

Private Benefits of Control, Ownership, and the Cross-listing Decision

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ABSTRACT

This paper investigates how a foreign firm's decision to cross-list on a U.S. stock exchange is related to the consumption of private benefits of control by its controlling shareholders. Theory has proposed that when private benefits are high, controlling shareholders are less likely to choose to cross-list in the United States because of constraints on the consumption of private benefits resulting from such listings. Using several proxies for private benefits related to the control and cash flow ownership rights of controlling shareholders, we find support for this hypothesis with a sample of more than 4,000 firms from 31 countries.

ACADEMICS AND PRACTITIONERS HAVE PROPOSED many reasons why it is beneficial for foreign firms to cross-list their shares on U.S. markets.¹ One of the most often cited rationales for listing is to lower the cost of capital by overcoming investment barriers that segment capital markets. While this market segmentation view has had some success in explaining why firms benefit from listing, its saliency has diminished as barriers to international investment have sharply decreased for many countries and it cannot explain why relatively few firms list. More recent studies argue that U.S. cross-listings are beneficial because they limit the ability of controlling shareholders to extract private benefits from

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¹ See surveys by Karolyi (1998, 2006), Claessens, Klingebiel, and Schmukler (2002), and Benos and Weisbach (2004) that evaluate more than 150 published studies on the cross-listing decision.

the companies they control. If such corporate governance benefits exist, they impose costs on controlling shareholders. As a result, the private benefits view of cross-listing may help to explain why many firms choose not to list, even though the benefits of listing for minority shareholders are potentially large.

In this paper, we study the role that private benefits of control play in the cross-listing decision. We draw our motivation from the recent corporate governance literature that argues, and provides supporting evidence, that the additional investor protection provided by a U.S. exchange listing constrains the consumption of private benefits of control (see, among others, Coffee (1999, 2002), Stulz (1999), Reese and Weisbach (2002), Doidge (2004), Doidge, Karolyi, and Stulz (2004)). We hypothesize that controlling shareholders who consume more private benefits of control will be more reluctant to give them up and hence will be less likely to cross-list their firms on a U.S. exchange, despite the many benefits that may accrue to their firm from such a listing.

There are both direct and indirect constraints associated with listing that might deter controlling shareholders with high private control benefits from listing their firms. Direct constraints include constraints pertaining to laws, disclosures, and enforcement actions that controlling shareholders face when they cross-list on a U.S. exchange (see Coffee (1999, 2002), Stulz (1999)). Firms must register with the SEC and subject themselves to U.S. securities laws, which are enforced through an extensive scheme of civil and criminal penalties. U.S. securities laws not only mandate disclosure and financial reporting standards, but also seek to reduce agency costs and restrain controlling shareholders by imposing substantive obligations on them. An exchange-listed firm also has the potential to face enforcement actions initiated by the SEC or class-action lawsuits filed in U.S. courts. Cross-listing on a U.S. exchange can also impose indirect constraints on the extraction of private benefits by increasing the scrutiny of “gatekeepers” such as analysts and the media (see Stulz (1999)). Baker, Nofsinger, and Weaver (2002) and Lang, Lins, and Miller (2003) show that firms have more analyst and media coverage and forecast accuracy improves after cross-listing. Further, if foreign firms raise funds in the United States, these firms are subjected to the scrutiny of underwriters.

At the same time, there is evidence that suggests a cross-listing may be an ineffective device to constrain the consumption of private benefits. For example, Siegel (2005) points out that there are limits to U.S. regulatory enforcement. Additionally, foreign firms that list in the United States do not become equivalent to U.S. firms in terms of disclosure and governance standards and can often obtain waivers from complying with some of the governance standards the exchanges impose on U.S. firms. In framing our private benefits hypothesis, it is important to note that we do not argue that foreign firms listed in the United States are subject to the same monitoring or scrutiny that is applied to U.S. firms. Rather, what is important for our hypothesis is that foreign firms listed in the United States face more constraints and potential enforcement actions than similar home-country firms that are not listed in the United States.

We use ownership of control rights as a proxy for private benefits of control to investigate their potential role in firms’ listing decisions. When private benefits

of control are more valuable, it is optimal for controlling shareholders to hold more control rights and exert tighter control on the firm.² Therefore, we would expect firms in which controlling shareholders have greater ownership of voting rights, and, consequently, tighter control of the firm, to be less likely to be listed on a U.S. stock exchange because the controlling shareholders would have to give up more private benefits of control if these firms were listed. We investigate this hypothesis using ownership and control data on more than 4,000 firms from 31 countries in Asia, Europe, Latin America, and elsewhere. The data include the control rights for controlling shareholders, such as top managers and their family members, governments, other corporations, and financial institutions.

One of the advantages of our ownership data is that they allow us to implement tests in which we can distinguish between ownership of voting rights and ownership of cash flow rights. The corporate governance literature notes that if controlling shareholders have a majority of voting rights but own negligible cash flow rights, they have little incentive to take steps to increase the value of the firm's equity. However, as controlling shareholders' ownership of cash flow rights increases, any action they take to benefit themselves at the expense of other equity holders has a cost in that it decreases the value of the shares controlling shareholders own. This reasoning suggests that for a given level of cash flow rights, a firm is less likely to be listed on a U.S. exchange if the controlling shareholders' control rights exceed their holdings of cash flow rights. Thus, we use the separation between the control and cash flow rights held by controlling shareholders as an additional private benefit of control proxy.

In multinomial logistic regressions, we find that when controlling shareholders have high levels of control, their firms are less likely to be listed on a U.S. exchange (Nasdaq, American Stock Exchange (Amex), or New York Stock Exchange (NYSE)). Further, when controlling shareholders' control rights exceed their cash flow rights, their firms are less likely to be listed on a U.S. exchange. We also find that firms controlled by top managers and their families are less likely to have a U.S. exchange listing. In contrast, we specifically test for and find no reliable evidence that the probability a firm will have a listing that does not entail substantial new regulation, such as a Rule 144a private placement, a Level 1 over-the-counter (OTC) listing, or even a listing on the London Stock Exchange (LSE), is related to any of our private benefit proxies. These results are consistent with a private benefits explanation and are inconsistent with a market segmentation explanation, which makes no predictions about the role of private benefits in the listing decision.

Our evidence indicates that the direct constraints associated with listing on a U.S. exchange are an important deterrent for foreign firms with high-expected private control benefits. We next construct tests aimed at identifying, to the extent we can, whether indirect constraints such as the scrutiny of

² Private benefits cannot usually be measured directly at the firm level. Though control premia for block transactions (Dyck and Zingales (2004)) and the value of voting rights (Nenova (2003)) provide estimates of private benefits, these estimates can only be constructed for firms for which information about large block transactions is available and for firms with prices available for different share classes.

“gatekeepers” might also be a deterrent. Specifically, we test for and find that analyst coverage increases for all types of U.S. cross-listings, even those that do not require adoption of the most stringent U.S. securities regulation, such as Level 1 OTC listings and Rule 144a private placements. Given our previous finding that controlling shareholders do not avoid these Level 1 OTC listings and Rule 144a private placements and instead avoid only the high regulation listings on a major U.S. exchange, this suggests that direct U.S. securities laws and enforcement are a more important constraint in the extraction of private benefits than is the scrutiny of financial analysts.

We also test whether direct or indirect constraints on controlling shareholders reduce the negative valuation impact of a wedge between control and cash flow rights frequently observed in the literature (see La Porta et al. (2002), Claessens et al. (2002), Lins (2003)). We find that the valuation discount associated with the control and cash flow rights wedge is reduced for cross-listings on U.S. exchanges but not for other types of listings. If indirect constraints were equally important as direct constraints, we would expect all types of listings to reduce this valuation discount. This particular evidence therefore suggests that investors perceive that direct constraints are more binding on controlling shareholders than indirect constraints.

Many discussions of why firms cross-list focus on the benefit of a cross-listing in enabling firms to raise additional equity. The market segmentation hypothesis implies that a cross-listing reduces the cost of capital and hence makes it optimal for firms to raise additional capital to finance investments that were not profitable before the reduction in the cost of capital. Other explanations for why firms cross-list also suggest that firms do so to issue more equity. For instance, firms that cross-list on U.S. exchanges often have access to more efficient, deeper, and more liquid markets than the markets in their home country, so that equity issues that were not possible in their home country can be made. It is also possible that cross-listing increases the recognition of the firm, which enables it to issue equity. We call the explanations for cross-listing that emphasize the role of equity issuance and assume there are no governance benefits from cross-listing the equity issuance explanation. If firms cross-list to issue equity and do so for reasons that have nothing to do with the governance benefits of cross-listings, we could still expect a negative relation between controlling shareholder ownership and the listing decision. This potential explanation for our results arises because our multinomial logistic regressions are based on a cross-sectional analysis; specifically, we observe firms' ownership in 1995 or 1996 and we classify firms as cross-listed or not at the end of 1997. For firms that cross-listed before the year in which we observe their ownership, our inferences regarding U.S. exchange listings may be incorrect if the stake of the controlling shareholder was diluted because the listing enables the firm to raise capital. In such instances, listed firms might have low controlling shareholder ownership not because they had low private benefits before listing, but because they listed and issued equity.

To investigate whether an equity issuance explanation could be behind our results, we collect information about firms' ownership in the year prior to the

exchange-listing event. Although we are forced to use a smaller sample when we use prelisting ownership data, we find that the probability of being listed on a U.S. exchange is negatively related to two of our three private benefits proxies. Together, these results are inconsistent with an equity issuance explanation. Moreover, we also find that there is a negative relation between these private benefits of control proxies and the probability of a listing on a U.S. exchange even when no equity-raising activity has taken place.

If private benefits are an important deterrent to listing on a U.S. exchange, then, in a given year, the probability that a firm not yet listed on a U.S. exchange will choose to acquire a listing will be lower when controlling shareholders have a high level of control, when controlling shareholders' control rights exceed their cash flow rights, and when the firm is controlled by family members and top managers. Using our snapshot of ownership structures in 1995 or 1996 for firms that have not cross-listed, and prelisting ownership data for firms that list, we pursue an "event-time" experiment using a Cox (1972) proportional hazard model or "duration" analysis. This allows us to investigate which factors influence the likelihood of a U.S. exchange listing across sample firms in each year from 1995 going forward through 2001. The estimates of the Cox models provide further support for the private benefits explanation.

To provide additional robustness to our interpretation of the empirical results, we also investigate several ancillary predictions of our private benefits hypothesis. To the extent that listing enables firms to raise funds at lower cost, we would expect, everything else being equal, to see firms with better investment opportunities pursue listings and firms that have not yet listed to become more likely to do so following an improvement in their expected investment opportunities. We find strong support for these predictions. Also, controlling shareholders receive fewer private benefits in countries with better protection of investor rights (Nenova (2003), Dyck and Zingales (2004)). This suggests that, everything else being equal, firms in countries with better investor protection would be more likely to list. We also find that this is the case.

Putting all of our findings together, our investigation shows that the increased cost of consuming private benefits following a cross-listing on a U.S. exchange plays an important role in the cross-listing decision. Further, we find evidence that the direct constraints resulting from the adoption of U.S. laws, disclosures, and potential threats of enforcement or civil actions are the main constraints facing controlling shareholders when their firms cross-list. Our evidence on the role of gatekeepers, such as analysts, is that they do not appear to constrain controlling shareholders directly. However, our evidence is consistent with the view that they do so indirectly insofar as their monitoring can lead to enforcement actions that would not otherwise have taken place.

We also note several limitations to our analysis. For one, our data are limited in the sense that, in our broadest sample, we only observe firm ownership in 1995 or 1996 and we classify firms as cross-listed or not at the end of 1997. While we attempt to mitigate this limitation by gathering prelisting ownership data for exchange-listed firms, it could be that the ownership of firms that are not exchange listed has changed substantially (although we note that La Porta,

Lopez-de-Silanes, and Shleifer (1999) argue that ownership structures around the world appear to be relatively stable). Further, while our evidence points to the importance of private benefits in the cross-listing decision, they are clearly not the only determinants of cross-listing. In our models, we control for other important determinants of cross-listing such as size, growth, relative industry valuation, and so forth, but it could be the case that firm characteristics we do not observe are associated with cross-listing. Additionally, while we show that many of our results are inconsistent with some of the more obvious alternative explanations, such as market segmentation or equity issuance, we clearly cannot rule all possible alternatives out. Finally, if controlling shareholders hold more control rights for a reason unrelated to private benefits and if that reason to hold more control rights also makes a listing less valuable, our interpretation would be incorrect.

The paper proceeds as follows. In Section I, we present our data. In Section II, we investigate whether the controlling shareholders' control (e.g., voting) rights, as well as the difference between their control rights and cash flow rights, help further our understanding of which firms among the population of firms are cross-listed and where they are cross-listed. In Section III, we investigate whether direct or indirect constraints are important in the cross-listing decision by testing analyst following around different types of cross-listings and by testing how the valuation discount associated with a control and cash flow rights wedge relates to different cross-listings. In Section IV, we estimate a Cox proportional hazard model that allows us to relate the listing decision to changing firm attributes and to ownership. Conclusions follow in Section V.

I. Data

A. Sample Selection and Private Benefit Proxies

To obtain our ownership measures, we use two main data sets, each of which contains different measures of ownership and control for a broad sample of firms from a large number of countries around the world. These two data sets are compiled from the raw ownership and control data available for Western European firms from Faccio and Lang (2002); for emerging market firms from Lins (2003); and for East Asian firms from Claessens, Djankov, and Lang (2000).³ Ownership and control data for East Asian and emerging market firms are

³ Claessens et al. (2002) examine the impact of ownership structure on firm value in East Asia. Fan and Wong (2002) examine the effect of ownership structure on earnings informativeness. Both studies exclude Japan from their tests based on the argument that Japan has a unique *keiretsu* governance system that features very little individual ownership or control and instead is dominated by widely held financial institutions that control a web of group-linked companies. Our empirical tests comprise a broader range of countries. While some countries arguably have unique characteristics, it is difficult for us to apply a consistent screening principle indicating uniqueness in country-level governance. Thus, we are reluctant to exclude any one country. None of our inferences are weakened by removing Japan and some are strengthened.

from the 1995 and 1996 period and those for Western Europe range from 1996 to 1999, with the majority of sample observations occurring in 1996. Finally, we confine our analysis to nonfinancial firms to maintain consistency across the ownership and control-structure data sets. Because we need a variety of firm-level financial data, we use only firms covered by the *Worldscope* database. Also, to make firms across countries more comparable, we limit our sample to firms with total assets of at least \$10 million.

Claessens, Djankov, and Lang (2000), Faccio and Lang (2002), and Lins (2003) report ownership and control statistics that could proxy for a firm's internal corporate governance environment. For instance, they compute the percentage of total ultimate control rights held by the following types of blockholders: family/management, government, widely held corporations, widely held financials, and miscellaneous (which includes ownership by trusts, cooperatives, foundations, employees, etc.). From these data it is possible to identify the largest blockholder of a firm's control rights. Unfortunately, the ownership data presented in Claessens et al. (2000), Faccio and Lang (2002), and Lins (2003) are categorized using different algorithms. Faccio and Lang and Claessens, Djankov, and Lang report the separation of ownership and control only for the largest blockholder of their sample firms (which may not be the family/management group), while Lins reports this measure for all holdings of the family/management group (which may not be the largest blockholder). Given these difficulties, we conduct our tests on two firm-level governance data sets, both of which measure in different ways the likelihood that controlling shareholders will consume private control benefits.

The first data set is called the "Controlling Blockholder" data set and it uses data for the ultimate control rights and cash flow rights held by the largest blockholder as detailed in Claessens, Djankov, and Lang (2000) and Faccio and Lang (2002). Our classification of the largest blockholder as the controlling shareholder follows much of the literature on international corporate governance (La Porta et al. (1999), Claessens, Djankov, and Lang (2000)). To make it clear that the largest blockholder is assumed to be the controlling shareholder in this data set, we call the controlling shareholder the controlling blockholder when this data set is used. The primary benefit of this data set is that we can conduct tests using two private control benefits proxies: the control rights held by the controlling blockholder and the difference between this entity's control and cash flow rights. This allows us to focus on both the controlling blockholder's capability to pursue its own agenda as well as its incentives to refrain from consumption of private benefits. One drawback of this data set is that the largest blockholder can sometimes be an outside entity, such as a multinational corporation, which might be associated with a lower likelihood of consuming private control benefits compared to a large management or family blockholding. One other drawback of this data set is that it has relatively few firms from emerging market countries. In particular, this data set has no countries from Latin America. At the end of 1997, this data set contains 4,280 firms across 22 countries with complete firm-level data.

Our second data set is called the “Family/Management Control” data set. In this data set, we proxy for the private benefits of control by compiling the ultimate control rights held by a firm’s officers, directors, top-level managers, and their family members. Since it is the management group that actually administers a firm, the private benefits of control may be especially pronounced when we observe high levels of control held by top managers and their families. Ultimate family/management control rights can consistently be identified across the data sets of Faccio and Lang (2002), Lins (2003), and Claessens, Djankov, and Lang (2000). An advantage of this data set is that we can include more firms from emerging market countries; the disadvantage is that we cannot consistently identify family/management cash flow rights. At the end of 1997, there are 4,516 firms from 31 countries in the Family/Management Control data set.

In compiling the Family/Management Control data set, we seek to construct measures that indicate a firm’s managers are, in effect, in full control of the firm because, all else equal, the opportunity to expropriate minority shareholders will be highest when managers’ control of a firm cannot be challenged internally. Because effective managerial control depends on the control rights held by management, as well as the control rights held by outside blockholders, we use both a nominal and a relative measure of effective managerial control in our analysis. The nominal measure is the percentage of control rights held by the management group and its family. We expect that higher levels of managerial control rights correspond to more effective control of a firm. Our relative measure is an indicator variable set equal to one if the control rights held by a firm’s family/management group exceed those held by any other blockholding entity. The relative measure corresponds to the idea that high raw levels of control may not always be necessary to establish effective managerial control; rather, managers need only obtain sufficient control rights so that they can avoid being influenced by other blockholders.⁴

We acknowledge that the Family/Management Control data set allows us to focus only on the opportunity for expropriation and not on the incentives to expropriate. To measure such incentives, we would need data for the ultimate cash flow ownership stakes held by the management group and its family for all of our firms, which we do not have.⁵ However, it is possible that this limitation will not materially affect our inferences. To the extent that effective managerial control can be established at some level below 100%, control and cash flow rights will inherently be separated. Generally, managerial control of 51% of the shares will confer unequivocal control rights. In such a case,

⁴ Results using the nominal and relative measures are similar. We focus our presentation of results on the nominal family/management control rights measure because it corresponds more closely to the control rights variable used in the Controlling Blockholder data set.

⁵ While we do not have data to separate the effect of managerial cash flow rights from control rights, the analysis in Faccio and Lang (2002) and Lins (2003) suggests that, for our sample, ultimate managerial control rights often exceed cash flow rights because of pyramid ownership structures and superior voting shares.

controlling managers that divert one dollar from the firm for personal gain will bear at most 51 cents of the cost. Any further separation of control from cash flow rights via pyramids and superior voting shares may be a second-order effect.

B. Cross-listing Variables and Prelisting Ownership Data

In each data set, we compile a complete list of firms listed in the United States at the end of each year from 1995 to 2001. Firms can list via SEC Rule 144a (private equity issues), via a Level 1 American Depositary Receipt (ADR), which trades over-the-counter (OTC) on the Pink Sheets or on the OTC Bulletin Board, and via a direct listing of shares on Nasdaq, Amex or NYSE, via New York Registered Shares, or via Level 2 or Level 3 (capital-raising) ADRs. To determine whether a firm is listed, we use information obtained from the Bank of New York, Citibank, NYSE, and Nasdaq. Listing dates are verified using Lexis-Nexis searches and by examining firms' annual reports as well as Form 20-F's filed with the SEC. In the Controlling Blockholder data set, 130 firms have a U.S. exchange listing and 218 firms have either a Rule 144a or Level 1 OTC listing as of 1997. In the Family/Management Control data set, 154 firms have an exchange listing and 244 have a Rule 144a or Level 1 listing.

We also obtain data on whether a firm is cross-listed in London from the LSE. In the Controlling Blockholder data set, there are 85 firms with London listings that meet our data requirements. Firms from 13 different countries have a London listing, with the most listings by firms from Ireland, Japan, Sweden, Germany, and South Korea. In the Family/Management Control data set, there are 108 firms with London listings that meet our data requirements. Firms from 15 different countries have a London listing, with the most listings by firms from Ireland, South Africa, Japan, Sweden, Germany, and South Korea. Some firms have a London listing as well as some type of U.S. listing. We always classify a firm by its most restrictive listing in terms of disclosure and governance requirements. Consequently, a firm with a London listing and a U.S. exchange listing is classified as having a U.S. listing; similarly, a firm with a Rule 144a/Level 1 OTC listing and a London listing is classified as having a London listing.

To investigate whether an equity issuance explanation could be behind our results, we also gather data on ownership of firms cross-listed on U.S. exchanges for the year prior to the cross-listing event. To assemble this data, we use firms' SEC filings, such as prospectuses for firms that raised equity capital, Form 20-F, which is required for all firms listed on a U.S. exchange, and in some cases, annual reports. Of the 4,280 firms in the Controlling Blockholder data set, 130 are U.S. exchange-listed firms for which we have ownership data from 1995 or 1996. We also searched for prelisting data for the 130 firms cross-listed on U.S. exchanges and identified ownership information for 110 out of 130 firms. For this subsample, we remove from the analysis 30 firms for which the U.S. listing

was part of an IPO or a privatization.⁶ In total, we have prelisting ownership for 80 firms in this data set. Of the 4,516 firms in the Family/Management Control data set, 154 are listed on U.S. exchanges, for which we have ownership data from 1995 or 1996. We are successful in collecting ownership information in the year prior to the U.S. listing for 133 out of the 154 firms. After removing 33 firms from this subsample that list as part of an IPO or a privatization, we have prelisting ownership data for 100 U.S. exchange-listed firms.

C. Control Variables

In our analysis, we include a number of firm- and country-level control variables.⁷ At the firm level, we include an indicator variable that denotes the presence of an additional blockholder with at least 10% of the voting rights (“2nd blockholder”). An additional large blockholder may serve to mitigate the actions of the controlling blockholder that are not in the interests of minority shareholders. However, it is also possible that the second blockholder’s nontrivial control stake could allow it to share some private benefits of control with the controlling blockholder. We control for growth opportunities using two proxies: sales growth over the last 2 years (“*Sales growth*”) and the median q of the global industry that a firm belongs to (“*Global industry q*”). We expect that controlling shareholders will be more likely to forgo private benefits of control if the need for external financing to fund growth opportunities is greater. Our sales growth proxy is a 2-year geometric average of annual inflation-adjusted growth in sales. Because Worldscope data for many countries is relatively sparse prior to 1994, this reduces our sample size by about 250 firms compared to using a 1-year sales growth measure. For robustness, we reestimate all of our models using a 1-year sales growth measure and find results that are virtually identical in magnitude and significance.

The existing literature suggests that a benefit for firms that list in the United States is access to deep capital markets, so that firms that access U.S. markets can relax financial constraints that arise because of imperfections in their home capital markets. For example, Lins, Strickland, and Zenner (2005) provide evidence that the capital expenditures of firms depend less on their cash flow after listing. This benefit of a listing would not be important for firms with financial flexibility. We therefore use in our models an index of financial flexibility.

⁶ We remove these firms for two reasons. First, the extremely high pre-listing control stakes (80% to 100%) of these firms are not meaningful for our theory because they do not correspond to control stakes that reflect the ability of the controlling shareholders to exploit private benefits in the future since the controlling shareholders are in the process of reducing their holdings. Second, the decisions made around privatizations often have a political component that is not part of our theory. For example, Hung, Wong, and Zhang (2007) show the cross-listing decision of Chinese SOEs is best explained by political connectedness of the CEO.

⁷ We use the same set of control variables in both the Controlling Blockholder data set and the Family/Management Control data set, with two exceptions. We do not have data for the 2nd blockholder dummy or the *Government owned* dummy in the Family/Management Control data set.

Firms with high financial constraints have low financial flexibility. Kaplan and Zingales (1997) construct a firm-level index of financial constraints (the KZ index). However, the ordered logit coefficients used to construct the KZ index are estimated from a sample of 49 low-dividend-paying U.S. manufacturing firms. We are not aware of any research showing that the ordered logit coefficients would be similar for samples of non-U.S. firms. Instead of using these coefficients, we construct a simple index of financial flexibility (*Financial flexibility index*). The *Financial flexibility index* is constructed as a count variable by adding one point for a firm with high cash and liquid assets, one point for high dividends, and one more point for low capital expenditures. For each firm, we identify whether their cash and liquid asset holdings are high or not depending on whether they are greater than the 75th percentile among firms within their country. We apply a similar rule based on whether a firm's dividends and capital expenditures are greater than the 75th percentile or below the 25th percentile among firms within their country.

In addition to proxies for growth opportunities and financial flexibility, we include *Leverage*, which is total debt divided by the total assets of the firm. Firms that have higher leverage (prior to listing) might be more likely to pursue a U.S. listing to raise new equity capital. Firm size, as proxied by the natural logarithm of total assets in U.S. dollars (*Log assets*), is included to control for cross-listing economies of scale. These economies of scale reflect the lower proportional fixed costs and potential benefits that increase with firm size. Firm profitability, as proxied by the return on assets (*ROA*), is included to control for the possibility that higher quality firms may be more likely to cross-list in order to signal their quality. Also, we would expect firms with international activities to be more subject to the discipline resulting from cross-listing their shares in the United States. For instance, it would be easier for minority shareholders to recover damages from a non-U.S. firm with U.S. assets than from one that has only domestic assets. Further, firms with a larger presence in foreign product markets may be expected to pursue cross-listings as a complement to an overall strategy of internationalization (Pagano, Roell, and Zechner, (2002)). As an indicator for the degree of international orientation, we include the ratio of foreign sales to total sales (*Foreign sales*).⁸ Finally, we include an indicator variable that equals one if the state is a firm's largest shareholder (*Government owned*) under the premise that governments might take into account different tradeoffs from private controlling shareholders. For instance, the government might be privatizing to raise funds, in which case it might choose to cross-list in more liquid markets in order to increase the proceeds from the sale of shares.

We also employ a number of country-level control variables. As noted earlier, controlling shareholders receive fewer private benefits in countries with better

⁸ Similar to Pagano, Roell, and Zechner (2002), we find that foreign sales data is missing for a significant fraction of the sample firms. Therefore, we follow the procedure they outline to impute missing values via regressions that generate predicted values of foreign sales—see footnote 22 on page 2678 in Pagano, Roell, and Zechner for further details. For robustness, we repeat all of our regressions using the actual foreign sales data on a necessarily smaller sample. None of the results reported in the paper are affected.

protection of investor rights. Therefore, we include a proxy for investor protection in a firm's home country. We estimate models with a number of different proxies such as the antidirector rights index, the index of judicial efficiency, and legal origin from La Porta et al. (1998). Following La Porta et al. we create the dummy variable "*Civil law*," which equals one for firms from countries with a civil law tradition and zero for firms from countries with a common law tradition. We also estimate models featuring three indices from La Porta, Lopez-de-Silanes, and Shleifer (2006), namely, their disclosure index, which measures the quality of disclosure requirements; their burden of proof index, which measures liability standards; and finally, their investor protection index, which is the principal component of disclosure, liability standards, and antidirector rights. While each of these investor protection variables has its own merits, we find that the specific choice of proxy makes little difference to our results. For brevity, we report only the results using the legal origin of the firm's home country as indicated by the *Civil law* dummy variable.

Recent research by Sarkissian and Schill (2004) shows that if a firm decides to cross-list, it is more likely to cross-list in a country that has more ties with the firm's home country. We therefore include a country characteristic that measures the extent to which the country interacts economically with the United States. This variable, which we call "*Economic proximity*," is from the 1996 International Trade Statistics Yearbook. It is the percentage of a given country's exports going to the United States (or to the U.K. for London listings). We also estimate our models using the historical 3-year stock market correlation (weekly dollar-denominated returns) with the U.S. market index as a proxy for familiarity. Finally, we control for overall economic development with the log of GNP per capita ("*Log of GNP per capita*"). As a final alternative, rather than using specific country characteristics as control variables, when it is possible, we include country dummies. This approach has the advantage of allowing us to control for all country effects.

D. Summary Statistics

We summarize basic firm-level governance statistics for each data set in Table I and break these out into three groups: firms without a U.S. listing as of year-end 1997, firms with a Rule 144a/Level 1 OTC listing, and firms with a U.S. exchange listing.⁹ Comparing firms with an exchange listing and firms

⁹ In terms of coverage, the ownership data sets that we use are very comprehensive when compared to data sets used in other studies. However, we recognize that we do not have complete coverage for all firms that have financial data available in Worldscope, which leads to two potential concerns. First, in terms of firm characteristics, the firms for which we have ownership data may be different from firms for which we do not have ownership data. Lins (2003) addresses this issue and finds no significant differences between the two groups of firms. Second, requiring ownership data may have an impact on the fraction of firms that have a U.S. exchange listing in our study. To address this concern, we compare the data set that is constrained to include ownership data to one that is not constrained. The mean (median) fraction of firms listed in the United States is similar in both samples and the differences across countries are not significant.

Table I
Summary Statistics for 1997

Summary statistics are computed as of December 31, 1997 for firms that are not cross-listed, for firms that are cross-listed via Rule 144a or Level 1 OTC ADRs, and for firms cross-listed on a U.S. exchange via a direct listing or via Level 2 or 3 ADRs. Data sources for information on U.S. listings and ownership are described in Section I. We use all firms available in the Worldwide for which we have ownership data except for financial firms and firms with assets less than \$10 million. Total assets (\$ billions), from Worldwide, is the average total assets of sample firms in a country. Panel A presents results using the Controlling Blockholder Data set. *Control rights* (*CF rights*) is equal to the average voting rights (cash flow rights) held by the controlling blockholder. *CR > CF* is the frequency with which control rights exceeds cash flow rights. Panel B presents results for the Family/Management Control Data set. *F/M control rights* are the voting rights held by the management group or controlling family. *F/M largest BH* is the frequency with which the management group or controlling family is the largest blockholder.

Country	Panel A: Controlling Blockholder Data Set									
	Not Cross-listed					Rule 144a/Level 1 OTC ADRs				
	Total Assets	Control Rights	CF Rights	CR > CF	N	Total Assets	Control Rights	CF Rights	CR > CF	N
Austria	43	0.45	0.56	0.48	9	2.87	0.46	0.44	0.11	0
Belgium	62	1.78	0.38	0.35	1	8.51	0.29	0.29	0.00	1
Finland	55	0.71	0.31	0.26	4	1.28	0.47	0.44	0.25	2
France	334	1.07	0.50	0.49	14	10.58	0.21	0.17	0.29	15
Germany	398	1.10	0.55	0.49	11	18.23	0.24	0.21	0.36	6
Hong Kong	98	0.56	0.35	0.30	42	1.91	0.30	0.26	0.33	1
Indonesia	89	0.44	0.37	0.30	1	0.58	0.42	0.34	1.00	2
Ireland	28	0.22	0.21	0.17	0	—	—	—	—	4
Italy	90	2.39	0.48	0.38	5	1.94	0.45	0.42	0.20	5
Japan	848	1.92	0.10	0.07	13	13.67	0.04	0.02	0.31	13
Malaysia	107	0.45	0.34	0.27	5	1.60	0.25	0.22	0.20	0
Norway	65	0.34	0.31	0.25	10	2.01	0.24	0.17	0.80	5
Philippines	41	0.19	0.29	0.26	9	1.23	0.27	0.24	0.44	1
Portugal	39	0.38	0.41	0.40	1	1.54	0.00	0.00	0.00	2
Singapore	119	0.34	0.31	0.24	8	3.72	0.26	0.14	0.75	1
South Korea	183	0.91	0.20	0.17	11	6.62	0.08	0.07	0.18	3
Spain	82	1.16	0.35	0.32	1	4.31	0.42	0.31	1.00	3
Sweden	97	0.77	0.26	0.20	6	2.97	0.25	0.18	0.67	8
Switzerland	103	0.83	0.42	0.30	4	28.96	0.29	0.16	0.50	3
Taiwan	86	0.53	0.23	0.18	11	1.74	0.25	0.23	0.27	0
Thailand	76	0.37	0.39	0.37	5	1.10	0.30	0.30	0.00	0
U.K.	889	0.32	0.20	0.18	47	3.69	0.11	0.10	0.11	55
Total	3,932				218					130
Mean	0.78	0.34	0.29	0.37		5.67	0.27	0.22	0.37	
Median	0.55	0.35	0.29	0.35		2.87	0.26	0.22	0.29	

(continued)

Table I—Continued

Panel B: Family/Management Control Data Set												
Country	Not Cross-listed				Rule 144a/Level 1 OTC ADRs				U.S. Exchange Listings			
	N	Total Assets	F/M Control Rights	F/M Largest BH	N	Total Assets	F/M Control Rights	F/M Largest BH	N	Total Assets	F/M Control Rights	F/M Largest BH
Argentina	2	1.21	0.00	0.00	0	—	—	—	4	6.69	0.00	0.00
Austria	43	0.45	0.44	0.65	9	2.87	0.25	0.44	0	—	—	—
Belgium	62	1.78	0.37	0.74	1	8.51	0.29	1.00	1	10.87	0.00	0.00
Brazil	17	1.43	0.14	0.29	10	2.02	0.22	0.30	1	7.82	0.00	0.00
Chile	9	1.17	0.50	0.89	0	—	—	—	5	6.17	0.20	0.60
Czech	7	0.14	0.28	0.71	0	—	—	—	0	—	—	—
Finland	55	0.71	0.33	0.65	4	1.28	0.04	0.25	2	4.08	0.00	0.00
France	335	1.07	0.50	0.82	14	10.58	0.18	0.57	15	16.03	0.18	0.53
Germany	402	1.10	0.54	0.82	11	18.23	0.24	0.55	6	40.70	0.03	0.17
Hong Kong	114	0.53	0.41	0.81	43	1.92	0.39	0.79	2	3.21	0.20	0.50
Indonesia	46	0.28	0.38	0.65	0	—	—	—	2	2.53	0.00	0.00
Ireland	28	0.22	0.12	0.25	0	—	—	—	4	2.90	0.05	0.25
Israel	4	1.65	0.26	0.50	0	—	—	—	6	1.05	0.35	0.67
Italy	90	2.39	0.46	0.80	5	1.94	0.45	1.00	5	27.72	0.28	0.60
Japan	848	1.92	0.03	0.15	13	13.67	0.00	0.00	13	33.70	0.02	0.15
Malaysia	199	0.39	0.30	0.70	7	1.37	0.33	0.86	0	—	—	—
Norway	68	0.33	0.25	0.59	10	2.01	0.22	0.50	5	4.69	0.22	0.60
Peru	2	0.32	0.25	0.50	1	0.21	0.25	1.00	2	1.82	0.00	0.00
Philippines	22	0.17	0.48	0.82	9	1.27	0.31	0.67	1	4.86	0.23	0.00
Portugal	40	0.38	0.39	0.80	2	1.17	0.28	0.50	2	9.11	0.00	0.00
Singapore	111	0.31	0.34	0.67	7	4.19	0.04	0.14	1	0.63	0.58	1.00
South Africa	55	0.59	0.45	0.73	13	1.75	0.25	0.46	4	1.65	0.00	0.00
South Korea	141	1.01	0.17	0.76	10	7.23	0.04	0.30	3	13.65	0.06	0.33
Spain	82	1.16	0.32	0.57	1	4.31	0.00	0.00	2	22.47	0.00	0.00
Sri Lanka	5	0.07	0.18	0.80	0	—	—	—	0	—	—	—
Sweden	97	0.77	0.25	0.59	7	3.27	0.22	0.57	8	8.13	0.03	0.13
Switzerland	103	0.83	0.40	0.70	4	28.96	0.37	0.75	3	2.95	0.36	0.67
Taiwan	101	0.42	0.18	0.83	10	0.78	0.18	0.80	2	2.26	0.08	0.50
Thailand	125	0.25	0.21	0.54	6	0.98	0.19	0.50	0	—	—	—
Turkey	16	0.23	0.20	0.31	0	—	—	—	0	—	—	—
U.K.	889	0.32	0.16	0.47	47	3.69	0.10	0.40	55	8.94	0.14	0.35
Total	4,118				244				154			
Mean		0.76	0.30	0.62		5.31	0.21	0.54		9.79	0.12	0.28
Median		0.53	0.30	0.67		2.02	0.22	0.50		6.17	0.05	0.17

that do not have any type of U.S. listing, Panel A shows that in the Controlling Blockholder data set, 130 firms are listed on a U.S. exchange, while 3,932 firms do not have a U.S. listing. Overall, we find that firms listed in the United States are significantly larger, as measured by total assets (in \$ billions), than firms that are not listed (p -value of the t -test of equality of means across the two subsamples is less than 0.01). Panel A also shows that mean control rights held by the controlling blockholder are higher for firms that are not listed on a U.S. exchange (p -value equals 0.04). At the same time, mean cash flow rights held by the controlling blockholder are not significantly higher for the firms that are not cross-listed (p -value equals 0.11). Tests on differences between the medians of these variables for listed and nonlisted subsamples show virtually identical significance levels.

Another primary variable of interest in the Controlling Blockholder data set is the separation of control and cash flow rights held by the largest blockholder. To measure the control wedge—the difference between control rights and cash flow rights—we use the percentage point difference in the control rights and cash flow rights held by the largest blockholder (see Claessens et al. (2002)). Such a measure directly assesses the percentage of control rights held for which there are no corresponding cash flow consequences of exercising the control.

In Panel A, we report the frequency with which the controlling blockholder's control rights exceed its cash flow rights (for the sake of brevity, we do not report mean percentage point values for this separation, conditional upon such a separation being present). We find that a separation between control and cash flow rights occurs with greater frequency in firms that are not listed in the United States (p -value equals 0.05). Finally, although it is not reported in the table, we note that there is no statistically significant difference in the frequency of the presence of a secondary blockholder between firms that have exchange listings and those that do not (p -value equals 0.46).

For nonexchange cross-listings, Panel A shows there are more firms with a Rule 144a or Level 1 OTC listing (218) than an exchange listing (130). Firms with a primary listing in Hong Kong (42) have almost as many Rule 144a/Level 1 OTC listings as U.K. firms (47). As we would expect, firms that have a Rule 144a/Level 1 OTC listing are smaller, have more concentrated control rights ownership, have more concentrated cash flow ownership, and are more likely to have a wedge than firms that have an exchange listing. Along these dimensions, firms with a Rule 144a/Level 1 OTC listing are between exchange-listed firms and firms that do not have a U.S. listing.

In Panel B of Table I, we report summary statistics for the Family/Management Control data set. Similar to the Controlling Blockholder data set, relatively few firms have a U.S. exchange listing (154 firms have a listing compared to 4,118 firms that do not have a U.S. listing), and those that are listed are much larger in size than those that are not (p -value less than 0.01). The panel also shows that mean levels of family/management control rights are significantly higher for firms that are not listed in the United States (30% vs. 12%, p -value of difference is 0.00). Similarly, the frequency with which the

family/management group holds the most control rights is significantly higher for firms that are not listed in the United States (p -value less than 0.01). As with Panel A of Table I, the size and private benefit proxies of firms with a Rule 144a/Level 1 OTC listing fall between those of firms that do not list and those of firms that have an exchange listing. Overall, the correlation across countries in the average control rights between the Controlling Blockholder and Family/Management Control data sets is 0.86 across all firms.¹⁰

Taken together, the summary statistics reported in Table I are consistent with our hypotheses that when controlling shareholders have more control rights and when there is a separation between control rights and cash flow rights, they are reluctant to subject themselves to the higher levels of disclosure and monitoring associated with a U.S. exchange listing. In the next section, we examine these hypotheses in more detail.

For the (untabulated) prelisting ownership information, we collected from prospectuses and Form 20-F filings for firms with exchange listings, prelisting cash flow ownership of controlling blockholders averages 18.95%, which is statistically significantly higher than the postlisting cash flow ownership of 14.05% (p -value of 0.02). Similarly, prelisting ownership of control rights of controlling blockholders exceeds postlisting ownership of control rights. The prelisting ownership stake averages 20.27% while the postlisting average is only 16.48%, also a statistically significant difference (p -value of 0.075). In contrast to the results with the Controlling Blockholder data set, there is no significant difference between prelisting and postlisting ownership of control rights in the Family/Management Control data set.

II. Is the Probability of Cross-listing Related to Ownership and Control?

In this section, we investigate the relation between private benefits and the probability a firm is cross-listed. If cross-listing significantly reduces the private benefits that controlling shareholders can extract from the firm they control, we expect that firms in which private benefits are large will avoid a U.S. exchange listing, all else equal. Further, this effect should be concentrated in firms that pursue a listing on a U.S. exchange, such as the NYSE, American, or Nasdaq stock exchanges, because of the more stringent governance and disclosure requirements associated with such listings. Firms that choose London listings, Rule 144a private placements, or Level 1 OTC cross-listings are generally not subject to the same SEC registration costs, continuous disclosure and reporting

¹⁰ For firms that are in both the Controlling Blockholder and Family/Management Control data sets, we compute a Kolmogorov–Smirnov test statistic of the equality of distributions for each measure of control rights for each country. The Kolmogorov–Smirnov test rejects the equality of the distributions for 16 of 22 countries. Given that the measures of control rights are distinct and capture different aspects of control, it is not surprising that the distributions are different. Moreover, a key feature of our test design is the fact that we employ two differently constructed data sets of control and cash flow rights and reach similar conclusions with each data set.

requirements, and subsequent monitoring by market agents—or, in the case of a London listing, to an equivalent mechanism.¹¹

We begin by examining whether ownership of cash flow rights and control rights is related to whether a firm has a listing in London (“*London*”), whether it has a Rule 144a/Level 1 OTC U.S. listing (“*144a/Level 1*”), and whether it has a listing on a U.S. exchange (“*Exchange*”), after controlling for other firm-level variables as well as country-level variables.¹² Our approach in conducting this investigation is to estimate multinomial logistic regressions with the three listing choices. The ownership data are measured in 1995 or 1996 and the firm and country characteristics are measured at the end of 1997. As a robustness check, we repeat all of the regressions using data for 1996 and find similar results. In all models, we report marginal effects evaluated at the average values of the independent variables (marginal effects for the intercept are not computed or reported). Marginal effects for dummy variables are calculated as the discrete change in the expected value of the dependent variable as the dummy variable changes from a value of zero to one. The standard errors are computed assuming that observations are independent across countries, but not within countries, so that the standard errors are clustered at the country level.

Panel A of Table II reports results using the Controlling Blockholder data set. The marginal effect for control rights (“*Control rights*”) is negative and significant (−0.0074 with a *t*-statistic of −2.72) for U.S. exchange listings and is insignificant for the other listings. The coefficient for U.S. listings corresponds to a 0.74% lower probability of a listing for a 1% increase in control rights. This constitutes an economically large decrease relative to the average probability of a U.S. exchange listing, which is 3.04% (130 out of 4,280 firms in the sample). We find that the *2nd blockholder* dummy variable has a significantly positive coefficient for nonexchange U.S. listings but not for U.S. exchange listings or London listings.

Though many of the firm-level characteristics are important in explaining whether a firm has a U.S. exchange listing, none help explain why a firm has a London listing. For U.S. exchange listings, *Global industry q* is positive and statistically significant. The marginal effect of firm size (*Log assets*) is also positive and statistically significant, which is consistent with the hypothesis that larger firms are more likely to find the costs of cross-listing lower and the benefits larger. Finally, firms with a more international orientation, as proxied by *Foreign sales*, are more likely to have a cross-listing; the economic importance of this relationship is strong with a 1% increase in foreign sales associated with

¹¹ See Baker, Nofsinger, and Weaver (2002) and Salva (2003) for a description of the listing requirements for foreign firms at the London Stock Exchange.

¹² We group the Rule 144a private placements and Level 1 OTC listings together for two reasons. First, they both constitute a U.S. market presence but in a form that affords relatively less liquid trading than on major exchanges and that represents easier access in terms of registration and disclosure requirements. Second, by grouping them together, we alleviate concerns that we may not find a significant effect due to a small sample size.

Table II
Cross-sectional Multinomial Logistic Regression: The Choice of Listing Type

The multinomial logistic regressions estimate the probability that a firm is cross-listed as of December 31, 1997. The table reports marginal effects evaluated at the means of the independent variables. Data sources for information on U.S. listings, London listings, and ownerships are described in Section I. There are four categories: not cross-listed (base category), London listings (*London*), Rule 144a or Level 1 OTC listings (*144a/Level 1*), and U.S. exchange listings (*Exchange*). We use all firms available in Worldscope for which we have ownership data except for financial firms and firms with assets less than \$10 million. Panel A shows results using the controlling blockholder's control rights (*Control rights*), or voting rights held by the controlling blockholder; Panel B shows results using the controlling blockholder's cash flow rights (*Cash flow rights*) and the difference between his control rights and cash flow rights (*Control – cash*); and Panel C shows results using family/management control rights (*Family/Mgmt control rights*), or fraction of voting rights held by the family/management group). *Second blockholder* (Panels A and B) equals one if there is another blockholder with at least 10% of the voting rights. *Government owned* (Panels A and B) equals one if the state is the controlling shareholder. All accounting data are from Worldscope. *Sales growth* is inflation adjusted 2-year sales growth. *Global industry q* is the firm's median global industry *q*. Total assets are in \$ thousands. *Foreign sales* is the percentage of foreign sales to total sales. *Leverage* is total debt divided by total assets. The *Financial flexibility index* ranges from 0–3, where higher values indicate that firms have more flexibility. *Civil law* is a dummy variable that equals one if the country's legal origin is based on civil law (from La Porta et al. 1998). *Log of GNP per capita* (\$) is from the World Bank WDI Database. *Economic proximity*, from the 1996 International Trade Statistics Yearbook, is the percentage of country *i*'s exports going to the United States (to the U.K. for London listings). Pseudo- R^2 is a goodness-of-fit measure based on the difference between unrestricted and restricted likelihood functions. The *t*-statistics reported in parentheses are adjusted for clustering on countries—they are computed assuming observations are independent across countries, but not within countries. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A			Panel B			Panel C		
	London	144a/Level 1	Exchange	London	144a/Level 1	Exchange	London	144a/Level 1	Exchange
Control rights	−0.0014 (−0.27)	−0.0210 (−1.48)	−0.0074 (−2.72)***						
Cash flow rights				−0.0015 (−0.26)	−0.0183 (−1.25)	−0.0063 (−2.13)**			
Control – cash				0.0022 (0.45)	−0.0517 (−1.51)	−0.0208 (−2.86)***			
2 nd blockholder	0.0004 (0.30)	0.0117 (1.68)*	0.0006 (0.57)	0.0003 (0.27)	0.0121 (1.78)*	0.0007 (0.74)			
Family/Mgmt control rights							−0.0030 (−1.12)	0.0011 (0.09)	−0.0082 (−2.54)**
Sales growth	0.0010 (0.30)	−0.0116 (−1.32)	0.0033 (1.76)*	0.0009 (0.28)	−0.0116 (−1.32)	0.0033 (1.85)*	0.0026 (1.35)	−0.0125 (−1.07)	0.0044 (1.35)

Global industry q	0.0002 (0.16)	0.0265 (2.47)**	0.0102 (5.42)**	0.0003 (0.19)	0.0261 (2.49)**	0.0098 (4.81)***	0.0020 (0.49)	0.0302 (2.76)***	0.0147 (3.66)***
Log assets	0.0004 (0.26)	0.0167 (4.30)**	0.0046 (7.08)**	0.0003 (0.24)	0.0167 (4.25)**	0.0046 (6.73)***	0.0008 (0.65)	0.0193 (4.49)***	0.0063 (4.15)***
Foreign sales	0.0028 (0.32)	0.0418 (3.21)**	0.0091 (5.51)**	0.0023 (0.29)	0.0421 (3.22)**	0.0091 (5.60)**	0.0062 (1.43)	0.0471 (3.70)**	0.0140 (3.50)**
Leverage	0.0010 (0.43)	0.0002 (0.01)	-0.0012 (-0.24)	0.0009 (0.37)	-0.0004 (-0.03)	-0.0016 (-0.32)	0.0018 (0.57)	-0.0017 (-0.11)	-0.0049 (-0.85)
Financial flexibility	0.0001 (0.24)	-0.0005 (-0.20)	0.0007 (0.89)	0.0001 (0.24)	-0.0006 (-0.26)	0.0007 (0.81)	-0.0001 (-0.38)	-0.0007 (-0.29)	0.0004 (0.38)
index	0.0000 (0.29)	0.0005 (1.00)	0.0000 (0.27)	0.0000 (0.27)	0.0005 (1.01)	0.0000 (0.30)	0.0000 (0.57)	0.0003 (0.94)	0.0001 (0.67)
ROA									
Government owned	-0.0006 (-0.25)	0.0026 (0.36)	0.0006 (0.31)	-0.0005 (-0.24)	0.0028 (0.36)	0.0005 (0.27)			
Civil	-0.0065 (-0.84)	-0.0285 (-1.77)*	-0.0086 (-2.14)**	-0.0068 (-0.48)	-0.0279 (-1.72)*	-0.0081 (-2.12)**	-0.0077 (-0.88)	-0.0284 (-1.79)*	-0.0081 (-1.78)*
Economic proximity	-0.0003 (-0.89)	0.0004 (0.47)	-0.0002 (-1.88)*	-0.0002 (-0.71)	0.0004 (0.48)	-0.0002 (-1.79)*	-0.0006 (-0.37)	-0.0157 (-2.84)***	-0.0041 (-2.39)**
Log of GNP per capita	-0.0008 (-0.54)	-0.0111 (-2.23)**	-0.0019 (-1.31)	-0.0007 (-0.48)	-0.0111 (-2.26)**	-0.0019 (-1.39)	-0.0003 (-1.08)	0.0004 (0.54)	-0.0001 (-0.56)
Number of observations		4,280			4,280			4,516	
Pseudo- R^2		0.3010			0.3038			0.2787	

approximately a 0.91% increase in the probability of listing.¹³ *Leverage*, *ROA*, *Financial flexibility index*, and the dummy variable for *Government ownership* are not statistically significant. Consistent with the predictions of the private benefits hypothesis, firms from civil law countries are less likely to list on a U.S. exchange (Doidge, Karolyi, and Stulz (2004)). Surprisingly, they are also less likely to list outside of an exchange in the U.S. *Log of GNP per capita* has an insignificant coefficient for exchange listings, but a significantly negative coefficient for Rule 144a/Level 1 OTC listings. Finally, our economic proximity measure has a negative and significant coefficient, which is surprising in light of the finding of Sarkissian and Schill (2004) that firms that list shares abroad are more likely to do so in an economically proximate country. We also estimate (but do not tabulate) the regressions using the antidirector index and the Investor protection index instead of the *Civil law* dummy variable and find similar results.¹⁴

The results in Panel A are supportive of our private benefits hypothesis: Ownership of control rights is negatively associated with the probability of having a U.S. exchange listing, but is not associated with the probability of having other types of listings.¹⁵ We next turn to tests that also use the Controlling Blockholder data set, but seek to capture both the ability to expropriate as well as the incentives to expropriate measured by the difference between the control rights and the cash flow rights held by the controlling shareholder. Theoretically, Bebchuk, Kraakman, and Triantis (2000) show that agency costs of controlling shareholders are higher when there is a separation of control rights and cash flow rights. Empirically, Claessens et al. (2002) find that separation of control rights and cash flow rights is associated with higher expected minority shareholder expropriation.

Panel B of Table II reports tests in which we examine the incidence of cross-listing as a function of both the cash flow rights ("*Cash flow rights*") and the control wedge—the difference between the control rights and cash flow rights ("*Control – cash*") of the controlling blockholder. We find that the two key variables of interest have negative and significant coefficients for U.S. exchange listings. The economic magnitudes are greater than those of Panel A: A 1% increase in the control wedge is associated with a 2% decline in the probability of listing, although it should be noted that a 1% increase in the control wedge is a much larger economic change, given that the median control wedge is about

¹³ The median level of foreign sales is 10.7% with an interquartile range of 0% to 40.8%, so a 1% change in foreign sales is a smaller economic event that is much more likely to occur than a similar change in sales growth.

¹⁴ We cannot include country dummies in these models because of thinness of the sample that results from its stratification across countries by type of listing. The model uses three different listing types. If there is not at least one firm from each country of each listing type, as is the case for several countries, the country dummy perfectly predicts the listing.

¹⁵ We also explore the possibility of a nonlinear relationship. If a controlling shareholder has control rights over 50%, it may matter little whether he holds 55% or 85% of these rights. Therefore, rather than using the control rights variable, we replace the control rights variable with an indicator variable if control rights exceed 50%. The results suggest that a threshold effect for control rights at 50% does not exist.

5% for our sample firms. To the extent that this wedge proxies for greater expected consumption of private benefits of control, our result is consistent with the idea that a significant number of controlling shareholders do not want to reduce their ability to extract private benefits of control by listing in the United States. None of the ownership measures has a significant coefficient for the other listing types. The coefficients on the other variables are similar to the coefficients in Panel A.

As mentioned previously, one drawback of the Controlling Blockholder data set is that the largest blockholder is not always directly associated with the firm's top management group or its controlling family. In Panel C, we use the Family/Management Control data set to examine the effect of the management group and its family on the incidence of cross-listing. This data set also allows us to examine a broader set of firms from both emerging and developed markets. We find consistent results in that firms with high family/management control rights are less likely to have an exchange listing but these firms do not shy away from other types of listings. There are only two noteworthy differences in our control variables. First, *Sales growth* is not significant for any type of listing. Second, *Log of GNP per capita* is not significant for Rule 144a/Level 1 OTC listings while it is significant in Panel A.

We find no evidence in Table II that private benefits play a role in the decision to list on the LSE. We interpret this finding as consistent with the hypothesis that a London listing does not significantly constrain controlling shareholders. While it is true that ordinary LSE listings require that firms provide financial information prepared and audited in accordance with U.K. GAAP or International Accounting or Financial Reporting Standards (IAS or IFRS), these regulations do not apply to global depositary receipt (GDR) listings, which require little incremental financial reporting relative to the home country. Salva (2003) documents almost half of all U.K. listings are in GDR form and these are concentrated in firms from emerging markets for which constraints on private benefits would bind most stringently.¹⁶ Our LSE findings are also consistent with recent evidence that U.K. enforcement of governance and disclosure requirements is simply weaker than in the United States (see La Porta, Lopez-de-Silanes, and Shleifer (2006)). We note, however, that our results may obtain simply because there are too few observations to generate sufficient power.¹⁷

Taken together, the results in Table II are consistent with the key predictions of the private benefits hypothesis: Ownership of control rights and the control

¹⁶ Additional details on the listing requirements of the U.K. Listing Authority are in the "Index to Listing, Disclosure and Prospectus Rules" (particularly LR 18.1–18.4 and 19.1–19.3) at the U.K. Financial Services Agency website (<http://fsahandbook.info/FSA/html/handbook/D85>).

¹⁷ We also estimate binomial logistic regression models of the decision to list in London relative to the decision to remain in the home market. This approach has two main advantages. First, we note that in the multinomial logits reported in Table II we include firms from the U.K., even though cross-listing in London is not a choice for these firms (cross-listing in the United States is though). By estimating a binomial logit that excludes U.K. firms we eliminate this problem. Second, in the binomial logit, we can use the full sample of London listings. The results for London listings in the binomial logits are similarly weak.

wedge are strongly negatively associated with the probability of having a U.S. exchange listing, but are not associated with the probability of having other types of listings.

We now examine an important alternative explanation for our results. As discussed earlier, if firms cross-list to issue equity and do so for reasons that have nothing to do with the governance benefits of cross-listings, we would still expect a negative relation between controlling shareholder ownership and the listing decision. For firms that cross-listed before the year in which we observe their ownership, controlling shareholder ownership could be low mechanically because they listed and issued equity previously.¹⁸ Under this equity issuance explanation, we would also expect a negative relation between ownership and a U.S. listing, but private benefits would play no role. One way to mitigate the concern that equity issuance explains our results is to document a negative relation between ownership and the probability of listing when controlling ownership stakes are measured *prior* to the actual cross-listing event. Of course, the difficulty with performing such an analysis is that we often use ownership data that predates 1997 by many years, so that the ownership may differ simply because of changes in firm characteristics, although this may not be an important problem as ownership patterns tend to be stable (La Porta, Lopez-de-Silanes, and Shleifer (1999)). It is only possible to obtain prelisting ownership systematically for U.S. exchange-listed firms—these firms are required to file prospectuses (for capital raising issues) and Form 20-F, in which they are required to disclose ownership information. Firms with other types of listings are not required to make such disclosures. We cannot, therefore, reestimate our multinomial regressions with prelisting ownership. Since we have already shown that ownership is unrelated to the probability of having a London listing or a Rule 144a/Level 1 OTC listing, this is not a serious issue. As a result, in Table III, we estimate logit regressions in which the dependent variable is a dummy variable, *Exchange*, equal to one if a firm has a U.S. exchange listing, and zero otherwise. Note that the benchmark firms are those that have no listing in London or another type of U.S. listing to be consistent with the benchmark sample in the multinomial regressions. For comparison purposes, we report logit regressions with the ownership data from 1995 to 1996 (as used in Table II) and the prelisting ownership data in Panels A and B of Table III, respectively. Of course, the Panel B tests are likely to have less power because, as mentioned earlier, we lose a number of exchange-listed firms from the sample due to data limitations.

Models (1) and (2) in Panel A and (4) and (5) in Panel B use the Controlling Blockholder data set. In model (1), we regress the dummy variable *Exchange* on the same set of variables used in Panel A of Table II. Not surprisingly, the coefficient estimates are similar in magnitude and precision. More interesting, however, is the comparison of the coefficient estimates in model (1) with those in model (4). Model (4) uses prelisting ownership for the exchange-listed firms.

¹⁸ Ayyagari and Doidge (2008) confirm that some controlling shareholders significantly decrease their ownership stakes after listing on major U.S. exchanges.

Table III
Cross-sectional Logistic Regressions: Listing on a U.S. Exchange

The logistic regressions estimate the probability that a firm is cross-listed on a U.S. exchange as of December 31, 1997. The table reports marginal effects evaluated at the means of the independent variables. Data sources for information on U.S. listings and ownership are described in Section I. We use all firms available in Worldscope for which we have ownership data except for financial firms, firms that list via Rule 144a and Level 1 OTC ADRs, and firms with assets less than \$10 million. Panel A shows results using ownership data from 1995 to 1996. Panel B shows results using prelisting ownership data. In this panel, privatizations and IPOs are excluded. Models (1) and (4) show results using the controlling blockholder's control rights (*Control rights*, or voting rights held by the controlling blockholder); models (2) and (5) show results using the controlling blockholder's cash flow rights (*Cash flow rights*) and the difference between control rights and cash flow rights (*Control – cash*); and, models (3) and (6) show results using family/management control rights (*Family/Mgmt control rights*, or fraction of voting rights held by the family/management group). See Table II for details on control variables. Pseudo- R^2 is a goodness-of-fit measure based on the difference between unrestricted and restricted likelihood functions. The t -statistics reported in parentheses are adjusted for clustering on countries—they are computed assuming observations are independent across countries, but not within countries. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A			Panel B		
	(1)	(2)	(3)	(4)	(5)	(6)
Control rights	–0.0078 (–2.73)***			–0.0020 (–1.56)		
Cash flow rights		–0.0067 (–2.18)**			–0.0011 (–1.08)	
Control – cash		–0.0209 (–3.04)***			–0.0150 (–2.62)***	
2 nd blockholder	0.0007 (0.73)	0.0009 (0.87)		0.0005 (0.94)	0.0005 (1.08)	
Family/Mgmt control rights			–0.0082 (–2.49)**			–0.0048 (–2.48)**
Sales growth	0.0031 (2.26)**	0.0030 (2.25)**	0.0043 (1.52)	0.0020 (2.86)***	0.0017 (2.92)***	0.0033 (1.78)*
Global industry q	0.0090 (5.46)***	0.0085 (4.73)***	0.0137 (3.64)***	0.0037 (3.29)***	0.0030 (2.89)***	0.0079 (3.10)***
Log assets	0.0041 (7.88)***	0.0041 (7.38)***	0.0061 (4.47)***	0.0018 (3.36)***	0.0016 (2.96)***	0.0035 (3.68)***
Foreign sales	0.0074 (4.85)***	0.0075 (5.00)***	0.0126 (3.32)***	0.0045 (2.97)***	0.0041 (2.72)***	0.0094 (2.94)***
Leverage	–0.0007 (–0.13)	–0.0010 (–0.19)	–0.0047 (–0.70)	0.0014 (0.89)	0.0008 (0.57)	–0.0012 (–0.32)
Financial flexibility index	0.0007 (1.02)	0.0006 (0.87)	0.0003 (0.33)	0.0005 (1.64)	0.0004 (1.29)	0.0005 (0.72)
ROA	0.0000 (0.04)	0.0000 (0.09)	0.0001 (0.53)	0.0000 (0.22)	0.0000 (0.27)	0.0001 (0.70)
Government owned	0.0008 (0.46)	0.0006 (0.33)		–0.0006 (–0.68)	–0.0006 (–0.66)	

(continued)

Table III—Continued

	Panel A			Panel B		
	(1)	(2)	(3)	(4)	(5)	(6)
Civil law	−0.0058 (−2.18)**	−0.0054 (−2.14)**	−0.0069 (−1.78)*	−0.0035 (−5.44)***	−0.0027 (−4.34)***	−0.0047 (−3.00)***
Log of GNP per capita	−0.0013 (−0.90)	−0.0014 (−1.04)	−0.0038 (−2.10)**	0.0002 (0.23)	0.0000 (0.01)	−0.0023 (−1.66)*
Economic proximity	−0.0002 (−2.33)**	−0.0002 (−2.16)**	−0.0001 (−0.81)	−0.0001 (−2.19)**	−0.0001 (−1.84)*	−0.0001 (−0.62)
Number of observations	4,062	4,062	4,272	4,012	4,012	4,218
Pseudo- R^2	0.4834	0.4862	0.4711	0.4504	0.4663	0.3811

The coefficient on ownership of control rights in this specification is negative, but the result is statistically insignificant (t -statistic is -1.56). When we use the cash flow rights and wedge variables in models (2) and (5), we find that the wedge has a significant negative coefficient when measured before the listing, but its magnitude is reduced by about 30%. Models (3) and (6) use the Family/Management Control data set (control rights variable here denoted “*Family/Mgmt control rights*”). In this comparison, prelisting ownership of control rights has a negative coefficient that is equivalent in terms of statistical significance, but somewhat smaller in magnitude than with the ownership data from 1995 to 1996. In sum, Table III, Panel A confirms the inferences from Table II that ownership of control rights and the control wedge are strongly negatively associated with the probability of having a U.S. exchange listing. Using prelisting ownership data, as in Panel B of Table III, we also find support for our private benefits hypothesis, but such support is somewhat weaker. One of the three coefficients of interest is not significant and the two significant coefficients are smaller in magnitude.

We perform many robustness tests on Panel A of Table III. One concern is that our results might be driven by the U.K. and Japan, two countries that have a large number of firms compared to other countries in our sample. When we reestimate the logit regression for U.S. exchange listings, we find that our results are not driven by firms from these countries.¹⁹ We also reestimate that regression on a subsample that excludes government-owned and privatized firms. For the Family/Management Control data set, we do not have government ownership data. For that sample, we therefore remove only the privatized firms. With these subsamples, all the conclusions we reach from our earlier regressions hold. We also estimate the regressions for the subsamples using

¹⁹ In fact, we reestimate all regressions in each table excluding the U.K. and excluding Japan. All our inferences hold for these additional regressions with one exception. When we reestimate the regressions of Table II excluding Japan and excluding the U.K. the coefficient on ownership for OTC listings becomes significant and negative for the Controlling Blockholder data set but not for the Family/Management Control data set.

different country-level control variables. All our conclusions hold up through these robustness tests except for one experiment in which the coefficient on the control wedge is negative but not significant when we remove government-owned firms and control for the antidirector index or the investor protection index. It is statistically significant when we control for country characteristics using country dummies though.

We consider, but ultimately rule out, including interaction effects in our models. A difficulty with interaction effects in logit and probit models that has only recently been recognized in the literature is that the implementation and interpretation of interactions in nonlinear models is not straightforward (see Ai and Norton (2003)). It is also important to note that the interaction effect is conditional on the value of independent variables. Therefore, the magnitude and statistical significance of the interaction effect can vary over different observations—in fact, the interaction effect will be positive for some observations and negative for others, which makes summary measures of the interaction effect less meaningful.²⁰

As an additional test to help us rule out the equity issuance explanation for our results, we differentiate cross-listing types based on whether they raise capital. Previously in Table II, we divide the types of listings into London listings, Rule 144a/Level 1 OTC listings, and U.S. exchange listings. Differentiating among types of listings in this way is appropriate when focusing on the private benefits hypothesis. However, if the listing decision is explained by the desire of some firms to raise funds in the United States and this results in a dilution of ownership stakes, then it is more appropriate to divide the listing types according to those that are eligible to raise capital and those that are not. Among listing types in the United States, Rule 144a private placements and those exchange listings that also file Form F-1 and F-6 to qualify for public offerings of securities (often referred to as Level 3 ADRs) are similar in this respect.²¹

²⁰ One important reason to explore such interaction effects in the logit regressions would be to help to evaluate potential endogeneity problems in our analysis. After all, our sample firms with low control rights and smaller wedges between control and cash flow rights may very well coincidentally be those that pursue U.S. cross-listings because there are unobservable or omitted forces that lead them to both decisions. We follow an approach similar to Bertrand, Mehta, and Mullanaithan (2002), who offer evidence that tunneling of resources by corporate insiders in Indian companies within business groups is more likely following positive shocks to operating profits. In our private benefits theory of cross-listing, firms that experience positive sales growth would be less likely to list if they have higher control rights or a larger wedge. The coefficient on the interaction between *Sales growth* and the *Control – cash* wedge is negative in extensions of models (1) and (2) in Table III, but it is not statistically significant. Of course, proper practice (Ai and Norton (2003)) dictates examination of the interaction across all probability outcomes and, when we do that, we do not find evidence that the interaction is significant for most ranges, except over low cross-listing probabilities. We explored other forms of interactions, but the reliability of the *Control rights* and *Control – cash* wedge is never eliminated by the addition of these interactions.

²¹ We explore the consequences of splitting up exchange-listed firms in our sample into noncapital-raising Level 2 and capital-raising Level 3 ADR programs given the incremental initial disclosure and continuous reporting requirements of the latter group. The results are consistent between the two groups, but not statistically distinguishable, probably due to the small numbers of observations that obtain.

This suggests that, in order to investigate the equity issuance hypothesis, we should instead divide the listings into three different groups: London listings, Level 1 OTC/Level 2 exchange listings, and Rule 144a/Level 3 exchange listings. When we divide listings into these groups, control and cash flow rights ownership stakes are negatively related to the probability of listing of both Level 1 OTC/Level 2 exchange listings and Rule 144a/Level 3 exchange listings (not tabulated for the sake of brevity). Thus, the negative relation between ownership of control rights and the probability of a Level 1 OTC/Level 2 listing is not explained strictly by a motivation to raise capital. Rather, a more plausible explanation for that relation is that controlling shareholders with high private benefits avoid a Level 2 or 3 exchange listing.

The results so far suggest that when the control rights held by controlling shareholders are higher, firms are less likely to pursue a listing on a U.S. exchange but they are not less likely to pursue other types of listings. This finding is robust to alternative definitions of the controlling shareholders and to firm- and country-level controls. The negative relation between the probability of a U.S. listing and ownership of control rights also holds when we use prelisting ownership data, but the magnitude and precision of these results are weaker. In addition to the results with control rights, we find that when there is a larger separation of control and cash flow rights (using both prelisting ownership for listed firms and ownership as of 1995 to 1996), firms are less likely to be listed. Further, we consistently find that firms with better growth opportunities, larger firms, firms with more international activity, and firms from countries with better legal protection for minority investors are more likely to be listed on a U.S. exchange. These variables do not affect the probability that a firm is listed on the London Stock Exchange. Overall, the results are consistent with the hypothesis that firms in which controlling shareholders have the capability and the incentives to expropriate minority shareholders are less likely to subject themselves to the increased transparency and monitoring of a U.S. exchange listing. None of these results can be explained by the hypothesis that firms list to overcome investment barriers.

III. Differentiating between Direct and Indirect Constraints

A. Cross-listing and Analyst Following

As discussed in the introduction, the literature considers both direct and indirect constraints on controlling shareholders. Direct constraints include constraints pertaining to laws, disclosures, and enforcement actions that controlling shareholders face when they cross-list on a U.S. exchange. Indirect constraints result from greater monitoring by gatekeepers, such as analysts. In this section, we examine whether analyst following is higher for listed firms with controlling shareholders that own more control rights than cash flow rights, for firms with greater family/management control, and for firms with more

concentrated ownership, relative to comparable firms that are not listed as a way to evaluate the role of indirect constraints in the listing decision. We investigate the role of indirect constraints using analyst following because (1) we can obtain analyst coverage data for a large number of firms in our sample and (2) previous research suggests that greater analyst following brings about more scrutiny and monitoring and is indicative of a better information environment. In particular, there are two key results in the literature that guide our analysis. First, Lang, Lins, and Miller (2003) and Bailey, Karolyi, and Salva (2006) focus on the role of analysts and show that analyst following is significantly higher for firms that cross-list on U.S. exchanges, that their forecasts are significantly more accurate, and that resulting forecast errors have larger capital market consequences. Second, Lang, Lins, and Miller (2004) show that firms with more concentrated control have less analyst following and conclude that analysts are less likely to follow firms with weak internal governance.

If analyst following increases only for firms that list on major exchanges, our earlier tests would not help us distinguish between the relative importance of direct and indirect constraints on the consumption of private benefits. This is the case since both direct and indirect constraints would increase when a firm lists on a major exchange and neither would increase if they list elsewhere. If, in contrast, analyst following increases for all types of listing, our earlier tests would imply that direct constraints are binding but indirect constraints—at least through analysts as gatekeepers—are not. This follows because controlling shareholders do not appear to avoid nonexchange U.S. listings even though these listings would also bring with them greater analyst following.

To perform this analysis, we collect information on analyst coverage for our sample firms from the I/B/E/S International Summary file. Using the I/B/E/S ticker to match firms, we extract information as of the annual earnings announcement in each year from 1997 through 2001. We construct three variables including: (1) the number of analysts, (2) the earnings surprise (*"Earnings Surprise"*), computed as the absolute value of the difference between actual earnings and the prevailing consensus (mean) forecast, scaled by the absolute value of the mean consensus forecast, and (3) the forecast dispersion across analysts (*"Dispersion"*), computed as the reported standard deviation of the forecasts that comprise the consensus forecast. We require that each firm must have at least one analyst per reporting year but require that it have two analysts to compute dispersion. The intersection of the samples is larger for this experiment than for those in previous sections as we do not require data on as many firm characteristics to be available. The final sample for the Controlling Blockholder (Family/Management Control) data set includes 17,311 (17,765) firm-years and includes as many as 176 (200) exchange-listed and another 260 (278) Rule 144a/Level 1 OTC listings.

Table IV presents the results. For each of the three panels (which correspond to the three panels in Table II), we report regressions of analyst coverage on ownership variables alone and on ownership variables and control variables

Table IV
Analyst Coverage Regressions

This table presents results from regressions that estimate the impact of cross-listing in the United States on analyst coverage. The sample period goes from 1997 to 2001. Data sources for information on U.S. listings and ownership are described in Section I. We use all firms available in Worldscope for which we have ownership data except for financial firms and firms with assets less than \$10 million. The dependent variable in each regression is the number of analysts. Analyst data are from the I/B/E/S International Summary File. Firms in our sample with at least one analyst in a given year are included. Panel A shows results using the controlling blockholder's control rights (*Control rights*), or voting rights held by the controlling blockholder; Panel B shows results using the controlling blockholder's cash flow rights (*Cash flow rights*) and the difference between control rights and cash flow rights (*Control – cash*); and Panel C shows results using family/management control rights (*Family/Mgmt control rights*, or fraction of voting rights held by the family/management group). *144a/Level 1* and *Exchange* are dummy variables that equal one if a firm cross-lists its shares in the United States, respectively, via a Rule 144a or Level 1 OTC ADR or on a U.S. exchange. *Earnings surprise* is computed as the absolute value of the difference between actual earnings and the last consensus (mean) forecast, scaled by the absolute value of the consensus forecast (defined only if there are at least two analysts in any given year). All accounting data are from Worldscope. All models include year, industry, and country dummies. The *t*-statistics reported in parentheses are adjusted for clustering on firms—they are computed assuming observations are independent across firms, but not across time. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A		Panel B		Panel C	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	5.39 (2.67)***	-25.90 -(17.66)***	5.37 (2.62)***	-25.90 -(17.71)***	5.19 (2.21)**	-24.44 -(15.02)***
Control rights	-6.16 -(10.25)***	-3.52 -(6.62)***				
Cash flow rights			-6.44 -(10.50)***	-3.51 -(6.46)***		
Control – cash			-2.98 -(2.01)**	-3.60 -(2.55)**		
Family/Mgmt control rights					-5.74 -(12.17)***	-2.86 -(6.54)***
144a/Level 1	7.03 (9.33)***	1.53 (2.75)***	7.02 (9.36)***	1.55 (2.83)***	7.00 (12.23)***	2.09 (4.86)***
Exchange	8.35 (11.13)***	0.99 (1.86)*	8.36 (10.91)***	0.96 (1.77)*	9.10 (15.07)***	2.00 (4.32)***

144a/Level 1 $\times$ Control rights	0.73 (0.27)	4.21 (2.18)**			
Exchange $\times$ Control rights	7.88 (2.87)***	8.11 (3.52)***			
144a/Level 1 $\times$ Cash flow rights			0.70 (0.25)	4.37 (2.15)**	
Exchange $\times$ Cash flow rights			8.12 (2.91)***	7.96 (3.40)***	
144a/Level 1 $\times$ Control – cash			2.32 (0.34)	2.16 (0.33)	
Exchange $\times$ Control – cash			6.23 (0.62)	10.31 (1.42)	
144a/Level 1 $\times$ Family/Mgmt control rights					–1.18 (1.18)
Exchange $\times$ Family/Mgmt control rights					–0.28 (1.34)
Log assets		2.71 (39.84)***		2.71 (39.91)***	2.68 (39.44)***
Earnings surprise		–0.17 (–5.04)***		–0.17 (–5.04)***	–0.20 (–5.46)***
Dispersion		–0.54 (–6.72)***		–0.54 (–6.72)***	–0.59 (–6.81)***
Number of observations	17,311	12,055	17,311	12,055	12,556
Adjusted- R^2	0.2911	0.5539	0.2923	0.5540	0.5497

(*Log assets*, *Earnings surprise*, and *Dispersion*).²² These specifications are similar to those in Lang, Lins, and Miller (2003). We also include country dummies, industry dummies, and year dummies in all regressions. The *t*-statistics are adjusted for clustering on firms—they are computed assuming observations are independent across firms, but not across time.

In Panel A, firms are covered by 5.39 analysts on average when the ownership variables are set equal to zero. The coefficient on *Control rights* is negative and statistically significant. For firms with a median holding of control rights of 34% (Table I), this implies 2.09 fewer analysts (-6.16×0.34). By contrast, both coefficients on *Rule 144a/Level 1 OTC* and *Exchange* are significantly positive and economically large (seven to eight more analysts per firm), which suggests that analyst coverage increases not only for exchange listings, but for all listing types. We also find in model (1) that the interaction of *Exchange* $\times$ *Control rights* is positive and significant, while that of *Rule 144a/Level 1 OTC* $\times$ *Control rights* is not. This result means that firms having shareholders with large control rights stakes that cross-list in the United States by way of an exchange listing have higher analyst coverage than those firms that do so by way of a Rule 144a/Level 1 OTC listing. Evaluated at the median control rights for these two types of listings from Table I (26% for Rule 144a/Level 1 OTC listings and 24% for exchange listings), the difference constitutes 3.09 more analysts per firm (10.53 analysts for Rule 144a/Level 1 OTC listings, 13.62 for exchange listings, and only 3.30 for firms that do not list in the United States at all). Model (2) shows an attenuation in these effects when control variables like *Log assets*, *Earnings surprise*, and *Dispersion* are included, which implies that these differences may reflect the different kinds of firms that pursue Rule 144/Level 1 OTC and exchange listings in the United States. Indeed, the overall explanatory power rises to 55.4% when these control variables are included.

Panel B evaluates the impact of cash flow rights and, incrementally, the control wedge from the Controlling Blockholder data set. Models (3) and (4) show, as before, that cross-listings have positive effects on analyst coverage for all listing types, but the adverse impact of cash flow rights and the control wedge on analyst following is attenuated for the exchange-listed firms and not Rule 144a/Level 1 OTC listings. The attenuation factor for the exchange listings is stronger for cash flow rights (significant coefficient of 8.116 on *Exchange* $\times$ *Cash Flow rights*) than for the control wedge (insignificant coefficient of 6.239 on *Exchange* $\times$ *Control – cash*). As in Panel A, however, any differences between Rule 144a/Level 1 OTC and exchange listings are considerably diminished when control variables are included in model (4). These models also show that firms with high cash flow rights have significantly fewer analysts and the number of analysts falls with the control wedge. These two distinct effects are statistically

²² We also evaluate all of the specifications in Table IV using Tobit (censored regression) models. Our concern stems from the fact that our sample is left-censored by the analyst coverage data requirement (which mandates one analyst per firm-year to qualify and two analysts per firm-year to compute the analyst dispersion control variable). For models with country, firm, and year fixed effects, we are able to discern no significant differences in the key inferences.

significant and economically large (6.44 fewer analysts and 2.98 fewer analysts, respectively, when evaluated at the medians of the ownership variables in Table I).

Finally, Panel C uses the Family/Management Control data set. Again we find that analyst coverage increases across all listing types. We also find that the impact of control rights and of cross-listing via Rule 144a/Level 1 OTC listings and exchange listings are the same as in Panel A. However, the interactions of the *Family/Mgmt control rights* variable with the listing dummy variables are much weaker. In this case, we can state that cross-listed firms for the family/management group have significantly higher analyst coverage than those of firms without a U.S. listing, regardless of the size of the control rights stake, and there are negligible differences across types of cross-listing. This inference applies regardless of whether control variables are included in the specification (models (5) versus (6)). We also note here that all results in Table IV hold if we use prelisting ownership for the exchange-listed firms.

Overall, the results in Table IV offer little support for the hypothesis that analyst monitoring constrains the consumption of private benefits. We find that analyst following increases after any type of listing. Consequently, greater analyst monitoring cannot explain why controlling shareholders with more private benefits are less likely to list only on U.S. exchanges. It follows that an explanation for the negative impact of private benefits on the probability of listing on U.S. exchanges has to be either the presence of direct constraints for such listings or, possibly, the role of indirect constraints other than analyst monitoring, such as monitoring through other capital market intermediaries like investment bankers, auditors, or large institutional shareholders.

B. Firm Value, the Control Wedge, and Cross-listing

It is known from the literature (see, for instance, Claessens et al. (2002), Lemmon and Lins (2003), Lins, (2003)) that there is a negative relation between firm value and the control wedge. Such a negative relation is consistent with the view that controlling shareholders of firms in which the control wedge is higher consume more private benefits. To the extent that consumption of private benefits is more expensive for firms listed on a U.S. exchange, we would expect this negative relation to be alleviated for these firms. We investigate this hypothesis in this section.

To perform this analysis, we follow Claessens et al. (2002), Lins (2003), and Doidge, Karolyi, and Stulz (2004) and use Tobin's q as our valuation measure. We use their procedures to compute Tobin's q ratios for our sample firms as of year-end 1997 from Worldscope. For the numerator, we take the book value of total assets, subtract the book value of equity, and add the market value of equity. For the denominator, we use the book value of total assets. All variables are in local currency. There are, of course, a number of cautions about this valuation ratio proxy, including the fact that we do not attempt to compute replacement cost in the denominator and we do not use a market value of debt in the numerator. But these simplifications are necessary in a data set that

spans so many countries and for which only standard financial accounting data are consistently available. We focus our analysis on the Controlling Blockholder data set as our interest is on the valuation impact of the control wedge (we have similar, but unreported, results with the Family/Management Control data set). This results in 4,275 nonfinancial firms (with assets above \$10 million) in 1997, of which 218 are listed via Rule 144a or Level 1 OTC ADRs and another 130 are exchange-listed (the sample is similar to the sample used in Table II with a small difference in sample size because we use different variables).

Table V presents the valuation regression results. As is standard (see, for example, La Porta, Karolyi, and Stulz (2002), Claessens et al. (2002)), we estimate the regressions with country random effects.²³ The control variables are similar to those in Doidge et al. (2004). In all regressions, firms that have a U.S. exchange listing have a higher Tobin's q . On average, these coefficients range from 0.31 to 0.40, which constitutes a premium of 23% to 30% of the average Tobin's q ratio of about 1.33. Though firms that pursue Rule 144a private placements and Level 1 OTC listings have a higher Tobin's q than firms that do not have a U.S. listing, these firms have a lower Tobin's q ratio than firms with an exchange listing, as expected. In Panel A, we report the results for three specifications using the control wedge variable. In model (1) we verify the result from the previous literature that firm value falls with the size of the control wedge. The wedge coefficient is -0.49 , which implies a Tobin's q discount of -0.025 for a firm with a mean control wedge of about 5% (-0.49×0.05). In model (2), we add the listing dummies, which confirms previous research that finds cross-listed firms are worth more, particularly those with exchange listings. In model (3), we find that the negative relation between the control wedge and firm value is attenuated by a U.S. listing, but the only significant coefficient on the interaction between the control wedge and listing dummies obtains for exchange listings. This is consistent with predictions from our private benefits theory. The coefficient equals 3.56 and has a t -statistic of 2.57, implying an economically large valuation premium in Tobin's q that grows with the size of the control wedge.

In Panel B, we report another three specifications with a simple dummy variable for firms that have a positive control wedge ("*Control – cash dummy*"). In models (4) and (5), the coefficient on this dummy variable is significantly negative as expected, but it is larger in magnitude than the implied Tobin's q discount from Panel A. In model (6), the coefficients on the interactions of the dummy variable with the exchange listing and the Rule 144a/Level 1 OTC listing dummy variables are insignificant, but they are positive, so that, for firms with such listings, the total impact of the control wedge becomes insignificant.

The results in Table V are consistent with the hypothesis that investors perceive direct constraints to be more binding than indirect constraints on the

²³ The Breusch-Pagan Lagrange multiplier test rejects the hypothesis that the residuals are independent within countries. We also check the assumptions of the random effects model with a Hausman test: Random effects models assume that the random effects are uncorrelated with the other regressors. Hausman tests show that this assumption is not rejected.

Table V
Valuation Regressions

This table presents results from (country) random effects regressions that estimate the valuation impact of cross-listing in the United States as of December 31, 1997. Data sources for information on U.S. listings and ownership are described in Section I. We use all firms available in *Worldscope* for which we have ownership data except for financial firms and firms with assets less than \$10 million. The dependent variable in each regression is Tobin's q , computed as $((Total\ Assets - Book\ Equity) + Market\ Value\ of\ Equity)/Total\ Assets$ (all variables are in local currency) on December 31, 1997. Panel A shows results using *Control - cash*, the difference between the controlling blockholder's control rights and cash flow rights (*Cash flow rights*). Panel B shows results using a dummy variable (*Control - cash dummy*) that equals one if *Control - cash* is greater than zero. *144a/Level 1* and *Exchange* are dummy variables that equal one if a firm cross-lists its shares in the United States, respectively, via a Rule 144a or Level 1 OTC ADR or on a U.S. exchange. Sales (*Log sales*) are in \$ millions. See Table II for details on other control variables. t -statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A			Panel B		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-0.90 -(1.79)*	-0.75 -(1.44)	-0.74 -(1.32)	-0.88 -(1.77)*	-0.73 -(1.42)	-0.73 -(1.38)
Cash flow rights	-0.01 -(0.20)	0.01 (0.11)	0.01 (0.12)	-0.02 -(0.27)	0.00 (0.02)	0.00 (0.01)
Control - cash	-0.49 -(2.64)***	-0.43 -(2.31)**	-0.51 -(2.73)***			
Control - cash dummy				-0.09 -(3.17)***	-0.08 -(2.87)***	-0.09 -(3.12)***
144a/Level 1		0.15 (2.35)**	0.12 (1.67)*		0.15 (2.34)**	0.11 (1.45)
Exchange		0.40 (5.05)***	0.31 (3.70)***		0.40 (5.03)***	0.36 (3.97)***
144a/Level 1 $\times$ Control-cash			0.93 (1.00)			
Exchange $\times$ Control-cash			3.56 (2.57)**			

(continued)

Table V—Continued

	Panel A			Panel B		
	(1)	(2)	(3)	(4)	(5)	(6)
144a/Level 1 × Control – cash dummy						0.13 (1.03)
Exchange × Control – cash dummy						0.15 (0.85)
2 nd blockholder	–0.04 (–1.49)	–0.04 (–1.48)	–0.05 (–1.55)	–0.04 (–1.33)	–0.04 (1.33)	–0.04 (–1.35)
Sales growth	0.76 (12.30)***	0.77 (12.40)***	0.76 (12.38)***	0.76 (12.23)***	0.76 (12.34)***	0.76 (12.33)***
Global industry <i>q</i>	1.15 (16.35)***	1.12 (15.91)***	1.12 (15.94)***	1.15 (16.27)***	1.12 (15.84)***	1.12 (15.86)***
Log sales	–0.02 (–1.98)**	–0.03 (–3.66)***	–0.03 (–3.62)***	–0.02 (–1.90)*	–0.03 (–3.58)***	–0.03 (–3.56)***
Government owned	0.04 (0.81)	0.03 (0.66)	0.04 (0.76)	0.04 (0.84)	0.04 (0.69)	0.04 (0.69)
Civil law	–0.04 (–0.37)	–0.03 (–0.27)	–0.03 (–0.26)	–0.04 (–0.39)	–0.03 (–0.29)	–0.03 (–0.29)
Log of GNP per capita	0.11 (2.19)**	0.12 (2.19)**	0.11 (2.00)**	0.11 (2.20)**	0.11 (2.20)**	0.11 (2.14)**
Number of observations	4,275	4,275	4,275	4,275	4,275	4,275
Overall <i>R</i> ²	0.1195	0.1284	0.1295	0.1208	0.1293	0.1295

consumption of private benefits of control: The value of private benefits of control is lower for firms with a U.S. exchange listing, so that for a given level of the control wedge, firm value is greater when the firm is listed on a U.S. exchange. The fact that a substantial increase in analyst following for nonexchange listings is not associated with a reduction in the valuation discount due to the wedge implies that investors do not believe that greater analyst following constrains the consumption of private benefits. Of course, caution is warranted. The evidence on higher valuations of firms that cross-list (Doidge, Karolyi, and Stulz (2004), Lang, Lins, and Miller (2003)) has been criticized because of concerns about biases due to omitted variables and sample selection. We control for a number of variables in the valuation equations inspired by other literature, but there may be an important omitted variable that jointly influences the decision to list in the United States and the valuation of such firms. We do estimate, but do not report, regressions that account for self-selection following those in Doidge, Karolyi, and Stulz (2004). In these regressions, the first-stage model (the exchange-listing decision) is a binary probit model (we do not consider Rule 144a/Level 1 OTC listings in these models) and the second-stage model is the valuation equation with an additional nonselection hazard or “selectivity” regressor from the first-stage model. We try a number of different specifications in both the listing and valuation equations. In some specifications, the magnitude of the premium for exchange listings is reduced, but it remains statistically and economically significant. None of our inferences are affected when we account for self-selection in this way. Finally, all results in Table V hold if we use prelisting ownership for the exchange-listed firms.

IV. Predicting Firms’ U.S. Exchange Listing Decisions

In Section II, we find that the population of firms cross-listed on a U.S. exchange has lower control rights ownership and a lower control wedge than non-cross-listed firms. These results are static in nature as they are estimated with ownership data at a particular point in time. In this section, we investigate the listing decision in a dynamic way using a Cox (1972) proportional hazard model. That is, we build a model to predict from among a set of firms that are not listed in year t those that are more likely to be listed in year $t + 1$. The prediction of the private benefits hypothesis is that firms with higher ownership of control rights and a larger control wedge in year t are less likely to list on a U.S. exchange and firms with greater growth opportunities are more likely to list.

There are two problems with such an analysis that warrant discussion from the outset. First, we need firm characteristics in year t for firms that are not listed. As we move back in time, data availability is constrained and is subject to a sample selection bias in that Worldscope added many firms to its data set in the first half of the 1990s. Second, we need ownership data for nonlisted firms across time. The data sets we use have ownership for nonlisted firms, but only in 1995 to 1996. To the extent that ownership is stable for nonlisted firms, our approach does not pose a serious problem. For firms with U.S. exchange

listings, we augment this data with ownership information for the year prior to listing.

In this analysis, we model the probability of listing in year t , given that the firm has not yet listed, as a function of firm-level and country-level variables using Cox models that allow us to predict firms' listing decisions in a panel setting, while allowing the independent variables to change over time. Similar approaches are used in Pagano, Roell, and Zechner (2002) and Claessens, Klingebiel, and Schmukler (2003). As discussed earlier, the cost of this approach is a reduction in power since the number of firms in our sample that list on a U.S. exchange over the relevant period is 58 (from 14 different countries) in the Controlling Blockholder data set and 67 (from 18 different countries) in the Family/Management Control data set.

The Cox proportional hazard model estimates the probability that a firm that has not yet listed on a U.S. exchange will list in a given year. The hazard rate in the model is the instantaneous rate of listing for firms that have not yet listed. The model assumes that the hazard rate for firm j , $h(t | \mathbf{x}_j)$, is a function of the independent variables, $\mathbf{x}_j$, and is written as

$$h(t | \mathbf{x}_j) = h_0(t) \exp(\mathbf{x}_j \beta_x), \quad (1)$$

where β_x is a vector of coefficients to be estimated. The hazard rate in equation (1) is composed of two separate parts. The first part, $h_0(t)$, is called the baseline hazard. It is obtained by setting $\mathbf{x}$ equal to zero so that the baseline hazard for firm j corresponds to the hazard rate with $\mathbf{x}_j$ set to zero. The Cox model is a semiparametric model in that β_x is estimated without specifying the baseline hazard; that is, the model makes no assumptions about the nature or shape of the hazard rate.²⁴ The second part, $\exp(\mathbf{x}_j \beta_x)$ is called the relative hazard, and is a function of explanatory variables. The model is proportional in that the hazard rate is obtained by shifting the baseline hazard as the explanatory variables change. For example, firm j 's hazard rate is a multiplicative transformation of firm i 's hazard rate. Therefore, the model assumes that, whatever the shape of the baseline hazard, it is the same for all firms.

In this "event time" experiment, we consider firms over the period from 1995 to 2001. In the Controlling Blockholder data set, there are 4,589 firms with complete data. Of these firms, 58 list on a U.S. exchange during this period. In the Family/Management Control data set, there are 4,731 firms, of which 67 list during this period. Once a firm lists in the United States, it is no longer used in the model estimation. For example, if a firm lists in 1998, then it is included in the sample from 1995 through 1998 and is excluded in subsequent years. Although our ownership data are observed at only one point in time, in 1995 or

²⁴ The baseline hazard cancels out of the partial likelihood function. Note also that the intercept is subsumed into the baseline hazard. Because the baseline hazard drops out, the Cox model has no intercept. If one knows, or is willing, to make assumptions about the form of the baseline hazard, parametric hazard models can be estimated. As a robustness check, we reestimate all hazard models assuming an exponential distribution, that is, the baseline hazard is constant. None of the results we report are affected.

1996 for firms that do not list and in the year prior to listing for listing firms, all other firm-level variables are allowed to change each year. As such, we are assuming that firms' ownership structures are constant, at least until they list in the United States. We use the same set of firm-level and country-level variables that we use in Section II. The difference in this section is that all firm-level variables are lagged by 1 year since we are trying to explain why a firm with specific characteristics at the end of 1 year chooses to list in the next year. For example, in 1996 we use 1995 values for *Sales growth*, *Global industry q*, *Total assets*, *Foreign sales*, *Leverage*, *Financial flexibility index*, and *ROA*. The Cox model is estimated by maximizing the "partial" likelihood function. We report the coefficients in exponentiated form. These coefficients thus represent relative (to the base) hazard ratios. The advantage of exponentiated coefficients is that they can be interpreted as the effect of a unit change in the explanatory variable on the hazard ratio. For example, an exponentiated coefficient of 1.2 (0.8) implies that a one-unit increase (decrease) in the explanatory variable increases (decreases) the hazard ratio by 20%. The standard errors are adjusted for clustering across firms, so that we assume errors are independent across firms, but not across time.

We begin by investigating the role of control rights on the listing decision. In Table VI, Panel A, we report results using the control rights of the controlling blockholder as an explanatory variable in our model of the probability that a firm will list in the United States in a given year (the Controlling Blockholder data set). We report a regression model with the same explanatory variables as in model (1) of Table III. The hazard ratio on the controlling blockholder's control rights is 0.25. This hazard ratio is statistically different from one (t -statistic = -1.92).²⁵ A one-unit increase in *Control rights* therefore decreases the relative hazard ratio by 75%. When control is more valuable, the cost to the controlling shareholders of listing is higher and firms are less likely to list. The hazard ratio for the second large blockholder dummy variable is greater than one but not statistically significantly so. This is consistent with the idea that the value of control is lower when there is a second large shareholder to monitor the actions of the controlling blockholder. For the sake of brevity, we do not discuss the coefficients of the control variables for the Cox regressions.

In Panel B, we include both the cash flow rights of the controlling blockholder and the control wedge. As expected, we find that higher cash flow rights and a higher control wedge make it less likely that a firm will list.

Finally, in Panel C, we report the results using the control rights of the family/management group. As noted earlier, a potential advantage of this analysis is that the effect of control rights on cross-listing may be sharpest in firms for which we can be certain the controlling shareholder is part of the management group or its family. The hazard ratio for the family/management group's

²⁵ The null hypothesis is written as $H_0: \exp(\beta_x) = 1$ since hazard ratios equal one when the coefficients equal zero.

Table VI
Cox Models: Estimating the Probability of Cross-listing on a U.S.
Exchange in Event Time

The Cox models estimate the probability of listing in year t , given that the firm has not listed yet. The sample includes observations on the dependent variable from 1995 to 2001. Data sources for information on U.S. listings and ownership are described in Section I. We use all firms available in Worldscope for which we have ownership data except for financial firms, firms that list via Rule 144a and Level 1 OTC ADRs, and firms with assets less than \$10 million. Panel A shows results using the controlling blockholder's control rights (*Control rights*, or voting rights held by the controlling blockholder); Panel B shows results using the controlling blockholder's cash flow rights (*Cash flow rights*) and the difference between control rights and cash flow rights (*Control – cash*); and Panel C shows results using family/management control rights (*Family/Mgmt control rights*, or fraction of voting rights held by the family/management group). See Table II for details on control variables. The table reports hazard ratios (e.g., $\exp(\beta_x)$, not β_x). The model does not estimate a constant. The t -statistics reported in parentheses are for the null hypothesis that the coefficient is equal to one. The t -statistics are adjusted for clustering on firms—they are computed assuming observations are independent across firms, but not across time. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Panel A	Panel B	Panel C
	No. of Subjects: 4,589 No. of Failures: 58	No. of Subjects: 4,589 No. of Failures: 58	No. of Subjects: 4,731 No. of Failures: 67
Control rights	0.25 –(1.92)*		
Cash flow rights		0.33 –(1.51)	
Control – cash		0.01 –(2.49)**	
2 nd blockholder	1.35 (0.95)	1.49 (1.31)	
Family/Mgmt control rights			0.19 –(2.67)***
Sales growth	1.32 (0.40)	1.51 (0.59)	2.01 (1.58)
Global industry q	5.10 (3.39)***	4.85 (3.25)***	4.11 (3.31)***
Log assets	2.37 (10.88)***	2.40 (11.15)***	2.27 (11.61)***
Foreign sales	6.71 (3.89)***	7.30 (4.11)***	6.12 (3.89)***
Leverage	0.56 –(0.45)	0.53 –(0.52)	0.46 –(0.70)
Financial flexibility index	0.72 –(1.82)*	0.72 –(1.78)*	0.77 –(1.43)
ROA	1.02 (2.47)**	1.01 (2.44)**	1.01 (2.74)***
Government owned	1.36 (0.83)	1.31 (0.71)	
Civil law	0.34 –(2.56)**	0.35 –(2.54)**	0.54 –(1.63)
Log of GNP per capita	1.25 (0.58)	1.23 (0.54)	0.60 –(2.28)**
Economic proximity	0.92 –(3.11)***	0.93 –(3.06)***	0.95 –(2.25)**
Log likelihood	–374.05	–372.67	–446.13
χ^2	206.45	252.15	238.29
Prob > χ^2	0.0000	0.0000	0.0000

control rights is 0.19 and is significant (t -statistic = -2.67). As we would expect, an increase in control rights decreases the probability of listing.

Similar to the logistic regressions in Section II, we investigate the robustness of the results of Table VI extensively. First, we reestimate each regression substituting for the *Civil law* dummy the *Antidirector* index, the *Efficiency of the judicial system* index, the *Disclosure* index, the *Burden of proof* index, or the *Investor protection* index. All the results of Table VI hold with these changes in the investor rights variable. With the Controlling Blockholder sample, firms are more likely to list if the investor rights are better. With the Family/Management Control sample, the hazard ratios on investor rights variables are not always significant. We then reestimate the Cox models excluding firms from the U.K. and Japan. Our results hold up for the resulting subsample. Next, we exclude the government-owned firms and the privatized firms (except for the Family/Management Control sample where we only exclude the privatized firms). Again, our results hold up. Finally, in addition to estimating Cox models that predict U.S. exchange listings, we also estimate (but do not report) separate Cox models that predict Rule 144a/Level 1 OTC listings and listings in London. Similar to the results in Table II, we find that the ownership variables, control rights, and the control wedge do not help to explain which firms have a Rule 144a/Level 1 OTC listing or a listing in London.

In regressions not tabulated, we explore the possibility of a nonlinear impact of the control wedge on the listing decision. For example, small differences may not have a major impact on incentives to cross-list in the United States, but a larger difference might. One simple nonlinear specification that we consider is the squared control wedge. However, for many firms the difference is zero, so that the squared term is highly correlated with the linear term. Therefore, we estimate a model that is piecewise linear in the control wedge, allowing for one change in the slope. Because choosing a point at which the slopes change is fairly arbitrary, we try a number of different cutoff points. For example, when we use a cutoff at 5% with the full set of control variables, the hazard ratio for cash flow rights is 0.40, but is not significant.²⁶ The hazard ratio for the control wedge from zero to 5% is 0.04 and is also insignificant. The hazard ratio for the control wedge greater than 5% declines to 0.01 and is significant at the 10% level. When we use a 10% cutoff, the hazard ratio for the control wedge from zero to 10% is 0.42 and is not significant. The hazard ratio for the control wedge greater than 10% is 0.001 and is significant at the 10% level. Overall, the evidence suggests that the control wedge has a nonlinear impact on firms' exchange listing decisions, but the exact form of this nonlinear relationship is somewhat unclear.

V. Conclusions

Recent research has shown that the private benefits of control are especially valuable in countries with weak investor protection. A related stream

²⁶ Recall from Table I that the control wedge is zero for the majority of the firms and that a 5% cutoff is almost associated with the 75th quantile of our sample of firms and it rises to a maximum value of 34%.

of research finds that the decision to cross-list in the United States is value enhancing because it commits firms to improved disclosure and better corporate governance, as well as to additional scrutiny by various capital market intermediaries (“gatekeepers”). However, despite the existence of benefits from cross-listing, it is well known that many non-U.S. firms do not cross-list their shares in the United States. One explanation is that the controlling shareholders of firms that choose not to list do so because a listing would force them to reduce their consumption of private benefits through the effects of direct constraints (legal and regulatory constraints resulting from the listing, for example, disclosure requirements) and/or of indirect constraints (changes in monitoring because the firm is listed in the United States, for example, analyst monitoring). In this paper, we find evidence in support of this hypothesis and we evaluate the relative importance of direct versus indirect constraints.

We posit that private benefits of control are more valuable when controlling shareholders own more control rights and when their control rights exceed their cash flow rights. Using two separate databases of ownership structure for over 4,000 firms from 31 countries around the world, we construct proxy measures of private benefits of control and find that, when controlling shareholders have high levels of control and when their control rights exceed their cash flow rights, their firms are less likely to list their shares in the United States. Similarly, we find that firms controlled by their top managers and their families are less likely to have a U.S. listing. In modeling the costs and benefits of listing, we also find an important role for other firm-level variables, such as growth opportunities and size, as well country-level variables, such as home-country investor protection. Further, we show that our basic inferences hold up if we use information about ownership of cash flow and control rights at the time of the cross-listing. We also employ duration analysis using a Cox proportional hazard model to show that the probability of listing in a given year over the 1995 to 2001 period, conditional on not yet having listed, is significantly lower when controlling shareholders have higher levels of control, when their control rights exceed their cash flow rights, and when firms are controlled by their top managers. While we find that our proxies for private benefits lower the probability of a U.S. exchange listing, they do not have a significant impact on other types of less constraining cross-listings. As evidence of a decrease in consumption of private benefits for firms listed on U.S. exchanges, we find that the known negative relation between Tobin’s q and the control wedge is sharply attenuated when firms list in the United States.

This paper also contributes to the debate in the literature about the relative importance of direct and indirect constraints on the consumption of private benefits of control resulting from a cross-listing. An example of indirect constraints is the greater monitoring from analysts after a cross-listing event. We find in our data that analyst following increases for all types of listing. Since private benefits affect only the probability of a U.S. exchange listing, our evidence suggests that analyst following may not constitute an important constraint on the consumption of private benefits of control. Though this evidence suggests that direct constraints are the most binding, we cannot exclude the possibility that

other gatekeepers, such as investment bankers, lawyers, and auditors, may significantly constrain the consumption of private benefits of control.

Taken together, our results support the view that a desire to either consume private benefits of control or retain the option to consume such benefits deters the controlling shareholders of many non-U.S. firms from listing on major U.S. exchanges.

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