to cross-list in the Chinese case.⁹

An integrated approach to the question of what the political economy determinants of cross-listing are—one that simultaneously considers the roles both firm-level and country-level factors play in the decision to cross-list—has yet to be undertaken. In the next section of the paper, I provide an analytical framework to structure this type of data analysis. In it I present three hypotheses that expose the natural tensions between these firm-level and country-level political economy factors in determining whether or not a firm chooses to cross-list. First, however, I review important ways in which both domestic political connections and high quality domestic economic institutions create value for firms.

⁹ Hung, Wong, and Zhang (2008) in a recent working paper suggest that politically connected firms may want to pursue cross-listings because they provide “a mechanism for constraining politicians’ pursuit of private benefits and improving efficiency”; they caution, however, that the effectiveness of this mechanisms may be limited if the connections are too strong.

2 Related Literature

2.1 Economic Institutions Set the Rules of the Game

North (1981) defines, in his seminal work, economic institutions as “the rules of the game in a society” that structure both political and economic exchange.¹⁰ As Desai (2008) argues, the economic institutions in a firm’s traditional home are critically important in its choice to maintain an alternate financial home: in many cases firms may cross-list in an attempt to opt out of certain aspects of their domestic institutional environment.

North’s definition of economic institutions can be somewhat difficult to apply to data analyses because in it they are monolithic. In an attempt to clarify what is meant by economic institutions and how we should think about them, Acemoglu and Johnson (2005) follow North’s (1981) lead in “unbundling” them. Acemoglu and Johnson assert that there are two varieties of economic institutions—those that apply to *contracting* and those that apply to *property rights*.¹¹ Acemoglu and Johnson (2005) further claim that *property rights* institutions should have first order effects on economic outcomes, while *contracting* institutions play a more minor role.

Acemoglu, Johnson, and Robinson (2001, 2002, 2005) focus on the role of *property rights* institutions in determining economic outcomes. They assert that *property rights* institutions are the fundamental cause of long-run economic growth.¹² Their primary measure of *property rights* institutions is the expropriation risk index published in Political Risk Services’ International Country Risk Guide.¹³ The logic is simply that the higher the risk of expropriation, either by the state or other actors in the economy, the weaker are *property rights* institutions.

In a series of papers on law and finance, La Porta, Lopez-de-Salinas, Shleifer, and Vishny (1998) study the role of *contracting* institutions, particularly with respect to financial market

¹⁰ Given that they apply to all firms in a specific location, economic institutions tend to be treated as being constant within a country; hence, they are a country-level variable in most analyses.

¹¹ A common analytic problem faced in the unbundling institutions is that both *property rights* institutions and *contracting* institutions tend to be highly correlated variables across countries. If one is weak, the other also tends to be weak. A visual representation of how common measures of *property rights* institutions and *contracting* institutions (those used by Acemoglu, Johnson and Robinson, and those used by La Porta, Lopez-de-Salinas, Shleifer, and Vishny) are correlated across countries can be seen in Figure A3 in the Appendix.

¹² While not important to my analysis, the empirical foundations of the claim that *property rights* institutions are the fundamental cause of economic growth, however, have come under considerable recent attack. For example, see Albouy (2008) who attacks the instrumental variable Acemoglu, Johnson, and Robinson (2001, 2002, 2005) use in their analysis. Glaeser, La Porta, Lopez de-Salinas, and Shleifer (2004) raise other questions about their argument.

¹³ A visual representation of the data Acemoglu, Johnson, and Robinson (2001, 2002, 2005) use is available in Figure A4 in the Appendix of this paper.

development. One of their primary measures of contracting institutions is an index of accounting disclosure quality published by the Center for International Financial Analysis and Research,¹⁴ this measure has also been used extensively in the empirical cross-listings literature, particularly with respect to the *regulatory bonding* theory. Again, the logic is simply that the higher the quality of accounting disclosures in a particular country, the more transparent are firms' activities, making it easier to write the contracts necessary to borrow or lend capital.

Conceptually, the area where *contracting* institutions can overlap with *property rights* institutions can lead to confusion. In practice, *contracting* institutions tend to refer to rules that require the provision of information enabling contracts to be written; whereas *property rights* institutions tend to refer to the enforcement of such contracts and protections from outright theft.

While most of the prominent studies on the role of economic institutions focus on macro-outcomes, these institutions also affect firm-level characteristics important to corporate financing options. Laeven and Woodruff (2007) show that optimal firm-size depends critically upon the quality of domestic economic institutions.¹⁵ Furthermore, it is unclear that every firm actually responds to a given set of formal domestic economic institutions in the same way: this is precisely where political connections to national governments may be important.

2.2 Political Connections Can Create Value for Firms in Various Ways

In his later work, North (1990) acknowledges that political interactions may be important to how agents can further reduce uncertainty in economic exchange. This may help us reconcile the many findings that those agents who maintain special relationships with institutional stewards (e.g. politically-connected firms) appear to play by slightly different rules within a given institutional context.

Political connections are valuable to the firms that maintain them within the confines of the institutional environment in which they operate. Studies of the value of political connections to firms cover the spectrum of political personalities, ranging from dictators like Hitler (Ferguson

¹⁴ A visual representation of the data La Porta, Lopez-de-Salinas, Shleifer, and Vishny (1998) use is available in Figure A5 in the Appendix of this paper.

¹⁵ Laeven and Woodruff (2007) exploit subtle state-level heterogeneity in the quality of institutions to show that economic institutions are a factor that explains differences in average firm-size across Mexico, in one of the few studies to look at how institutions matter at a sub-national level.

and Voth 2008) and Suharto (Fisman 2001, Leuz and Oberholzer-Gee 2006) to democratically elected officials like US senators (Roberts 1990, Jayachandran 2006).¹⁶

Despite the breadth of studies, there are only a few numerical estimates of the explicit value of political connections in different countries in the literature; these tend to be measured in terms of how sensitive the market capitalization of firms are to unexpected changes in the status of political connections.¹⁷ In Indonesia connections to Suharto were worth an estimated 25% of the market capitalization of firms (Fisman 2001; Fisman and Miguel 2008). In Italy connections to Giovanni Angelli (Fiat Chairman and Member of Parliament) were worth an estimated 3.4% of market capitalization (Faccio 2006; Fisman and Miguel 2008). In the United Kingdom, connections to Sir John Moore (Rolls-Royce Chairman and Member of Parliament) were estimated to be worth approximately 0% (Faccio 2006; Fisman and Miguel 2008); similarly in the U.S., connections to former Vice President Cheney were estimated to be worth approximately 0% (Fisman, Fisman, Galef, and Khurana 2006). The results of these last two cases—that of the Minister or Parliament in the UK and of the Vice President in the US—are somewhat surprising in that they show that there is no measurable (market) value to firms being politically connected in some contexts despite the widely-held belief that all political connections are inherently valuable. Furthermore, we have no explanation on how and why the value of political connections varies from country to country in the literature; in their recent popular book, *Economic Gangsters*, Fisman and Miguel (2008) write that this is “a centuries old question.”

In an important study, Faccio (2006) collects data on the prevalence of domestic political connections in countries around the world.¹⁸ She documents the somewhat surprising result that firms with political connections to their national governments are common both in developed and developing countries, despite the popular belief that such connections are most prevalent in

¹⁶ The benefits of political connections tend to be studied within one country or a fixed institutional context which is a shortcoming of the literature on the value of political connections. The primary reason for this is that it is difficult to have a comparable definition of political connections across countries. Another reason that political connections tend to be studied within a single country context is that empirical approaches have tended to rely on natural experiments—that shock the connected individuals—for identification.

¹⁷ A visual display of the results from Fisman and Faccio’s natural experiment estimates of the value of political connections in terms of market capitalization is available in the Appendix as Figure A6.

¹⁸ A visual display of the incidence of political connections around the world from Faccio (2006) is available in the Appendix as Figure A7.

the poorest countries with the weakest economic institutions.¹⁹ Regulations against politicians maintaining ties to firms vary greatly across countries of varying institutional quality and of varying levels of development, explaining Faccio's result.²⁰ Indonesia, Italy, and the United Kingdom all fall into the top quartile of countries based on their incidence of political connections; however, the value of political connections in these three countries (in market capitalization terms) varies dramatically from 25% to 0%. A similarly surprising fact is that in a country with relatively abundant political connections (the United Kingdom), their value can be worth just as little as in a country with relative scarce connections (the United States).

Preferential access to domestic debt finance is one channel through which politically connected firms receive benefits, possibly through coercion. Studies showing firms with political ties obtain better terms on domestic finance than the population of non-connected firms include those on Brazil (Claessens, Feijen, and Laeven 2008), China (Cull and Xu 2005; Li, Meng, Wang, and Zhou 2008), Indonesia (Leuz and Oberholzer-Gee 2006), Pakistan (Khwaja and Mitton 2005), the Philippines (Hutchcroft 1998), Malaysia (Johnson and Mitton 2003), and South Korea (Kang 2002). Despite the volume of research on preferential access to domestic debt finance, little is known about whether or not foreign capital markets respond favorably to firms with domestic political connections.

There are a variety of other ways that political connections can create value for firms. Political ties can: increase the probability that a firm is the recipient of a government bailout (Faccio, Masulis, McConnell 2006); help firms secure favorable regulation or receive government contracts (Agrawal and Knoeber 2001); and, ensure that entities win government appropriations (Roberts 1990; de Figueiredo and Silverman 2006). Many authors on business ties with politicians see the connections resulting in inefficient allocations of resources that mean some firms "will benefit at the expense of others" (Kang 2002) and that "are likely to have an adverse effect on economic growth" (Faccio, Masulis, McConnell 2006). This stands in opposition to a view that financiers may rationally choose to offer better financing terms to politically connected firms.

¹⁹ Figure A8 in the Appendix illustrates in scatterplot form that there is no discernable relationship between the incidence of political connections and the quality of *property rights* institutions around the world.

²⁰ Faccio (2006) also collects data on these regulations and creates an index of regulations against political connections around the world.

3 Analytical Framework for the Political Economy of Global Financing (Cross-listing) Decisions

An integrated analysis of what the political economy determinants of firms' cross-listing decisions are should simultaneously consider the roles of both firm-level and country-level factors. Past research into this question has looked at either domestic political connections at the firm level or the quality of domestic economic institutions at the country level. Interactions between domestic economic institutions and domestic political connections have not been considered—nor has the role of domestic political connections across countries, rather than within a single environment.

Firm-level domestic political connections and country-level domestic economic institutions each have been hypothesized to play an independent role in determining whether or not a firm chooses to be cross-listed. There are, however, reasons to believe the roles these variables play may be interrelated. Tensions between domestic political connections and the institutional environment, in which both connected and unconnected firms are embedded, may be of particular importance when it comes to cross-listing.

In the three hypotheses that I present next, I explore these natural tensions. I show how they have led researchers to argue both *for* and *against* the hypotheses that domestic political connections and stronger domestic economic institutions make firms more likely to cross-list—rather than considering how they play off each other in an integrated framework. The independent roles of these firm-level and country-level political economy factors are the subject of the first two hypotheses I present; studying these relationships has been proposed as an interesting research question in the past. With my third hypothesis, I propose an idea for how we might be able to reconcile tensions in the independent roles these firm-level and country-level variables play. The manner in which I do so is consistent with both the *regulatory bonding* theory (focused on the country-level) and an updated version of the *reputational bonding* theory (focused on the firm-level)—theories which tend to be pitted against each other, rather than being seen as complementary.

3.1 The Role of Domestic Political Connections

Domestic political connectedness is a firm-level attribute believed to have some impact on a firm's decision to cross-list its securities in equity markets outside of its traditional home.

We know that there are large potential benefits to cross-listing that past research has uncovered; however, we also know that there can be substantial costs. Whether or not these benefits dominate the costs, on average, for firms with domestic political connections is purely an empirical question, since there are theoretical reasons to believe either the costs or the benefits dominate. This leads me to the first hypothesis, I will test:

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

Existing empirical tests of the role domestic political connections play in cross-listing decisions find evidence both for and against the hypothesis that cross-listings are more likely if a firm is politically connected. Hung, Wong, and Zhang (2008) find support for this hypothesis using data from China, while Leuz and Oberholzer-Gee (2006) and Siegler (2004) cast doubt on this hypothesis using data from Indonesia and Mexico. Each of these findings, while independently credible, only applies in the context of the countries studied. No one has yet run a cross-country test of the hypothesis that domestic political connections make it more likely that a firm cross-lists, allowing for domestic political connections in several different home country environments simultaneously. We might expect that politically connected firms are more likely to have cross-listed securities, because cross-listing has enormous benefits. On the other hand, we might expect firms with domestic political connections to be less likely to have cross-listed securities, because they have less need or willingness to maintain financing homes in foreign markets given the potential costs.

Hung, Wong, and Zhang (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. Of course there are other reasons to believe that politically-connected firms are more likely to cross-list. We know that political connections have a market value in firms' domestic securities markets and that domestic political connections can enable firms to take advantage of opportunities unavailable to their unconnected counterparts. Given these facts, it is reasonable to expect that foreign financial markets may also value certain non-market capabilities of firms with domestic political connections enabling them to take better advantage of cross-listing.

On the other hand, variants of the *reputational bonding* theory suggest that I will not be able to find empirical support for the hypothesis that politically-connected firms are more likely to cross-list. It is possible, despite potentially being more able to cross-list, that politically connected firms may be less willing to do so or may have less need to do so. Leuz and Oberholzer-Gee (2006) claim that domestic political connections are substitutes for foreign financing in the Indonesian case because domestic political connections provide firms with a better domestic financing alternative (in that they can use the government to fund them through back channel contracting arrangements). Siegel (2004) also argues that firms with domestic political connections have less need to cross-list because the connections serve as a *reputational bond* enabling them cheaper access to domestic credit. Using a slightly different logic Rajan and Zingales (2003) argue that firms with political power have a reduced need to tap foreign capital markets.²¹

Figure 1 illustrates the various financing options that both politically connected and unconnected firms can choose from, starting from their traditional home. It shows that both *regulatory bonding* and *reputational bonding* can co-exist for a given type of firm in a given institutional environment. It cannot answer the question of which logic—for how and where a firm chooses to finance—overshadows the other across or within all institutional contexts. This is precisely why we need an empirical test and why we need to understand the independent role of the institutional context on firms' cross-listing decisions.

<Insert Figure 1 Here>

3.2 The Role of Domestic Economic Institutions

Since domestic economic institutions define the rules of the game, it would be imprudent to consider the decisions of firms to seek out an alternative set of rules without examining the role of their traditional home's institutional environment in such decisions. Furthermore, much of the past inquiry into the determinants of cross-listing, and on the *regulatory bonding* theory in particular, has focused prominently on the role of differential accounting and disclosure requirements as a type of *contracting* institution. The role of *property rights* institutions, which are highly correlated with *contracting* institutions, however, has received little to no empirical

²¹ Rajan and Zingales' (2003) argument is that incumbent industrialists, with political power, have incentives to support policy agendas that keep domestic financial markets underdeveloped as a means of stifling competition, in turn reducing their external financing needs.

attention in determining firms' decisions to cross-list. This leads me to the second hypothesis I want to test:

Hypothesis 2: Cross-listing is more likely the higher the quality of domestic economic institutions for contracting and for property rights.

Since both the literatures on *contracting* and *property rights* suggest that higher quality economic institutions bring about superior economic outcomes, it is reasonable to expect that a firm whose traditional home has high quality economic institutions would be more able to cross-list if it is in fact a desirable economic outcome. On the other hand, some firms might not be able to cross-list because the costs of operating in a weak institutional environment are simply too burdensome.²²

Doidge, Karolyi, and Stulz (2004) explore the natural tensions in whether or not domestic *contracting* institutions influence firms' cross-listing decisions. They show that while the benefits of cross-listing are greatest for the average firm from a country with the *weakest* contracting institutions, the costs of cross-listing may also be higher.²³ Following from this, they argue that a higher proportion of firms cross-list from countries with strong *contracting* institutions given firm-level costs are likely to be lower in these countries, making cross-listing easier. Whether the costs or the benefits dominate for the average firm from a country with strong or weak *contracting* institutions remains an open empirical question across cross-listing destinations. Hence, the data could or could not support the hypothesis that stronger domestic *contracting* institutions make it more likely that a firm cross-lists. A reason to expect that I will not find support for the hypothesis with respect to *contracting* institutions is that firms do not need to cross-list to reveal greater information about themselves, but that the act of cross-listing imposes additional costs.²⁴

²² A third possibility, more consistent with *reputational bonding* rather than *regulatory bonding*, is that certain firms might choose not to cross-list because they may be relatively unaffected by operating in a weak institutional environment, reducing the benefits gained from opting out.

²³ Doidge, Karolyi and Stulz (2004) find that the benefits dominate the costs for firms who choose to cross-list, supporting the regulatory bonding hypothesis; if the regulatory bonding hypothesis holds, however, the costs should dominate the benefits for those firms who choose not to cross-list.

²⁴ Bradshaw, Bushee, and Miller (2004) show that firms who choose to comply with US Generally Accepted Accounting Practices (GAAP) benefit, from greater foreign investment in their securities, regardless of whether or not they choose cross-list securities.

The literature on cross-listings is less focused on *property rights* institutions than on *contracting* institutions. What it does conjecture supports the idea that firms whose operations are located in countries with strong *property rights* institutions are more likely to cross-list. Siegel (2005) argues that cross-listings “are far from a perfect substitute for ... preventing fraud, theft, embezzlement, and asset taking.” Likewise Ammer, Holland, Smith, and Warnock (2006) suggest cross-listing cannot substitute for protections from expropriation in the traditional home country. Stulz (2005) argues that weak *property rights* institutions limit all forms of financial globalization because “financial globalization reduces the state’s [and other’s] ability to expropriate.” All of these statements suggest that I should find support for the hypothesis that firms whose domestic *property rights* institutions are stronger should be more likely to cross-list. Furthermore, foreign financiers operating in major capital markets may not have a strong appetite for investing in firms from countries with weak *property rights* institutions. The relative unpredictability of cash flows generated in those environments may scare away money-center investors. Domestic financiers may have a comparative advantage in understanding the exact risks the weak *property rights* institutions pose for particular firms, making domestic financiers more willing to extend capital to firms in those environments on more favorable terms than foreign financiers could.

Figure 2 below provides a visual representation of Hypothesis 2, showing that firms located in countries with the highest quality domestic institutions are most likely to cross-list, since on average the costs are lower, even though the benefits (while positive) are also lower. It is largely consistent with the results from empirical tests of the *regulatory bonding* theory run across countries, particularly those in Doidge, Karolyi and Stulz (2004) who also provide a formal, theoretical/mathematical model of the *regulatory bonding* theory.

<Insert Figure 2 Here>

3.3 Domestic Political Connections in the Context of Economic Institutions

Now that we have a framework for how domestic political connections and domestic economic institutions might independently affect firms’ cross-listing choices, we can begin to consider how the two factors might be interrelated. This is important since all firms, those with and without connections, are embedded in the “rules” that domestic institutions dictate.

Some firms are (politically) connected to the institutional stewards who both set and enforce the rules, while other firms are not. Depending upon the institutional context, the rules may be more, or less, manipulable to create superior implicit institutional environments in which connected firms can operate. It is reasonable to expect that the institutional stewards (or governments) in the weakest institutional environments have the greatest ability to manipulate the implicit rules. In countries with lower quality economic institutions the shadowy background to the institutional environment—where back channel contracts can be written and where property rights can be selectively enforced—becomes larger such that there can be substantial variability in the implicit domestic environment. Domestic political connections can functionally enable firms to operate as if they were in a much stronger institutional environment because their political ties can protect them from a weak institutional environment.

A key difference between the *contracting* and *property rights* institutions is where they can be enforced. *Property rights* institutions can only be enforced in the domestic environment, as proponents of the *reputational bonding* theory argue; however, *contracting* institutions can be enforced either in the domestic environment or in a foreign environment, as proponents of the *regulatory bonding* theory argue. The difference in where *property rights* and *contracting* institutions can be enforced should help explain the difference in the ability of firms with domestic political connections to cross-list securities. While both types of institutions are malleable in ways that could favor firms with domestic political connections, domestic *contracting* institutions should matter less than domestic *property rights* institutions for financing choices.²⁵ All firms can improve their *contracting* environment by cross-listing; however, only the few firms privileged enough to be politically connected can improve their domestic *property rights* environment. This leads me to the third, and final, hypothesis I would like to test:

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

I should expect to find support for this hypothesis if political connections create a superior implicit domestic *property rights* environment that is important to foreign financiers.

²⁵ This is consistent with Acemoglu and Johnson (2005) who argue that *property rights* institutions matter more than *contracting* institutions for most economic outcomes in their article on “Unbundling Institutions.”

This is reasonable if we expect domestic governments to protect firms with which they have strong political connections from the ills of a weak *property rights* environment better than they protect firms with which they have weaker connections (or no connections at all). Implicit domestic *property rights* protections should help reduce uncertainty in politically-connected firms' domestic cash flows, which in turn should make them more attractive to both domestic and foreign investors. Foreign investors in particular may have a difficult time assessing the particular risks a weak domestic *property rights* environment poses to a specific firm they are considering financing unless it maintains implicit protects from the ills of that environment.

If in addition to protecting firms with domestic political connections from the ills of weak *property rights*, governments also coerce domestic banks into giving the connected firms financing terms at below market rates, this would pose a challenge to Hypothesis 3.²⁶ The reason domestic banks extend favorable debt financing terms to politically-connected firms, however, may have little to do with government coercion. The favorable terms a politically-connected firm receives on domestic debt may actually be a rational market response by financiers to the explicit signal (the political connection provides) that the firm receives implicit *property rights* protections. This logic would remain consistent with Hypothesis 3.

Figure 3 illustrates the complexity of Hypothesis 3 by combining all three of the financing possibilities shown in Figure 1 with the various institutional environments shown in Figure 2. The weaker the formal domestic institutional environment is, the larger the shadow background is that represents the informal domestic financing arrangements potentially available to firms with domestic political connections. The thickness of the arrows originating from the different home country environments in Figure 3 represents the intensity with which I expect the domestically connected firms to use different financing channels. Whether or not thicker arrows, representing more intensive financing choices, land in the back-channels (representing informal below market rate domestic financing) or in alternative financial homes (representing cross-listings) remains an open empirical question. Hypothesis 3 predicts that the arrows should be thicker to the institutional environments where politically connected firms choose to cross-list.

<Insert Figure 3 Here>

²⁶ Foreign investors may be less interested in firms with political connections used to access below-market financing terms. Those investors would have to be compensated for the additional risk this poses. As a result of this type of market manipulation, the foreign investors would then have to charge above-market financing terms to be compensated for the existence of back channel financial contracting (to restore the equilibrium financing terms).

4 Building a Multi-level, Cross-Sectional Dataset

To test the three hypotheses presented above (and to achieve this paper's purpose of investigating the role domestic firm-level political connections and domestic country-level economic 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. The firms included in my sample are congruent with those in Faccio's (2006) work on political connections as well as those in Doidge, Karolyi, and Stulz's (2004) work on the value of cross-listing. Both use the Worldscope database, which covers 96% of the world's market capitalization, as the foundation for their analysis; I use the Datastream/Worldscope database.²⁷ Once I exclude firms from the sample that fall outside of Faccio's (2006) and Doidge, Karolyi, and Stulz's (2004) core samples, I am left with 12,395 firms.²⁸

While there may be some concern that there is no time-dimension to my dataset, a cross-sectional setting is well suited for testing the hypotheses presented in the prior sections because there is little meaningful time variation in the key independent (political economy) variables. Acemoglu, Johnson, and Robinson (2001) assert that country-level economic institutions are highly persistent. Likewise, political connections demonstrate very little year-to-year variation and when they do it is often the result of a surprise event such as an unexpected election outcome (Faccio 2006) or health shocks to political leadership (Fisman 2001; Fisman, Fisman, Galef, and Khurana 2006). Furthermore, no data on how domestic firm-level political connections vary over time is available for a large cross-section of countries, precluding the use of such a dataset.

²⁷ Datastream/Worldscope fully incorporates all information that was in the original Worldscope database, but merges into it other information on the same firms found in the original Datastream database. If there is a limitation to the dataset, it is that it does not include firms without public securities listings.

²⁸ Doidge, Karolyi, and Stulz (2004) exclude firms in the United States from their core sample since they are looking specifically at the value of cross-listing in the United States. Faccio (2006) excludes firms for which data on the top managers and large shareholders was unavailable on the compact disc version of Worldscope at the time she conducted her analysis. Mara Faccio gave me the Worldscope ID of all the firms in her core sample on which she investigated the existence of political connections since that information is not publicly available (although the names of all politically connected firms and the number in a given country are).

4.1 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 traditional home. It is a dummy variable that will be used in binary choice regressions:²⁹

$$y_i = \begin{cases} 1, & \text{if firm } i \text{ issues securities on any market outside its traditional home} \\ 0, & \text{if firm } i \text{ issues securities in its traditional home, but nowhere else} \end{cases}$$

My definition of cross-listing is based on the one used in Doidge, Karolyi, and Stulz (2004), but expands on it by incorporating firms that cross-list on any capital market around the world rather than focusing exclusively on U.S. cross-listings.³⁰ As in Doidge, Karolyi, and Stulz (2005), I use the Bank of New York's American Depository Receipt dataset and JP Morgan's ADR.com dataset to check if a firm is cross-listed in the U.S.³¹ For cross-listings in destinations other than the U.S. (and to check the accuracy of cross-listings in the U.S.), I use data contained in Datastream/Worldscope; it provides information on every other market on which a firm lists securities outside its traditional home. Furthermore, I only set $y_i = 1$ if a firm actively maintains a cross-listing as of January 2003, to match the timing of the political connections data I am about to introduce. Note that when $y_i = 0$, a firm may finance itself through regular domestic channels or through back-channels arrangements resulting from government coercion of banks as either outcome is observationally equivalent in the data.³²

²⁹ The use of this binary indicator for cross-listing means it is observationally equivalent whether a firm uses political connections to obtain back-channel financing at below-market rates or whether it approaches domestic financial markets through traditional channels. Since the use of back-channel finance is inherently unobservable, I can only test whether a firm cross-lists or not—not whether a firm cross-lists, uses government coercion to obtain back-channel loans, or uses traditional financing in its domestic market.

³⁰ Pagano, Roell, and Zechner (2002) and Sarkissian and Schill (2004) both suggest that it is important to consider cross-listings in countries other than the United States, despite much of the academic literature focusing on the American Depository Receipts given the analytic attractiveness of data on U.S. cross-listings. Furthermore, including cross-listings other than those in the U.S. is consistent with Leuz and Oberholzer-Gee's (2006) study of the relationship between political connections and foreign financing in Indonesia.

³¹ I am able to match these datasets to my core sample in Datastream/Worldscope using International Securities Identification Numbers (ISINs) as a unique identifier.

³² While possible, it is unlikely that when $y_i = 1$ a firm may also finance itself through back channels resulting from 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.

4.2 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.³³ The dummy variable, $d_connect_i$, takes on a value of one if a firm is politically connected and a value of zero if a firm is not connected in the year 2002.³⁴

The measure of political connections in the Faccio dataset is a strict and objective one since it is constructed from directly observable criteria for all market participants. A firm is only defined as politically connected 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, Masulis, McConnell 2006). Another advantage of this strict definition of political connections is that they are not types 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.³⁵

³³ The key advantage of the Faccio dataset is that it spans across countries. It is very difficult to gather data on political connections across countries, making studies of them rare in the literature. One of the problems with collecting data on political connections across countries is that often in the literature the definition of just what constitutes a connection changes depending upon context. Sometimes subjective assessments of the strength of connectedness is used as a measure; for example Fisman (2001) used The Castle Group’s Suharto Dependency Index. In other instances, firm’s monetary support of politicians through campaign contributions or lobbying dollars are used as a measure of political connections; for example, see Claessens, Feijen, and Laeven (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; in addition to Faccio (2006), see also Ferguson and Voth (2008).

³⁴ There are two key limitations of the Faccio dataset. 1) Since it is only accurate in 2002, it limits me to being able to perform only cross-sectional analyses since there is no explicit time-variation in the measure of political connections. Another concern about timing may be that the Sarbanes-Oxley legislation went into effect July 2002 which caused many firms to de-list in the US. This should not, however, be a major concern because many firms are cross-listed in multiple markets or if they did de-list in the US, they often re-listed on a European exchange with less stringent reporting requirements than in the US, but with more stringent reporting requirements than in their traditional homes. 2) Faccio’s measure of political connections is strictly a binary indicator; hence another limitation of her dataset is that it contains no informational content about the intensity of political connections like that used by some other researchers. It is, however, a fairly safe assumption that the connections she captures are about as intense as possible given how rare they are and how stringent her criteria for a connection is.

³⁵ These properties of the type of political connection on which I have data should relieve some concerns about political connections being endogenous to economic outcomes. If politicians are perceived to be abusing their office for personal gain they are likely to be held accountable (and kicked out of office either by election in democratic systems or forcibly in authoritarian ones.) Nevertheless, I will dedicate a portion of the paper following the empirical analysis to concerns about the potential for the endogeneity of political connections to my outcome

Country-level Economic Institutions

The data used for my key country-level independent variable, the quality of economic institutions, is consistent with that used in Acemoglu, Johnson, and Robinson (2000) and La Porta, Lopez-de-Salinas, Shleifer, and Vishny (1998). The key difference with their measures of *property rights* and *contracting* institutions and mine is that I re-scale the underlying variables: this makes the values more readily interpretable and makes the coefficients on them from the regressions I will run larger, so we don't have to look at the n -th decimal place.

The variable $PropInst_k$ will represent *property rights* institutions throughout my empirical analyses. Higher values of this measure indicate weaker *property rights*. It is based on two different popular measures used commonly in that literature: The International Risk Services Groups' International Country Risk Guid (ICRG) Expropriation Risk Index and the Freedom House Property Rights Index.³⁶ Both were used by Acemoglu and Johnson (2005) in their article unbundling institutions. Rather than using each separately, I rescale and combine the data using a principal components technique to project the Freedom House measure onto the ICRG measure, creating in essence a weighted average that should maximize the underlying signal in each data series.³⁷

The variable $ContractInst_k$ will represent *contracting* institutions throughout my empirical analyses. Higher values of this measure indicate stronger *contracting*. The data I will use was coded originally by the Center for International Financial Analysis & Research for their International Accounting and Auditing Trends report. It was featured prominently as a measure of *contracting* institutions, in La Porta, Lopez-de Silanes, Shleifer, and Vishny's (1998) seminal

variable: observed cross-listing choices of firms (i.e. observed behavior in competitive global financial markets, rather than in more readily manipulable and less competitive domestic financial markets).

³⁶ I take the *Exprop_ICRG* data from La Porta, Lopez-de Silanes, Shleifer, and Vishny (1998) since the data is posted on La Porta's website. They define it as "ICR's assessment of the risk of 'outright confiscation' or 'forced nationalization'. Average of the months of April and October of the monthly index between 1982 and 1995. Scale from 0 to 10, with lower scores for higher risks." I also take the *PropRights_FH* data from La Porta, Lopez-de-Silanes and Shleifer (2002) since the data is also posted on La Porta's website. They define it as "an index of property rights in each country (on a scale from 1 to 5). The more protection private property receives, the higher the score. 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, the probability that the government will expropriate private property, and the country's legal protection to private property."

³⁷ 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. I then use principal components which suggests I created a weighted average such that my final measure is 86% the ICRG measure and 14% the Freedom House measure. I get similar regression results in my analysis, however, if I use either rescale measure separately or an equally weighted average.

work on law and finance. The measure also features prominently in those authors' follow-on research on the subject and in tests of the *regulatory bonding* theory.³⁸ The original data is coded on a scale of 0 to 90 depending upon the inclusion or omission of 90 items that could appear on accounting statements within a given country; on that scale a score of 90 would represent perfect accounting disclosure while a score of zero would represent no disclosures whatsoever.³⁹ I rescale the data so that a value of zero corresponds to accounting disclosures in the United States and so that positive value indicate higher quality disclosures, while negative values correspond to lower quality disclosures.⁴⁰

4.3 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—are those in Leuz and Oberholzer-Gee (2006), who include these four variables as their only controls in their core regressions (on whether or not political connections to Suharto in Indonesia affect the probability of a given firm cross-listing securities in a market outside of its home-country). I use their definitions.⁴¹ Doidge, Karolyi, Lins, Miller, and Stulz (2009) also use the four variables—size, leverage, and return on assets—as independent variables in a regression on the determinants of cross-listing.

Industry-level Indicators

Indicators of which industries, broadly-defined, a particular firm belongs to come from Datastream/Worldscope. The industries included are Basic Materials, Consumer Goods, Consumer Services, Financials, Health Care, Industrials, Oil & Gas, Technology, Telecommunications, and Utilities. I use these indicators to create dummy variables for each

³⁸ Unfortunately the data is available for only 37 of the 46 countries in my sample, so econometric tests that require it will be run on a slightly smaller sample than those that do not.

³⁹ The country that scores the highest in the data is Sweden at 83. The United States as a benchmark for best, generally accepted practices comes in at 71. The country that scores the lowest is Egypt at 24, followed up by Uruguay at 31.

⁴⁰ I take the value of each, divide it by the level in the United States, and subtract 1.

⁴¹ 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. These definitions are the same as those used by Leuz and Oberholzer-Gee (2006) and Doidge, Karolyi, Lins, Miller, and Stulz (2009).

industry: d_j . I also use these indicators to exclude firms in the financial industry from the sample on which I run regressions as in Doidge, Karolyi, and Stulz's (2004) work on cross-listings and the majority of work on firm's corporate financing activities, given the different financing patterns/needs of such firms.

Country-level Indicators and Other Country-level Variables

Indicators of which country is a particular firm's traditional home also come from Datastream/Worldscope. These were also used by Faccio (2006) in constructing her dataset of politically connected firms around the world. I use these to create dummy variables for each firm's traditional home country: d_k . In regressions, where I am not trying to test the direct effects of country-level factors on firm's cross-listing decisions, I will use these.

To round out my analysis, I also gather additional country-level data and construct country-level variables that aggregate information contained in firm-level variables already discussed. These include the level of development (as measured by the natural logarithm of GDP) and country-level incidences of both political connections and cross-listing. I construct the country-level incidences of political connections and of cross-listing 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.

4.4 Summary Statistics

Table 1 provides a country-level correlation matrix. There are several things in it to notice. First, the correlation coefficient between the incidence of political connections and the incidence of cross-listings (-0.093) is negative and low at a country-level, suggesting little if any country-level support for Hypothesis 1 that political connections make it more likely a firm will cross-list its securities. This, however, does not preclude firm-level support for Hypothesis 1, which is what we are really interested in testing. Second, the correlation coefficient between the incidence of political connections and the quality of economic institutions (0.029) is relatively low, as should be expected given Faccio's (2006) finding that political connections are equally abundant in both developed and developing countries. Third and finally, measures of both types of economic institutions—*property rights* institutions and *contracting* institutions—are highly correlated (0.551), as should be expected given the difficulty in empirically unbundling institutions as done by Acemoglu and Johnson (2005).

<Insert Table 1 Here>

Table 2 slices the data a different way, displaying categorical means at a country rather than firm-level, and provides preliminary support for all three hypotheses at a country level. That the values in the third row (displaying the difference in property rights the country-level incidence of cross-listing for firms of a given political status) are positive supports the first hypothesis that firm are more likely to cross-list if they are politically connected. That the values in the first and second rows are generally decreasing supports the second hypothesis that fewer firms cross-list from countries with weaker property rights institutions. That the values in the fourth row are generally increasing supports the hypothesis that politically connected firms are more likely to cross-list than unconnected firms the weaker property rights institutions are.

<Insert Table 2 Here>

Additional summary statistics and views of the data appear in this paper's Appendix.

5 Empirical Tests of the Political Economy Determinants of Cross-listing Hypotheses

In this section of the paper, 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, just described, to binary choice regressions predicting whether or not a firm cross-lists its securities, using alternative explanatory factors (namely firm size) and other corporate financing characteristics of firms as control variables.

Ideally, I could accurately estimate a single fully-specified regression model that could be used to simultaneously test all three of my hypotheses. Such a model would take the form:

$$Pr[y_i = 1] = \Lambda(\beta \cdot [Variables\ of\ Interest] + \gamma_i X_i + \gamma_k Y_k + \gamma_j Z_j + \varepsilon_{ijk})$$

where *Variables of Interest* represents the political economy variables of interest; X_i represents firm-level control variables; Y_k represents the country-level control variables; and Z_j represents industry-level control variables.

This form for the estimation is logical because I am trying to estimate the determinants of a discrete choice outcome in a multi-level setting. Using multinomial logit as the type of binary choice regression makes sense because it is the most widely-used discrete choice method (Train

2003) and because it is used by Doidge, Karolyi, Lins, Miller, and Stulz (2009) in their study of the determinants of cross-listing.⁴² Furthermore, this form employs the maximum amount of information content in the data from the prior section.

Attempting to estimate a fully-specified version of the statistical model could be problematic, however, because 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. For cases where I am interested in estimating coefficients on firm-level determinants of cross-listing behavior or interaction between firm-level determinants and country-level factors, I will estimate regressions of the following form:

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

where *Variable of Interest* represents a different political economy variable depending upon the hypothesis being tested; X_i represents the firm-level control variables; d_j , industry dummies; and d_k , country dummies.

The advantage of this model, over the fully-specified one, is that in doing so 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. This eliminates any concerns country- and industry- omitted variable bias or about mis-specifying the model by including incorrect country- or firm-level variables. While my estimate of β will remain consistent in estimating this model, the dis-advantage vis-à-vis a fully specified alternative model (as suggested above) is that my coefficient estimates will lose econometric efficiency, making it less likely that I find statistically significant values.

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 whether or not a firm will choose to cross-list; the reasons for this are varied: larger firms can better bear direct costs of cross-listing, larger firms tend to be better appreciated by foreign investors, and larger

⁴² I find similar results if the density function inverted as part of the regression procedure were either Gaussian (as in probit) or Extreme Value (as in gompit). Likewise, I find similar results using a linear probability model.

firms are more likely to hit limits in domestic capital markets. Return on assets helps control for firm-profitability to ensure that the results do not simply capture firms that are more profitable/successful being more likely to cross-list. 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.⁴³ In principle we may want to consider the inclusion of other firm level controls; however, the combination of corporate financing characteristics I will include should be sufficient.⁴⁴

By including industry and country dummy variables, we eliminate any concerns about industry- and country- omitted variable bias; however, we do lose some efficiency in our estimates of β , making it less likely we find statistically significant values of the coefficient. By including industry dummies, I am effectively holding the average of firm-level characteristics for a particular industry constant, so the firm-level variables really capture variation from industry averages rather than their effects across all industries. In cases, where I include country dummies, I am effectively holding all possible country-level factors fixed. This is important because it assuages any concerns about alternative country-level variables that may explain why a firm would choose to cross-list.

In cases where I test Hypothesis 2 about the role of country-level economic institutions in firms' cross-listing decisions, I can no longer include the country dummy variable (d_k), since its inclusion effectively holds all country-level factors, including economic institutions, constant

⁴³ While some argue that political connections make firms more highly leveraged (Faccio, Masulis, McConnell 2006), others find that firms with stronger political ties are less likely to be highly leveraged (Myers 2007).

⁴⁴ For example, Doidge, Karolyi, Lins, Miller, and Stulz (DKLMS) (2009), also include as 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 DKLMS 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 extreme version of political connectedness than the measure I use from Faccio (2006) captures. Leuz and Oberholzer-Gee (2006) use a measure of firm 'age' in some of their robustness checks. They construct this variable based on original research into the incorporation dates of the 130 Indonesian firms in their sample; however, this is infeasible to do for the 12,395 firms in my sample.

across countries. A longer discussion of issues I need to be concerned about in this case is included with my test of Hypothesis 2.

5.1 The Role of Domestic Political Connections

The first of several relationships I want to test is if and how domestic political connections influence the observed propensity of a firm to cross-list securities in an equity market outside of its traditional home country (Hypothesis 1). To do so, I will estimate variations of the following regression, including and omitting different combinations of industry and country dummy variables:

$$Pr[y_i = 1] = \Lambda \left(\beta \cdot d_{connect}_i + \gamma_i X_i + \sum_{\forall j} \gamma_j d_j + \sum_{\forall k} \gamma_k d_k + \varepsilon_{ijk} \right)$$

where $d_{connect}_i$ represents a dummy variable for politically connected firms using Faccio's (2006) definition and data; X_i represents the firm-level control variables; d_j , industry dummies; and d_k , country dummies. In these regressions, I will treat a firm having a political connection (of the sort identified by Faccio) as being exogenous to whether it cross-lists, an assumption which I believe to be both reasonable and intuitive, but one that I will return to in a discussion of the robustness of my econometric results nevertheless.

If the coefficient (β) 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 serious doubts on Hypothesis 1. If the coefficient is not significant, then political connections play an indeterminate role in whether or not a firm has cross-listed securities.

The results of this estimation appear in Table3 and provide qualified support for the hypothesis that firms with political connections are more likely than those without them to have cross-listed securities. In all cases, the results in Table 3 indicate a positive coefficient of interest (β) when the regressions are run on the full sample of countries for which data is available.

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 for the sample of countries included.⁴⁵ This supports Hypothesis 1 strongly. For the cases where I include country dummy variables as controls, in columns 3 and 4, and run the test *within* rather than *across* countries; however, the statistical significance of the results falls just below the 10% level but remains above the 15% level.⁴⁶

<Insert Table 3 Here>

The average value of domestic political connections *across* all countries in my sample may be an uninteresting number if the value of them depends upon the institutional environment of the home country. Nonetheless, the marginal effect of going from unconnected to connected, for the average firm, across all countries based on the regressions shown in the first two columns of Table 3 is approximately 0.15.⁴⁷ This means that a 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.⁴⁸ Furthermore, this magnitude of this estimate and its statistical significance may be understated to the extent that some political connections are unobserved or missing in the dataset I use (such as those gained through affiliations with business groups such as Korean *chaebol* or Japanese *keiretsu*).

Column 5 of Table 3 which has no firm-level, industry-level, or country-level controls is included as a robustness check. It can be compared to Column 1 of Table 3 which has no

⁴⁵ While the regression in column 2 of Table 2 is of the exact same form as Leuz and Oberholzer-Gee's (2006) baseline regression, I find the opposite result when expanding the sample of firms beyond Indonesia. My finding could be reconciled with Leuz and Oberholzer-Gee's (2006) finding if the role of political connections depends critically on country-specific factors in Indonesia including the economic institutions firms face there. Furthermore, my data does not suggest that Leuz and Oberholzer-Gee's (2006) finding is invalid, since I find a negative coefficient on my measure of political connections, just as they do, when I restrict the sample of firms to those whose traditional home is in Indonesia; in my regression, while this coefficient is negative, it is not statistically significant. The one difference between my regression and Leuz and Oberholzer-Gee's (2006) baseline regression (other than expanding the sample of countries involved) is that Leuz and Oberholzer-Gee's measure of political connectedness attempts to incorporate the strength of political ties a firm has, while my measure from Faccio (2006) is only a dummy variable that captures whether or not a firm is politically connected. This difference may explain why their result is statistically significant and mine is not. A direct replication of Leuz and Oberholzer-Gee's (2006) baseline regression using my dataset appear in a table in the Appendix.

⁴⁶ If I run Gompit regressions (inverting an Extreme Value distribution) rather than logit regressions (inverting a Logistic distribution) then the *across* country results are statistically significant at the 10% level. 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 is not symmetric and more likely to fit the data since closer to thirty percent of firms actually choose to cross-list.

⁴⁷ This marginal effect was calculated simply by dividing the logit coefficient by 4 which is a simple rule of thumb for evaluating marginal effects around the mean of other variables in logit regressions suggested in the graduate econometrics texts written by both Wooldridge and Greene. The approximation was originally derived by Amemiya.

⁴⁸ Since these estimates come from regressions run *across* countries, these estimates do not control for institutions, the level of development, or any other country-level factors.

industry-level or country-level controls to check for the magnitude of potential biases caused by omitted, firm-level variables that are either unobservable or on which reliable data does not exist. As in Bertrand, Luttmer, and Mullainathan (2000), we can surmise that unobservable firm-level characteristics are unlikely to be driving the results since the coefficient of the politically connected firm variable (β) hardly changes (between Column 1 and Column 5) when we increase the effective set of firm-level unobservable variables by dropping variables that are indeed observable.

As another sort of robustness check, I have tried altering the measure of firm size included in the regression to be something other than the natural log of a firm's total assets. These results are shown in a table in the Appendix. When I use either total assets or total assets squared as a measure of firm size—both of which would emphasize the larger firms even more than just the log of total assets—my results become stronger. This finding is inconsistent with a skeptical interpretation of my results suggesting that the only reason that I find a statistically significant relationship between my firm-level political connections variable and cross-listing decisions is because only larger firms are politically connected. Furthermore, as can be seen in boxplots in the Appendix depicting categorical firm level summary statistics (broken out for politically connected and unconnected firms), there are no major differences in the size distribution of connected firms versus unconnected firms across countries.

There are at least three possible explanations for why the *within* country tests of political connections effects on cross-listing are statistically insignificant at the 10% level: 1) it could be the result of an econometric efficiency loss from estimating a model with country dummies rather than country attributes; 2) domestic political connections may indeed have no independent effect on firms' propensities to cross-list *within* countries once we control for economic institutions, so they may only have an effect *across* countries; or 3) domestic political connections have different effects on firms' propensities to cross-list depending upon the different national (institutional) environments in which they exist, as Hypothesis 3 suggests.⁴⁹

Given the ambiguous finding for the *within* country tests of the role of political connections on cross-listing decisions of firms (shown in Columns 3 and 4 of Table 3), it may be

⁴⁹ A fourth reason, I might be getting an inconclusive result is that I am treating the intensity of all political connections as being equally strong when the reality is that all connections even using Faccio's strict definition probably are not created equal. Unfortunately, nothing can be done about this since Faccio's (2006) data is the best available on political connections across countries.

important to consider how political connections work in different institutional contexts (as in Hypothesis 3). Before we can do that effectively, however, it is important understand how the institutional environment independent of firms’ political status correlates with firm-level cross-listing decisions.

5.2 The Role of Economic Institutions

Hypothesis 2 (that firms are more likely to cross-list if they operate in higher quality domestic economic institutional environments) is in many ways a country-level prediction, so as a first pass, I examine the relationship between domestic economic institutions and the country-level incidence of cross-listings variable. I run country-level bivariate regressions of my measure of economic institutions against the incidence of cross-listing. The results, shown in Figure 4 for *property rights* institutions and Figure 5 for *contracting* institutions, confirm the country-level correlations predicted by Hypothesis 2, consistent with the *regulatory bonding* theory. I also run country-level multivariate regressions that include both varieties of domestic economic institutions and obtain the same signs on the coefficients for each.⁵⁰

<Insert Figure 4 Here>

<Insert Figure 5 Here>

To test the relationship between the economic institutions in firms’ traditional home country and firms’ propensity to cross-list securities in equity markets in another country (rather than just the country-level incidences of cross-listing), I will run regressions of the general form:

$$Pr[y_i = 1] = \Lambda \left(\beta \cdot Y_k + \gamma_i X_i + \sum_{\forall j} \gamma_j d_j + \varepsilon_{ijk} \right)$$

where Y_k is represents either my measure of *property rights* institutions ($PropInst_k$) or my measure of *contracting* institutions ($ContractInst_k$); X_i represents the firm-level control variables; and, d_j , industry dummies.⁵¹

⁵⁰ The results from these regressions where I include both *contracting* and *property rights* institutions (available upon request) are not statistically significant because there is a small number of observations (as there are only 37 countries with sufficient data). The signs on the coefficients remain as predicted by Hypothesis 2, however, when I estimate these regression including just the domestic economic institutions variables or when I also include country averages of the firm-level controls—size, return on assets, leverage, and capital intensity.

⁵¹ It is important to test this relationship at a firm-level as well as at a country-level to ensure that the country-level variation is not an artifact of underlying firm-level characteristics that depend upon the institutional environment.

Since the underlying measure of *property rights* institutions ($PropInst_k$) takes on higher values when they are weaker, a negative and significant coefficient (β) on the variable supports Hypothesis 2 (indicating that higher quality domestic *property rights* institutions make firms more likely to cross-list), while a positive and significant coefficient (β) on the variable for *contracting* institutions ($ContractInst_k$) supports Hypothesis 2 (indicating that firms with higher quality domestic *contracting* institutions are more likely to cross-list). Of course, insignificant coefficients or those with signs other than those predicted would cast doubts on the hypothesis.

Results of my firm-level tests of Hypothesis 2 appear in Table 4. Each broadly supports Hypothesis 2, showing that firms are more likely to cross-list if they have higher quality domestic economic institutions. This should not be surprising given the aggregated country-level regressions just shown and given that nearly every test of the *regulatory bonding* theory run on cross-country data finds the same results (despite a few tests within single country contexts that raise doubts and support the *reputational bonding* theory instead).

This setting poses a number of empirical challenges because I can no longer include the country dummy-variable (d_k) to hold constant all of the country-level factors that influence whether or not a firm cross-lists, since I am interested in testing the role of a specific country-level political economy variable in firms' cross-listing decisions. If the country-level factor of interest does not sufficiently capture the roles of all country-level factors, as country dummies would, these regressions may suffer from a country-level omitted variable bias. It is empirically challenging, however, to meaningfully control for other country-level factors that we might expect to influence firms' cross-listing decisions, such as the level of development, since these variables are known to be highly correlated with economic institutions (as shown in Table 1). Even if my regressions were to suffer from omitted variables bias, I should still be picking up meaningful partial correlations between country-level economic institutions and firms' cross-listing decisions—correlations that will be useful in evaluating Hypothesis 3, the role of political connections in different contexts (which is the primary point of this exercise and in discussing Hypothesis 2). Furthermore, multicollinearity issues would be a concern if we were interested in

testing the roles of both *property rights* institutions and *contracting* institutions simultaneously, since they are highly correlated (as shown in Table 1).⁵²

Given the challenge multicollinearity poses, in Table 4 I run tests for the sign of the coefficient on each of the institutions variables separately. This is a common solution to multicollinearity problems.⁵³ 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 as expected based on Hypothesis 2.

<Insert Table 4 Here>

Attempting to ‘Unpack’ the Country Dummy Variables

It would be desirable to know what country-level factors other than *contracting* and *property rights* institutions are absorbed by the country dummies included in the multi-level logit regressions I am estimating. This way I could also estimate a fully-specified regression model and test all three of my hypotheses simultaneously.

Unfortunately, when trying to determine which country-level variables are the determinants of cross-listing the analysis suffers from the same type of problems as in attempting to estimate cross-country growth regressions. That is 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 I only have observations for 46 countries in the best case). As a result both the signs and significance levels change depending upon the various combinations of conditioning variables included in the specification search. Consequently, these variables have difficulty standing up to the scrutiny of an extreme bounds analysis as suggested by Leamer (1985) or Sala-i-Matin (1997).

Possibilities, I have explored nonetheless include: domestic financial development as captured by legal origin as in La Porta, Lopez-de-Salinas, Shleifer, and Vishny (1998)⁵⁴; domestic cultures of rule breaking, as measured by unpaid diplomatic parking tickets as in

⁵² Multicollinearity makes it difficult to find individually statistically significant results if we were to test the roles of both *property rights* institutions and *contracting* institutions simultaneously.

⁵³ See Woolridge’s graduate-level econometrics text.

⁵⁴ I also tried using other measures of financial development such as stock-market capitalization to GDP and the fraction of government owned banks; however, these results are not shown because they were even more fragile.

Fisman and Miguel (2008); domestic social values such as trust, importance of religion, and importance of politics from the World Values Surveys; and the fraction of the domestic population belonging to certain religions. Firm-level regressions showing these results appear in the Appendix.⁵⁵

5.3 Domestic Political Connections interacted with Economic Institutions

Now that we have a basis for understanding the independent roles domestic political connections and domestic economic institutions play in firms' cross-listing choices, we can explore how their roles interact. To test the relationship between domestic firm-level political connections conditioned by the economic institutions of the firms' traditional home country and how they jointly affect cross-listing choices (Hypothesis 3), I will first run regressions of the following form:

$$Pr[y_i = 1] = \Lambda \left(\beta \cdot d_connect_i \cdot Y_k + \gamma_i X_i + \sum_{\forall j} \gamma_j d_j + \sum_{\forall k} \gamma_k d_k + \varepsilon_{ijk} \right)$$

where $d_connect_i$ represents a dummy variable for politically connected firms using Faccio's (2006) definition and data; 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 control variables; d_j , industry dummies; and d_k , country dummies. Again I will treat a firm having a political connection (of the sort identified by Faccio) as being exogenous to whether it cross-lists; as mentioned when I did this in my test of Hypothesis 1, I include a discussion of why this is reasonable in a discussion of the robustness of my econometric results. I also will treat the existence of political connections as being independent from the institutional environment.⁵⁶

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 (β) to be

⁵⁵ It should be noted that in univariate county-level regressions, rather than firm-level regressions shown, these variables appear to have even less explanatory power and are often not significant by themselves as are the institutions variables.

⁵⁶ Treating the existence of political connections as being independent from the economic institutional environment is quite reasonable. Country-level correlation tables reveal that the incidence of political connections in a given country is relatively uncorrelated with that country's economic institutions. The correlation coefficient for the incidence of political connections and *property rights* institutions is a meager 0.029 as show in Table 1. Furthermore the few numerical estimates other researchers have arrived at for the value of political connections (in terms of the fraction of firms market capitalization, estimated using natural experiment approaches) appear to be uncorrelated with the incidence of political connections in a given country, as mentioned in the introductory section of this paper on political connections.

positive and significant on the interaction between the political connections dummy variable and my measure of property rights institutions (i.e. the coefficient on $d_connect_i \cdot Y_k$ when $Y_k = PropInst_k$ should be positive). The results of this regression appear in Table 5, column 1. The same regression, including an independent effect for political connections, (which Hypothesis 3 predicts to be zero) appears in Table 5, column 2.

I can also use the same general framework to test several variations of potential alternative hypotheses that involve the interaction between firm-level political connections and alternate country-level factors ($d_connect_i \cdot Y_k$) that could explain whether or not firms choose to cross-list their securities. Column 3, of Table 5, shows a test of whether an interaction between firm-level political connections and *contracting* institutions has an effect on cross-listing. Column 4, of Table 5, shows a test of whether an interaction between political connections and the level of development (measured as the natural logarithm of per-capita GDP) has an effect on cross-listing. Both of these alternative country-level factors (Y_k) are highly correlated with *contracting* 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 within a given home-country environment.

The results displayed in column 1 of Table 5 appear to confirm Hypothesis 3, as it suggests that politically connected firms located in countries with weak formal *property rights* institutions are indeed more likely to cross-list.⁵⁷ We can also calculate partial marginal effects from this regression; those suggest that 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.⁵⁸ As suggested

⁵⁷ Alternative measures of *property rights* institutions such as Henisz' Political Constraint Index or the World Bank's Database of Political Institution's measure of Checks on political power yield similar results.

⁵⁸ If we used instead, the results in column 4 of Table 4 our marginal effects might be larger, so these numbers may be understated. There may also be concerns about the interpretation of interaction effects in a non-linear model, since Ai and Norton (2003) show that this is not as straightforward as it in linear models. What I am interested in here really is a partial marginal effect, not a total marginal effect, so Ai and Norton's concerns do not completely apply. Nonetheless, one simple solution to the problem of interpreting interaction terms in non-linear probability models in the literature has been to estimate instead linear probability models, despite their shortcomings, and

when Hypothesis 3 was introduced in the conceptual framework section of this paper, political connections may enable firms to cross-list despite weak formal *property rights* institutions because those connections may allow firms to operate as if they were in a higher quality *property rights* environment at least in terms of how it affects their cross-listing abilities/decisions. The results in column 2 of Table 5 indicate, as suggested in Hypothesis 3, that the role political connections play in cross-listing depends critically on the quality of the *property rights* institutions in the firms' traditional home (since it indicates that there is no independent effect on cross-listing aside from mitigating the negative consequences of a weak *property rights* environment).⁵⁹

The result in column 3 of Table 5 suggests that domestic political connections do not work through *contracting* institutions. This result, while not significant, is possibly consistent with a story about how in low quality *contracting* environments governments may be more likely to coerce banks into making loans at below competitive market rates as an alternative method of contracting that does not require firms to use *regulatory bonding* if they maintain *reputational bonds*. The result in column 4 of Table 5, which includes interactions between political connections and both *property rights* and *contracting*, seems to support this possibility as well; this result shows that when interactions between weak *contracting* and political connections are included in the regression as a control, the magnitude of the interaction effect between political connections and *property rights* institutions becomes even stronger, which would be consistent with that story.

<Insert Table 5 Here>

The result in column 5 shows that, in addition to it not being the interaction with *contracting* institutions, it is not the level of development either that determines whether domestic political connections affect firms' cross-listing. Taking all of the results in Table 5 together seems to support Hypothesis 3 in that the weaker the *property rights* institutions

interpret interaction terms in them instead. See for example Osili and Paulson (2008) who do this. When I do this, I get approximately the same results. As a second solution to address concerns about interpreting coefficients in a non-linear regression, see the next subsection of the paper and Table 5 in it, where I stratify the regression sample.⁵⁹ We can reject 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 incorrectly when we include an independent effect of political connections in it. The regression in column 2 also shows that when we include an independent effect we lose the statistical significance of the interaction term; however, the estimate of the coefficient on the interaction term hardly changes. Furthermore, a Wald test confirms at the 10% level that including both an independent effect of cross-listing and an interaction with *property rights* institutions is jointly significant.

environment is, the stronger an effect domestic political connections seem to have on whether or not a firm chooses to cross-list.

Another country-level variable, I have tried interacting with the politically connected firm dummy variable in this framework is the percentage of bank assets in a country that are state-owned (taken from La Porta, Lopez-de-Salinas, and Shleifer 2002). The logic for testing this relationship is that it might be that the political connections are most valuable in terms of manipulating domestic capital markets in contexts where the government has the greatest authority over banks. The results appear in Column 6 of Table 5; they yield a positive coefficient that is highly statistically insignificant, suggesting that if anything politically connected firms are more not less likely to cross-list in locations where they can directly benefit from the governments' ability to coerce banks into giving them preferential financing.

Potential for More Complex Interaction between Domestic Political Connectedness and the Quality of Domestic Property Rights Institutions

The above regression assumes a simple interaction relationship between domestic political connections and domestic *property rights* institutions—as *property rights* institutions become weaker, political connections make it more likely that a firm cross-lists. It is possible that the total relationship between domestic economic institutions and domestic political connections is more complex than that captured in the above regression because other country-level factors are correlated with weak *property rights*. This is particularly a concern if we believe Leuz and Oberholzer-Gee's (2006) finding that in Indonesia, a country with poor *property rights* protections, domestic political connections make it less—not more—likely that a firm cross-lists its securities, since my last set of findings (in Table 5) suggests we should find the opposite result.

One potential way to understand how the total effect of political connections varies with *property rights* (without a fully specified econometric model choosing the country-level variables) is stratifying the sample into groups of countries by their relative levels of *property rights* institutions and re-running my regressions with country dummies and looking at those results instead. This will better enable us to identify the effects of political connections conditional on a firm being located in a country with different levels of *property rights* institutions. This should also help assuage concerns identified by Ai and Norton (2003) about

the interpretation of coefficients on interacted variables in non-linear regressions, like the multinomial logit regression I employ.

Results of this analysis, where the sample is stratified by country-level quartiles of the quality of *property rights* institutions, appear in Table 6.⁶⁰ 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 6.⁶¹

The results in Table 6 seem to indicate that the simple interaction considered in the first test of Hypothesis 3 (shown in Table 5) may have been a bit too restrictive. If the relationship found in the prior regressions continued to hold, we would expect the coefficient on the political connections variable to get larger the weaker the *property rights* institutions become (i.e as we move from the first column to the last). It does not. Furthermore, the coefficient is not statistically significant in either the countries with the weakest or the strongest *property rights* institutions. The coefficient on the political connections variable is statistically significant only for the middle two quartiles. This suggests some attenuation in the relationship between domestic political connections and firms' cross-listing decisions when they operate in environments that are at the extremes of institutional quality.

There are logical reasons why we might observe an attenuation of the role that domestic political connections play in both the strongest *property rights* institutions environment and the weakest *property rights* institutions environments. In the environments with the strongest *property rights* institutions (Q1 in the regression sample or column 1 of Table 6), it is possible that because domestic *property rights* protections are already very secure in these countries that being politically connected does little to help firms. Since few firms in these economies alter their behavior substantially to avoid expropriation either by the government or other actors,

⁶⁰ 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.

⁶¹ These results are not shown, but available upon request.

political connections do little to improve the implicit *property rights* environment; any impact they might have might simply not be statistically measurable because it is so small. On the other hand, in the environments with the weakest *property rights* institutions (Q4 in the regression sample or column 4 of Table 6), it is possible that in addition to receiving *property rights* protections, politically-connected firms located in these extremely weak institutional environments might also receive below market rate loans through back channel contracts.

These latter results may be consistent with the *reputational bonding* theory of why firms choose not to cross-list, particularly since this quartile includes Indonesia, which is one of the single country contexts in which a variant of the *reputational bonding* hypothesis has received empirical support in a study by Leuz and Oberholzer-Gee (2006). The *reputational bonding* theory might also hold in the many countries not in my sample that have weak *property rights* institutions and few cross-listings or major firms; unfortunately, we know little about the incidence of political connections in these countries so we cannot include them in this study. Perhaps in the contexts where *property rights* institutions are weak, despite the fact that political connections could be mitigating the negative consequences of those institutions, back channel loans become more prevalent and serve as an alternative to global financing as suggested by Leuz and Oberholzer-Gee (2006). Below market rate back channel contracts might reduce the ability of politically-connected firms that benefit from them domestically to cross-list, since prudent foreign financiers would require a higher risk premium from firms that receive such domestic loans.

<Insert Table 6 Here>

5.4 Robustness of Results

I believe that my findings are robust; however, it is possible that some readers may be concerned about some of the assumptions I made throughout my analysis. The first of these assumptions is that my key political economy variables, firm-level domestic political connections and country-level domestic economic institutions, are exogenous to firms' decision to cross-list. A second concern might be about the assumption that the existence and incidence of political connections is independent of the institutional environment. A third and final concern that I address is whether domestic political connections have a direct effect on cross-listing or if they simply work through some other observable firm-level characteristic.

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

Endogeneity questions are essentially about the direction of causality. Surely, a firm choosing to cross-list its securities does not cause it to gain domestic political connections of the sort in Faccio's dataset. Recall that 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" (Faccio 2006). Given this definition, it is unclear how a large shareholder or top officer of a firm advising that firm to maintain a cross-listing has any direct influence on whether or not they could go on to win an election or secure a political appointment as a result. 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 its securities in a foreign market.⁶² In several instances, it has been argued that the types of political connections I am observing are a function of chance rather than an explicit calculation on either the firms' or the politicians' part (Johnson and Mitton 2003; Faccio 2006; Fisman and Miguel 2008).

It is also unlikely that politicians can directly manipulate foreign capital market participants who reside outside of the institutional environment over which they have some dominion. It is possible, however, that politicians could manipulate domestic capital market participants into financing a connected firm on preferential terms. Politicians' lack of power over foreign capital market participants should relieve concerns about this source of causality for the relationship. Consequently, the average foreign market participant must respond favorably to firms' observable domestic political connections. In many ways the identification strategy (of observing firm behavior in foreign markets) is similar to the one successfully implemented by Osili and Paulson (2008) who look at the financial behavior of international migrants to the United States. They claim to isolate hold-over home country effects on individuals' behavior when their US market participation is observed instead; in my identification, I am observing the behavior of firms outside their domestic market, rather than individuals as Osili and Paulson do.

Part of the argument made in the version of *reputational bonding* in Leuz and Oberholzer-Gee's (2006) paper on Indonesian connections and cross-listing is that if firms chose to cross-list they might have to give up some of the value of their domestic political connection by revealing information about what illicit benefits it affords them such as an ability to

⁶² In fact, we might expect that domestic political actors would see their stake in firms fall as a result of cross-listing since the action typically attracts a more diverse shareholder base.

manipulate domestic capital markets.⁶³ This causal story would run the opposite direction, biasing against my primary finding that politically connected firms are more likely to cross-list the weaker their domestic institutional environment. (Perhaps this is also why I see some attenuation in the net relationship when I stratify the sample by *property rights* institutions as in Table 6.)

The type of political connections I observe seem unlikely to be maintained for the explicit purpose of cross-listing. Likewise, the cross-listings firms have seem unlikely to be maintained for the explicit purpose of gaining political connections. This of course does not overturn the vast literature on how many other types of political connections that firms maintain, such as those gained through lobbying or campaign contributions, may in fact represent an exchange of dollars or other services for explicit policy outcomes.⁶⁴

Endogeneity of Domestic Institutions to Firms' Cross-listing is Unlikely

Concerns about the endogeneity of economic institutions primarily afflict studies that are done solely at the country level, such that the dependent variable could be determined simultaneously as the independent variable. By choosing to study a firm-level outcome, in a world where firms have little ability to influence their home institutional environment, concerns about domestic institutions being endogenous to the dependent variable (firms' cross-listing choices) should be mute. Low quality domestic institutions really do serve as a constraint to firms when most of them have little or no ability to directly influence the quality of domestic institutions they face.

In my study, we must believe that firms do not choose the country in which they locate operations based on the way the domestic institutional environment shapes their financing

⁶³ In Luez and Oberholzer-Gee's (2006) case, they do need to instrument for political connections, however, because they may be endogenous to the decision not to reveal information about what the connections afford the firm. Leuz and Oberholzer-Gee (2006) use as instruments for political connections: (1) whether or not the firm has a director of Chinese descent; and (2) what the firms' age is. Data on both of these instruments is unavailable for the cross-section of firms in the 46 countries in my sample. Furthermore, the former instrument (Chinese descent of directors) is unlikely to apply in a cross-country setting. The later could be interpreted differently in different settings too, so it is unclear that it would be worthwhile even if the data were available. Leuz and Oberholzer-Gee argue that in Indonesia younger firms are more likely to be connected because they need more help; it could easily be argued, however, that older firms are more likely to have connections in other settings because they have had longer to develop them.

⁶⁴ Of course there are also those who argue that some of these types of corporate political activity merely represent a form of corporate consumption, at least within given institutional contexts. (Ansolabehere, deFigueiredo, and Snyder 2003)

abilities. This seems reasonable. Many firms around the world are simply located where their founders were born or where they wish to reside. Furthermore, if all firms selected their traditional home-country institutional environment based on access to finance then we would expect all firms to be located in the countries with the best combinations of *property rights* and *contracting* institutions. That means that we would expect to see every firm in the world locate in places like Canada, Finland, Sweden or Switzerland—when in fact we know that many firms, including the bulk of those in my sample, choose to locate elsewhere. This is especially true of multinationals who often intentionally locate a portion of their operations in weak institutional environments to arbitrage wage and skill differentials between countries.⁶⁵

Should Correlations Between Domestic Political Connections and Domestic Economic Institutions be a Concern?

Another concern (particularly relevant to Hypothesis 3 about interactions between domestic political connections and domestic institutional environments) might be that political connections may be more common in places where they might be most valuable—possibly the weakest institutional environments. Again this is something that we do not need to be concerned about since it is not borne out in the data. Faccio (2006) showed that political connections are prevalent around the world in both developed and developing countries with both weak and strong institutional environments.

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 A8 in the Appendix shows a scatterplot that illustrates that the types of political connections I observe are not any more prevalent in weak or strong institutional environments. The source of variation in the domestic political connections data across countries that I use has more to do with regulations in individual countries against explicit, observable political connections than it has to do with the countries' level of development or their domestic institutional environment (Faccio 2006). Furthermore the few numerical estimates other researchers have arrived at for the value of political connections (in terms of firms' market

⁶⁵ To the extent that traditional multinationals are in my sample they may actually bias against my findings since they are likely to be cross-listed, but not likely to have the type of domestic political connections Faccio observes in the environments where the firms' operations occur.

capitalization) appear to be uncorrelated with the incidence of political connections in a given country, as mentioned in the introductory section of this paper.

Concerns about Differences in the Ability to Cross-list Depending upon Different Treatment of Firm-level Attributes for Politically Connected Firms

While an endogenous relationship between domestic political connections and cross-listing should not be a concern, it could be possible that foreign financial markets respond differently to the core corporate financing characteristics of connected firms vis-à-vis unconnected ones. If this were true, then we might be concerned that this somehow may bias the core findings or that political connections work through some alternate observable firm-level variable.

I should be able to test for such biases by running a regression of the form:

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

where $d_connect_i$ represents a dummy variable for politically connected firms using Faccio's (2006) definition and data; X_i represents the firm-level control variables; d_j , industry dummies; and d_k , country dummies. Results from regressions of this form appear in the Appendix.

The goal of such an analysis would be to test whether or not the coefficient β , on the interaction between firm-level control variables and the politically connected firm dummy variable, is measurably different than zero. If β is measurably different than zero, then we should be concerned that foreign capital markets in which firms seek to cross-list securities respond differently to firms' core corporate financing characteristics depending upon their political status. If β is not measurably different than zero, concerns about how foreign capital markets may respond differently to firm-level corporate financing characteristics should not adulterate the prior analysis.

As seen in a table in the Appendix the results of this robustness check seem to indicate that foreign financial markets do not respond differently to the core observable corporate financing characteristics of connected firms vis-à-vis unconnected ones. This means that observable, domestic political connections do indeed play a direct role in firms' cross-listing choices.

6 Discussion: Role of Political Connections in Access to Finance

The empirics suggest that the role domestic political connections play in firms' cross-listing choices depend critically upon the home institutional environment in which the firms operate. I find support for the hypothesis that in weaker *property rights* environments firms with domestic political connections are more likely to cross-list, controlling for firm-, industry-, and country-level characteristics. This finding contrasts with the typical relationship for firms operating in weak *property rights* environments, since on average they are less likely to cross-list.⁶⁶ The finding also contrasts with the *reputational bonding* theory that says firms with political connections are less likely to cross-list (Siegel 2004).

Taken together these results suggest we should update how we characterize the role political connections play in accessing finance. In most existing characterizations, political connections represent *reputational bonds* that are useful only for accessing back-channel domestic contracts through capital market manipulation (such as having governments coerce local banks into giving connected firms below market rate financing). My findings suggest an alternate way to characterize the value of political connections: they provide implicit *property rights* protections that enable firms to take actions in the real economy they would not have been otherwise able to that lead to their better financing terms (and more ready access to foreign capital markets). Political connections create efficiencies in the markets' ability to allocate capital by reducing the uncertainty in the *property rights* environment—rather than inefficiencies in how the market allocates capital as countless past researchers have suggested through explicit, but typically unobservable, capital market manipulation.

In most cases the preferential financing terms that politically connected firms receive are not likely to be the result of coercion. Rather, preferential financing terms are more likely the result of rational decisions making by financiers: financiers who observe firms' explicit political connections 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.

⁶⁶ The relationship between domestic political connections and domestic *property rights* institutions attenuates, however, as both *property rights* and *contracting* institutions become extremely weak. We may find this attenuation because in extremely weak institutional environments firms may use domestic political connections to access back-channel domestic financing at below market rates.

If political connections afford firms the ability to operate as if they were in a superior *property rights* environment, their cash flows will be more secure than otherwise similar firms. This means that firms with domestic political connections simply represent less risky investments—consequently they should receive relatively better financing terms in well functioning capital markets.

Why shouldn't domestic political connections matter to *foreign* investors in addition to *domestic* investors? In fact, in a paper by Siegel (2005) on *reputational bonding*, he writes "it is necessary to examine both how the institutions are written and how they are implemented." *Domestic* investors are likely to have a comparative advantage over *foreign* investors in understanding how domestic institutions are implemented in the absence of *reputational bonds*, since they are likely to better understand their home country and the specific risks its weak *property rights* institutions pose to any given firm. Given this possibility, it could even be argued that domestic political connections may matter even more to *foreign* investors since *foreign* investors may find it more difficult to evaluate the specific risks a weak domestic institutional environment poses to a firm that does not maintain domestic political connections as a form of implicit *property rights* protection.

The reason we may see the attenuation in the effect of domestic political connections on firms' cross-listing choices (in the data on extremely weak institutional environments), could stem from the few cases where domestic political connections are used to manipulate financial markets creating aggregate inefficiencies, rather than being used solely to provide a signal of implicit protections from a weak *property rights* environment creating aggregate efficiencies. Rather than using increased voluntary disclosures to improve their *contracting* environment as the *regulatory bonding* theory postulates they can, politically-connected firms may be using government coercion to obtain superior financing terms domestically, at below market rates. A rational response by financiers who sit outside the reach of the domestic government would be to offer above market rate financing terms to firms that obtain below market rate domestic financing (as this would be necessary to restore the equilibrium financing terms for a given firm). Since foreign capital market participants are beyond the reach of domestic governments, they are likely to offer above market rate financing terms to politically connected firms that use back channel contracts. Firms that use these back channel contracts then are less likely to cross-list since the costs of doing so would be higher. It is in this case (where below-market-rate, back

channel contracts are used and cross-listing is not) that we may see the real allocation inefficiencies. The average politically connected firm, however, does not create capital allocation inefficiencies. If domestic banks are offering seemingly preferential terms simply because they recognize that firms with domestic political connections are insulated from the weak domestic *property rights* environment, this may actually be a rational market response to observable political connections.

7 Conclusion

This paper has shown that political economy variables at both the firm-level and country-level affect firms' global financing activity. Specifically, this paper finds: (1) that domestic firm-level political connections increase the probability that a given firm cross-lists *across* countries; (2) that stronger domestic country-level economic institutions increase the probability that firms cross-list *across* countries; and (3) that the role domestic political connections play in cross-listing choices depends critically on the domestic *property rights* institutions environment *within* countries. These findings are important because they help reconcile existing *reputational bonding* theories of why firms would chose not to cross-list with *regulatory bonding* theories of why firms would choose to cross-list. Furthermore, the results require us to update how we think about the role political connections play in firms' receiving preferential financing; this may not be the result of capital market manipulation after all, but rather a rational response to the favorable position politically connected firms face in the real economy.

The results have broader implications because the value of political connections in other contexts may also depend critically on the institutional context in which they are studied. The characterization that the value of political connections across countries stems from the implicit protections they provide from weak *property rights* environments may carry over to other outcomes, where econometric identification is more difficult to achieve than it is for cross-listing. Nevertheless, most studies of the value of political connections still tend to be done within a single country context in which the institutional environment is fixed. While these studies are and will continue to be valuable, it is important to be cautious about generalizing their results beyond the location studied. Furthermore, these studies cannot help us understand where the value of political connections is derived if it is conditional on the country context.

8 References

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9 Figures and Tables

Figure 1 – Financing Location Options for both Connected and Unconnected Firms

Firms with domestic political connections (which are represented by the arrows with a circle at the base) can choose among three financing options: (1) financing domestically through normal channels (represented by a loop back to the traditional home); (2) financing abroad in an alternate financial home with higher quality contracting institutions (represented by arrows from the traditional home to the alternative home), consistent with the regulatory bonding theory; or (3) using their connections to access back channel financial contracts if the government can arrange for them (represented by arrows to the shadow behind the traditional home), consistent with the reputational bonding theory. The firms that are not politically connected (represented by the arrows without a circle at the base) do not have the third option—using back channels since they do not have the political connections required to access this potential source of cheaper finance—although they can choose among either of the first two financing options, depending upon the relative costs and benefits. Whether option (2) or option (3) dominates for politically connected firms across countries is an open empirical question and the subject of Hypothesis 1. It is also important to note that financing options (1) and (3) are observationally equivalent, since back-channel financing cannot be directly observed.

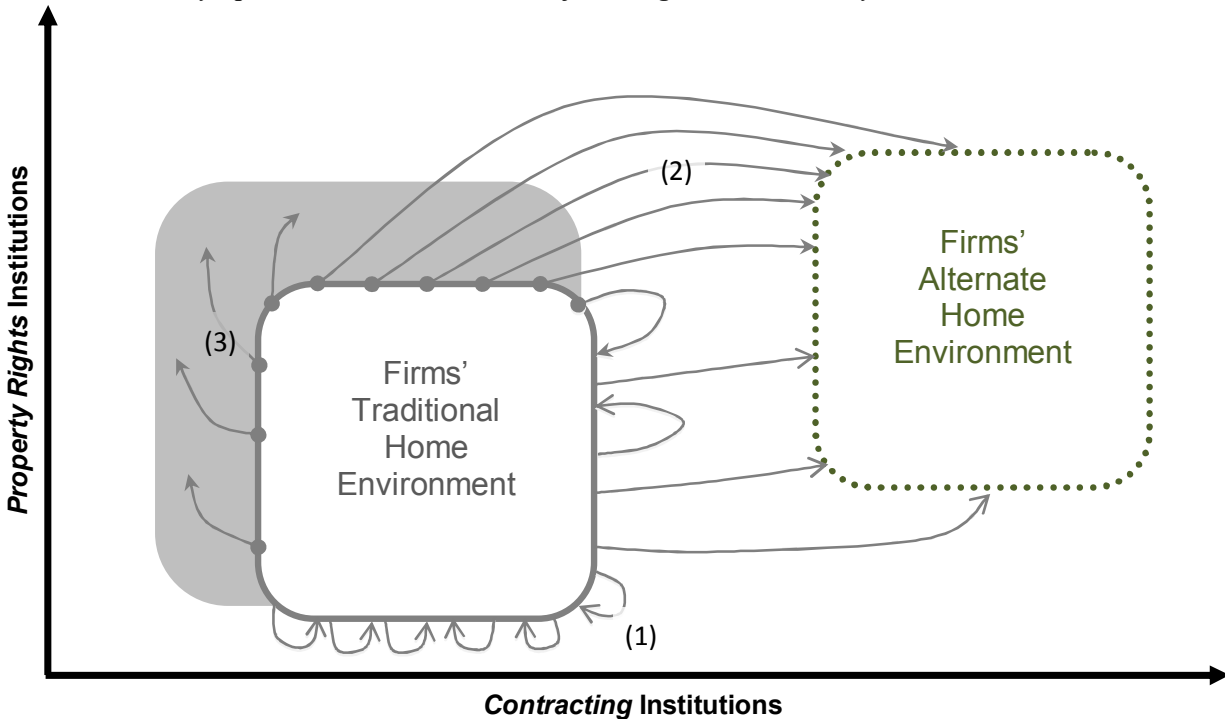


Figure 2 – Cross-listing across Formal Institutional Contexts

Locations (countries) to the top-right of the figure represent those with stronger contracting and property rights institutions; these are the ones that make the most desirable cross-listing destinations since they are where the world's deepest and most efficient financial markets are located. Each arrow-line on the graph represents a firm from the country where the arrow begins choosing to cross-list in the country where the arrow ends. The closer, in the institutions space represented below, a country is to the attractive cross-listing destinations, the easier it is for firms to cross-list securities in them because the costs of compliance with the alternative contracting institutions in institutionally-close destinations are lower; however, the potential benefits are lower too. Benefits can be thought of graphically by the length of the line to a given cross-listing decision.

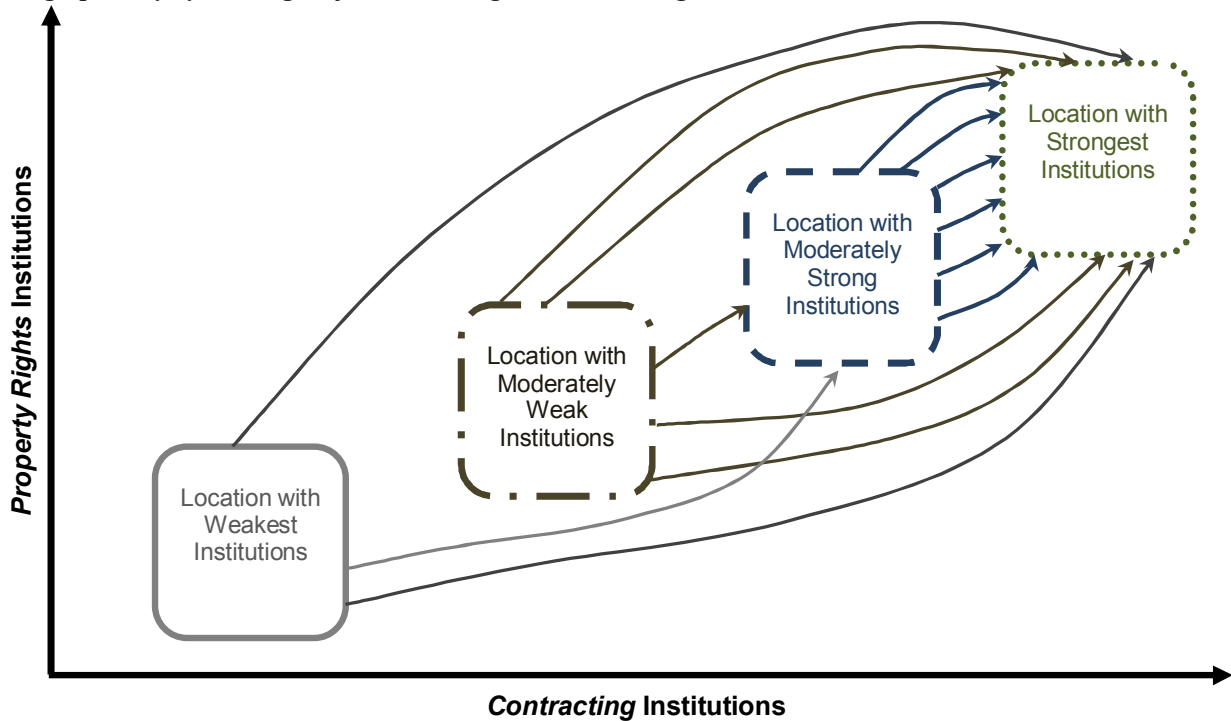


TABLE 1 - Country Level Correlation Matrix

	<i>Property Rights Institutions</i>	<i>Contracting Institutions</i>	<i>Log(GDP per Capita)</i>	<i>Incidence of Political Connections</i>	<i>Incidence of Cross-Listings</i>
<i>Property Rights Institutions</i>	1.000				
<i>Contracting Institutions</i>	-0.551	1.000			
<i>Log(GDP per Capita)</i>	-0.858	0.485	1.000		
<i>Incidence of Political Connections</i>	0.029	0.270	-0.144	1.000	
<i>Incidence of Cross-Listings</i>	-0.327	0.326	0.441	-0.093	1.000

TABLE 2 - Percentage of Firms Cross-listing, Categorical Means (at the Country-Level)

	Country-level Property Rights Quartiles				
	All Countries	Q1 (Strongest)	Q2	Q3	Q4 (Weakest)
Politically-Connected Firms	46.7%	49.7%	51.6%	50.7%	24.0%
Unconnected Firms	38.4%	45.6%	44.4%	38.8%	29.7%
Difference (Connected-Unconnected)	8.4%	4.1%	7.2%	12.0%	-5.7%
Ratio (Connected/Unconnected)	121.8%	109.0%	116.2%	130.9%	80.7%

To be included in the sample of countries here there must be at least one firm that is politically-connected within the country. Similar patterns hold if the unit of analysis is the firm within countries rather than country-level averages.

TABLE 3 - Does being politically increase the probability of cross-listing?

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>			
Politically Connected	0.613*** (0.119)	0.583*** (0.123)	0.239 [†] (0.156)	0.233 [†] (0.157)	0.621*** (0.118)
<i>Firm Level Controls</i>					
Size	-0.007 (0.008)	-0.001 (0.008)	0.687*** (0.029)	0.718*** (0.028)	
Return on Assets	-0.001 (0.038)	0.043 (0.040)	-0.450 (0.520)	-0.265 (0.433)	
Capital Intensity	-0.476*** (0.147)	-0.473*** (0.153)	-1.084*** (0.174)	-0.660*** (0.182)	
Leverage	0.588 (0.369)	0.497 (0.380)	-0.256 (0.245)	-0.208 (0.252)	
<i>Industry Level Controls</i>					
Industry Dummies	No	Yes	No	Yes	No
<i>Country Level Controls</i>					
Country Dummies	No	No	Yes	Yes	No
<i>Scenario being Tested:</i>		<i>Wald Tests (F-Statistics)</i>			
Joint Insignificance of Industry Dummies	-	41.336	-	37.654	-
Joint Insignificance of Country Dummies	-	-	31.928	34.237	-
Percentage of Obs. Predicted Correctly	60.27%	64.15%	77.82%	79.27%	60.33%
Number of Observations	7461	7461	7461	7461	7461

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level; † indicates significant 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.

Figure 4 – 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.

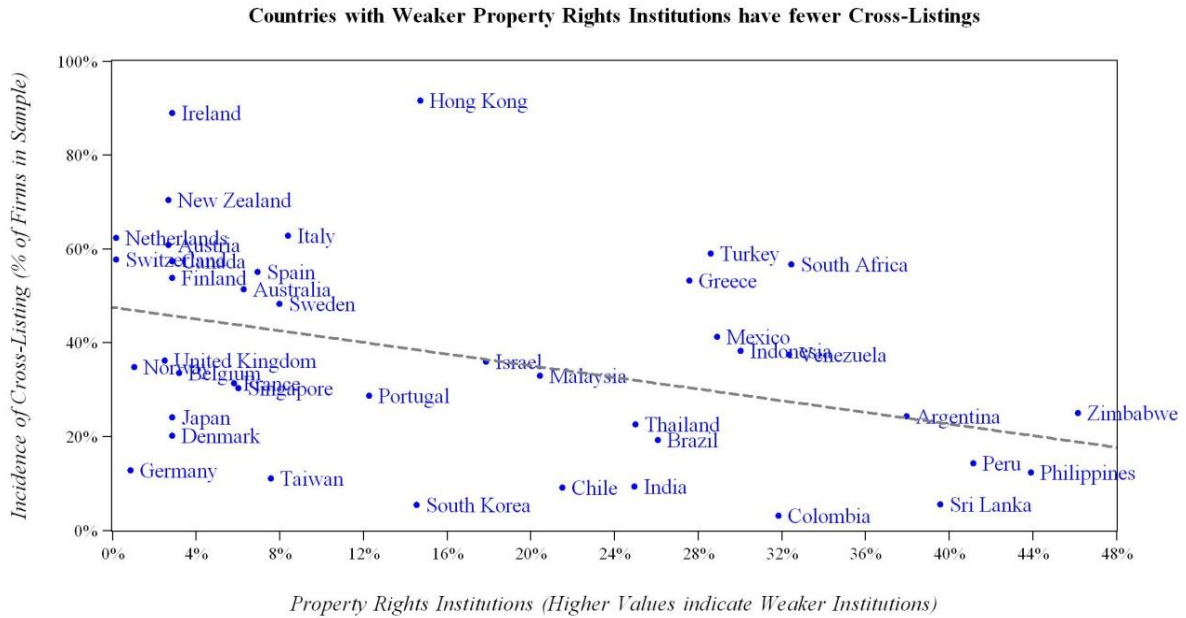


Figure 5 – Country-level Relationship between Cross-listing Incidence and *Contracting* Institutions
The following figure shows the results of a bivariate country-level regression of contracting institutions on the incidence of cross-listings for a particular country in my sample.

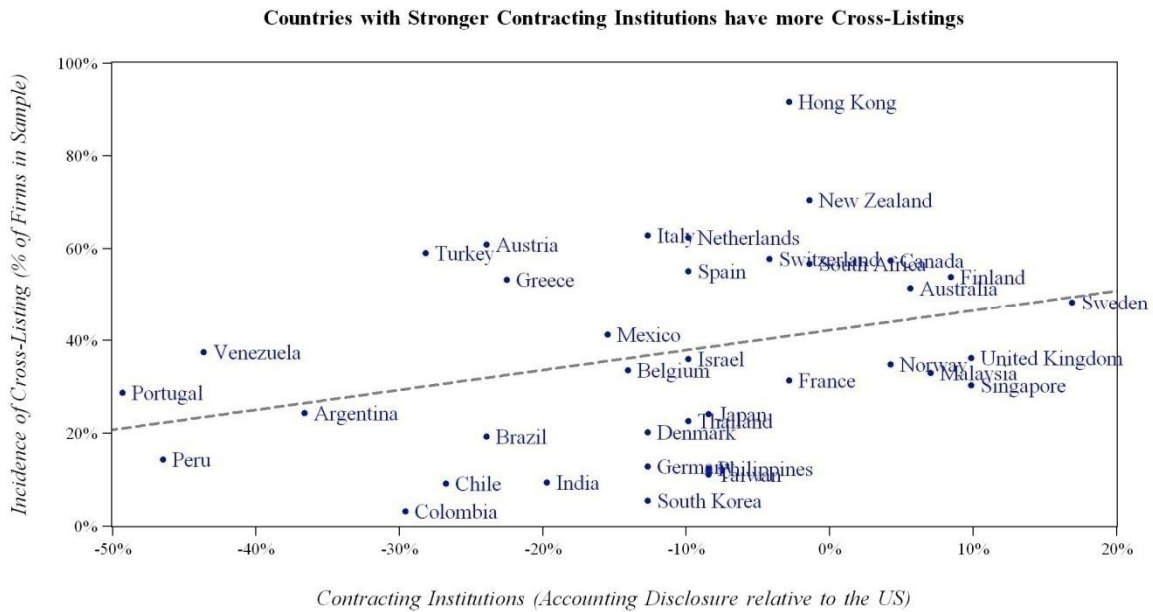


TABLE 4 - How Institutions affect Firms' Probability of Cross-listing

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>			
Property Rights Institutions	-0.627** (0.263)	-0.736*** (0.268)			
Contracting Institutions				4.242*** (0.326)	4.146*** (0.326)
<i>Firm Level Controls</i>					
Politically Connected		0.594*** (0.124)		0.433*** (0.131)	
Size	-0.004 (0.008)	-0.004 (0.008)	0.046*** (0.009)		0.045*** (0.009)
Return on Assets	0.043 (0.040)	0.044 (0.040)	0.041 (0.042)		0.040 (0.042)
Capital Intensity	-0.450*** (0.158)	-0.437** (0.158)	-0.511*** (0.161)		-0.507*** (0.161)
Leverage	0.577 (0.390)	0.543 (0.390)	0.488 (0.414)		0.467 (0.414)
<i>Industry Level Controls</i>					
Industry Dummies	Yes	Yes	Yes	Yes	Yes
<i>Scenario being Tested:</i>		<i>Wald Tests (F-Statistics)</i>			
Joint Insig. of Ind. Dummies	40.052	39.736	36.787	36.724	
% of Obs. Predicted Correctly	64.12%	64.31%	65.94%	66.01%	
Number of Observations	7355	7355	7191	7191	

*** 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 5 - Testing the Interaction between Political Connections and Economic Institutions

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>				
Prop. Rights Inst. * Connected	1.725* (0.971)	1.623 [†] (1.433)	2.877* (1.619)			
Contracting Inst. * Connected			-0.774 (1.396)	-0.190 (1.363)		
ln_GDP * Connected					0.023 (0.016)	
Government Ownership of Banks (% of Assets) * Connected					0.676 (0.718)	
<i>Firm Level Controls</i>						
Politically Connected	0.023 [†] (0.229)		-0.037 (0.227)			
Size	0.718*** (0.027)	0.718*** (0.028)	0.716*** (0.028)	0.714*** (0.028)	0.719*** (0.028)	0.721*** (0.028)
Return on Assets	-0.263 (0.424)	-0.263 (0.424)	-0.254 (0.394)	-0.252 (0.388)	-0.265 (0.433)	-0.266 (0.435)
Capital Intensity	-0.635*** (0.183)	-0.635*** (0.183)	-0.624*** (0.185)	-0.601*** (0.186)	-0.661*** (0.182)	-0.663*** (0.182)
Leverage	-0.217 (0.254)	-0.216 (0.254)	-0.235 (0.264)	-0.275 (0.267)	-0.206 (0.252)	-0.201 (0.252)
<i>Industry Level Controls</i>						
Industry Dummies	Yes	Yes	Yes	Yes	Yes	Yes
<i>Country Level Controls</i>						
Country Dummies	Yes	Yes	Yes	Yes	Yes	Yes
<i>Scenario being Tested:</i>		<i>Wald Tests (F-Statistics)</i>				
Joint Insig. of Industry Dummies	33.862	33.867	33.887	33.959	34.037	33.932
Joint Insig. of Country Dummies	37.827	37.628	41.457	41.183	34.223	37.168
% of Obs. Predicted Correctly	79.32%	79.29%	79.24%	79.22%	79.27%	79.30%
Number of Observations	7355	7355	7191	7191	7461	7455

*** 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 are strongly rejected in all regressions, meaning that the including the dummy variables has joint significance at the 1% level.

TABLE 6 - Stratified Sample Approach to Connections and Institutions as Joint Determinants

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>			
<i>Sample:</i>		<i>Property Rights Institutions' Quartiles (Country-Level)</i>			
	<i>Strongest</i>	<i>Moderately Strong</i>	<i>Moderately Weak</i>	<i>Weakest</i>	
	<i>(0-25%)</i>	<i>(26-50%)</i>	<i>(51-75%)</i>	<i>(76-100%)</i>	
Politically Connected	-0.224 (0.023)	1.056** (0.452)	0.614** (0.298)	-0.086 (0.488)	
		<i>Firm Level Controls</i>			
Size	0.691*** (0.032)	0.780*** (0.057)	0.866*** (0.083)	0.752*** (0.138)	
Return on Assets	-0.180 (0.149)	-1.765*** (0.554)	-0.855 (0.627)	0.702 (2.352)	
Capital Intensity	-0.568** (0.251)	-0.865* (0.448)	-0.494 (0.413)	-0.564 (0.701)	
Leverage	-0.192 (0.392)	-0.119 (0.474)	-0.743 (0.527)	0.237 (0.685)	
		<i>Industry Level Controls</i>			
Industry Fixed Effects	Yes	Yes	Yes	Yes	
		<i>Country Level Controls</i>			
Country Fixed Effect	Yes	Yes	Yes	Yes	
		<i>Wald Tests (F-Statistics)</i>			
Joint Insig. of Ind. Dummies	39.309	22.549	17.025	4.590	
Joint Insig. of Country Dummies	66.403	33.292	29.274	8.947	
% 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 $H_0: \gamma_j=0$ 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. 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.

Appendix

Figure A1- Fraction of Foreign Firms Listed on Equity Markets around the World

This figure illustrates cross-listing destinations around the world. The darker the shading, the higher the fraction of firms listed on a given country's exchange are foreign firms. For the New York Stock Exchange in the United States this number is over 20%. The underlying data comes from Karolyi's (2006) Table 1.

Cross-listing Destinations - Fraction of Foreign Firms' in Domestic Equity Market *based on Karolyi (2006), Table 1*

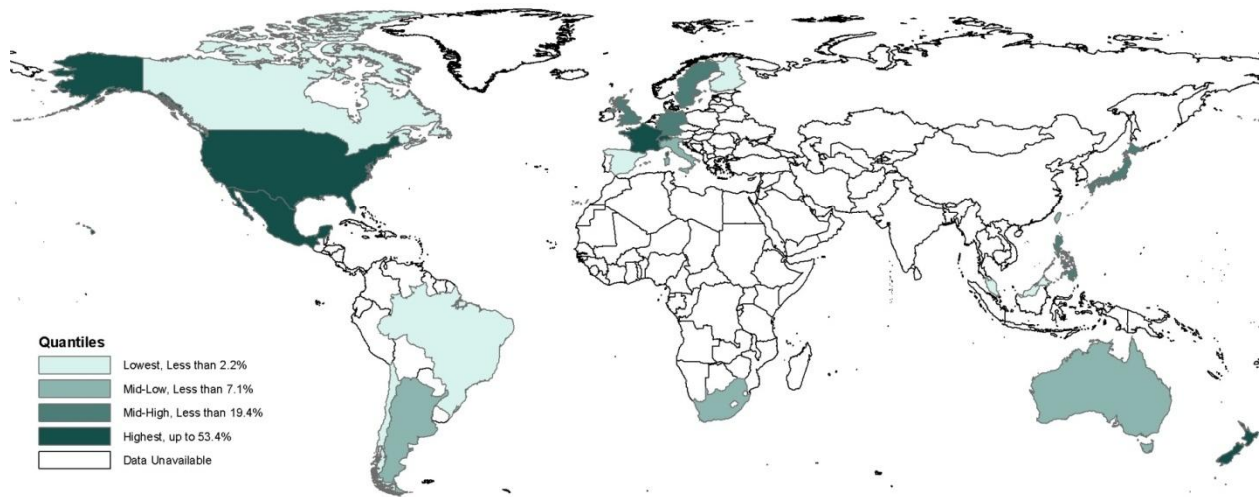


Figure A2- Fraction of Foreign Firms Listed on Equity Markets around the World

This figure illustrates cross-listing destinations around the world. The darker the shading, the higher the fraction of the aggregate market capitalization of a country's stock exchange come from foreign firms' listings. For the London Stock Exchange this number is over 50%. In Switzerland it is greater than 90%. The underlying data comes from Karolyi's (2006) Table 2.

Cross-listing Destinations - Fraction of Domestic Market Capitalization from Foreign Firms

based on Karolyi (2006), Table 2

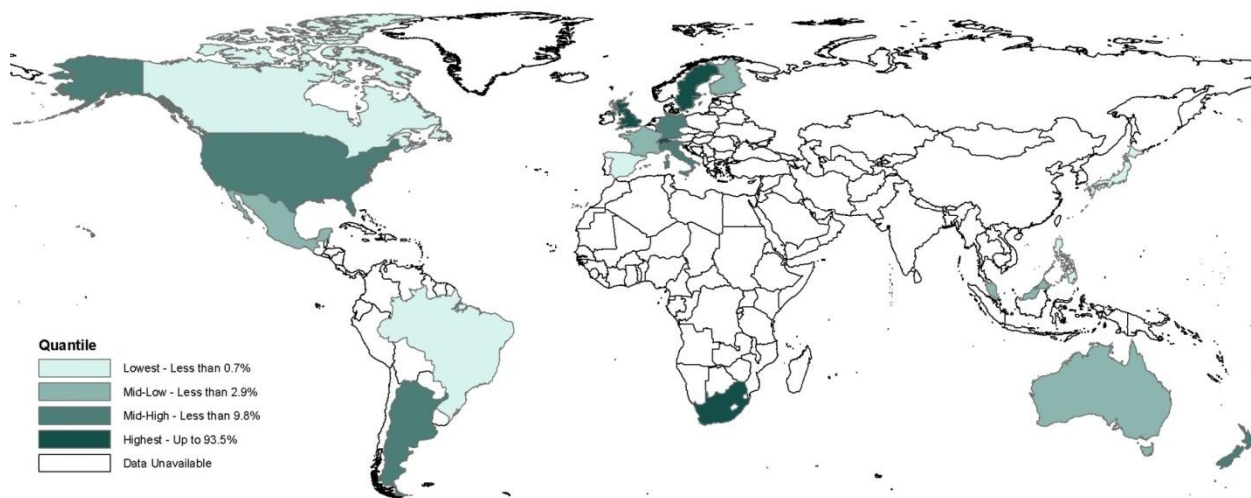


Figure A3- Property Rights vs. Contracting Institutions

This figure illustrates the relationship between property rights institutions and contracting institutions using the same measures as Acemoglu, Johnson, and Robinson (2001) and La Porta, Lopez-de-Salines, Shleifer, and Vishny (1998), for property rights and contracting institutions respectively. Higher values of each represent higher quality institutions in this case.

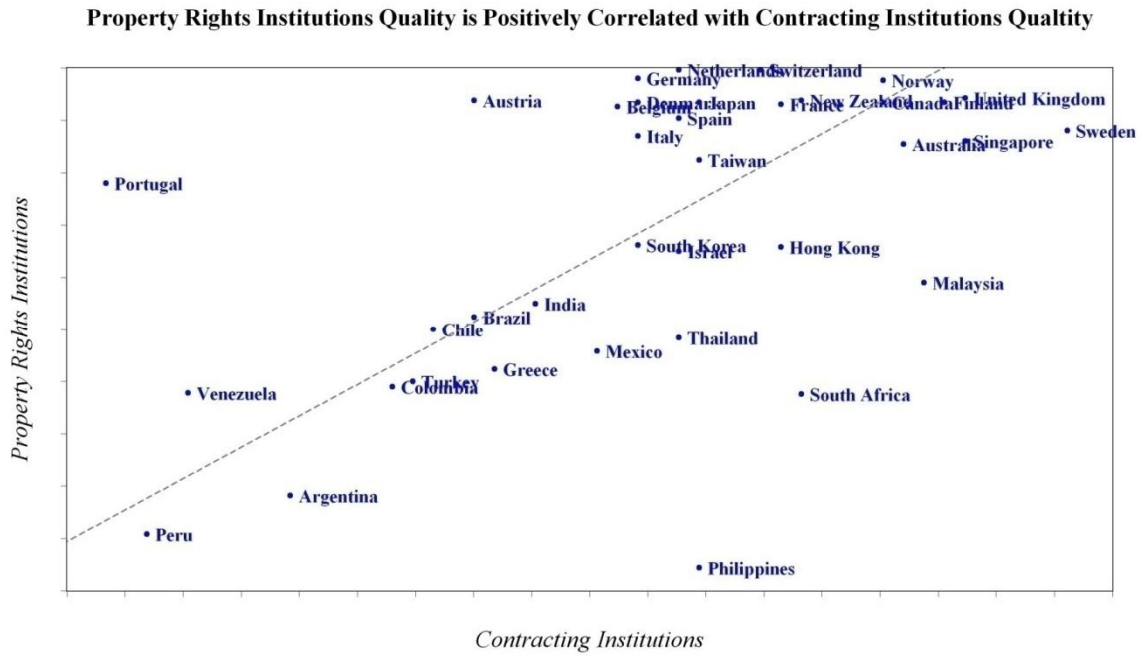


Figure A4- Property Rights Institutions

This figure illustrates how Acemoglu, Johnson, and Robinson's (2001) measure of property rights institutions varies around the world. The darker the shading the weaker the property rights institutions are in a given locale.

Property Rights Insecurity around the World

(based on ICRG Expropriation Index)

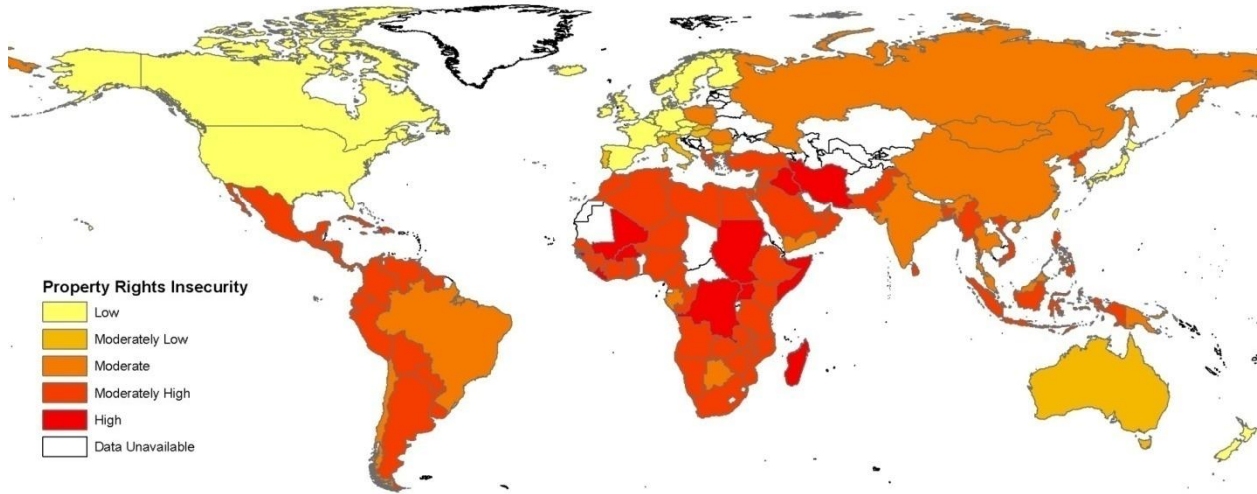


Figure A5- Contracting Institutions

This figure illustrates how La Porta, Lopez-de-Salines, Shleifer, and Vishny's (1998) measure of contracting varies around the world. If the accounting disclosure quality is within 5% of the level in the United States, the countries are shaded in grey (cross-hatched); if the quality level is greater than that the country is shaded in green (solid); if the disclosure quality is worse than that the country is shaded in red (diagonal stripes).

Contracting Institutions (Accounting Disclosure) around the World

(Accounting Disclosure Quality based on Center for International Financial Analysis & Research, Inc. Data)

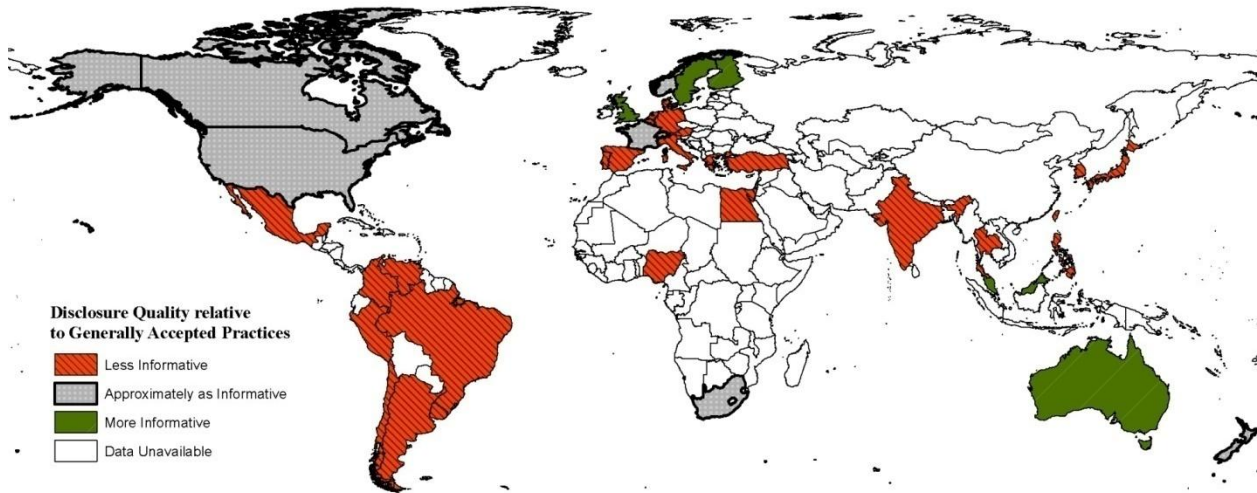


Figure A6- Estimates of the Value of Political Connections around the World

This figure illustrates Fisman's (2001), Faccio's (2006), and Fisman et. al's estimates of the value of political connections around the world. The darker the shading the more valuable the connections is estimated to be. In each of those papers the researchers exploit natural experiments or health-, surprise-election-outcome- shocks to political connections to estimate their value as a percentage of firm's market capitalizations. Fisman, et. al (2001) do this for Dick Cheney, the former United States Vice President; Faccio (2006) does this for Sir John Moore in the United Kingdom was a member of parliament and Rolls-Royce's chairman; Faccio (2006) also does this for Giovanni Angelli who was a member of parliament and Fiat's Chairman; Fisman (2001) famously does this for Suharto's many affiliated firms in Indonesia. For more on this, see Fisman and Miguel's (2008) Economic Gangsters.

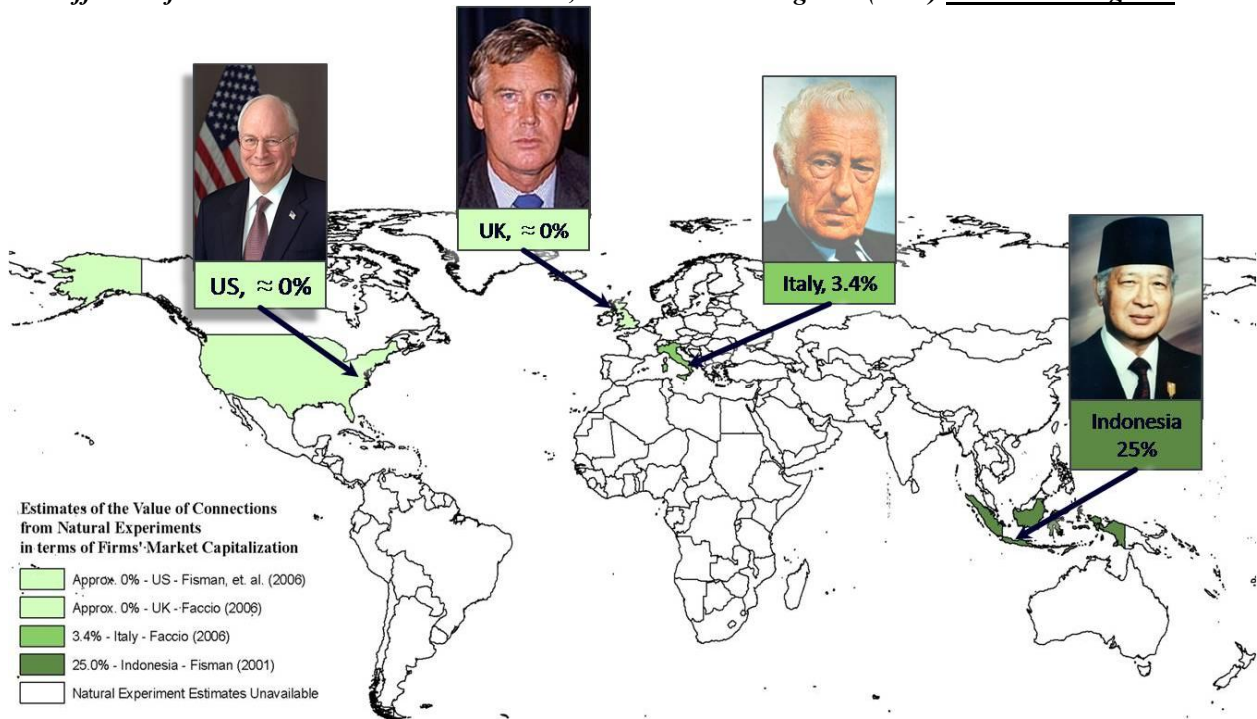


Figure A7- Incidence of Political Connections around the World

This figure illustrates how Faccio's (2006) measure for the incidence of political connections around the world varies across countries. The darker the shading is the higher the fraction of firms in that country is that maintains domestic political connections. A major source of the variance in the underlying data is country specific regulations against political connections. Faccio (2006) finds that political connections are common both in developed and developing countries. In fact, by some of her measures, political connections are most common in the United Kingdom which is also one of the richest countries in the world.

Incidence of Politically Connected Firms around the World

(Percentage of Politically Connected Firms in the sample, Faccio data)

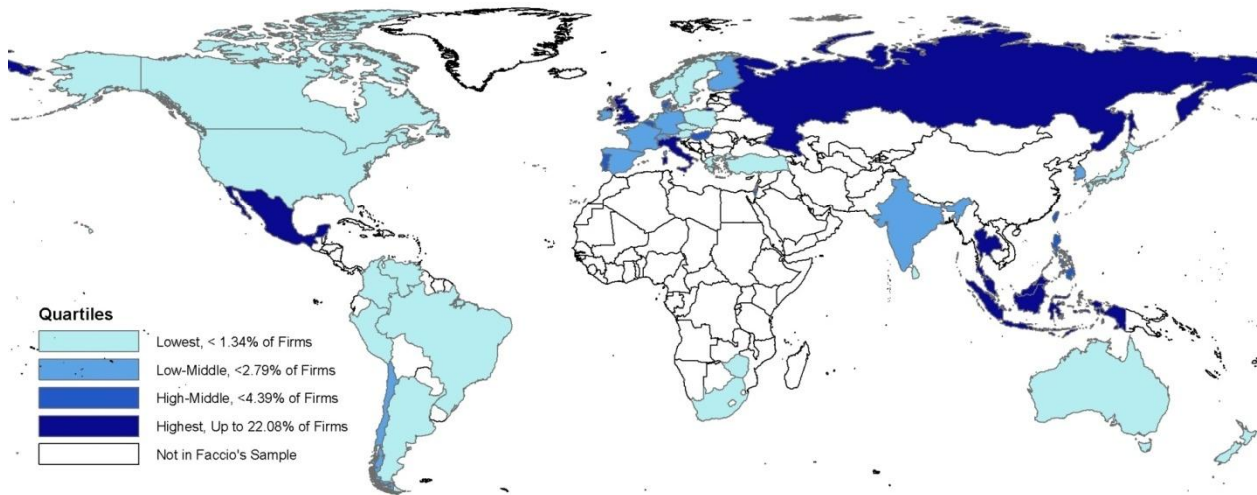


Figure A8 – Scatterplot of Property Rights Institutions vs. Incidence of Political Connections

This figure illustrates the lack of any relationship between the incidence of political connections around the world (Faccio 2006) and the quality of property rights institutions. It is a surprise to many people that there is not a stronger correlation or pattern. Part of the reason there might not be a relationship is that regulations against politicians maintaining political ties abound.

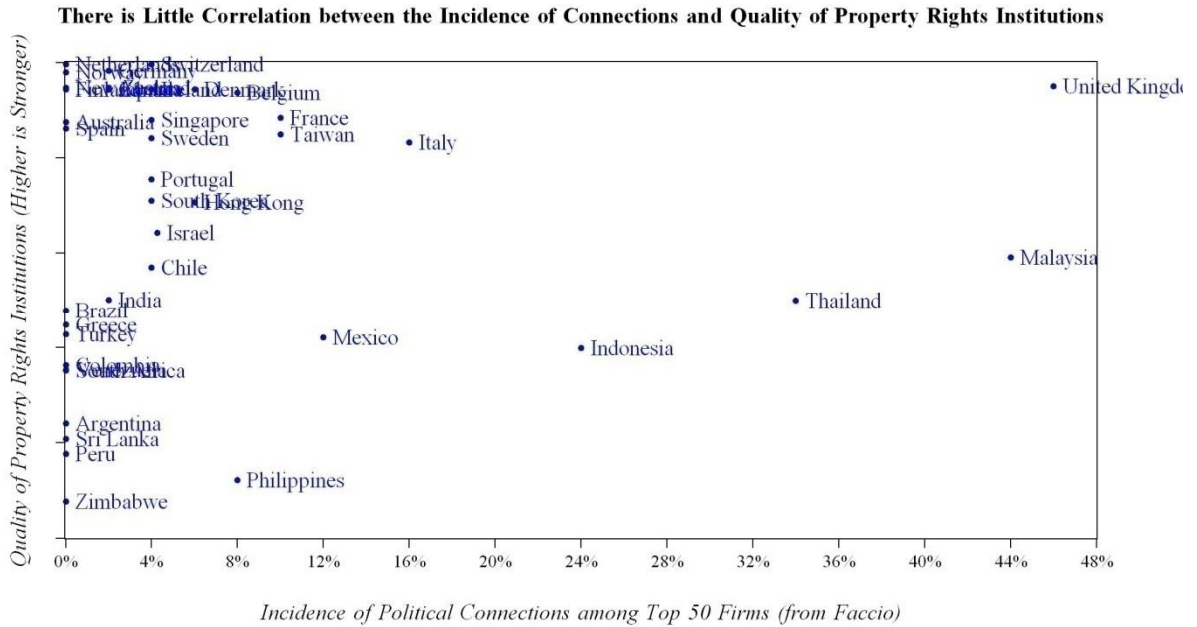


Table A1 - Country-by-Country Statistics

TABLE A1 - Country-by-Country Average(s) for the Full Sample

Country	Number of Firms (in sample)	Average Firm Size (ln of total assets)	Incidence of Firms Cross-listing (% of all firms)	Incidence of Politically Connected Firms (% of all firms)	Property Rights Institutions	Contracting Institutions
Argentina	37	14.02	24.32%	0.00%	39.58%	-36.62%
Australia	280	13.48	51.43%	0.70%	7.29%	5.63%
Austria	110	13.13	60.91%	0.91%	3.93%	-23.94%
Belgium	152	12.98	33.55%	3.82%	5.16%	-14.08%
Brazil	166	14.62	19.28%	0.00%	26.90%	-23.94%
Canada	496	13.68	57.46%	1.31%	3.19%	4.23%
Chile	87	19.40	9.20%	2.25%	23.26%	-26.76%
Colombia	31	20.73	3.23%	0.00%	33.30%	-29.58%
Czech Republic	58	16.18	58.62%	0.00%	NA	NA
Denmark	222	14.18	20.27%	3.07%	3.12%	-12.68%
Finland	128	12.53	53.91%	1.52%	3.16%	8.45%
France	903	12.45	31.45%	2.19%	6.28%	-2.82%
Germany	739	12.40	12.86%	1.55%	1.94%	-12.68%
Greece	150	12.13	53.33%	0.65%	28.15%	-22.54%
Hong Kong	394	14.69	91.62%	1.98%	15.73%	-2.82%
Hungary	23	17.73	82.61%	3.70%	NA	NA
India	309	15.77	9.39%	2.79%	26.55%	-19.72%
Indonesia	154	20.95	38.31%	22.08%	32.46%	NA
Ireland	73	12.74	89.04%	2.44%	3.99%	NA
Israel	50	14.86	36.00%	3.64%	18.08%	-9.86%
Italy	226	14.31	62.83%	10.30%	9.87%	-12.68%
Japan	2338	18.27	24.12%	1.34%	4.84%	-8.45%
Luxembourg	22	13.55	40.91%	4.35%	NA	NA
Malaysia	421	13.43	33.02%	19.78%	21.42%	7.04%
Mexico	92	16.17	41.30%	8.51%	30.31%	-15.49%
Netherlands	213	12.95	62.44%	0.42%	0.64%	-9.86%

New Zealand	44	13.13	70.45%	0.00%	3.01%	-1.41%
Norway	192	14.43	34.90%	0.00%	1.82%	4.23%
Peru	35	13.23	14.29%	0.00%	42.86%	-46.48%
Philippines	113	15.94	12.39%	4.39%	45.92%	-8.45%
Poland	55	13.24	20.00%	0.00%	NA	NA
Portugal	101	12.66	28.71%	2.97%	12.93%	-49.30%
Russia	24	17.98	58.33%	20.00%	NA	NA
Singapore	227	12.81	30.40%	7.86%	6.75%	9.86%
South Africa	199	14.82	56.78%	0.00%	32.27%	-1.41%
South Korea	313	20.57	5.43%	2.56%	17.39%	-12.68%
Spain	185	13.35	55.14%	1.50%	7.78%	-9.86%
Sri Lanka	18	15.76	5.56%	0.00%	40.42%	NA
Sweden	269	14.88	48.33%	1.07%	7.35%	16.90%
Switzerland	239	13.91	57.74%	2.47%	0.60%	-4.23%
Taiwan	233	16.76	11.16%	3.38%	9.86%	-8.45%
Thailand	278	15.40	22.66%	15.05%	27.09%	-9.86%
Turkey	83	12.63	59.04%	1.19%	30.77%	-28.17%
United Kingdom	1889	11.48	36.21%	7.17%	3.40%	9.86%
Venezuela	16	12.90	37.50%	0.00%	34.10%	-43.66%
Zimbabwe	8	8.39	25.00%	0.00%	46.13%	NA
Average	269	14.60	38.94%	3.67%	17.55%	-11.95%

Table A2 - Firm-level Correlation Matrix

The following table contains the correlation matrix for all five of the firm-level variables used throughout my analysis.

TABLE A2 - Firm Level Correlation Matrix

	<i>Politically Connected</i>	<i>Size</i>	<i>Return on Assets</i>	<i>Capital Intensity</i>	<i>Leverage</i>
<i>Politically Connected</i>	1.000				
<i>Size</i>	0.021	1.000			
<i>Return on Assets</i>	0.001	0.112	1.000		
<i>Capital Intensity</i>	0.015	0.135	0.043	1.000	
<i>Leverage</i>	0.039	0.084	-0.011	0.219	1.000

Table A3 - Industry-by-Industry Statistics for Cross-listing/Political Connections

TABLE A3 - Industry-by-Industry Average(s) for the Full Sample

Country	Number of Firms (in sample)	Average Firm Size (ln of total assets)	Incidence of Firms Cross-listing (% of all firms)	Incidence of Politically Connected Firms (% of all firms)
Basic Materials	1235	15.4	35.6%	3.2%
Consumer Goods	2155	14.8	27.3%	2.3%
Consumer Services	1429	14.4	36.0%	5.5%
Financials	2084	15.5	35.2%	5.1%
Health Care	357	14.2	54.9%	3.1%
Industrials	3219	14.9	29.3%	4.0%
Oil & Gas	257	14.9	57.6%	5.8%
Technology	592	13.7	59.1%	2.7%
Telecommunications	139	16.4	67.6%	7.2%
Utilities	245	16.2	43.7%	4.5%
All	11712	14.9	35.1%	4.0%

Figure A9 - Politically-connected vs. Unconnected Firm-level Summary Statistics

The following box plots illustrate the summary statistics for the key firm-level variables by political connection status, across countries. The edges of the box represent the 25% quartile and the 75% quartile; the solid line through the middle of the box represents the median; the solid black square represents the mean; the whiskers extend out to plus/minus two standard deviations from the mean; and, dots represent outliers.

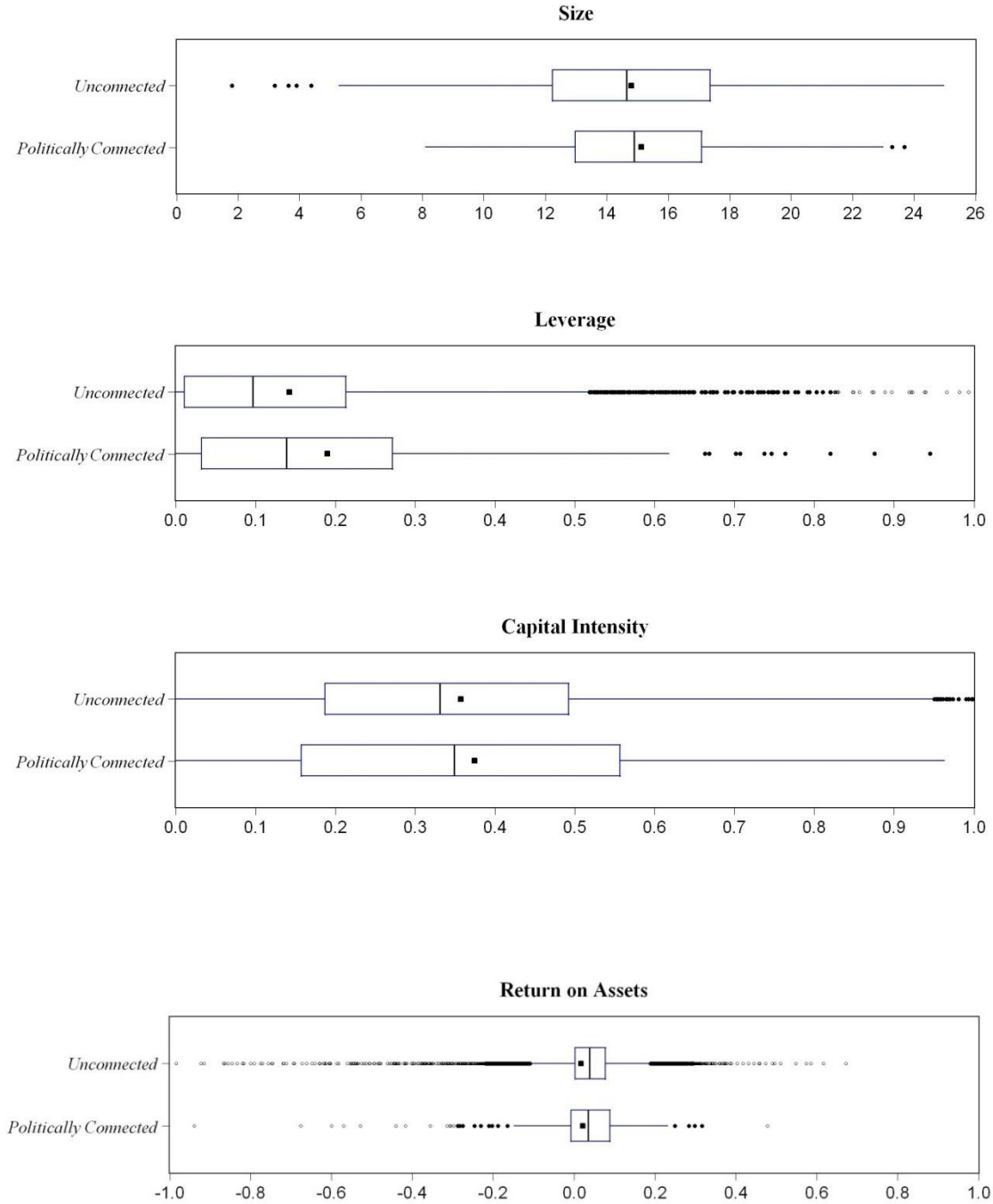


Table A4 – Size Measure Robustness Checks for Direct Effect of Political Connections Regressions

This regression tests alternative size metrics other than the standard log(Total Assets) used throughout the paper as a robustness check. This should help alleviate concerns that log(Total Assets) does not adequately control for firm size, since Total Assets and Total Assets squared should emphasize the larger firms even more rather than normalizing the data through a log transformation.

TABLE A4 - Size Robustness Checks -- Does being politically increase the probability of cross-listing?

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>			
Politically Connected	0.233 [†] (0.157)	0.718*** (0.135)	0.687*** (0.130)	0.253* (0.134)	
		<i>Firm Size Control Variable(s)</i>			
log(Total Assets)	0.718*** (0.028)			0.471*** (0.019)	
Total Assets		0.000** (0.000)		0.000*** (0.000)	
(Total Assets)²			0.000 (0.000)	0.000*** (0.000)	
		<i>Other Firm Level Controls</i>			
Return on Assets	-0.265 (0.433)	0.085* (0.050)	0.068* (0.041)	-0.132 (0.118)	
Capital Intensity	-0.660*** (0.182)	-0.426*** (0.149)	-0.269 (0.167)	-0.492*** (0.142)	
Leverage	-0.208 (0.252)	0.547*** (0.186)	0.376 (0.409)	0.071 (0.149)	
		<i>Industry Level Controls</i>			
Industry Dummies	Yes	Yes	Yes	Yes	
		<i>Country Level Controls</i>			
Country Dummies	Yes	Yes	Yes	Yes	
<i>Scenario being Tested:</i>		<i>Wald Tests (F-Statistics)</i>			
Joint Insignificance of Industry Dummies	37.654	39.918	36.635	30.310	
Joint Insignificance of Country Dummies	34.237	22.606	32.575	49.555	
Percentage of Obs. Predicted Correctly	79.27%	75.49%	74.31%	79.82%	
Number of Observations	7461	7461	7461	7461	

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level; † indicates significant 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. Column 1 replicates Column 4 of the Table in the body of the text of the paper. The results with alternative size measures emphasizing larger firms in Columns 2-4 show that the significance of the politically connected firm indicator actually become stronger when they are used. 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 A5 - Replication of Leuz and Oberholzer-Gee (2006) Baseline Regression

This regression replicates Leuz and Oberholzer-Gee's (2006) baseline regression on the role of political connections as a determinant (or deterrent) in firms' decisions to cross-list in Indonesia. This should be viewed as complimentary to the regression I run in Table 3 (Column 2) in the body of the paper because that regression is exactly the same; however, it is run on all countries with available data rather than being restricted to the Indonesian context. The difference between my regression and there are our measure of political connections and the year in which the underlying regression comes from. In their regression the coefficient of political connections is negative and statistically significant. In mine, it is negative; however, it is not statistically significant. The aforementioned difference may be why. Nevertheless, the results from this single country case are different than those found for the average case across countries as seen in Table 3 (Column 2).

TABLE A5 - Does being politically increase the probability of cross-listing? (All Countries, Indonesia Only)

<i>Dependent Variable:</i>	<i>Cross List Dummy</i>	
<i>Sample:</i>	<i>All Countries</i>	<i>Indonesia Only</i>
Politically Connected	0.583*** (0.123)	-0.303 (0.688)
	<i>Firm Level Controls</i>	
Size	-0.001 (0.008)	1.533*** (0.304)
Return on Assets	0.043 (0.040)	1.682 (3.069)
Capital Intensity	-0.473*** (0.153)	-0.626 (1.484)
Leverage	0.497 (0.380)	-0.098 (1.565)
	<i>Industry Level Controls</i>	
Industry Dummies	Yes	Yes
	<i>Country Level Controls</i>	
Country Dummies	No	n/a
<i>Scenario being Tested:</i>	<i>Wald Tests (F-Statistics)</i>	
Joint Insignificance of Industry Dummies	41.336	4.827
Percentage of Obs. Predicted Correctly	64.15%	83.81%
Number of Observations	7461	105

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level. In parentheses below the estimated coefficient values are Huber/White robust standard errors. Column 1 shows the same results as in column 2 of Table 3 as a benchmark. Column 2 shows Leuz and Oberholzer-Gee's (2006) baseline regression which was a single country study done in Indonesia only, replicated using my dataset which employs a slightly different measure of political connection. For the Wald tests, the null hypotheses is $H_0: \gamma_j=0$ for all j ; 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 A6 – Attempting to Unpack the Country Dummy Variables in the Multilevel Regressions

This table of regressions attempts to ‘unpack’ what is in the country dummy variable in the multi-level regression framework beyond contracting and property rights institutions. See Section 5.2 of the paper for a discussion of the challenges in assessing the importance of these additional co-variables.

TABLE A6 - 'Unpacking' the Country Dummy in the Multi-level Regression

<i>Dependent Variable:</i>	<i>Cross List Dummy</i>					
Property Rights Institutions	-6.551*** (0.448)	-1.466*** (0.509)	-6.468*** (0.512)	-6.815*** (0.877)	-5.124*** (0.929)	-6.662*** (0.963)
Contracting Institutions	1.154*** (0.420)	4.347*** (0.377)	1.88*** (0.444)	4.044*** (0.359)	6.839*** (0.526)	3.695*** (0.737)
Legal Origin, UK	0.689*** (0.117)		0.436*** (0.117)			2.198*** (0.33)
Legal Origin, France	0.784*** (0.141)		0.883*** (0.139)			2.692*** (0.613)
Legal Origin, Germany	-2.224*** (0.141)		-1.989*** (0.142)			0.100 (0.388)
Dip Parking Tix (FM)		0.034*** (0.003)	0.002 (0.007)			0.016* (0.010)
Trust Neighbors (WVS)				1.346*** (0.317)	2.42*** (0.477)	0.939* (0.632)
Religion Important (WVS)				3.677*** (0.363)	2.607*** (0.376)	0.591 (0.503)
Politics Important (WVS)				-4.545*** (0.361)	-3.235*** (0.425)	-0.818* (0.561)
Muslim %					0.035*** (0.004)	0.029*** (0.007)
Catholic %					0.020*** (0.002)	0.007* (0.006)
Protesant %					0.005*** (0.002)	0.028*** (0.003)
				<i>Firm Level Controls</i>		
Politically Connected	0.236** (0.136)	0.533*** (0.132)	0.388*** (0.135)	0.243* (0.158)	0.280** (0.153)	0.233* (0.159)
Size	0.305*** (0.017)	0.060*** (0.136)	0.281*** (0.017)	0.242*** (0.016)	0.298*** (0.018)	0.381*** (0.021)
Return on Assets	-0.070 (0.073)	0.049 (0.046)	-0.042 (0.047)	-0.026 (.051)	-0.055 (0.054)	-0.082* (0.06)
Capital Intensity	-0.561*** (0.137)	-0.544*** (0.137)	-0.503*** (0.142)	-1.045*** (0.164)	-0.873*** (0.163)	-0.746*** (0.165)
Leverage	0.027 (0.108)	0.639* (0.457)	0.165* (0.103)	0.168 (0.221)	0.127 (0.118)	0.124* (0.096)
				<i>Industry Level Controls</i>		
Industry Dummies	Yes	Yes	Yes	Yes	Yes	Yes
% of Obs. Predicted Correctly	71.26%	68.30%	71.91%	70.54%	73.12%	74.20%
Number of Observations	7191	6757	6757	5815	5815	5815

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level

Table A7 – Firm-level controls interacted with politically connected firm dummy variable

TABLE A7 - Robustness Check: Interactions of Firm Level Variables with Connectedness

<i>Dependent Variable:</i>		<i>Cross List Dummy</i>			
Politically Connected		0.233 [†]	-0.055		
		(0.157)	(0.787)		
		<i>Firm Level Controls</i>			
Size	0.721***	0.718***	0.719***	0.719***	
	(0.028)	(0.028)	(0.027)	(0.027)	
Return on Assets	-0.267	-0.265	-0.256	-0.256	
	(0.437)	(0.433)	(0.393)	(0.394)	
Capital Intensity	-0.670***	-0.660***	-0.632***	-0.631***	
	(0.182)	(0.182)	(0.188)	(0.188)	
Leverage	-0.195	-0.208	-0.224	-0.224	
	(0.252)	(0.252)	(0.267)	(0.266)	
Size * Connected			0.031	0.028	
			(0.052)	(0.020)	
Return on Assets * Connected			-1.101	-1.081	
			(1.400)	(1.344)	
Capital Intensity * Connected			-0.397	-0.404	
			(0.658)	(0.641)	
Leverage * Connected			-0.031	-0.024	
			(0.771)	(0.762)	
		<i>Industry Level Controls</i>			
Industry Dummies	Yes	Yes	Yes	Yes	
		<i>Country Level Controls</i>			
Country Dummies	Yes	Yes	Yes	Yes	
<i>Scenario being Tested:</i>		<i>Wald Tests (F-Statistics)</i>			
Joint Insig. of Ind. Dummies	33.990	34.029	34.116	34.133	
Joint Insig. of Country Dummies	34.409	34.237	34.683	34.684	
% of Obs. Predicted Correctly	79.31%	79.27%	79.27%	79.27%	
Number of Observations	7461	7461	7461	7461	

*** represents statistical significance at the 1% level; ** at the 5% level and * at the 10% level; † indicates significant 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. For the two Wald tests shown, 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. For the regression in the final two columns terms including the political connections dummy variable are jointly significant at between the 10% and 15% level just as they are in Column 2.