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ESSAYS ON THE GLOBALIZATION OF FIRM OWNERSHIP

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Essays on the globalization of firm ownership

Xiqian ZHANG

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FACULTÉ DES SCIENCES ÉCONOMIQUES

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Abstract

This dissertation is constituted of three distinct chapters. The first chapter seeks to resolve a recent debate on whether sovereign wealth funds(SWFs) have non-financial objectives and politically distort firm performance. I find that, once stake size is considered, previous belief that SWFs would affect firm performance should be reformulated. SWFs rarely seek an influential stake in listed firms. Even in firms where they take significant stake size, SWFs do not affect firm global performance. But at the plant level, this chapter provides case-study evidence that hints at the influence of SWFs on firm choices of business location. Overall, findings of this chapter indicate that SWFs do not affect economic gain accrued to global investors, but it awaits to be examined how SWFs and other stakeholders split economic gain in terms of technology transfer, employment, and tax revenue.

The second chapter examines if financial bidders exploit underpricing as a motive in acquisitions by studying acquisition bids that fail to complete.[1] Any revaluation remaining after failure should be unrelated to the value creation envisioned by the acquisition attempt, and in that light, could be driven by underpricing. We find a 30-percent cumulative abnormal return from bid offer to bid failure to firms that are targeted by financial bidders and subject to severe information asymmetry. Compared to financial bidders, we further find that strategic bidders have other motives behind acquisitions and fall short to reap underpricing as a driver in choosing target firms. These results indicate that financial bidders specialize or are more skilled in identifying and exploiting mispricing opportunities. This work successfully tackles a long-standing empirical challenge to separate value-selection versus value-creation motives for acquisitions and provides evidence that one of the motives for financial investors to acquire firms is to benefit from mispricing.

The third chapter takes the perspective of acquisition process and focuses on cross-border vertical acquisitions. I show that target country-level productivity in global value chains is important in explaining the extent of control transferred from targets to foreign acquirers. Foreign control is less common in target countries with higher relative productivity. This finding is robust to alternative determinants of cross-border acquisition decisions. These results suggest that one pertinent source of bargaining power for targets vis-à-vis foreign acquirers in the negotiation process of cross-border acquisitions is target countries' relative position in global value chains.

Key words: Sovereign wealth fund, Government ownership, Real effect, Undervaluation, Mispricing, Acquisitions, Financial acquirers, Revaluation, Cross-border, Global value chains, Bargain, Integration.

[1] Joint work with Carolina Salva.

Résumé

Cette dissertation est constituée de trois chapitres distincts. Le premier chapitre vise à résoudre un récent débat sur la question de savoir si les fonds souverains(SWFs) ont des objectifs non financiers et faussent politiquement la performance des entreprises. Je trouve que, après avoir examiné la taille de la participation détenue par SWFs, la croyance antérieure que SWFs affecteraient la performance de l'entreprise devrait être reformulée. SWFs cherchent rarement une participation influente dans les sociétés cotées. Même dans les entreprises où SWFs detiennent une participation significative, SWFs n'affectent pas la performance globale de l'entreprise. Mais au niveau de l'usine, ce chapitre fournit deux études de cas qui font une allusion à l'influence des SWFs sur les choix du lieu de l'usine des entreprises. Dans l'ensemble, les conclusions de ce chapitre indiquent que SWFs n'affectent pas le gain économique des investisseurs mondiaux. Elles indiquent également qu'il est intéressant à voir comment SWFs et les autres parties prenantes répartissent les gains économiques en termes de transfert de technologie, d'emploi et de recettes fiscales.

Le deuxième chapitre examine si les acquéreurs financiers exploitent la sous-évaluation comme motif des acquisitions en étudiant les échecs d'acquisition, c'est-à-dire les offres d'acquisition qui ne parviennent pas à se terminer.[2] Toute réévaluation subsistante après un échec d'acquisition ne devrait pas être liée à la création de valeur envisagée par la tentative d'acquisition. Dans cette optique, elle pourrait s'explique par une sous-évaluation. Nous constatons un rendement anormal cumulatif de 30% de l'offre d'achat à l'échec de l'acquisition aux entreprises qui sont ciblées par les acquéreurs financiers et souffrent d'une asymétrie d'information sévère. Par rapport aux acquéreurs financiers, nous constatons en outre que les acquéreurs stratégiques ont d'autres motifs des acquisitions et ne parviennent pas à tirer profit de la sous-évaluation en tant que facteur déterminant dans le choix des entreprises cibles. Ces résultats indiquent que les acquéreurs financiers se spécialisent ou sont plus compétents dans l'identification et l'exploitation de la sous-évaluation. Ce travail aborde avec succès un défi empirique de longue date consistant à séparer la sélection de valeur et de la création de valeur dans les acquisitions, en fournissant la preuve que l'un des motifs pour les acquéreurs financiers est de profiter de la sous-évaluation.

Le troisième chapitre prend la perspective du processus d'acquisition et se concentre sur les acquisitions verticales transfrontalières. Je montre que la productivité au niveau des pays cibles dans les chaînes de valeur mondiales est importante pour expliquer l'étendue du contrôle transféré des cibles aux acquéreurs étrangers. Le contrôle étranger est moins courant dans les pays cibles où la productivité relative est plus élevée. Cette conclusion est robuste aux déterminants alternatifs des décisions d'acquisition transfrontalières. Ces résultats suggèrent que la position relative des pays cibles dans les

[2] Collaboration avec Carolina Salva.

chaînes de valeur mondiales constitue une source pertinente de pouvoir de négociation pour les cibles vis-à-vis des acquéreurs étrangers dans le processus de négociation des acquisitions transfrontalières.

Mots clés : Fonds souverains, Propriété publique, Effet réel, Sous-évaluation, Erreur d'évaluation du prix, Acquisitions, Acquéreurs financiers, Réévaluation, Transfrontalier, Chaînes de valeur mondiales, Négociation, Intégration.

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Introduction

In recent years, we witness the rise of global ownership. It is reflected by the increasing acquisitions of controlling and non-controlling stake in foreign firms. What are the motives and impacts in buying foreign ownership? What is the best timing to launch a purchase? What are major determinants of a successful purchase? These questions receive growing research interest but await deeper investigation.

Existing literature uncovers four main motives. The first motive is to avoid tax (Huizinga and Voget, 2009). If tax in the host destination is lower, firms that merge enjoy a drop in effective tax rate and an increase in firm value (Col, Liao, and Zeume, 2016). The second motive is to take advantage of regulatory arbitrage. For example, banks buy financial institutions located in weaker regimes to execute profitable investment opportunities which are not allowed in their home countries (Karolyi and Taboada, 2015). The third motive is to extend the usage of superior special knowledge over immobile assets located in foreign countries (Frésard, Hege, and Phillips, 2017). Acquirers that act on this motive experience higher acquisition gain and post-deal better operation performance. The fourth motive is to gain additional value from improving target corporate governance (Rossi and Volpin, 2004). In deals where acquirers are better governed than targets, both targets and acquirers experience positive acquisition gain (Bris and Cabolis, 2008; Chari, Ouimet, and Tesar, 2010). However, the literature has mixed views on the motive and impact of a major global investor type: government-related acquirers. Some believe that government-controlled acquirers (state-owned enterprises) and government-backed institutional investors (sovereign wealth funds) are profit partakers in foreign firms (Kotter and Lel, 2011; Karolyi and Liao, 2017). Others argue that they aim to advance political agenda through foreign investment and distort foreign firm performance (Dewenter, Han, and Malatesta, 2010; Bortolotti, Fotak, Megginson, 2015). This discrepancy calls for better understanding whether government investors buy global ownership for profit or politics.

The timing of a foreign acquisition is shown to be affected by difference in stock market valuation and currency appreciation between acquirer and target countries (Baker, Foley, and Wurgler, 2009; Erel, Liao, and Weisbach, 2012). An undervaluation in targets can motivate acquirers to set an acquisition in motion which might be too costly in absence of target undervaluation (Edmans, Goldstein, and Jiang, 2012). It is unclear, however, whether the observed valuation gap in cross-border settings reflects a temporary bidding opportunity or systematic underperformance of foreign targets. Therefore, there is a need to disentangle the value-creation and value-selection motives.

Compared to previous two aspects, the process of obtaining global ownership receives surprisingly scant research attention. In fact, cross-country differences provide interesting features that can reshape cross-border procedures and cannot be examined in a domestic setting. For instance, unlike domestic deals, to choose to pay in cash is not so much at acquirers' discretion but a necessary response to foreign target demand (Faccio and Masulis, 2005). Another feature that is specific to cross-border settings is

government protectionism. Government intervention in foreign bids is shown to deter foreign capital (Dinc and Erel, 2013). There is much about the purchase process of global ownership that we do not fully understand, including determinants of bargaining outcome between foreign acquirers and local targets.

Taken together, the globalization of firm ownership has high potential for further exploration. To advance knowledge in this field, my dissertation aims to dedicate first two chapters to motives and impact, and the third chapter to the process of obtaining global ownership.

The first chapter reconciles a recent debate on whether sovereign wealth funds(SWFs) have non-financial social objectives and politically distort firm performance. I provide two remedies to major causes of inconsistency in the existing literature by: (1) identifying target firms where SWFs take substantial stake and (2) searching for evidence of real effects. The findings of this chapter suggest that the previous belief that SWFs worsen or improve firm performance should be reformulated once stake size is considered. SWFs rarely seek an influential stake in listed firms. This investment strategy does not, in turn, cause political distortion of firm operations at the global level. Yet, at the plant level, I show case-study evidence that hints at the influence of SWFs on firm choices of business location. Altogether, the results of this work suggest that SWFs do not affect economic gain accrued to global investors. Future research should focus on conflicts between SWFs and other stakeholders in splitting economic gain in terms of technology transfer, employment, and tax revenue. To advance research in this direction, private firms and subsidiaries of global firms are suitable subjects for future study.

In the second chapter, I work together with Carolina Salva and examine if financial bidders exploit underpricing as a motive in acquisitions in the international setting. To this end, we study acquisition bids that fail to complete. Any revaluation remaining after failure should be unrelated to the value creation envisioned by the acquisition attempt and could be driven by underpricing. We compute the market reaction from bid offer to bid failure, and we find a 30-percent cumulative abnormal return to firms that are targeted by financial bidders and subject to severe information asymmetry. This revaluation effect does not revert to prior-bid price level. After a battery of tests, we interpret this finding as suggesting that mispricing is a relevant acquisition motive for financial bidders and, in some cases, the ripped benefits can be substantial. We compare these results to revaluation effects around failed bids issued by strategic bidders. We find that strategic bidders have other motives behind acquisitions and fall short to reap underpricing as a driver in choosing target firms. Furthermore, we find that geographic proximity does not add additional advantage to identify mispricing targets. Overall, our results indicate that financial bidders seem to specialize or are more skilled in identifying and exploiting mispricing opportunities. This work successfully tackles a long-standing empirical challenge to separate value-selection versus value-creation motives for acquisitions and provides evidence that one of the motives for financial investors to acquire firms is to benefit from mispricing.

The third chapter takes the perspective of acquisition process and investigates the role of country-level productivity in vertical acquisitions across borders. In this chapter, I propose and provide new evidence that higher target-country productivity in global value chains lends targets more bargaining power in negotiation with foreign acquirers. Targets from countries with higher productivity can leverage their exclusive access to sources of productivity and charge a higher price for a transfer of ownership. If foreign acquirers do not want to overpay, they would settle for a non-controlling stake or switch to the next best target country. I find that, indeed, controlling acquisitions are less common in target countries with higher productivity in global value chains. This result is robust to alternative explanations for the choice between controlling and non-controlling acquisition. Findings of this chapter suggest that countries' relative position in international product market reshapes bargaining outcome of cross-border acquisitions.

Overall, this thesis sheds new light on the motives and impacts as well as the process of obtaining global ownership. In that light, I believe that valuable insights provided by this thesis help advance knowledge on the globalization of firm ownership.

Chapter 1: Do sovereign wealth funds (really) affect firm performance?

1.1. Introduction

Sovereign wealth funds(SWFs) have become increasingly important in the global capital market since their sudden rise in 2007.[3] By the end of 2014, assets managed by SWFs were estimated to be worth USD 7.1 trillion, far exceeding private equity (USD 4.1 trillion) and hedge funds (USD 2.7 trillion).[4] Do these government-controlled funds have political agendas and cause political distortion in firms in which they invest? This is a multi-trillion-dollar question, the answer to which can affect how policy makers in host countries address this new form of governmental investor. However, there is no clear answer. From the perspective of other global shareholders investing in the same firm as SWFs, the literature presents four different views on the role of SWFs. They are (1) passive shareholders (Kotter and Lel, 2011), (2) governmental friend (Sojli and Tham, 2011; Fernandes, 2014; Bertoni and Lugo, 2014); (3) governmental enemy (Bortolotti, Fotak, Megginson, 2015); and (4) peer shareholders with a possible political agenda, yet the damage of which awaits to be verified (Dewenter, Han, Malatesta, 2010; Dyck and Morse, 2011; Knill, Lee, Mauck, 2012; Calluzzo, Dong, Godsell, 2017). Since to get a clear view on SWFs not only concerns academia but also has real-world consequences, I aim to resolve the debate on whether SWFs politically distort firm performance by providing two remedies to major causes of inconsistency in existing studies.

The first remedy is to spot listed firms in which SWFs take a substantial stake. Given the proliferation of studies on the investment motives and strategies of SWFs, one might presume that there has been careful treatment on stake size, a crucial indicator for SWFs' intention and power to intervene. Surprisingly, this is not so. In fact, there is large discrepancy in the mean (median) stake size documented in existing literature: 27.5 (20) percent in Dewenter et al. (2010); 5.61 (0.08) percent in Dyck and Morse (2011); 12.7 (0.05) percent in Kotter and Lel (2011); 8.45 (1.23) percent in Bortolotti et al. (2015); and 1.13 (0.61) percent in Calluzzo et al. (2017). Apparently, the first step towards reconciliation is to have a sample in which SWFs truly have intention and power to impose political agenda. To do so, I focus on firms where SWFs invest no less than five percent, a minimum threshold for any hidden impact of SWFs to appear.

The second remedy is to search for evidence of real effects. Most of previous studies draw conclusion from revealed target preferences and stock market reaction. In other words, speculation over political motives of SWFs is intense, but evidence of real consequences is scant and unclear. If perceived non-financial motives are truly and largely at work in target firms, we should eventually see distortion on firm operation. However, other than univariate analysis and inconclusive results (Kotter and Lel, 2011; Sojli and Tham, 2011; Fernandes, 2014), only Bortolotti et al. (2015) conduct multivariate

[3] See for example, the Sovereign Wealth Funds Explosion, November 1st, 2007, Wall Street Journal.
[4] TheCityUK Fund Management Report, September 2015.

analysis on sales growth and operation efficiency. But Bortolotti et al. overlooked real investment, a key firm policy also likely to be exposed to political influence. For example, if a firm already runs at full capacity before SWFs becoming shareholders, new business in home countries of SWFs obtained through political connections (Dewenter et al., 2010; Fernandes, 2014) would lead to an upscale in firm capital expenditures and a temporary decrease in operation efficiency. Therefore, to have a better view on changes in firm operation, we need to test, in parallel, real investment, sales growth, and operation efficiency.

With those two remedies in mind, I construct a sample of listed firms invested in by SWFs from 1989 to 2012 and run pooled OLS regressions on an event window of three years before and five years after SWF investment events. I use three separate variables to measure, respectively, (1) average difference in operation performance between SWF target firms and control groups before SWFs investment, (2) average difference in operation performance of control groups before and after the year of SWFs investment, and (3) post-event incremental effect of SWFs on target operation performance. By design, this model easily accommodates various ways of forming control groups and sensitivity analysis on different thresholds of stake size.

This chapter presents several important empirical findings. First, a detailed analysis on stake size distribution reveals that SWFs rarely seek an influential stake in listed firms. Second, I find little evidence supporting either positive or negative political effects of SWFs on real investment, sales growth, or operation efficiency. This absence of incremental change is persistent across various thresholds of influential block size, control groups based on different matching specifications, and alternative proxies of operating performance. Put together, the findings are consistent with Kotter and Lel (2011), who conclude that SWFs do not impact firm performance globally.

Next, I reconcile my findings with Bortolotti et al. (2015) who show that SWFs damage firm operation performance. To start with, I follow Bortolotti et al. and construct a benchmark of non-government financial investors. I find a sharp contrast in investment patterns of SWFs and this benchmark. SWFs typically buy a non-influential stake in firms via cross-border transactions, whereas benchmark investors mostly seek influential stakes in domestic firms. Here, two major discrepancies emerge: stake size and geographic proximity. The two can jointly lead to misinterpretation. To illustrate with an extreme case, a 0.05 percent allocation of Asian SWFs in an American bank might be forced into comparison with (1) domestic investment by a private financial investor with better access to private information or (2) a merger between two American banks. As a result, SWFs that are worse at picking stock due to informational disadvantage or having less influence might be misinterpreted as SWFs damaging firm performance relative to the benchmark. For this reason, I keep deals where SWFs invest no less than five percent of shares. In this way, I preserve firms in which SWFs should have, relative to benchmark investors, either comparable private information to make large investments or similar

intention and power to intervene. It turns out that target firms of both types of investors perform similarly after investment events. The finding is robust to fund-specific characteristics such as a subgroup of SWFs more likely to be subject to political interference. Taken together, the conclusion of Bortolotti et al. seems to suffer from a motive-mismatched bias built into their benchmark formation.

Furthermore, I re-examine why Fernandes (2014) concludes that SWFs improve firm value. Fernandes uses Tobin's q to measure firm value and apply a one-percent threshold to identifying influential stake size. I analyze the association of Tobin's q and SWF investment events at various stake size thresholds. Again, I do not find evidence of incremental change in Tobin's q of firms targeted by SWFs after SWF investment. Furthermore, I find that, for the subsample of firms in which SWFs buy a stake size below five percent, target firms of SWFs have on average a higher Tobin's q than control groups defined by Fernandes before SWFs becoming shareholders. This suggests that a one-percent threshold adopted by Fernandes is insufficient to filter out firms that have, in general, a higher value than control groups before SWFs invest. And a stake size less than five percent is found to account for 80 percent of the SWFs investment sample. That means results of Fernandes (2014) might be driven by over-proportionate presence of cases where SWFs select themselves into firms with higher valuation.

Thus far, I have only show evidence of listed firms that, once stake size is considered, previous belief that SWFs worsen or improve firm performance should be reformulated. While I can conclude that SWFs do not impact the economic gain accrued to global shareholders, I cannot exclude the possibility that SWFs may affect how the economic gain accrued to government is redistributed. Neither can I generalize the no-impact conclusion to other asset classes of SWFs, such as unlisted firms. Unfortunately, data limitation makes it more difficult to conduct deep empirical analysis in that regard. Therefore, I resort to two brief case studies of two Middle East SWFs. These two SWFs have similar political agendas to diversify industrial structure but adopt different approaches to execute it. In the first case, Mubadala, an Abu Dhabi SWF that bought 8 percent of Advanced Micro Devices (AMD) in 2007, participates in AMD's multi-year plan to divest its manufacturing arm. Mubadala eventually owned the entire manufacturing business of AMD globally. However, so far Mubadala has not shifted any existing plant to Abu Dhabi or launched any new plant in the Middle East region. The opposite case concerns Oman SWF, which clearly aims to immediately transfer foreign technology to its homeland and create local employment by setting up manufacturing branches after investing in unlisted foreign firms in the high-tech sector. These two distinct cases hint that, even though SWFs do have political agendas, it is not global shareholders whose interests are at risk. Rather, it is the other stakeholders of target firms who may have conflicts of interests with SWFs. More precisely, SWFs might come into conflict with the governments of host countries who want to retain technological innovation, employment, and tax revenue at home. This type of conflict among country players is a promising direction for future research, in which private firms and subsidiaries of multinational firms serve as a valid topic of study.

This chapter mainly contributes to the literature on SWFs. First, I offer new evidence to resolve the debate on the motives and real impact of SWFs. Among the studies on the real effect of SWFs, only the conclusion of Kotter and Lel (2011) continues to hold across the various tests proposed in this chapter. That is, SWFs do not affect the global performance of listed firms. I further explain the divergence of this study from Bortolotti et al. (2015) by identifying a motive-mismatched bias built into their sample construction and in turn an over-proportionate presence of an incomparable benchmark with different incentives. I also provide an explanation to opposing conclusion of Sojli and Tham (2011) and Fernandes (2014), in that his results may be driven by a disproportionately large sub-sample in which SWFs select firms with better growth prospect than the benchmark. Second, this work adds to the literature by proposing a simpler yet more reliable method to gauge the real effect of SWFs. Results of this study suggests that, given the heterogeneous background of SWFs, stake size should be the primary filter to effectively detect SWFs' intention and power to intervene. Furthermore, due to the lack of consensus on SWFs' investment motive, we must use matching techniques with great caution. Otherwise, ad hoc model specification might lead us to unstable and incomparable results. My empirical design circumvents such obstacles without compromising rigorousness in distinguishing selection and real effects.

This chapter sheds light on the broader literature on government-firm connection by revealing that SWFs, as a new form of governmental investor, deviate from conventional wisdom about the role of government in firms. A largely held belief is that government always intends to keep control (Bortolotti and Faccio, 2009) or execute political agenda against firm profit maximization (Faccio, Masulis, and McConnell, 2006); Claessens, Feijen and Laeven, 2008). On the contrary, this work provides systematic evidence supporting the idea that SWFs rarely seek an influential stake in listed firms and do not impact profit accrued to global shareholders. Furthermore, using the illustration of case-study evidence, this work notes a new direction for future research on the interplay between politics and the economy. An overlooked form of conflict is arising – a conflict of how the pie is split among difference country players. While SWFs want to transfer technology and create jobs to their own countries, policy-makers of host countries want to retain local technological innovation, employment, and tax revenue.

1.2. Data and methodology

1.2.1. SWFs equity investment

The event of SWFs becoming shareholders is defined as SWFs completing a purchase of existing common shares. I do not consider new shares purchase, including subscribing to seasonal equity offering, private placement, and convertibles. That is because in these transactions it is companies that motivate

investors to invest in company prospect, which introduces self-selection bias into my exploration for a causal effect of SWFs on companies.[5]

I start to collect SWFs investment from SDC Merger & Acquisitions(SDC) database from the first record towards 2012 December 31. I keep records that satisfy the following criteria: (1) transactions that are flagged as "Y" for "Buyside: sovereign wealth funds Involvement"; (2) the deal status is marked "Completed". Next, I exclude privatization, repurchases, self-tenders, exchange offer, recapitalization, spinoffs. To ensure comparability of firm performance before and after SWFs investment, I require available accounting data prior and post to SWFs investment. Hence, I drop leveraged buyout and transactions involving unlisted targets. In addition, I drop transactions in which deal terms or acquirers are not disclosed, i.e. information on SWFs' names and stake size is unavailable.[6] Two reasons motivate this filter. First, SWFs are shown to conduct heterogeneous investment strategies, which implies that an across-the-board analysis might underestimate real influence of subgroups (Dyck and Morse, 2011). Second, stake size is a key variable that determines SWFs' incentive and power to affect firm management. To precisely detect potential impact of SWFs, I must know their identity and stake of interest. From SDC, I collect 724 SWFs investment events with complete deal information.

Then I go to the transaction database provided by Sovereign Wealth Fund Institute and add to my initial sample the transactions that satisfy the following criteria: (1) the summary does not contain key words indicating purchase of new shares, such as "new shares", "new ordinary shares", "preferred shares", "subscribe", and "subscription"; (2) deal term and acquirer name are disclosed; (3) the investment type is marked as "Listed Equity"; (4) transactions contain target name and target country different from those records in my initial sample from SDC. Till now, my raw deal sample contains 3578 SWFs transactions.[7]

If a single firm receives multiple SWF investments, either from the same fund or other SWFs, in the same year, I count once. By doing so, I do not imply homogeneous investment objectives among SWFs. Yet investing in the same firm in the same year, that firm must attract those SWFs for common reasons. By further controlling stake size, I can assume similar objectives of SWFs in that specific. But if a firm receives SWF investment in different years, either from the same fund or another SWF, that firm is counted by the number of events.

Key variables collected at this step include announcement date, effective date, target company name, country where target companies are primarily listed, the home country of SWF country, SWF name, transaction amount in U.S. dollars, stake purchased, and stake owned post transaction.

[5] I double check both SDC Global New Issues database and SWF Institute transaction database. SWFs are involved in only 55 purchases of new shares.
[6] Prior to this filter, I extensively search for complementary information on SWF name and stake size in SDC synopsis and news.
[7] 25 transactions involve a group of several SWFs as buyer.

1.2.2. Stake size as a key indicator for SWFs motive and influence

I use five-percent stake size as a minimum threshold for SWFs influential investment. There is no one-size-fit-all threshold to indicate a shareholder's effective power in a firm. In studies focused on U.S. data, five percent is widely used. SEC considers five percent as the starting point for a shareholder to be influential and in turn requires firms to disclose the identify and intention of shareholders in SEC 13-Filings. Probably because of this, five percent is a commonly applied threshold in previous studies on blockholder and corporate governance. Five percent is also found to be the effective average holding of top five institutional investors (see for example, Cronqvist and Fahlenbrach, 2009; Michaely and Vincent, 2012). The cut-off level outside U.S. is much less clear, probably lower than the five percent threshold. As Ferreira and Matos (2008) shows, holding a 2.7 percent is already ranked as the top one shareholder in large non-U.S. international firms. Thus, by employing five percent as the minimum threshold for influential stake, I might put a stricter standard on non-U.S. target firms.

[Insert Table 1.1 Here]

Table 1.1 reports the stake size distribution of SWFs equity investment. To put in perspective, I compare it with that of non-government financial investors who share the same home and host countries with SWFs.[8] Panel A presents the mean stake size taken by SWFs is 4.09 percent, whereas that of non-government financial investors is 16.28 percent. The contrast is even more striking under the measure of median. While non-government financial investors seek a large stake of 10 percent, SWFs typically purchase as small as 0.5 percent. The upper part of Panel B decomposes the sample by national border. SWFs typically buy non-influential stake overseas, while non-government financial investors have a clear focus at home but seek influential stake size regardless where they invest. In terms of industry (lower part of Panel B), SWFs and non-government financial investors investment share similar frequency in each industry, but again with distinct interest in firm stake size. To sum up, SWFs do not seem to intend to seek for influence in target firms as opposed to non-government financial investors. This serves as a first evidence that SWFs are more of profit partakers than delegates with political mission.

1.2.2. Firm operation performance

To measure operating performance, I collect firm-level accounting data from Worldscope. To identify firms that receive SWFs investment, I match firm name and country to those in my transaction sample described above. Next, I drop observations with negative book equity or missing values on total assets. Till now, 2143 SWFs investment events are matched to accounting data.

[8] Due to this matching, the sample size reported here is smaller than that of the main sample used in empirical analysis, except for reconciliation with Bortolotti et al. (2015) in Table 1.5.

The relevant metric of operating performance for this study are real investment level, sales growth, and operation efficiency. These three aspects are predicted by prior theories to reflect political impact (see, e.g., Atkinson and Stiglitz, 1980; Pfeffer, 1978; Shleifer and Vishny, 1994). As a friend to target firms, SWFs may help management to communicate more efficiently with regulators, to obtain private information, to reduce political uncertainty, and ultimately to optimize operation. As a foe, SWFs could attempt to impose political agenda by altering product mix, changing choices of technology, or thrust requirement for plant location and employment. Either role being in action would lead to changes in aforementioned three accounting measures. To investigate real investment, sales growth, and efficiency in parallel allow me to cover the following scenarios. If SWFs invest in firms that have idle production capacity prior to SWF investment, network transactions on product market, as mentioned by Dewenter et al. (2010) and Fernandes (2014), might boost the firm sales in a short run but not necessarily the capital expenditure. This scenario would be reflected in an increase in 1-year sales growth and operation efficiency. However, if a firm already runs at full capacity before SWF investment, a new long-term government procurement contract, as accentuated in Sojli and Tham (2011), would lead to increase in firm investment level and a decrease in operation efficiency in the short run.

I also include other key firm variables as alternative proxies for performance and control variables. They are Tobin's q for market valuation, operating cash flow to total assets for efficiency, cash to total assets and leverage for financial health, total assets and market capitalization for firm size, paid dividends to total assets for dividend policy in place.

1.2.3. Benchmarks

I use five benchmarks. The first benchmark is simply all listed firms in Worldscope that have never been invested by SWFs, i.e. the universe average. The advantage of using this universe average is that I do not have to decide *a prior* determinants of target selection. Existing literature does not provide consensus on whether and how different investment strategies translate into different criteria for firm selection. For example, Kotter and Lel (2011) use the whole Worldscope universe as the candidate pool to analyze determinants of SWF target selection but do not find consistent predictors for subgroups of SWFs investment classified by stake size.[9]

The next three benchmarks are matched control groups. Previous studies use *ad hoc* combinations of firm-level variables to construct matched control group probably because literature has no consensus on determinants of SWFs target selection. Nevertheless, for robustness purpose, I matched control firms to target firms by 2-digit SIC industry and host country and thereby my second benchmark. To build the third one, I match by 2-digit SIC industry, host country, and firm size. My fourth control group is composed by firms matched to targets by 2-digit SIC industry, host country, and a firm performance

[9] See Table IA1. in internet appendix of Kotter and Lel (2011).

measure. Specifically, when operating efficiency (sales growth, real investment level) is the variable to be tested, then the matching criterion for firm performance is Tobin's q (operating efficiency).

The last benchmark is proposed by Bortolotti et al. (2015): firms invested by non-government financial investors. Compared to control groups solely matched by firm-side characteristics, this approach incorporates shareholder-side characteristics in benchmark and help single out political influence of SWFs on firm operation. Though primary evidence in Section 1.2.2 shows that SWFs are far less likely than non-government financial investors to seek non-influential stake, I still consider this benchmark in a more rigorous regression setting.

In following empirical analysis, I exclude target firms that fall in SIC codes between 6000 and 6999. This is the economics of financial service firms are different from regular firms. Take capital expenditure as an example, investment activities of financial service firms are not clearly defined (Damodaran, 2013).

1.2.3. Model

I assess incremental effect of SWFs on firm operation using an event study approach. More specifically, I consider a time frame of three years before a SWF investment and five years after it. This approach is motivated by two important reasons. First, it mitigates potential inflation in estimation errors. After merging transaction and firm accounting data, my sample becomes a panel that comprises firms invested by SWFs or paired private financial investors. This panel is highly unbalanced because the earliest tractable investment of SWFs takes place in 1989 and not all international data is available throughout such long period. The attrition of international data is unlikely to be random. Thus, to estimate impact on a long historical window can inflate estimation errors (Cameron and Trivedi, 2010), Second, it makes more economic sense to focus on a shortened sample window because the further into a firm's historical operation performance, the less relevant to its future performance and to an investor's investment decision. The purpose to extend prior-event window to three years is to incorporate the possibility that investors start to affect target operation before their investment is disclosed to the public, i.e. acquisition announcement. I keep deals that have non-missing accounting information at least one year before and one year after investment events. The controlled regression setting is as follows.

$$y_{i,t} = \alpha + \beta_1 SWF_i + \beta_2 After_t + \beta_3 SWF_i \times After_t + \boldsymbol{\delta X_{i,t-1}} + \varepsilon_{i,t} \qquad (1.1)$$

where i indexes firms and t indexes fiscal years. The dependent variable of interest, $y_{i,t}$, is one of the following three measures: capital expenditure over year-start net book value of property, plants and equipment (CapexPPE), one-year sales growth (SalesGrowth) measured by the yearly change of net revenue, and operation efficiency measured by operating income over year-start book value of total assets (ROA). SWF_i is a time-invariant dummy variable that equals one for firms that are going to be invested by SWFs at some point in my sample period and equals zero for benchmark firms. $After_t$ is a

dummy that equals one for both SWF target firms and benchmark firms during the [+1Year,+5Year] event window period. β_1 captures average difference in operation performance between SWFs target firms and control groups before SWFs investment, i.e. [-3Year, 0]. β_2 captures average difference in operation performance of control groups between the two event windows, i.e. [-3Year, 0] versus [+1Year,+5Year]. The coefficients of interest, β_3, capture post-event incremental effect of SWFs on $y_{i,t}$. By construction, this model easily accommodates various ways of forming control groups and sensitivity analysis on different thresholds of stake size. As explained in Section 1.2.2, SWFs' potential roles as friends versus foes contrast one another. Thus, at this step, I do not give prediction on the sign of β_3.

$X_{i,t-1}$ is a vector of time-varying firm- and industry-level control variables. All control variables here are with one-year lag, unless stated otherwise. When $y_{i,t}$ is capital expenditure, I include in the vector $X_{i,t-1}$ operating cash flow over total assets, Tobin's q, firm size, and 2-digit SIC industry average of Tobin's q. When $y_{i,t}$ is one-year sales growth, I include in the vector $X_{i,t-1}$ sales growth, capital expenditure, cash over total assets, firm size, and 2-digit SIC industry average of Tobin's q. When $y_{i,t}$ is operating efficiency, I include in the vector $X_{i,t-1}$ sales growth, capital expenditure, firm size, and 2-digit SIC industry average of Tobin's q.

Regarding possible presence of a fixed effect, I do not consider year fixed effect because $After_t$ already controls for changes in macroeconomic environment that affect all firms. Furthermore, I do not consider country, industry or firm fixed effects primarily because they are embedded in fixed effects of SWFs investment strategies. A fund decides which country and industry to invest simultaneously with which firm to pick. All relevant information to SWFs investment decisions, either at country-, industry-, or firm-level, are observable to SWFs. Thus, the ultimate unobservable fixed effects that may bias my estimation derive from heterogeneous fund investment strategies. Statistically, I confirm my belief by following the diagnosis procedures suggested by Petersen (2009) and Cameron and Miller (2015). That is, while I add fixed effect one dimension after another, fund fixed effect leads to the largest variation in standard errors of β_3. Ideally, I should control for investment strategy fixed effect. Unfortunately, as investment strategies are driven by different investment goals, it is infeasible to clearly classify them unless all investment goals are observable. SWF fund-level fixed effect is not an appropriate alternative because a fund can implement multiple strategies simultaneously (Dyck and Morse, 2011). Alternatively, I use various stake size thresholds to mitigate fund-related unobservable effects because stake size sought by SWFs determines not only the intention but also the ability of SWFs to affect firm operation. And in following estimations, I use the sample where SWFs buy no less than five percent of common shares, unless otherwise mentioned.

1.3. Empirical results

1.3.1. Univariate results

Table 1.2 Panel A presents summary statistics of key firm characteristics. Compared to universe average, target firms of SWFs have a distinct profile. They spend more in real investment (CapexPPE), generate cash flow more efficiently (CfTA), pay more dividends (DvdTA) and have larger in firm size (TA, MktCap). However, they have less sales growth (SalesGrowth) and lower operation efficiency (ROA). Despite these two groups of opposing features, target firms of SWFs have a higher market valuation (Q).

To better understand the difference between SWFs targets and the universe, Table 1.2 Panel B decompose firm operating performance by event year and stake size. Each performance measure is defined as the difference between its value and the average value of all firms in Worldscope (Universe) from the same SIC 2-digit industry classification. I also include Tobin's q (Q) in my examination. Having been adjusted for industry average, the profile of SWFs targets becomes clearer. These firms seem to systematically upscale fixed asset or production capacity to maintain their sales level and experience a lower operation efficiency. Their market valuation is largely consistent with industry average except for the year when SWFs place investment (t). Overall, target firms present little evidence on incremental improvement or deterioration after SWFs become shareholders.

It is worth noting that the majority interest group (50%-100%) normally contains no more than 10 observations per year. Such small sample size does not allow me to draw reliable inference on the few values that are significantly different from zero. In addition, there appears to be an increase in market valuation from the year of SWFs investment and to two years afterward. However, it is difficult to tell whether SWFs improve firm value or select into higher value. Therefore, I am going to investigate into this short dynamic around SWFs investment year in a controlled regression.

[Insert Table 1.2 Here]

1.3.2. Multivariate results

Table 1.3 reports estimates of Model (1.1). Panel A display results for the dependent variable CapexPPE, Panel B for SalesGrowth, and Panel C for ROA. Column 1 presents the results obtained from Model (1.1) benchmarked to the universe average. In line with descriptive statistics of firm characteristics in Table 1.2, before SWFs investment, target firms of SWFs have similar sales growth to universe average, but invest more in real investment and experience lower operation efficiency. The variable of interest, SWFAfter, does not indicate any incremental change resulted from the presence of SWFs as shareholders. And this is the case for all three operation measures.

Column 2-4 adopt more refined control groups to account for omitted variables at country, industry, and firm levels. Colum 2 uses a control group matched by 2-digit SIC code and host country. Compared to this industry-country-adjusted benchmark, SWFs targets have similar sales growth and operating efficiency before SWFs become shareholders, while SWFs targets continue to have a higher real investment level. Again, the coefficient of SWFAfter is still insignificant from zero. Column 3 adopts a stricter matching metric. To account for unobservable firm characteristics that vary slowly with time, I construct a control group matched by 2-digit SIC, host country, and firm total assets. Estimates of key coefficients stays largely the same, except for a reappearance of lower operating efficiency for SWFs targets before investment event. Column 4 imposes an additional matching criterion: a firm-level variable shown by previous studies to be related to each of the three operation measures. For real investment and sales growth, I choose one-year lagged ROA. When ROA is the dependent variable, I use Tobin's q. Even benchmarked to such strictly matching pair, I do not find any evidence that SWFs improve or damage firm operating performance.

Column 5 examines year-to-year changes in the operation of SWFs targets around SWFs investment event. It could be that governments intervene by investing in firms to stabilize short-term volatility in operation, trigger a short-term adjustment, or discontinue a change in operation policy that is just launched by management and against political interest. To detect whether there is any dynamic effect of SWFs investment on firm, I interact the indicator variable for SWFs targets and time dummies to explicitly account for year-to-year additional change in SWFs targets relative to the benchmark average over [-3Year, 5Year] period. $Before^{-2}$ is a dummy variable that equals one for the second year prior to SWFs investment. $Before^{-1}$ is a dummy variable that equals one for the year prior to SWF investment. $After^{1}$ is a dummy that equals one for the year after SWFs investment. $After^{2}$ is a dummy that equals one for the second year after SWFs investment. I use the same benchmark as in Column (1).

I observe same patterns as in Column (1). As for real investment and operation efficiency, SWFs target firms are on average different from universe. But there is no evident change emerge along the four timepoints for SWFs targets. As suggested by p-value of F-test at the table bottom, the four coefficients are not statistically different.

In sum, there are two important messages from this baseline estimation. First, the absence of incremental change for SWFs targets prevails through various empirical settings. Second, I obtain new insight on how SWFs select target firms. Using the whole Worldscope stock universe as the pool for candidates, Kotter and Lel (2011) discover that SWFs select past losers measured by negative operating efficiency. Using the same benchmark, I also find a strong negative sign on SWF in Panel C (ROA), which captures the average difference between SWFs target firms and universe before SWFs investment event. However, this difference diminishes, when I use industry-country-adjusted benchmarks. This suggests SWFs do not necessarily select past losers. Furthermore, the real investment of target firms

maintains its higher level regardless the choices of benchmark. This extends the conclusion drawn by Karolyi and Liao (2017) that only firm size matters for SWFs target selection. The persistently positive coefficient on SWF indicates the scale of capital expenditure is a key determinant of target selection, not firm size by its very nature.

[Insert Table 1.3 Here]

1.3.3. Sensitivity to stake size thresholds

So far I obtain consistent evidences that SWFs do not cause major change in firm operation. However, it could be that my baseline threshold of five percent is too low to detect the sub-sample where SWFs are willing and able to be active. Thus, I test the sensitivity of my conclusion to multiple stake thresholds. As SWFs do not easily take influential block of shares, the number of transactions drops sharply when I increase stake cut-off level. Accordingly, I remove all benchmarks and only examine whether there is any incremental effect on a SWFs target operation after a certain SWF becomes shareholders with respect to that target firm's average over the sample period. Table 1.4 summarizes the results. I enhance the threshold by five percent at a time until the number of remaining observations falls below 30. No subgroup generates any incremental change in firm operation after SWFs become shareholders, except for a slightly significant negative sign on ROA shown in Column 9. However, this negative effect disappears when I re-estimate the subgroup of stake no less than 10 percent in the strict regression setting as in Table 1.3 Panel C.[10]

[Insert Table 1.4 Here]

1.3.4. Reconcile with Bortolotti, Fotak, and Megginson (2015)

In this section, I re-examine the effect of political SWFs documented in Bortolotti et al. (2015). Instead of strictly replicating their regression, I directly use Model (1.1) as described in Section 1.2.3. That is because I do not obtain the same propensity score matching result as reported by Bortolotti et al. and thus unable to continue to replicate their benchmark in regression.

Results are displayed in Table 1.5. Panel A examines effects on sales growth, Panel B on operation efficiency, and Panel C on market valuation. The variable of interest, SWFAfterPolitical, captures potential incremental effect of SWFs that are subject to political interference. Column 1-3 cover the event window three years before investment and five years after it. Column 4-6 strictly follow Bortolotti et al. and estimate operation performance only for the third year after investment events. Column 2 and 5 contain target firms where SWFs or benchmarked private financial investor purchase no less than 5 percent, in Column 3 and 6 no less than 10 percent.

[10] Re-estimated results are not reported here.

Overall, there is no evidence suggesting political SWFs harm firm operating performance. But there are some results worth elaboration. In Panel A, the coefficient on SWFAfterPolitical is even positive and statistically significant in Column (1). If it is political SWFs that cause an acceleration in sales growth, the effect should be more pronounced in cases where SWFs have higher incentive and power. Yet this effect immediately disappears once I impose a minimum stake size threshold. A disproportionate effect of non-influential deals seems to impact the coefficient estimation. In Column 6, the highly significant coefficient should also be interpreted with caution. As sample size drops sharply, Column 6 only contain five investment transaction by political SWFs and thus do not provide sensible analysis results.

As for operation efficiency, in Panel B Column 1, the coefficient of SWFAfterPolitical is negative but insignificant. When I enhance the stake size threshold, its coefficient gradually turns from negative to positive both for full period or Year 3. This finding is at odds with the conclusion of Bortolotti et al. that political SWFs negatively affect firm operation efficiency. Note that the stake size distribution presented in Table 1.1 shows that SWFs typically buy a non-influential stake in firms via cross-border transactions, whereas private financial investors mostly seek influential stakes in domestic firms. Here, two major discrepancies emerge: stake size and geographic proximity. It is therefore important to impose a minimum threshold for stake size to make the two investor groups truly comparable. Absent of it, a "stake-motive mismatch" bias might have been built into the sample used by Bortolotti et al.. For this reason, the negative association between SWFs investment and firm operation efficiency found by the Bortolotti et al. may be alternatively driven by an over-proportionate presence of target firms in which SWFs buy a non-influential stake. SWFs that are worse at picking stock due to informational disadvantage or having less influence might be misinterpreted as SWFs damaging firm performance relative to the benchmark.

[Insert Table 1.5 Here]

1.3.5. Reconcile with Sojli and Tham (2011) and Fernandes (2014)

Thus far, all my evidences suggest SWFs neither improve firm performance nor cause any economic ill. Yet this finding contradicts to conclusion drawn in Sojli and Tham (2011) and Fernandes (2014), which find that SWFs improve firm value, measured by Q via channels of new growth opportunities in the home country of SWFs.

To further understand this discrepancy, I use Model (1.1) to re-examine whether SWFs lead to any incremental increase in Q compared to a control group matched by 2-digit SIC, host country, and ROA, a similar control group used by aforementioned two papers. Table 1.6 reports the results. In Column 1 and 2, I maintain the five percent cut-off level. I neither observe any incremental change indicated by SWFAfter nor any prior-investment difference in average between SWFs target firms and control group

indicated by SWF. Next, I lower the threshold to one percent, which is employed by Fernandes (2014). SWF becomes positive and statistically significant, but there is still no support for any improvement resulted from SWFs investment. Similar pattern emerges once I fully relax stake threshold. While mean stake size decreases, the statistical significance grows for indicators that capture SWF-related difference of firm operation prior to the presence of SWFs as shareholders, i.e. $SWFBefore^{-2}$ and $SWFBefore^{-1}$.

These findings reveal two characteristics with respect to SWFs' investment strategies. First, different target selection criteria are applied to purchases of large block size (≥ five percent) and purchases of tiny stake (< fiver percent) but in a wider range of equities. Second, in the latter type of purchase, SWFs select target firms that have high growth prospective prior to their investment, which introduces a self-selection bias into the estimation of aforementioned two studies. Fernandes (2014) used Heckman selection model and two-stage regressions to address this self-selection issue. This technique might have not fully addressed the disproportionately presence of large sub-sample of firms where SWFs lack incentive or power to play any active role. Regarding Sojli and Tham (2011), their usage of SEC 13-Filings does not rule out the possibility that SWFs accumulate their holding via very small but frequent allocation. Thus, it is challenging to distinguish whether SWFs select firms with good market valuation or SWFs actively improve target firm performance.

[Insert Table 1.6 Here]

1.4. Discussion

1.4.1. Do no-change results mean shirking from monitoring?

The absence of significant impact does not mean SWFs shirk their responsibility to monitor when they are entitled to. Note that the type of firms preferred by SWFs are large firms listed in developed financial markets that pay high dividend. The existing ownership structure of these well-established firms implies their stock shares are not always readily available in large amount for potential investors. As predicted by Dhillon and Rossetto (2015), initial control shareholder or active block holders do not easily put their right for sell only if their slice of firm profit offsets active monitoring cost. Thus, SWFs may only be able to obtain stock shares from liquid shareholders on open market or through private negotiation, unless the initially pivot shareholder, being a single majority holder or multiple blockholders, agrees to tender new shares or put their control right for sale. If SWFs buy shares from liquid shareholders, it is just a switch of hands of non-influential shareholders.[11] If SWFs obtain shares from a former blockholder though private negotiation, SWFs are likely to monitor only to the extend the slice of profit compensates the cost of monitoring.

[11] I do not exclude the mechanism of governance through trading Edmans and Manso (2011)

1.4.2. Reconcile with small but positive valuation effect of SWFs investment

Up to this point, I find that SWFs do not cause major change in firm operation. It seems they aim to just partake stable profit from well-established firms, where the cost of monitoring is affordable to SWFs. In fact, this motive is logically reconciled to the positive yet small market reaction towards SWFs investment (Dewenter et al. 2010; Kotter and Lel, 2011; Bortolotti et al., 2015; Karolyi and Liao, 2017). A plausible yet unverified driver for this valuation effect could be undervaluation. Through investing in a firm, SWFs signals to the market that the firm is underpriced, which, if recognized by the market, would lead to upward price correction. This adjustment is however small because investors rationally anticipate SWFs' limited role in making any big change in those firms. This explanation differs from the view of Dewenter et al. (2010) that the observed net valuation effect is a trade-off between an actively harmful and an actively beneficial role of SWFs.

1.4.3. Do we take the right perspective to understand SWFs' motives and impacts?

Most of studies on SWFs' motives and impacts, including the present work, have only looked at listed firms. While I can conclude that SWFs do not impact the economic gain accrued to global shareholders, I cannot exclude the possibility that SWFs may affect how the economic gain accrued to government is split among different country players. Neither can I generalize the no-impact conclusion to other asset classes of SWFs, such as private firms. Unfortunately, data limitation makes it more difficult to conduct deep empirical analysis in that regard. Therefore, I resort to two mini case-studies of Middle East SWFs, who have similar political agendas to diversify the economy beyond oil and gas exports but adopt different approaches to execute it.

The first case concerns Mubadala, a wholly-owned subsidiary located in Abu Dhabi and controlled by the SWF of the United Arab Emirates, and its investment in Advanced Micro Devices (AMD). On November 16 of 2007, AMD announced that Mubadala took an 8.1 percent stake in AMD. Mubadala CEO and Managing Director, Khaldoon Khalifa Al Mubarak, stated that investing in AMD fit Mubadala's investment goal to seek for long-term growth and value creation. AMD Chairman and CEO Hector Ruiz stressed that this was a non-controlling, minority investment and Mubadala would not receive any board representation as part of the deal. Nonetheless, this investment made Mubadala the single largest shareholder of AMD. In subsequent years, Mubadala participated in AMD's multi-year plan to divest its manufacturing arm. In 2009, a partnership between AMD and the technology unite of Mubadala launched Globalfoundries, a leading full-service semiconductor design, development, fabrication and innovation company. Since 2012, Globalfoundries is 100 percent owned by Mubadala. Though Mubadala owns the entire manufacturing business of AMD globally, till 2015 Mubadala has not shifted any existing plant to Abu Dhabi or launched any new plant in the Middle East at large.

The other case concerns the Omani State General Reserve Fund (SGRF) and its serial investments in high-tech private firms. On October 31 of 2016, SGRF acquired a 32.2 percent stake in Mecanizados Escribano, a Madrid-based private firm specializing in the manufacture of precision mechanical components and develops its own systems for the aerospace and defense sectors. Abdulsalam al Murshidi, the executive president of SGRF, openly stated that "(the agreement) is to replicate Mecanizados Escribano's business model in Oman, with respect to machining capabilities and technologies, but encompassing it to the startup of the local business. This will no doubt help create a lot of job opportunities and train our national cadres to work in this booming sector.".[12] This invest-and-replicate approach has been adopted in earlier investments of SGRF. For example, in August 2011, SGRF invested in GlassPoint, a California-based firm specializing in solar steam generators for the oil and gas industry. Soon after that, a plant was built in South Oman. In February 2012, regional headquarters of GlassPoint were set up in Oman.

These two distinct cases indicate that, even though SWFs do have political agendas, it is not global shareholders whose interests are at risk. Rather, it is the other stakeholders of target firms with whom SWFs may have conflicts of interests. More precisely, SWFs may encounter conflict with the governments of host countries who want to retain technological innovation, employment, and tax revenue at home. This type of conflict among country players is a promising direction for future research, in which private firms and subsidiaries of multinational firms serve as a valid topic of study.

1.5. Conclusion

This chapter seeks to resolve the debate on whether SWFs politically distort firm performance by focusing on firms in which SWFs take a substantial stake and searching for evidence of real effects. Previous beliefs that SWFs harm or improve firm performance should be reformulated once stake size is carefully considered. SWFs rarely seek controlling or influential stakes in a firm. I find no evidence that SWFs as shareholders affect firm' real investment level, sales growth, or operation efficiency at a consolidated accounting level. To wit, SWFs do not affect economic gain accrued to global investors.

Since my primary goal is to reconcile the existing debate on the motives and real effect of SWFs, this work is subject to three limitations. The first limitation is that systematic evidence is limited to listed firms. Thus, the no-impact conclusion should not be generalized to other asset classes invested by SWFs, such as private firms and subsidiaries of multinational firms. As a remedy, I provide case-study evidence that reflects the influence of SWFs on firm choices of business location. Based on that, I propose that future research should focus on the conflicts of interest between SWFs and other country players in terms of technology transfer, employment, and tax revenue. To advance research in this direction, private firms and subsidiaries of global firms are suitable subjects for future study. The second limitation

[12] Source: October 31, 2016, "Oman sovereign wealth fund buys into Spanish manufacturer Escribano", www.reuters.com.

is that this work is unable to conduct analysis with respect to actual portfolios of SWFs due to the lack of data. I relied on absolute stake size thresholds to spot firms where hidden impact of SWFs can appear. Alternatively, SWFs would be more inclined to impact target firms not because their stake size is larger but because the deal value represents a larger share of their own portfolios. This caveat is hard to overcome unless SWFs are obliged to continuously disclose their detailed asset allocation. The third limitation is that I do not consider firm payout policy, as I focus on the metric of operating performance predicted by prior theories to reflect political impact. In fact, there is a possibility that SWFs intend to push firms to return more cash to shareholders to partake more profit from listed firms, though this is an issue best left for future research.

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Table 1.1: Stake size distribution

This table compares the distribution of stake size taken by SWFs to that of non-government financial investors who share the same home and host countries with SWFs. The sample period of investment deals is from 1989 to 2012. Panel A reports the overall descriptive statistics for stake bought in the deal. The upper part of Panel B decomposes sample by national border: deals where target firms locate in the same country as the investor(Domestic), where targets firms locate in a different country from the investor(Cross-border). The lower part of Panel B decompose stake bought by 1-digit SIC industries. *, **, and *** indicate variable mean is significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

Panel A.

	N	Mean	Median	Std. Dev.	Min	Max
SWFs	577	4.09	0.50	9.20	0.00	100.00
Non-government financial investors	1720	16.28	10.00	17.32	0.02	100.00
					Mean difference t-stat	-20.95***
					Median rank z-stat	-27.73***

Panel B.

	SWFs				Non-government financial investors			
	N	% of N	Mean	Median	N	% of N	Mean	Median
Domestic	34	6.34%	16.66	11.34	1436	83.49%	15.79	9.78
Cross-border	502	93.66%	3.57	0.56	284	16.51%	18.75	12.38
By one-digit SIC code classification								
Agriculture, forest, fishing	5	0.87%	3.59	0.58	22	1.27%	10.75	7.63
Construction	18	3.12%	3.69	1.59	70	4.03%	18.09	12.69
Finance, insurance, and real estate	102	17.68%	5.8	0.64	213	12.27%	21.33	12.91
Manufacturing	211	36.57%	2.46	0.25	574	33.06%	16.08	10
Mining	35	6.07%	4.71	0.63	401	23.10%	13.68	9.37
Public administration	2	0.35%	1.7	1.7				
Retail trade	36	6.24%	5.25	0.55	59	3.40%	15.49	8.8
Services	68	11.79%	2.53	0.38	225	12.96%	15.44	9.79
Transportation and public utility	87	15.08%	5.57	0.26	101	5.82%	18.19	10.28
Wholesale trade	13	2.25%	9.26	3.32	71	4.09%	18.46	10.15

Table 1.2: Summary statistics and univariate results of firm operation performance

This table reports summary statistics of key accounting variables in Panel A and univariate test results of firm operation performance in Panel B. In Panel A, SWFs are target firms invested by SWFs. Universe are the rest of firms in Worldscope. The sample period of investment deals is from 1989 to 2012. Accounting data is aggregated under event window three years before and five years after the investment event. CapexPPE is capital expenditures scaled by the net balance of property, plants and equipment at year start. ROA is operating income over total assets at year end. SalesGrowth is yearly change in net sales. Q is Tobin's q, computed as the sum (total assets-book equity + market capitalization) divided by total assets at year end. CfTA, DebtTA and DvdTA are net operating cash flow, paid dividend and financial debt scaled by total assets at year start. TA and MktCap are natural logarithm of total assets and market capitalization at year end. All are winsorized at the 5th and 95th percentiles throughout the analysis. Relevant variables are converted into U.S. dollar. Panel B reports the industry-adjusted CapexPPE, SalesGrowth, ROA, and Q of target companies from three years prior to the year of SWFs investment (t) to three years afterwards (t+3). Each performance measure is defined as the difference between its value and the average value of Universe from the same SIC 2-digit industry classification. The first row shows results for my full sample. The second row is for majority interest purchase. The third row is for purchases of influential stake size between 5% and 50%. *, **, and *** indicate variable mean is significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

Panel A

	SWFs				Universe				SWFs-Universe
	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	t-stat
CapexPPE	565	0.57	0.3	0.71	290,744	0.43	0.19	0.61	0.227***
SalesGrowth	387	0.12	0.1	0.28	140,816	0.14	0.08	0.4	-0.039***
ROA	481	0.22	0.17	0.18	305,907	0.29	0.17	0.35	-0.089***
Q	529	1.55	1.18	1.28	299,694	1.47	0.92	1.48	0.082***
CfTA	524	0.12	0.1	0.12	277,025	0.11	0.06	0.27	0.005***
DebtTA	534	0.2	0.17	0.17	303,983	0.31	0.13	0.29	-0.123
TA	565	11.5	11.5	2.62	319,755	9.05	9.63	3.39	3.648***
MktCap	529	11.43	11.46	2.49	292,775	8.68	9.3	3.36	3.670***
DvdTA	349	0.01	0.01	0.01	365,402	0	0	0.02	0.008***

Panel B

	Before				After			Before - After
	t-3	*t-2*	*t-1*	*t*	*t+1*	*t+2*	*t+3*	
CapexPPE								
Full sample	0.41***	0.46***	0.51***	0.41***	0.32***	0.29***	0.22***	-0.13
50%-100%	0.76*	0.54**	0.61**	0.50*	0.26**	0.18	-0.04	-0.41
5%-50%	0.37***	0.42***	0.46***	0.40***	0.38***	0.31***	0.23***	-0.13
SalesGrowth								
Full sample	-0.03	0.03	0.08	-0.11	-0.04	-0.01	0.06	0.05
50%-100%	-0.13***	0.13	-0-02	-0.35**	0.03	-0.14*	0.27***	0.17***
5%-50%	-0.03	0.03	0.09	-0.07	-0.05	-0.08	0.05	0.00
ROA								
Full sample	-0.07***	-0.12***	-0.13***	-0.14***	-0.13***	-0.13***	-0.15***	0.00
50%-100%	-0.08	-0.05	-0.11**	-0.12**	-0.17***	-0.06	-0.1	0.00
5%-50%	-0.11***	-0.12***	-0.14***	-0.14***	-0.13***	-0.13***	-0.15***	0.00
Q								
Full sample	-0.38*	0.1	0.15	-0.34***	0.13	0.17	0.12	0.00
50%-100%	-0.46*	-0.51*	-0.54	-0.41	-0.36	-0.29	-0.37	0.00
5%-50%	-0.17*	-0.1	-0.15	-0.32***	-0.12	-0.17	-0.12	0.00

Table 1.3: Do SWFs affect firm performance? Baseline regression results

(Continued)

This table reports estimation results of Model (1.1) for an event window [-3year,+5year] around SWFs investment events from 1989 to 2012. The sample only include firms in which SWFs invest no less than five percent and their benchmarks. SWF is a time-invariant dummy that equals one for firms that are going to be invested by SWFs at some point in the sample period and equals zero for benchmark firms. After is a dummy that equals one for both SWF target firms and benchmark firms during the [+1Year,+5Year] event window period. SWFAfter is an interaction term of the former two. Panel A display results for the dependent variable CapexPPE, Panel B for SalesGrowth, Panel C for ROA. In Column 1, I benchmark results against an average of all firms from Worldscope that have not been invested by SWFs. In Column 2, I use a control group of firms matched by 2-digit SIC number and host country. In Column 3, I use a control group of firms matched by total assets. In Column 4, the control group is built by matching firms to previous two criteria plus a third factor, i.e. one of firm-level performance measures. In Panel A (CapexPPE), this third matching factor is ROA, in Panel B (SalesGrowth) ROA, and in Panel C (ROA) Q. Column 5 uses the same benchmark as in Column 1 and tests explicitly changes at time points -2year, -1year, 1year, and 2year. The standard errors are in brackets and adjusted for heteroscedasticity and within-firm clustering. *, **, and *** indicate coefficients are significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

Panel A: CapexPPE

	Baseline	SIC & Country	Size	ROA	Short dynamics
	(1)	(2)	(3)	(4)	(5)
SWF	0.461***	0.414***	0.444***	0.430***	
	[0.073]	[0.082]	[0.081]	[0.088]	
After	-0.022***	0.006	0.002	-0.006	
	[0.004]	[0.011]	[0.026]	[0.030]	
SWFAfter	-0.007	-0.064	-0.059	-0.031	
	[0.070]	[0.079]	[0.077]	[0.084]	
SWFBefore^{-2} (1)					0.439*
					[0.088]
SWFBefore^{-1} (2)					0.531*
					[0.090]
SWFAfter^{1} (3)					0.526*
					[0.084]
SWFAfter^{2} (4)					0.467*
					[0.089]
$CfTA_{t-1}$	0.278***	0.114*	0.560*	0.628*	0.248*
	[0.013]	[0.049]	[0.216]	[0.217]	[0.013]
Q_{t-1}	0.050***	0.082***	0.051*	0.078*	0.058*
	[0.002]	[0.006]	[0.023]	[0.028]	[0.002]
TA_{t-1}	-0.015***	-0.017***		-0.013	-0.018*
	[0.001]	[0.002]		[0.012]	[0.001]
$IndustryQ_{t-1}$	0.166***	0.117***	0.143***	0.132***	0.166***
	[0.008]	[0.009]	[0.021]	[0.022]	[0.008]
Constant	0.227***	0.549***	0.284***	0.443*	0.471*
	[0.015]	[0.026]	[0.037]	[0.154]	[0.010]
Observations	188637	21080	2360	2356	188637
adj-R^2	0.064	0.047	0.085	0.08	0.056
p:(1)=(3)					0.361
p:(1)=(4)					0.799
p:(2)=(3)					0.952
p:(2)=(4)					0.515

Table 1.3: Do SWFs affect firm performance? Baseline regression results

(Continued)

Panel B: SalesGrowth

	Baseline	SIC & Country	Size	ROA	Short dynamics
	(1)	(2)	(3)	(4)	(5)
SWF	-0.066	0.041	0.027	0.031	
	[0.035]	[0.031]	[0.040]	[0.046]	
After	-0.066***	0.029***	0.030	0.042	
	[0.003]	[0.007]	[0.026]	[0.032]	
SWFAfter	0.070	-0.057	-0.070	-0.071	
	[0.046]	[0.041]	[0.050]	[0.056]	
SWFBefore^{-2} (1)					0.071
					[0.067]
SWFBefore^{-1} (2)					0.075
					[0.074]
SWFAfter^{1} (3)					-0.024
					[0.052]
SWFAfter^{2} (4)					-0.106
					[0.067]
SalesGrowth$_{t-1}$	0.017***	0.050***	-0.194***	-0.204***	0.023***
	[0.005]	[0.015]	[0.045]	[0.044]	[0.005]
CapexPPE$_{t-1}$	0.057***	0.056***	0.027	0.010	0.060***
	[0.003]	[0.007]	[0.023]	[0.018]	[0.003]
CashTA$_{t-1}$	0.111***	0.131***	0.195**	0.253**	0.110***
	[0.006]	[0.018]	[0.074]	[0.080]	[0.006]
TA$_{t-1}$	-0.000	-0.002**		-0.006	-0.001**
	[0.000]	[0.001]		[0.004]	[0.000]
IndustryQ$_{t-1}$	0.071***	0.028***	0.077***	0.115***	0.075***
	[0.004]	[0.005]	[0.021]	[0.028]	[0.004]
Constant	0.023**	-0.008	-0.064	-0.042	-0.028***
	[0.008]	[0.017]	[0.060]	[0.067]	[0.007]
Observations	89430	9010	1105	1022	89430
adj-R^2	0.033	0.044	0.048	0.065	0.027
p:(1)=(3)					0.310
p:(1)=(4)					0.059
p:(2)=(3)					0.247
p:(2)=(4)					0.059

Table 1.3: Do SWFs affect firm performance? Baseline regression results

Panel C: ROA

	Baseline	SIC & Country	Size	Q	Short dynamics
	(1)	(2)	(3)	(4)	(5)
SWF	-0.108***	-0.044	-0.060*	-0.054	
	[0.025]	[0.024]	[0.025]	[0.032]	
After	0.004	0.027***	-0.014	0.030	
	[0.003]	[0.006]	[0.012]	[0.017]	
SWFAfter	-0.009	-0.026	0.021	-0.017	
	[0.020]	[0.019]	[0.021]	[0.027]	
SWFBefore^{-2} (1)					-0.108***
					[0.028]
SWFBefore^{-1} (2)					-0.115***
					[0.025]
SWFAfter^{1} (3)					-0.116***
					[0.025]
SWFAfter^{2} (4)					-0.102***
					[0.024]
SalesGrowth$_{t-1}$	-0.023***	0.014	0.011	0.014	-0.024***
	[0.003]	[0.009]	[0.011]	[0.017]	[0.003]
CapexPPE$_{t-1}$	0.064***	0.023*	-0.004	0.026	0.064***
	[0.006]	[0.008]	[0.019]	[0.025]	[0.006]
TA$_{t-1}$	-0.016***	-0.021***		-0.010*	-0.016***
	[0.001]	[0.001]		[0.004]	[0.001]
IndustryQ$_{t-1}$	0.036***	0.063***	0.122***	0.143***	0.036***
	[0.007]	[0.005]	[0.022]	[0.021]	[0.007]
Constant	0.377***	0.350***	0.191*	0.117	0.380***
	[0.014]	[0.016]	[0.062]	[0.060]	[0.013]
Observations	84440	8325	1056	987	84440
adj-R^2	0.055	0.195	0.123	0.230	0.055
p:(1)=(3)					0.715
p:(1)=(4)					0.845
p:(2)=(3)					0.956
p:(2)=(4)					0.645

Table 1.4: Do SWFs affect firm performance? Sensitivity to stake size thresholds

This table reports estimation results at different stake thresholds of Model (1.1) for an event window [-3year,+5year] around SWFs investment events from 1989 to 2012. Only target firms of SWFs are included in the model. After is a dummy that equals one for both SWF target firms during the [+1Year,+5Year] event window period. Column 1-4 display results for the dependent variable CapexPPE, Column 5-7 for SalesGrowth, Column 8-10 for ROA. For each dependent variable, I start with all target firms in which SWFs invest no less than five percent, Then I increase the threshold by five percent at a step the number of target firms drop under 30. The standard errors are in brackets and adjusted for heteroscedasticity and within-firm clustering. *, **, and *** indicate coefficients are significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

	CapexPPE				SalesGrowth			ROA		
	Stake ≥ 5%	Stake ≥ 10%	Stake ≥ 15%	Stake ≥ 20%	Stake ≥ 5%	Stake ≥ 10%	Stake ≥ 15%	Stake ≥ 5%	Stake ≥ 10%	Stake ≥ 15%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	-0.022	0	-0.039	-0.086	-0.023	-0.043	-0.106	-0.004	-0.035*	-0.02
	[0.072]	[0.103]	[0.115]	[0.132]	[0.049]	[0.053]	[0.057]	[0.019]	[0.014]	[0.015]
$CfTA_{t-1}$	0.603	0.263	-0.097	0.236						
	[0.315]	[0.474]	[0.650]	[0.918]						
Q_{t-1}	0.072	0.1	0.045	-0.058						
	[0.037]	[0.056]	[0.089]	[0.105]						
$SalesGrowth_{t-1}$					-0.250**	-0.387***	-0.315*	-0.003	0.009	0.021*
					[0.084]	[0.103]	[0.126]	[0.017]	[0.013]	[0.010]
$CapexPPE_{t-1}$					0.039	-0.039	-0.101*	-0.031*	-0.007	-0.01
					[0.041]	[0.044]	[0.040]	[0.014]	[0.012]	[0.016]
$CashTA_{t-1}$					0.408*	0.307	0.36			
					[0.177]	[0.191]	[0.207]			
TA_{t-1}	0.014	0.019	0.032	-0.005	-0.007	0.001	0.003	-0.022	-0.001	-0.004
	[0.019]	[0.029]	[0.035]	[0.041]	[0.011]	[0.016]	[0.023]	[0.013]	[0.005]	[0.006]
$IndustryQ_{t-1}$	0.070	0.306	0.595*	0.896	-0.064	0.016	0.178	0.100*	0.031	0.032
	[0.149]	[0.239]	[0.261]	[0.557]	[0.074]	[0.125]	[0.147]	[0.049]	[0.027]	[0.037]
Constant	0.414	0.038	-0.44	-0.322	0.262	0.146	-0.025	0.320*	0.103	0.13
	[0.352]	[0.488]	[0.572]	[0.958]	[0.211]	[0.279]	[0.291]	[0.137]	[0.069]	[0.094]
Observations	565	300	183	110	282	137	94	278	140	97
adj-R^2	0.018	0.02	0.03	0.026	0.071	0.115	0.082	0.214	0.057	0.029

Table 1.5: Do political SWFs harm firm performance? Multivariate results

(Continued)

This table reports estimation results of Model (1.1) for an event window [-3year,+5year] around SWFs investment events from 1989 to 2012. The sample include target firms in real sectors invested by SWFs and benchmarked non-government financial investors. SWF is a time-invariant dummy that equals one for firms that are going to be invested by SWFs at some point in the sample period and equals zero for benchmark firms. After is a dummy that equals one for both SWF target firms and benchmark firms during the [+1Year,+5Year] event window period. Political is a dummy that equals one for firms, the investors of which are from countries where political interference is likely, according to Table 2 of Bortolotti, Fotak, and Megginson(2015). Panel A display results for the dependent variable SalesGrowth, Panel B for ROA, Panel C for Q. Column (1)-(3) cover an event window [-3year,+5year] around SWFs investment events. Column (4)-(6) only keep the third year after SWFs investment. Column (2) and (5) contain target firms where SWFs or benchmarked private financial investor purchase no less than 5 percent, in Column (3) and (6) no less than 10 percent. The standard errors are in brackets and adjusted for heteroscedasticity and within-firm clustering. *, **, and *** indicate coefficients are significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

Panel A. SalesGrowth

	[-3Year, +5Year]	[-3Year, +5Year] stake ≥ 5%	[-3Year, +5Year] stake ≥ 10%	Year 3	Year 3 stake ≥ 5%	Year 3 stake ≥ 10%
	(1)	(2)	(3)	(4)	(5)	(6)
SWF	0.015	-0.034	-0.034	0.134*	0.035	0.014
	[0.037]	[0.066]	[0.109]	[0.064]	[0.075]	[0.084]
After	0.001	-0.009	-0.03			
	[0.044]	[0.048]	[0.064]			
SWFAfter	-0.01	-0.071	-0.073			
	[0.046]	[0.065]	[0.075]			
SWFAfterPolitical	0.071*	0.134	0.089	-0.055	-0.01	0.534**
	[0.031]	[0.086]	[0.055]	[0.045]	[0.139]	[0.177]
$SalesGrowth_{t-1}$	-0.127***	-0.168**	-0.221***	-0.071	-0.084	-0.252***
	[0.029]	[0.052]	[0.057]	[0.070]	[0.084]	[0.055]
$CapexPPE_{t-1}$	0.024	0.033	0.023	0.002	-0.008	-0.146***
	[0.013]	[0.024]	[0.036]	[0.032]	[0.044]	[0.029]
$CashTA_{t-1}$	0.131**	0.108	0.108	-0.078	0.026	0.361**
	[0.042]	[0.065]	[0.070]	[0.113]	[0.127]	[0.109]
TA_{t-1}	0	0.008	0.016	0.023*	0.03	0.042*
	[0.006]	[0.012]	[0.020]	[0.011]	[0.016]	[0.019]
Constant	0.064	-0.207	-0.053	-0.121	0.003	0.011
	[0.089]	[0.186]	[0.236]	[0.182]	[0.276]	[0.284]
Observations	358748	88507	48002	26118	8508	5130
adj-R^2	0.047	0.074	0.105	0.47	0.568	0.815

Table 1.5: Do political SWFs harm firm performance? Multivariate results

Panel B. ROA

	[-3Year, +5Year]	[-3Year, +5Year] stake ≥ 5%	[-3Year, +5Year] stake ≥ 10%	Year 3	Year 3 stake ≥ 5%	Year 3 stake ≥ 10%
	(1)	(2)	(3)	(4)	(5)	(6)
SWF	0.073**	-0.013	-0.057	0.099**	0.115	-0.02
	[0.027]	[0.030]	[0.054]	[0.037]	[0.060]	[0.091]
After	0.02	0.011	0.016			
	[0.015]	[0.015]	[0.019]			
SWFAfter	-0.022	-0.003	-0.007			
	[0.015]	[0.025]	[0.036]			
SWFAfterPolitical	-0.035	-0.018	0.002	-0.013	-0.081	0.155
	[0.029]	[0.033]	[0.031]	[0.041]	[0.068]	[0.117]
$SalesGrowth_{t-1}$	0.02	0.033**	0.028	0.004	0.014	0.092
	[0.011]	[0.016]	[0.018]	[0.053]	[0.075]	[0.056]
$CapexPPE_{t-1}$	-0.015	-0.026**	-0.022	-0.011	-0.036	-0.062**
	[0.008]	[0.011]	[0.012]	[0.025]	[0.032]	[0.023]
TA_{t-1}	-0.024***	-0.01	-0.030**	-0.042***	-0.044**	-0.040**
	[0.006]	[0.008]	[0.012]	[0.011]	[0.015]	[0.019]
Constant	0.440***	0.406**	0.588**	0.610***	0.327	0.447
	[0.099]	[0.151]	[0.189]	[0.144]	[0.267]	[0.235]
Observations	340795	91462	50472	24797	8500	5132
adj-R^2	0.369	0.236	0.343	0.485	0.494	0.754

Panel C. Tobin's q

	[-3Year, +5Year]	[-3Year, +5Year] stake ≥ 5%	[-3Year, +5Year] stake ≥ 10%	Year 3	Year 3 stake ≥ 5%	Year 3 stake ≥ 10%
	(1)	(2)	(3)	(4)	(5)	(6)
SWF	0.239	0.206	-0.013	0.141	0.253	0.344
	[0.195]	[0.317]	[0.572]	[0.177]	[0.235]	[0.271]
After	-0.122	-0.079	-0.055			
	[0.115]	[0.119]	[0.164]			
SWFAfter	0.143	0.031	-0.013			
	[0.121]	[0.152]	[0.200]			
SWFAfterPolitical	0.036	0.18	0.46	-0.383	-0.59	0.115
	[0.175]	[0.240]	[0.309]	[0.252]	[0.404]	[0.628]
$SalesGrowth_{t-1}$	-0.058	-0.182	-0.069	0.501**	0.306	0.924**
	[0.080]	[0.105]	[0.125]	[0.233]	[0.350]	[0.220]
ROA_{t-1}	1.636**	0.6	-0.81	2.426**	1.971**	0.611
	[0.291]	[0.575]	[0.705]	[0.562]	[0.595]	[0.383]
$DebtTA_{t-1}$	-0.127	0.16	0.459	-0.32	-1.462**	0.738
	[0.211]	[0.519]	[0.970]	[0.370]	[0.736]	[0.443]
TA_{t-1}	-0.208**	-0.226**	-0.331**	-0.017	0.037	-0.169**
	[0.035]	[0.056]	[0.105]	[0.048]	[0.053]	[0.048]
Constant	3.938**	4.213**	5.672**	1.801**	0.388	2.161**
	[0.548]	[0.838]	[1.342]	[0.657]	[0.855]	[0.702]
Observations	339944	91447	50325	24795	8499	5001
adj-R^2	0.295	0.25	0.313	0.5	0.59	0.825

Table 1.6: Do SWFs create or select value? Multivariate results

This table reports estimation results of the association between Tobin's q and SWFs becoming shareholders based on Model (1.1) for an event window [-3year,+5year] around SWFs investment events between 1989 and 2012. The dependent variable is \$Q\$. In Column 1-2, I keep target firms in which SWFs invest more than five percent. In Column 3-4, the stake threshold is decreased to one percent. In Column 5-6, I use the full sample. The benchmark is a control group matched by 2-digit SIC, host country, and ROA. The standard errors are in brackets and adjusted for heteroscedasticity and within-firm clustering. *, **, and *** indicate coefficients are significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

	Stake ≥ 5% (1)	Stake ≥ 5% (2)	Stake ≥ 1% (3)	Stake ≥ 1% (4)	All stake (5)	All stake (6)
SWF	0.079		0.215**		0.350***	
	[0.138]		[0.093]		[0.060]	
After	-0.123		-0.076		0.011	
	[0.074]		[0.053]		[0.028]	
SWFAfter	0.109		0.069		-0.009	
	[0.125]		[0.098]		[0.057]	
$SWFBefore^{-2}$ *(1)*		0.374		0.348**		0.333***
		[0.208]		[0.120]		[0.068]
$SWFBefore^{-1}$ *(2)*		0.102		0.270**		0.301***
		[0.150]		[0.106]		[0.062]
$SWFAfter^{1}$ *(3)*		0.112		0.277**		0.342***
		[0.129]		[0.101]		[0.063]
$SWFAfter^{2}$ *(4)*		0.156		0.213		0.324***
		[0.125]		[0.110]		[0.068]
$SalesGrowth_{t-1}$	0.058	0.056	0.067	0.064	0.160***	0.158***
	[0.067]	[0.067]	[0.049]	[0.049]	[0.036]	[0.036]
$DebtTA_{t-1}$	-0.242	-0.241	-0.057	-0.066	-0.125	-0.127**
	[0.201]	[0.200]	[0.142]	[0.141]	[0.064]	[0.064]
TA_{t-1}	-0.086***	-0.086***	-0.092***	-0.089***	-0.067***	-0.060***
	[0.016]	[0.016]	[0.013]	[0.013]	[0.008]	[0.007]
Constant	2.158***	2.104***	2.226***	2.184***	1.816***	1.784***
	[0.205]	[0.207]	[0.167]	[0.168]	[0.079]	[0.078]
Observations	1041	1041	2268	2268	5872	5872
adj-R^2	0.067	0.066	0.06	0.058	0.049	0.044
p:(1)=(3)		0.238		0.558		0.888
p:(1)=(4)		0.311		0.312		0.904
p:(2)=(3)		0.941		0.947		0.506
p:(2)=(4)		0.713		0.673		0.737

Chapter 2: Do financial bidders exploit underpricing as a motive in acquisitions?

2.1. Introduction

Financial bidders pay an average premium of about 35 percent in acquisitions even though they do not benefit from operational synergies with target firms.[13] How do they justify such premiums? Their ability to unlock significant value is typically attributed to two main factors. First, to the operational improvements that financial acquirers can implement after the acquisition. Existing studies show that financial bidders are especially good at acquiring mismanaged firms and improving their operations and governance (Gorbenko and Malenko, 2014; Hege, Lovo, Solvin, and Sushka, 2013). And second, to the benefit they obtain from the heavy use of debt financing at favorable terms (Axelson, Jenkinson, Strömberg, and Weisbach, 2013; Guo, Hotchkiss, and Song, 2011; Martos-Vila, Rhodes-Kropf, and Harford, 2013). An alternative plausible hypothesis is that financial bidders are skilled at identifying mispriced targets whose prospects are not clearly understood by the market. Under this hypothesis, mispricing creates a profit opportunity and financial acquirers can benefit by acquiring firms whose market value is below its fundamental value. [14,15] Although theoretically appealing, in practice, it is not clear whether such opportunities exist, whether they can be successfully exploited and if so, whether financial acquirers are the smart investors. In this study, we aim to explore these questions.

Profit opportunities may exist to the extent that market values deviate from intrinsic values. Market values should reflect intrinsic values; however, there can be moments where the two are misaligned and watchful investors can benefit from that. In this respect, Shleifer and Vishny (2003) and Rhodes-Kropf and Viswanathan (2004) develop theories where stock market misvaluation, interpreted as mispricing or valuation errors, drives acquisition activity. Several other papers provide empirical support (see, e.g. Rhodes-Kropf, Robinson, and Viswanathan, 2005; Dong, Hirshleifer, Richardson, and Teoh, 2006; and Edmans et al., 2012). Even if valuation errors may affect the stock price of any firm, deviations from intrinsic values may be more prevalent in less efficient markets or when information asymmetries are particularly important. Thus, if acquirers can successfully benefit from mispricing, we would expect that they do so especially in acquisitions where the target is subject to larger information asymmetries.

To the extent that profits opportunities can be successfully exploited, we hypothesize that the identity of the acquirer may matter. In the absence of synergistic gains, financial bidders may specialize or may be more skilled at exploiting mispricing. Dittmar, Li, and Nain (2012) suggest that financial bidders are particularly skilled at identifying undervalued targets. That financial acquirers are good at

[13] For instance, both Bargeron, Schlingemann, Stulz, and Zutter (2008) and Gorbenko and Malenko (2014) document a premium of 35 percent offered by financial bidders.
[14] In this study, acquirer refers indistinctively to bidder who fails or succeeds in completing a takeover bid. Undervaluation refers indistinctively to mismanagement and/or mispricing.
[15] Gaughan (2014) in Chapter 8 "Private equity market" explain that "Privat equity funds seek out investments that are undervalued". Damodaran (2001) in Chapter 25 "Motives for acquisitions" refers to undervaluation as a motive driving acquisitions by financial acquirers.

identifying undervalued targets is also prevalent in professional circles.[16] Alternatively, financial bidders may enjoy an information advantage that makes them be better placed to benefit from price deviations. In contrast, strategic acquirers focusing on synergistic gains may neglect other sources of gains or simply may not be so skilled at detecting mispricing. Both under the view that each type of bidder specializes in different sources of gains (Gorbenko and Malenko, 2014) or that it has differential skills (Dittmar et al., 2012), financial bidders are more likely to exploit and reveal mispricing.

Testing our hypothesis is challenging because measuring mispricing is not straightforward. To date, existing studies have used different approaches to measure mispricing. Some studies relate valuation ratios such as the market-to-book ratio to takeover premiums or the likelihood of being acquired to evaluate whether undervaluation motivates acquisition activity. Renneboog, Simons, and Wright (2007) show financial acquirers offer higher premiums for firms with low market-to-book ratios.[17] With these findings, existing studies conclude that undervaluation is one of the motives behind acquisitions and a source of gains for financial acquirers. However, their main limitation is the use of valuation ratios that are both correlated to poor management and mispricing and therefore make a vague use of the term undervaluation to refer indistinctively to mispricing and mismanagement. Other studies have the goal of testing whether misevaluation, interpreted as mispricing, drives acquisition activity and use valuation ratios (Dong et al., 2006) or a decomposition of them (Rhodes-Kropf et al., 2005) that depend on the estimation of fundamental values. These papers estimate fundamental values by using accounting-based valuation models to predict mispricing errors. With this approach, any interpretation critically rests on the estimation of fundamental values and the adequate integration of growth options and of managerial efficiency. Their main challenge rests on disentangling mispricing from other sources driving valuations such as the inefficient running of the firm or variation in growth options. Last, Edmans et al. (2012) use non-fundamental shocks to stock prices to infer valuation errors.

To circumvent the above difficulties, we study acquisition bids that fail to complete. We estimate the market reaction from the announcement of the offer to the failure of the bid to evaluate whether the target price upon failure fully reverts to the pre-announcement price level or whether a revaluation effect remains. By construction, a potential revaluation should be unrelated to value-creation hypothesis because any value to be added by the envisioned takeover becomes invalid once the takeover attempt fails. Therefore, any potential revaluation can be read as a reassessment of the stand-alone value of the target and interpreted as ex-ante mispricing.[18] To further evaluate whether a potential revaluation is related to the identity of the acquirer, we compare the revaluation effect around failed bids by financial bidders to that of strategic bidders. In this set-up, according to our hypothesis, we would expect a larger

[16] For example, *The Strategic Secret of Private Equity*, Harvard Business Review, September 2007.
[17] Dittmar et al. (2012) infer target undervaluation while examining bidder wealth effects. This approach does not distinguish either between a reassessment of value or value creation by the acquisition.
[18] We may not observe any revaluation effect even if mispricing is motivating an acquisition. This is when the announcement of the acquisition does not convey information to the market about the sources of gains.

revaluation effect for financial bidders. Since we posit that informational effects drive revaluation, we would also expect that the revaluation attached to financial bidders is more relevant when informational asymmetries are more likely to be at play.

Our dataset includes a panel of 789 acquisition bids that failed over the period 1980-2015, of which, 258 bids are initiated by financial acquirers and 531 by strategic acquirers. To start with we conduct an event study on target firm stock returns from the announcement of the bid to the announcement of the failure and evaluate whether there is any revaluation effect. Consistent with previous studies, we find that the target price upon failure does not fully revert to the pre-announcement price level and a positive revaluation remains. This revaluation is mostly associated to the identity of the bidder and it is significantly larger for failed bids initiated by financial bidders and when target firms are small. We interpret this finding as evidence that financial bidders seem to exploit mispricing opportunities to a larger extent than strategic bidders. Though, this profit opportunity seems to motivate acquisition bids only when information asymmetries are significant or in the presence of information advantage.

Although our approach circumvents the need to estimate fundamental values, it is still subject to some challenges because the revaluation effect could be the product of other forces and not just a reflection of mispricing. First, we should be concerned with the possibility that selection into bid failure is different for deals involving financial bidders and targets with large information asymmetries from other failed deals. If an unobservable variable correlated with the type of bidder leads to failure, and simultaneously impacts the stock price, our findings may be biased. To address this concern, we collect and identify failure reasons and classify bids in two subgroups. Subgroup one includes bids whose failure reason may be correlated to target stock prices and the second subgroup includes bids that failed due to exogenous reasons and bid failures for which no relevant information was disclosed over the event window. In instances where new information is not conveyed to the market from the announcement of the offer to the announcement of the failure, we presume that failure itself is unlikely to be correlated to an abnormal price run up. We then re-do our analysis for the second subgroup and find a significantly larger revaluation effect for failed bids initiated by financial bidders and targeting small firms event when bids failed for exogenous reasons or in circumstances where no information was disclosed to the market. Furthermore, we also evaluate the distribution of failure reasons for bids launched by financial bidders for small firms and compare it to that of bids launched by strategic bidders. We do not find significant differences or that certain reasons appear more frequently for bids involving a given type of acquirer. We conclude that our findings are not driven by the possibility that selection into bid failure depends on the type of acquirer.

Second, the revaluation effect could be the outcome of anticipation effects. Bradley, Desai, and Kim (1983) show that the revaluation effect could reflect the anticipation of future takeovers and thus target firms subject to failed bids may have a higher probability of being acquired in the future. In our set-up,

this would imply that the revaluation effect around unsuccessful bids by financial bidders for small target simply reflects that those targets are more likely to be acquired in the future, or be acquired at a higher speed or at a higher premium than other targets subject to unsuccessful bids. We perform a survival analysis and we also evaluate if a higher revaluation predicts a higher offer price and we do not find evidence supporting this conjecture.

Alternatively, the revaluation effect could simply reflect the anticipation of future operational improvements by current management (Hirshleifer and Titman, 1990; Safieddine and Titman, 1999). To deal with this concern, we drop from the sample failed bids where targets are presumably more likely to have a margin for operational improvements such as hostile bids and bids that failed due to a rejection by the board of directors of the target firm. Our results remain unchanged. All in all, we do not find evidence that calls for an alternative interpretation of our findings.

Our analysis contributes to several strands of the literature. First, we add to the literature that explores the motives driving acquisitions and the sources of acquisition gains and, in particular, to the literature that studies how financial acquirers justify their acquisition premiums. Previous studies document acquisition gains that emanate from improvements in sales and operating margin (Acharya, Gottschalg, Hahn, and Kehoe, 2013), from favorable debt financing terms (Axelson et al., 2013; Martos-Vila et al., 2013), or from realized tax benefits and sector returns (Guo et al., 2011). Other studies show that misvaluation also drives acquisition activity (Shleifer and Vishny, 2003; Rhodes-Kropf et al., 2004, 2005; Dong et al., 2006). This work shows that particularly underpricing motivates acquisitions especially when bidders are financial acquirers that target small firms. And as such, the identity of the bidder conveys information to the market about the stand-alone value of the target firm.

We also contribute to the literature that evaluates the rational of the revaluation effect around unsuccessful takeovers (Bradley et al.,1983; Safieddine and Titman, 1999; Savor and Lu, 2009; Malmendier et al., 2016). More precisely, this work complements the findings in Malmendier et al. (2016) who show that revaluations are partly related to the information embedded in the medium of exchange. We show that revaluations are also and importantly related to the type of acquirer and the size of the target. Thus, the revaluation effect seems to be partly due to purely informational effects related to the identity of the acquirer and, to a lower extent, the medium of exchange. The informational effects conveyed by the identity of the acquirer seem to be more relevant than those conveyed by the medium of exchange when information asymmetries about the value of the target are significant or in the presence of an informational advantage.[19]

[19] Our results also strengthen the role of informational effects as opposed to Bradley et al. (1983). Contrary to the results of their study, the informational effects seem to be larger than the synergy effects. If synergies would be the main driver of the revaluation effects, then we should have observed stronger revaluation for strategic acquirers.

Last, our results add some insights to studies that examine the differences between financial and strategic bidders (see, e.g., Gorbenko and Malenko, 2014; Dittmar et al., 2012; Bargeron et al., 2008).[20] In particular, Gorbenko and Malenko (2014) show that the two types of acquirers target different firms in auctions, suggesting that they are driven by different motives in acquisitions. To our knowledge, our study is the first to study informational effects attached to the identity of the buyer in the context of failed bids. By doing so, it reveals that underpricing seems to motivate financial bidders to engage in acquisitions and in the business of fishing underpriced firms. That is not the case for strategic bidders that are moved by other drivers when acquiring target firms.

2.2. Review of the literature, contribution and hypothesis development

2.2.1. Undervaluation as a motive in mergers

We build on the idea that mispricing is, among others, a motive in acquisitions (Shleifer and Vishny, 2003; Rhodes-Kropf et al., 2004, 2005; Dong et al., 2006). That stock market misvaluation encourages acquisitions is at the heart of Shleifer and Vishny (2003) and Rhodes-Kropf et al. (2004) models. While Shleifer and Vishny's model generates a set of predictions, their prediction that undervalued firms become natural targets is the most relevant in our context.[21] Thus, potential bidders are more likely to bid when they perceive that the target firm is underpriced by the market. This incentive is explained by information differences between bidders and outside investors. Consequently, outside investors observing that will bid up the target stock price.

In this study, we propose that underpricing is especially a driving motive in the case of financial bidders. That is, bidder identity matters when it comes to exploiting inefficient valuation in the context of acquisitions. This is for two reasons: first, financial bidders need to exploit sources of value other than synergies and as such spotting underpricing may be at the center of their activities and second, they specialize and develop skills that they use to profit from buying underpriced targets. Thus, financial bidders may have both an incentive to specialize is exploiting pricing errors and an information advantage and those together will be more rewarding when target firms are exposed to larger information asymmetries.

Various studies have attempted to empirically evaluate the predictions of Shleifer and Vishny (2003) and Rhodes-Kropf et al. (2004) models. For that, they have tackled the task of measuring mispricing. One approach uses valuation ratios such as the price-to-book or the price-to-residual value (Dong et al., 2006) or their decomposition (Rhodes-Kropf et al., 2005) to measure mispricing. For that, they need to estimate fundamental values and do so by using accounting-based valuation models to predict

[20] Papers that study differences between financial and strategic bidders often evaluate announcement returns and related them to the identity of the acquirer (see, e.g., Bargeron et al. 2008; Dittmar et al. 2012; Hege et al. 2013). From a different perspective, Gorbenko and Malenko (2014) studies competition and bids within each auction and Fidrmuc et al (2012) compare the selling process of firms acquired by strategic acquirers to that of financial acquirers.

[21] They also predict that cash acquisitions are more likely when targets are undervalued.

mispricing errors. With this approach, any interpretation critically rests on the estimation of fundamental values and the adequate integration of growth options and of managerial efficiency. Their main challenge rests on disentangling mispricing from other sources driving valuations such as the inefficient running of the firm or variation in growth options.

Regarding Shleifer and Vishny's prediction that undervalued firms become natural targets, Rhodes-Kropf et al. (2005) shows that cash targets are undervalued while stock targets are overvalued. In parallel, Dong et al. (2006) find that cash bids are associated to bidders willing to acquire undervalued targets. In face of this finding, Dong et al. (2006) also examine and rationalize why target shareholders would sell if the target firm is undervalued. They propose that target management should be willing to cash out in cases of lower undervaluation. Although it is possible that target management is pressed to sell even if underpricing is significant. This can happen if shareholders are excited about the offer premium and eager to cash out or if they receive side payments by bidders or alternatively, if they are less informed that bidders about the true value of the firm. In this sense, Dessaint, Foucault, and Frésard (2016) gives support to the later as they show that managers are not always able to filter out noise in prices and to distinguish fundamental from no fundamental changes in stock prices. Dong et al. (2006) findings are open to two interpretations: that it is target mispricing that motivates acquisitions or that it is the undervaluation related to inefficient management. With our tests we aim to disentangle and distinguish among the interpretations more sharply and evaluate whether the mispricing interpretation is valid.

An alternative approach used by Barraclough, Robinson, Smith, and Whaley (2013) and Borochin (2014) consists on using option prices to estimate synergies and information effects around mergers and acquisitions. While the main objective of these papers is to estimate synergies and value creation, their approach allows estimating whether acquisition announcements reveal news about the stand-alone value of bidders and targets. Specifically, Barraclough et al. (2013) find that M&A announcements reveal good news about targets and Borochin (2014) adds this is specially the case for cash bids. However, these papers do not evaluate which sort of news they may be at play or whether the reassessment of bidder and target value is simply and adjustment of valuation errors. Thought this approach could bring interesting insights in our case, we would need data on call prices which are not available in for a significant part of our sample.

Last, Edmans et al. (2012) propose another method to evaluate whether undervaluation is related to increased takeover activity. Instead of measuring mispricing directly, they use mutual fund redemptions as an exogenous shock to stock prices and document that the resulting non-fundamental price fall translates in low target valuations and increases takeover probability.[22]

[22] In a robustness check, they show that takeover probabilities increase more for financially driven acquisitions.

To go around the need to estimate fundamental value directly and the lack of option data, we directly focus on failed deals and study target prices to measure mispricing. Studying price reactions around failed deals allows pinning down mispricing as a motive in acquisitions because any price reaction or revaluation remaining after deal failure should only reflect a reassessment of the stand-alone value of the target firm.

2.2.2. Failed deals and revaluation effects: sources of informational effects

While returns around bid announcements factor in information about acquisition gains, the probability of success and a reassessment of stand-alone values of both the bidder and the target firm, target returns following failed deals result solely from the reassessment of the stand-alone value of the target firm thus providing an ideal set-up to examine informational effects.

The early papers of Dodd and Ruback (1977) and Bradley (1980) document a permanent and positive revaluation of unsuccessful targets and attribute that revaluation to informational effects. Since then, the context of failed has been used and studied in several papers. Bradley et al. (1983) attribute target revaluation effects to the anticipation of future acquisition activity and therefore to premiums that target shareholders discount to have soon. Safieddine and Titman (1999) provide evidence that targets, following unsuccessful bids, engage in leverage increasing transactions and other corporate actions in the interest of shareholders who anticipate operational improvements. With these results, Safieddine and Titman (1999) attribute revaluation effects to the anticipation of operational improvements rather than purely informational effects. In contrast, Savor and Lu (2009) study bidder revaluation effects around failed deals and use it as a counterfactual in their tests. More recently, also in the context of fail deals, Malmendier et al (2016) find that only cash bids are subject to a revaluation while there is none around stock bids. They also rule out that the anticipation of future takeover activities or operational changes are responsible for these findings and attribute them solely to informational effects conveyed by the medium of exchange about undervaluation of the target. In contrast, Boyson, Gantchev, and Shivdasani (2017) find that there is no revaluation around fail bids for activism targets, but those targets experience long term abnormal returns that are associated to value-enhancing operational and financial policy changes.

The above evidence shows that indeed revaluation effects may be the source of different forces at work and warns us that the use of fail bids as an ideal set-up to study informational effects is not absent of several challenges than will need to be carefully examined. We carefully do so latter in our study. In this sense, we also contribute to understand the drivers of revaluation gains around bid failure and, particularly, we expand on the sources of informational effects by evaluating to what extend the identity of the bidder plays a role. We hypothesize that the identity of the bidder matters as it conveys information to the market about the true value of the target. We also propose that the revelation of new information to the market is more relevant a) in the presence of information asymmetries and b) when the acquirer

has an information advantage. Information asymmetries are typically more prevalent is smaller firms. Thus, we use the size of the firm to proxy for the extent of information asymmetries and we conjecture that the revaluation effect attached to the presence of financial bidders will be larger the smaller is the target firm. Also, when the bidder is proximate to the target firm, he may enjoy an information advantage.[23] To capture this idea, we posit that the information advantage of financial bidders will be larger when they are in the same country as target firms. In contrast with cross-border transactions, domestic transactions imply a closeness of the parties involved in a bid, not only in terms of physical proximity but also cultural, economic and institutional proximity. Financial bidders having a better knowledge of country characteristics such as language, fiscal policies, regulations, cultural and institutional dimensions will be able to better assess the extent of mispricing and their bid may be more informative about the stand-alone value of the target firm.[24] In this set-up, cross-border transactions will benefit less from this information advantage and, consequently, the revaluation effect will be less prominent.

2.3. Data

2.1. Sample construction

Our sample of failed acquisition bids comes from SDC Mergers and Acquisition database for the period 1980 to 2015. We collect bids with deal status "withdrawn", "intention withdrawn", "discontinued rumored" and "seek buyer withdrawn" and thus, we discard bids with deal status "completed" or "pending".[25] To start with, we include all bids regardless of the country of incorporation of the bidder and the target. We then retain bids that comply with the following restrictions. First, the target is a public firm for which we can obtain market prices and identify announcement and withdrawal dates.[26] Second, the deal involves a bid of more than 50 percent and up to 100 percent of the target. This is to ensure a real change of ownership if the deal was completed.[27] Furthermore, to filter noise posed by target insiders, we exclude bids by the target management or employees. And finally, we also exclude bids subject to a third party offer for the target while the original bid under analysis is pending. This choice allows us to reduce noise brought by any valuation effect related to a third party outbid.

To identify the bidder type, financial or strategic, we look at bidders' 4-digit SIC code. Financial bidders have SIC codes between 6000 and 6900. Strategic bidders have SIC codes either below 6000 or

[23] In the context of financial analysts, proximity has been shown to be a relevant factor explaining superior performance (Orpurt (2004), Malloy (2005), Bae et al. (2005).
[24] The location of the bidder is not critical for the extent of information asymmetries embedded into the market price but rather the location of the marginal investor.
[25] At this stage, these bids include "Withdrawn", "Discontinued Rumoured", "Seek Buyer Withdrawn", and "Intention Withdrawn". We note that 99 percent of our cleaned sample refers to "Withdrawn" and 1 percent to "Intention Withdrawn".
[26] SDC considers firms with more than 50 percent free-float to be public firms. Thus, in this step we retain listed firms with more of 50% free float before the deal announcement and exclude all other firms.
[27] Tough the undervaluation hypothesis may also apply to minority interest bids, we use takeover bids for two reasons. First, a takeover bid catches much more market attention than a minority purchase and enables us to measure more clearly reactions in market prices from the announcement of the bid to its withdrawal. Second, the sample size of minority bid failures is too small to provide statistical significance once we require disclosure of the premium and public status of the targets.

over 6999. Once we have identified the type of bidder, we exclude financial services' targets with SIC codes between 6000 and 6999. This choice allows us to rule out bids by financial bidders that could be motivated by operational synergies and allows us to focus on non-synergistic motives driving acquisition decisions.[28] After that step, our sample includes 3'815 failed deals; that is, 1'277 involving financial bidders and 2'538 involving strategic bidders.

Next, for every target firm involved in a deal, we obtain stock prices and financial data from Datastream and Worldscope databases for non-US firms and from CRSP and Compustat for US firms. To add price and financial data to our sample of failed deals we use as matching variables the Datastream code, the firm name, and the country of primary listing for non-US firms.[29] Then, we use the 6-digit CUSIP and the firm name for US firms since the Datastream code for these firms in largely missing in the SDC database. Adding available price and financial data reduces our sample to 2'945 failed deals; that is 1'019 (1'926) involving financial (strategic) bidders. We then drop deals for which target stock price data is not available 275 days prior to the bid announcement date[30] and deals where the offer price is lower than the market price. With this step, our sample shrinks to a total of 2'204 failed deals involving 877 (1'327) failed deals by financial (strategic) bidders.

To finalize our sample, we impose two last but relevant restrictions. Consistent with existing studies, we keep only deals with non-missing observations for key variables that can impact target revaluation as identified by previous literature. These are the medium of payment, the offer price, an indicator for hostile attitude, an indicator for tender offer and the target market capitalization.[31] We then use these variables as control variables in the empirical analysis. Last following Officer (2003), we retain only bids when bid premiums, computed as offer price over target market capitalization, are positive and lower than 200%.[32]

The medium of payment captures how an acquirer pays for the acquisition. Shleifer and Vishny (2003) predict that deals with an underpriced target are more likely to be paid in cash. Empirically, Malmendier et al. (2016) show that target revaluation upon deal failure is mostly driven by deals involving cash payments. To measure medium of payment we define 3 variables. *Cash* is the percentage of deal value to be paid in cash. *Stock* refers to the percentage of deal value to be paid in stock and *Other* refers to the percentage of deal value to be paid by other means.

[28] In un-reported tests, we re-do all estimations including bids where targets are from the financial service sector and acquirers are strategic. Our results do not change.
[29] Datastream assigns different DS codes to cross-listed firms by stock exchange and issued security. Matching by DS code assigned to the primary stock exchange, we neglect trades on other stock exchanges. We note that his should not impact our measure of cumulative abnormal returns for a multiple-week event window because price parity deviation are only relevant in multi-market intraday trading (Gagnon and Karolyi, 2010). Datastream also assigns different DS codes for a single firm that issues multiple classes of shares, such as China. This has a bearing on only 1 percent of our sample, this again should not cause any big problem.
[30] With this step, our sample shrinks to 948 (1'458) failed deals by financial (strategic) bidders.
[31] We do not include deal value because it is highly correlated with target firm market capitalization, the key firm-level control variable in our regression. Previous studies (e.g. Malmendier et al., 2016) include deal value to gauge acquisition cost relative to bidders' business scale by scaling deal value by bidder's market capitalization. This is infeasible in our setting because most financial bidders are private.
[32] See for example, Officer (2003), Bates and Lemmon (2003), Moeller et al. (2004), etc.

The offer price, once announced, immediately sets a reference for other investors of the target value in the eyes of its bidder. Thus, it should also serve as a benchmark for price correction on prior-bid mispricing. We use the offer price over the target stock price four weeks before the bid announcement to compute the *Premium* that we then use in our tests. Karolyi and Liao (2017) and Malmendier et al. (2016) document a positive association between the premium and the short-run stock price reaction to the acquisition announcement and similarly, we expect a positive relation to the revaluation effect.

We also construct an indicator for hostile attitude. *Hostile* is a dummy that equals one when the acquisition attempt is hostile. An hostile takeover bid, even if unsuccessful, can trigger market anticipation that target management would improve future operational performance after resisting the bid (Hirshleifer and Titman, 1990). In a similar vein, *TenderOffer* is a dummy that equals one when the bid is instrumented as a tender offer. A tender offer can build up market expectation that the target may end up receiving a premium from a subsequent offer (Jensen and Ruback, 1983).

Finally, target market capitalization is the last key variable that we require to be non-missing. It refers to the total number of target shares outstanding times target stock price 4 weeks prior to the announcement of the bid and aims to measure target firm size and to control for information asymmetries. Our final sample of failed deals consists of a total of 789 deals, including 258 failed deals by financial bidders and 531 by strategic bidders.

As discussed in introduction, we consider failed deals as a counterfactual to completed deals. That requires us to assess the comparability between failed and completed deals. To construct a comparable sample of completed deals, we collect deals with deal status "completed" in SDC database and we follow the same procedures just described above. The final sample of completed deals consists of a total of 6'583 deals, including 1'283 completed deals by financial bidders and 5'300 by strategic bidders.

2.2. Summary statistics

Table 2.1, Panel A reports summary statistics for bids made by financial bidders and Panel B for bids made by strategic bidders. In both panels, we precisely report summary statistics for various deal-level characteristics. Panel A shows that, both completed and failed bids issued by financial bidders, are similar regarding the medium of payment (*Cash, Stock, Other*), days between the announcement and the completion/withdrawal date (*Days*) , market-to-book value (*MB*), premium (*Premium*) and the size of the stake sought by the bidder (*StakeSought*). Panel B shows that, both completed and failed bids issued by strategic bidders, are similar in terms of market-to-book value (*MB*), premium *(Premium)* and the size of the stake sought by the bidder (*StakeSought*). However, completed and failed bids issued by strategic bidders are marginally different with respect to the medium of payment (*Cash, Stock*), and days between the announcement and the completion/withdrawal date (*Days*). Unsuccessful deal attempts by

strategic bidders tend to be resolved (withdrawn) faster than completed deals and there is a higher proportion of attempted deals to be paid with stocks that fail.

Panel A and B also outline differences between completed and failed bids that are common to financial and strategic bidders. Failed bids tend to involve targets of bigger size (*DealValue* and *TargetSize*) when financial bidders are involved and of smaller size when strategic bidders are involved. For both financial and strategic bidders, failed bids are more likely to be hostile (*Hostile*), and less likely to be tender offers (*TenderOffer*), involve target fees in case of termination (*TTerm*), or hold a smaller block size prior to bid (*Toehold*) than completed bids. These findings are largely consistent with previous studies[33] and these variables highly correlated with failure and eventually can be related to target price run-ups despite failure.[34] For example, Hirshleifer and Titman (1990) argue that, while resisting a hostile bid, the target firm may also learn about and take actions to implement policies planned by the bidder and which in turn increases the target stock price. Thus, it is important to account for such effects in our tests, and for that, we will include these variables as controls in the regression setting described in Section 2.3.

Table 2.1, Panel C evaluates whether there is any significant difference between bids issued by financial and strategic bidders both for completed and failed samples. The most relevant difference relates to the premium offered and to the medium of payment, regardless if a bid is completed or not. Financial bidders offer on average premiums that are 7.8 percent lower than those offered by strategic bidders.[35] The lower premium can be explained by the absence of operational synergies.[36] Moreover, financial bidders tend to pay all bids with cash while it is not the same for strategic bidders that often also use stock or a combination of stock and cash. Malmendier et al. (2016) shows that cash payments explain the revaluation effect at deal failure and therefore reveal prior-bid underpricing of the target firm. Thus, it is crucial for us to make sure that the identity of the bidders is not simply capturing the medium of payment she uses for acquisitions. More precisely, in our tests, we are careful that our indicator of financial bidder is not actually a proxy for cash payments. To account for that, we run our tests controlling for *Cash* and verify that all our results hold for a subsample composed only of cash deals.

[Insert Table 2.1 Here]

2.4. Empirical analysis

By looking at failed deals, we aim to evaluate whether there is any revaluation effect. Market revaluation refers to the market reaction from the announcement to the failure of the bid. After the

[33] See for example Bates and Lemmon (2003).
[34] In an unreported table, we confirm that they are determinants for failure in a logistic regression setting.
[35] Focusing on U.S. domestic completed bids where acquirers own 100 percent stake, Bargeron et al. (2008) find the premium from financial bidders is around 12 percent lower than that from strategic bidders.
[36] See Bargeron et al. (2008) for alternative explanations.

announcement, prices incorporate deal specific information as the bid advances towards completion or failure. In the case of failure, prices should go back to their initial level unless there is revaluation. The presence of a revaluation effect can be attributed to undervaluation prior to the bid and just reflect a price correction. In that case, the bid per se conveys information to the market. However, a revaluation could also be the result of the anticipation of future takeovers or operational improvements.

In this section, first we aim to estimate whether there is any revaluation effect and then we perform additional tests to pin down its likely driver.

2.4.1. Measuring market revaluation for target firms

To estimate market revaluation, we conduct an event study on target firm stock returns around the announcement of the bid and of the withdrawal. The estimation window corresponds to the interval (-275, -25) with respect to the bid announcement date. Following standard event study methodology as in Brown and Warner (1985), we compute daily abnormal returns as prediction errors from a market model using local market indices.[37] Stock and local market index returns are expressed in home market currency. To account for illiquid stocks, we adjust abnormal returns following the "trade-to-trade" approach of Maynes and Rumsey (1993) and exclude bids where target firms are not traded for more than 225 out of 250 trading days (90 percent of the estimation window). [38]

Then we compute the cumulative abnormal returns (CARs (B-25,F+25)) from 25 days before the announcement of the bid to 25 days after the announcement of the withdrawal following Malmendier et al. (2016). The choice of 25 days before the bid announcement allows us to capture price run-ups caused by rumors (Schwert, 1996). The choice of 25 days after bid failure allows us to incorporate the possibility that stock markets react slowly to bad (withdrawal) news (Chan, 2003).

2.4.2. CARs summary statistics and univariate tests: Is there any revaluation effect?

Figure 2.1 depicts the revaluation effect for both failed bids by financial and strategic bidders. It plots the evolution of target firm CARs (B-25,F+25) for the two subgroups.[39] The solid line refers to target firm CARs for failed bids by financial bidders. And similarly, the dashed line for bids by strategic bidders. The first part of the graph, when we move from B-25 to B along the x-axis, captures price run-ups prior to the bid. At B, we observe that the average market reaction around the bid is around 15 percent. Surprisingly, it is only one third of the market reaction documented by previous studies with focus on completed acquisitions. But our results are similar to Malmendier et al. (2016) and are consistent with Bhagat, Dong, Hirshleifer, and Noah (2005) who explain that the price reaction also

[37] In robustness check, we replace local market indices with Datastream value-weighted global market index in U.S. dollar and local market currency exchange rates to U.S. dollar. We also re-estimate CARs using a constant mean model. Our findings are qualitatively similar.
[38] With this restriction we exclude 20 (55) failed bids by financial (strategic) bidders. We also compute abnormal returns without adjusting for illiquid stocks. Our results do not change.
[39] To normalize the length between bid and failure, we follow the same approximation procedure as described in Malmendier et al. (2016).

factors in investors' views on the probabilities of failure together with the premium expected for the deal. Thus, it is natural to observe a lower price reaction around failed bids because those deals may have a higher ex-ante probability of failure that deals that evolve to completion.

The middle part of the graph, when we move from B to F along the x-axis, depicts how the stock market incorporates deal-specific information as the bid progresses towards failure in our case. The number of trading days from B to F is different for different bids and, for an easy representation, we standardize the period between B and F following the approach in Malmendier et al. (2016). In the absence of a revaluation effect, we would observe CARs falling back to zero after the announcement of the failure. This is what we observe for firms targeted by strategic bidders (the dashed line). In contrast, we observe a revaluation effect of about 10% for firms targeted by financial bidders because their CARs of do not revert to zero upon the announcement of failure but stay at around 10 percent. The gap between the two lines is wide and remarkable and it indicates that the revaluation effect is only prevalent in the presence of financial bidders. Finally, when we move from F to F+25, we observe how the target price settles after failure and to what extent it reverts to its preannouncement level.

[Insert Figure 2.1 Here]

[Insert Table 2.2 Here]

Table 2.2 provides CARs summary statistics and univariate tests. We present CARs for the whole sample and by bidder type for all bids and for various subgrouping. Asterisks next to means indicate statistical significance using skewness-adjusted t-test. Asterisks next to medians indicate statistical significance using Wilcoxon sign-rank test. For the overall sample that we label "Main sample", we observe a statistically significant revaluation effect of around 11 percent (median of 5.9 percent) for bids involving financial bidders. In contrast, we do not observe a significant revaluation effect around failed bids by strategic bidders (mean of 1.6% and median of 3.1% but no statistical significance). The difference in revaluation effect between the two types of bidders is statistically significant. Overall our results indicate that financial bidders are involved in failed deals that experience a revaluation effect, but it is not the case for strategic bidders.

Failed bids by financial bidders may show a higher revaluation effect only because they tend to be paid in cash while strategic bidders frequently use also shares as a method of payment (see Table 2.1, Panel C). Existing literature (Shleifer and Vishny, 2003; Rhodes-Kropf and Viswanathan, 2004; and Malmendier et al., 2016) shows that it is important to separate stock and cash financed deals before making judgements on value effects specially for bidding firms but also for target firms. Specifically, Malmendier et al. (2016) show that cash bids reveal prior undervaluation of the target firm, but it is not the case for paid-in-stock bids. Thus, the inclusion of paid-in-stock bids may overshadow the impact that strategic bidders may have on the revaluation effect. To account for that possibility, we need to

further investigate whether our results are merely reflecting the method of payment that is frequently used by each type of bidder. To this end, we form subgroups based on the medium of payment and split our sample in deals involving only cash (*PureCash*) and those involving also some or only shares (*Paid in Stock >0*) and provide CARs by type of bidder. We observe that in our sample, while almost all financial bids would imply only a cash payment, around 50 percent of strategic bids would involve payment only in shares or a mixture of cash and shares. We focus on cash-only deals (*PureCash*), and we observe a statistically significant revaluation effect of around 12 percent (median of 5 percent) for bids involving financial bidders and a significant revaluation effect of 7.9% (median of 9.7%) around failed bids by strategic bidders.[40] Furthermore, there is no statistical difference between the two groups. As expected, bids that would involve shares as a means of payment are not subject to a significant revaluation for any type of bidder. These findings contrast with our previous results but are consistent with Malmendier et al. (2016) who show that the medium of payment plays a key role and conveys information to the market about prior undervaluation of the target.

To further evaluate our hypothesis that a potential revaluation effect could be related to the amount of information asymmetry or to informational advantage. We split our sample in deals involving targets of different size and provide CARs by type of bidder. We use size as a proxy for the extent of information asymmetries because smaller firms seem to evolve in poorer information environments (Atiase, 1987). We rank targets by market capitalization, form quartiles and label quartiles Small(Q1), Q2, Q3, and Big(Q4) respectively. We also use the location of the bidder with respect to the target as a proxy for information advantage and create the dummies *Domestic* equal to one when both are in the same country and *Crossborder* equal to one if they are in different countries (Orpurt, 2004; Bae et al., 2005; Cooper, 2013).

Our results give support to our hypothesis that a revaluation effect should mostly be observed for firms subject to larger information asymmetries and targeted by financial acquirers. These firms experience a significant mean revaluation effect of around 35.6 percent (median of 24.3 percent), whereas there is no significant revaluation effect for small targets by strategic bidders. The difference between the two groups is statistically significant and confirms our prediction that only financial bidders exploit opportunities where mispricing is most likely to occur. We note that there is also a positive and significant revaluation effect for targets with size above the median (Q3) and when strategic bidders are involved, though the difference with that of financial bidders is only statistically significant at 10% level.

Our results also support the notion that the revaluation effect is larger when bidders are in the same country as target firms because they enjoy an informational advantage. As expected, the coefficient on *Domestic* is positive and significant and higher than *Purecash* both for financial and strategic bids,

[40] As discussed in Fu et al. (2013), to focus on pure cash or stock bids captures more clearly acquisition motives, while interpretation on hybrid payments might be arbitrary.

whereas there is no revaluation effect attached to *Crossborder* failed bids. These preliminary results show that both financial and strategic bidders seem to enjoy an information advantage, but it is economically and statistically relevant for financial bidders in a domestic setting and when the target firm is small.

Put together, the univariate tests support our conjecture that, compared to strategic bidders, financial bidders are more skilled or better exploit prior mispricing when information asymmetry is severe or when they enjoy informational advantages. However, there is the need to further evaluate our hypothesis in a multivariate setting where we can explicitly control for other deal-level and firm-level characteristics that can be correlated with the revaluation effect that we try to explain.

2.4.3. Multivariate tests

To further evaluate our hypothesis while accounting for deal- and firm-level characteristics, we evaluate CARs in a cross-sectional regression setting. Our base model is:

$$CAR_i = \alpha + \beta Financial_i + \delta Log(TargetSize)_i + \gamma Deal_i + \varphi Yeardummies + \theta Industrydummies + \varepsilon_i \quad (2.1)$$

where i indicates the ith bid. CAR_i is the cumulative abnormal return for target firms from 25 days before the bid announcement to 25 days after bid withdrawal. $Financial_i$ is our variable of interest that represents the type of bidder. It equals one (zero) if the ith bid is initiated by a financial (strategic) bidders. Its coefficient, β, captures any target revaluation effect associated with the bidder type "Financial" . We follow previous studies and control for other factors that may be related to the revaluation effect as measured by CAR_i. $Deal_i$ is a vector of deal-level control variables, including *Cash, Premium, Hostile,* and *TenderOffer*.[41] Target return regressions also typically control for other variables such as target size, but unlike in bidder regressions, the theoretical relevance of adding this control is not clear (Malmendier et al., 2016; Bargeron et al., 2008)[42]. Malmendier et al (2016) find that big targets have a lower revaluation effect. We measure target size with *Log(TargetSize)* the natural logarithm of the 2014 CPI-adjusted target market capitalization. Target size can affect valuation multiples and thus a restatement of those based on new information. If revaluation effects are larger when information asymmetries are more important we would expect to see higher revaluation effects for small firms. Without this control, if financial bidders tend to bid for smaller firms we may falsely attach a revaluation effect to bidder type when it is associated instead to the size of the target. Thus, it is important to add this control to rule out this possibility. Thus, keeping size constant, we aim to evaluate if financial bidders are associated with a higher revaluation effect than strategic bidders. Finally,

[41] Cash is a dummy equal to one if more than 50% is paid in cash. In unreported robustness check, we use the percentage of payment in cash. Our results remain qualitatively the same.
[42] Andrade et al (2001) control for size (deal size) and find larger deals having smaller premia. Malmendier et al (2016) find negative coefficients and other papers show insignificant coefficients .

Yeardummies and *Industrydummies* are dummies for bid announcement year and 1-digit SIC industries that control for macroeconomic conditions and regulation changes common to all buyers, target firms or certain industries.[43]

Table 2.3 presents baseline regression results. Column 1 complements the graphical results shown in Figure 2.1 and shows that the revaluation effect around failed bids associated to financial bidders is significantly larger than that associated to strategic bidders. There is an average revaluation effect of 9.7% around failed bids by financial bidders and a positive but insignificant 1.5% for failed bids by strategic bidders. The coefficient on *Financial* shows that the difference of 9.7 % is economically and statistically significant (t-statistic of 2.56). Column 2 and column 3 introduces year and industry dummies and a set of control variables. While the signs of coefficients on control variables are largely consistent with previous studies, we observe that the significance of the coefficient on *Financial* vanishes and does not bring additional explanatory power beyond the set of controls.[44] This takes place especially when we add *Cash and TenderOffer*. In reported correlation analysis, and split regressions, we observe that the association of *log(Targetsize)* and *TenderOffer* with CARs depends on the identity of the acquirer and thus, it could be that the introduction of those variables in the regressions artificially attenuate the association of *Financial* and CARs. In addition, the rational to include *Cash* in the regressions as a control is that the choice of the method of payment conveys information to the market about the value of the bidder and the target. Yet this rational may well apply for strategic bidder because they do have the choice of how to pay for a deal, but it may apply less for financial bidders since often they do not have that choice and pay with cash. In other words, for financial bidders paying with cash may not send a signal to the market because they always do, but it is their nature and identity that makes the market update target valuations.

To make sure that our inference is not misstated, we extend our analysis in several dimensions. First, if financial and strategic bidders are driven by different motives, have different choices when bidding for a target, or induce different market reaction, imposing the same slopes on all control variables, as we do in the basic specification, would be inadequate. To correct for that, we interact all control variables with *Financial*. Column 4 reports the results. Alternatively, in unreported results we interact this dummy with each control variable separately. We observe that, consistent with our main hypothesis, *Financial* is again positive and statistically significant indicating that there is an incremental revaluation associated to financial bidders. Thus, bids by financial bidders seem to convey information to the market about the stand-alone value of the target firm and this effect is particularly larger for smaller firms when information asymmetries are more prevalent. This finding reinforces our interpretation that the

[43] We do not add target valuation measures as a control, such as the market-to-book, because by focusing on failed bids we would only expect such measures to be related to the revaluation effect if there is prior mispricing. Thus, such a control would be picking up the effect that we aim to capture with categorical variables. See for example Rhodes-Kropf, Robinson, Viswanathan (2005), Dong et al., (2006), Edmans et al., (2012), and Fu, Lin, Officer (2013).

[44] Our estimation of the coefficient on *Cash* is 10 percent, similar to the estimation of Malmendier (2016) when both public and private acquirers are included.

revaluation effect is mainly reflecting informational effects. It is worthwhile to note that *Cash* is positive but becomes insignificant which contrasts with Malmendier et al (2016) conclusions.

Second, we narrow our sample to cash-only bids and report results in columns 5-8. In this way, we can directly evaluate the identity of the bidder conveys information to the market about the stand-alone value of the target beyond the medium of payment. Consistent with univariate tests, columns 5-6 show that there does not seem to be larger revaluation effect associated to financial bidders. But when we control for the deal-level and firm-level variables and for the different relation they have with the type of bidder, we again observe that there is an incremental revaluation associated to financial bidders that is larger for smaller firms. Thus, the type of bidder seems to convey incremental information to the market about the stand-alone value of the target beyond what is transmitted by the medium of payment, the offer premium, hostile attitude, and tender offer.

We also gain additional insights that are worth mentioning. The coefficient on *TenderOffer* is positive and significant for strategic acquirers but the effect is reversed and insignificant for financial acquirers. Thus, Bradley et al (1983) result that the effect of tender offers on CARs reflects the anticipation of future acquisition activity may only be valid in the case of bids by strategic bidders. Similarly, the association of *log(Targetsize)* and bidder type really depends on the type of bidder. For strategic bidders, there is a positive and significant association with target size. In contrast, this association is negative and significant for financial bidders. The effect attached to financial bidder type most likely stems from his ability to identify underpriced targets (due to skill or to focus on that activity) or to the possession of private information about the target rather than to the fact that he is using cash as a means of payment.

[Insert Table 2.3 Here]

2.4.4. Information asymmetry and information advantage

Our baseline results show that the type of bidder is associated to the revaluation effect. Failed bids by financial bidders have associated a higher revaluation than bids by strategic bidders and this effect is larger the smaller is the target firm. Thus, financial bidders seem to exploit deviations between value and price and as such underpricing seems to be a motive guiding their acquisition activity.[45] Next we aim to further explore the size effect and its relation to the identity of the bidder as well as the potential sources of financial bidders' advantage to identify and exploit mispricing.

Our previous results indicate that the revaluation effect is larger for small firms targeted by financial bidders where information asymmetries are likely to be more prevalent. To further examine this observation, we consider the variable *Q1(Small)* that equals one if the target market capitalization is in

[45] In contrast, Edmans et al. (2012) show that a non-fundamental discount on stock price attracts both strategic and financial acquirers, but takeover probabilities seem to increase more for the later.

the lowest quantile and zero otherwise. By taking a categorical variable we can directly and alternatively examine to what extent the revaluation effect is particularly relevant for small firms. Table 2.4 shows the results. Column 1-3 refers to the main sample and column 4-6 to the sample including cash only bids. In column 1 and 4, we find that financial bidders are positively and significantly associated with CARs in the case of small firms. This result holds even after adding *Log(Targetsize)* in column 2 and 5. Adding jointly *Q1(Small)* and *Log(Targetsize)* represents a more stringent test. The finding that *Q1(Small)* plays a role for financial bidders beyond *Log(Targetsize)* reinforces our interpretation that informational effects are important in the case of small firms. In column 3 and 6, we present the results for the model specification that allows for different loadings on control variables by each type of bidder. We find that there is no incremental effect related to the sub-sample of very small firms *Q1(Small)* beyond the size effect captured by *Financial×Log(Targetsize).* Clearly in column 6, we observe a significant association between financial bidder type and the revaluation effect and, consistent with our hypothesis, this effect is larger for smaller firms.

[Insert Table 2.4 Here]

In unreported tests, we also include Q1(Small) and interact it with *Strategic*. We then evaluate whether informational effects can be detected in the case of small targets by strategic bidders. We find no evidence that CARs are either associated with strategic bidders or with small firms targeted by strategic bidders.

In Table 2.5, we further explore the role of target size buy looking at other size quantiles and we do that both for financial and strategic bidders. For ease of interpretation, we report the full model with quantiles interacted first with *Financial* (column 1) and then with *Strategic* (column 2) as well as separate regressions for financial and strategic bidders. In all regressions, we drop *Log(Targetsize)* since it can be mechanically correlated with size quantiles. In unreported tests, we also include all bidder interactions with controls and find virtually the same results. Overall, we observe that the association of financial bidder type with the revaluation effect is mostly concentrated in small firms, but this pattern is not present in the case of bids by strategic bidders. Thus, the informational effect seems to be only attached to financial bidders. [46]

[Insert Table 2.5 Here]

To evaluate the potential sources of financial bidders' advantage that allow them to identify and exploit mispricing, we focus on the location of the bidder-target pair. It could be that financial bidder advantage stems from skill or from an informational advantage. If financial bidders benefit from informational advantage rather than skill (or focus) we can expect that the information advantage will

[46] In Section 2.3, univariate tests suggest thin evidence that strategic bidders have slightly higher revaluation for targets of medium size. Thus, we replace the *Small* with *Medium* in the two interaction items and do not observe any significance, which indicates that the valuation effect is driven by other control variables. For reporting brevity, we do not tabulate a column for these two pairs.

be larger the more proximate is the bidder to the target. This is consistent with findings in existing studies in the context of financial analysts. To capture that possibility, we use *Domestic* a dummy that equals one for domestic transactions. Domestic transactions imply a closeness of the parties involved in the bid, not only in terms of physical proximity but also cultural, economic and institutional that can lead to an informational advantage. If information advantage is the driving force, domestic transactions will benefit more and should have therefore a larger relation to CARs.

We report the corresponding results in Table 2.6. Based on location, our results indicate that it is rather skill than information advantage.

[Insert Table 2.6 Here]

Summing up, our results show that the revaluation effect is larger in the presence of financial bidders when information asymmetries are high. We also document the absence of a revaluation effect attached to bids by strategic bidders. We find that identity of the bidder seems to convey information to the market about the stand-alone value of the target firm beyond and to a larger extent than what is conveyed by the medium of payment. Financial bidders seem to consider underpricing as a driver of their acquisition choice while underpricing does not seem to be a driving force in acquisitions aimed at by strategic bidders. The magnitude of the revaluation unlocked by the identity of the bidder is significant and subsumes the effect of the medium of payment documented in Malmendier et al. (2016). Alternative interpretations of our findings such as anticipation effects are plausible. For those alternative explanations to be valid, they have to be more pertinent for the subsample of fail bids by financial bidders than for those by strategic bidders. We may also be concerned with selection into failure biases. We explore these possibilities and concerns next.

2.4.5. Is selection into bid failure driving our findings?

Our tests so far are subject to some challenges and require further examination. In our setup, we should be concerned with selection into bid failure, i.e. the cause of deal termination is related to the valuation of a target. For example, as a defense tactic, firms can release positive news to hype up stock price, squeeze the profit margin for bids, and drive away bidders.[47] In this case, we equally observe a revaluation at bid failure but cannot conclude it is stand-alone firm value that is previously mispriced and then revealed by financial bidders. Therefore, the key to address selection into failure is to set apart bids by whether their failure reasons affect stock price. Specifically, we follow Savor and Lu (2009) and Malmendier et al. (2016) to collect detailed information on failure reasons and group them into exogenous ones, i.e. unrelated to firm stock price, and those endogenously related to firm stock price.[48]

[47] Literature has long established evidence that firms consciously release news to influence stock price. See (Ahern and Sosyura, 2014) for recent evidences.
[48] We use three information sources, SDC deal synopsis, SDC event history and news from Nexis-Lexis.

Our objective to use this exogenous-endogenous classification is two-fold. On the one hand, we want to verify whether the differential results between small targets of financial bidders and the rest bids still hold for the exogenous group. On the other hand, we want to ensure that, even if selection into failure exists, its presence has similar distribution for small targets of financial bidders and the remaining bids. If the cause of failure makes the market to react, such stock price reaction should be larger the larger are information asymmetries, leading to an overproportion of small firm bids failing for a reason involving high revaluation. Furthermore, deals by financial bidder are more likely to fail than strategic ones because financial bidders do not enjoy synergies and their negotiation margins are thinner and more easily drop the bid, leading to an overproportion of financial bidder deals failing for a reason involving high revaluation. The two, putting together, imply that the greater revaluation of small firms targeted by financial bidders may simply be driven by over-presence of selection into failure for small firms and financial bidders. Alternatively, there could be an overproportion of deals involving strategic bidders failing due to negative news about the target and adversely affects target stock price, such as bidders withdrawing during due diligence.

[Insert Table 2.7 Here]

Table 2.7 summarize analysis results by failure categories. The very left column of Table 2.7 lists main failure categories. Categories from row (1) to (4) constitute our exogenous sample because they contain little additional information to the market that affects stock price. The first row is labeled (1) No more information because in those bids nothing but the date of bid withdrawal is found in public news resources. The next row, (2) Term mutually disagree, contains bids with withdrawal news stating terms are modified but eventually both bidder and target agree to terminate negotiation. No further information is available on what term is modified or what drives both parties agree to drop. In those two categories, there is no more public information than the sheer fact that any value creation of bidder becoming a controlling shareholder is no longer there. In this sense, these two failure categories are the most exogenous to revaluation effect. We also consider two other categories as exogenous. One is (3) Bidder no financing, in which news explicitly states that bids are withdrawn because bidders have difficulties to finance the deal. The other is (4) Regulator reject, in which bids failed to obtain approval from target country regulatory authorities such as Australian Competition and Consumer Commission, Federal Trade Commission in U.S., European Union, Office of Fair Trading or Competition and Markets Authority in U.K., etc. Those two causes of failure are unlikely to carry underlying information that affects investors' view about a target firm's governance, operation or growth prospect, apart from reversing anticipated value effect of a bid being completed. Putting together, the four failure categories are labeled as Strict sample in the last but one row.

The next six categories, from (5) to (10), are considered endogenous because the cause of failure is likely to stimulate stock market reaction. (5) Bidder withdraw, refers to bids that are terminated by

bidders either during due diligence or at any other phase of the bid negotiation. A bid terminated in this way might signal that the bidder discovers the true but worse than expected situation of its target firm. Convinced by this signal, investors may reverse more than what to be added by the bid. In (6) Target released news, other news about the target is released around bid withdrawal, for instance earnings announcement or other accounting information, plans for new project, updates on court ruling, and so on. This category also includes bids in which bidders say they terminate because of recent released news about its target firm. Good or bad, these pieces of confounding news make it hard to distinguish price effect of bid failure. (7) Price too low covers bids where target board of directors decline the offer by arguing the offer price is inadequate. It also includes bids where bidders openly reject to sweeten their offer. (8) Tender failed contain bids where bidders fail to tender sufficient shares before offer expires. These two categories literally tell the market that shareholders and management of target firms believe the firm fair value should be higher than offer price. In this sense, it actually supports our underlying hypothesis. However, one might argue that the so-called fair price in the mind of target management and shareholders can only be attained if they could learn about and implement policies planned by the bidder. If outside investors are convinced that performance-improving policies are to be executed by target management in place of bidders, they would not reverse initial revaluation at bid announcement. This undermines our conclusion because the observed positive revaluation is in fact anticipation of future operational improvement. We are going to address this anticipation effect in the next section. For now, we simply regard the two categories as endogenous to target price effect. The last two categories, (9) Board reject and (10) Shareholder reject, stand for bids that are vetoed by target board or shareholders without explicitly stating it is due to inadequate offer price. These two failure causes may disperse market perception on firm value. While some can follow a similar line of thinking to the categories of Price too low or Tender failed, others might regard it as an indicator for stagnated board or dominant block holder whose interest conflicts with other shareholders.[49] Despite diverse directions of price implications, the aforementioned six failure categories, from Bidder withdraw to Shareholder reject, all hinder us from cleanly draw conclusion on undervaluation hypothesis. Therefore, we classify them as endogenous failure sample.

Having classified our sample by failure reasons, we further divide the sample into $Financial \times Small$, Other Financial, and Strategic. They refer to, respectively, small firms targeted by financial bidders, non-small firms targeted by financial bidders, all firms targeted by strategic bidders. Furthermore, we keep cash-only bids. N(% of N) indicates the number(fraction) of total observations for each of the three subgroups falling into each of the ten failure categories. In total, the main sample is divided into thirty subgroups. Then we regress CARs on each of these thirty subgroups by fully

[49] Of course, *Price too low* or *Tender Failed* are not free from governance problems. For example, the former can be viewed as an excuse used by entrenched management. The latter can occur if a majority block holder refuses to tender and hurt minority shareholder interest.

controlling for Log(TargetSize), Premium, Hostile and TenderOffer.[50] Coefficient captures any revaluation effect loaded on each subgroup. It is left empty if there are insufficient observations to run regression.

As results suggest, our two-fold objective is satisfied and our concern of selection into failure is alleviated. First, the distribution patterns of failed bids into each category are similar across the three subgroups, $l \times Small$, *Other Financial*, and *Strategic*. Particularly, in the failure categories prone to endogeneity, i.e. (5)-(10), none of the three subgroups has significantly greater or lower presence. This mitigates our concern that a cluster into either good or bad compounding news drives our results. Second, all three subgroups have around 40 percent of their observations present in the *Strict sample*, the failure categories least contaminated by price-sensitive information. In this exogenous sample, the greater revaluation effect of $Financial \times Small$ persists. In a word, our findings are unlikely to be driven by confounding factors related to both failure and target stock price.

One possible criticism on the current failure categories is that our so-called robust results are mainly driven by (1) No more information. But no more information on the news does not exclude the possibility that investors with private information can trade and cause an increase in price. Accordingly, (1) No more information would not be a valid exogenous category. Even if investors with private information take new action between bid and failure, such new move would be likely to be triggered by financial bidders. In this sense, financial bidders' role in revealing new information still holds.

It is also worth noting that financial and strategic bidders have different revaluation effect for (1) No more information and (8) Tender failed. Again, such discrepancy hints at different roles played by the two types of bidders. For this reason, in future versions of this work, we are going to investigate in depth to obtain additional insights on the bidder types.

2.4.6. Is target revaluation driven by future takeover activities?

The differential effect observed so far may be alternatively explained by future takeover activities Bradley et al. (1983). That is, the market believes small target firms of financial bidders have a greater probability to be acquired and hence a higher expected value. And this higher likelihood is further decomposed into two dimensions, shorter acquiring time or higher offer premium. For instance, a future bid to be realized within two years is to be valued more than one completed ten years later. Likewise, a 30 percent premium is preferred to a 10 percent premium. We approach the two dimensions separately.

To analyze the time until a successful acquisition, we conduct a survivor analysis.[51] This method is originally used in biology to examine, for instance, whether certain factors accelerate death. As an

[50] We also estimate the coefficient without any control, or dropping collinear control variable such as *TenderOffer* for the failure category *Tender failed*. Estimated results remain the same.

[51] It suits better our setup than a logistic regression because the former accommodates both occurrence and timing, whereas the latter only examines the overall presence and absence of an event. See Allison (2010) for a more detailed discussion on survival analysis techniques.

analogue to it, we test whether a combination of three factors, bidder types as financial acquirers, small firm size and higher market revaluation predicts a shorter survival time to future acquisition. Since our primary interest is the differential effect in revaluation, we focus on the marginal difference in time to acquisition between $Financial \times Small$ and the rest failed bids, instead of comparing to all un-acquired firms in the history.[52]

To begin with, we plot Kaplan-Meier survival curve to visualize the survival paths of $Financial \times Small$, and the rest. For each target firm in our sample, the observation period is measured as the time interval between the withdrawal date and the censoring date. The censoring date is the effective date on which a target firm is successfully acquired. In case no bid is completed, it is the date of officially delisted date. We take June 30 2016, the last trading date of our sample period, as the censoring date for companies stay listed and independent. For each year, survival probability is calculated as the number of firms surviving divided by the number of firms at takeover risk. Firms that are delisted for other reasons are not included in the denominator.

Figure 2.2 plot the cumulative survival ratios at each year end, calculated as the product of preceding probabilities of surviving a takeover risk in previous years. At first glance, the survivor ratio of small firms targeted by financial bidders drop faster than that of the other firms. For example, after two years following the initial bid failure, the subgroup $Financial \times Small$ is taken over 1.4 times (35 percent vs. 25 percent) faster than the other three groups. Yet such seemingly lower survival ratio turns out to be statistically insignificant, suggested by the large p-value (0.403) of log-rank test on the four categories.

[Insert Figure 2.2 Here]

So far we show firms grouped by their size and the type of bidders share similar time to acquisitions. But what we are ultimately interested is whether the higher revaluation associated to $Financial \times Small$ reflects a higher completion speed. Thus, we switch to Cox proportional-hazard regression to accommodate continuous variables, such as revaluation (CARs), and multiple covariates. In fact, all control variables included to estimate revaluation effect would be subject to the same query as the indicator for subgroup of financial small, if the latter is doubted to be driven by the rate of future takeover activities. Using Cox regression to adjust for other covariates, we can set apart the marginal effect of higher revaluation on the rate of future takeover.

Table 2.8 summarizes regression results. The dependent variable is the rate of subsequent takeovers following a bid failure. The explanatory variable of our primary interest is $Financial \times Small \times$

[52] Besides its thin relevance to our setting, to include all un-acquired firms in the history may induce a negative bias on the survivor ratio of our sample. As demonstrated by (Malmendier et al., 2016), the survival ratio of recipients of a unconsummated bid is persistently much lower than firms that share similar firm and industry characteristics but never receive an offer for a five-year period before bid announcement. This gap suggests the comparison of future survival in our setting should be conditioned on the set of takeover risks following the very recent bid event.

$Cars$, the revaluation associated to small firms targeted by financial bidders around bid failure. Its coefficient represents a relative increase in the rate of future takeover for one unit increase in revaluation. If the positive revaluation documented in Table 2.4 reflects faster successful takeover in the future, we would observe a positive and statistically significant coefficient. However, as shown in Column 1, the coefficient is insignificant from zero. That means revaluation possessed by small targets of financial bidders reveals prior mispriced intrinsic value of a firm. Next, in Column 2 of Table 2.8, we add the same array of control variables as used for estimating revaluation effect in order to set apart the marginal effect of Financial Small Car. We see clearly that it remains insignificant. The cash coefficients, contrary to Malmendier et al. (2016), is significantly positive. But once we follow their sample filtering and restrict to domestic bids in U.S., the coefficient becomes insignificant from zero. This is consistent to Faccio and Masulis (2005) that an alternative role of cash as payment is to secure a bid, which reveals the true market for a targets' shares and in turn reflect future takeover activities. Then in Column 3, we introduce a variable for average *CARs* and an indicator for financial bidder to further understand the role of revaluation and types of bidder respectively. Neither $Financial$ nor $Financial \times Small \times Cars$ possesses a significant coefficient. However, *CARs* become highly influential. For every one percent increase in revaluation, there is a 37.2 percent increase in takeover rate (HR=$e^{.316}$=1.372). Once we decompose *CARs* by the type of bidders, i.e. the revaluation allotted respectively to financial and strategic bidders, we see in Column 4 that the significant effect is solely driven by strategic bidders. The strong link between revaluation of firms targeted by strategic bidders and faster future takeover extends (Bradley et al., 1983) that revaluation at bid failure reflects not only synergies brought in by a subsequent successful acquisition but also a shorter waiting time before it eventually happens. In contrast, absent of synergy, the higher revaluation of $Financial \times Small$ is not related to a higher speed of future takeover activities.

[Insert Table 2.8 Here]

[Insert Table 2.9 Here]

Next, we estimate whether this subgroup is related to a higher wealth created by future takeover. We proxy future wealth with the premium of the ultimately completed bid, measured by inflation-adjusted deal value divided by target market capitalization. As a start, in Column 1 of Table 2.9, we regress this future wealth on $Financial \times Small \times Cars$ without any control. Then in Column 2-4, we account for the medium of payment, premium, bid attitude, and tender offer. The advantage to use the target market capitalization before bid failure to normalize future premium is to be aligned with other control variables in terms of time. Yet it leads us to imprecise measure for long-term stock return. Thus, we include the time it takes to be eventually acquired (*Years*) to partly control for the long period of estimation. As shown in Column 2-4, $Financial \times Small \times Cars$ does not carry any significant effect. This insignificance lasts when we narrow down the sample to initial cash-only bids. All these

evidences demonstrate that future higher offer value is unlikely to drive the higher revaluation of small firms targeted by financial bidders.

To sum up, neither the speed nor the offer price of future takeovers differs significantly between small firms targeted by financial bidders and the rest of firms. The greater revaluation of the former should derive from market correction for a prior underpriced intrinsic firm value. And financial bidders are capable of profiting from it.

2.4.7. Is target revaluation driven by anticipation of future operation improvement?

Another type of anticipation effect, i.e. expectation of future operation improvement, might drive financial bidders' higher evaluation on small firms and invalidate our conclusion. Hirshleifer and Titman (1990) point out the possibility for hostile bids that, after targets firms resist a bid, they could learn about and implement policies planned by the bidder. Predicting this change, stock market can increase their expectation of the firm's prospect. In our setting, one might conjecture that financial bidders could have better policy in mind than strategic bidders. So only those firms targeted by financial bidders preempt policies planned by bidder. If it is financial bidders' superior management skills than strategic bidders that drive differential stock price reaction, we should see positive revaluation effect for all firms targeted by financial bidders. In reality, revaluation on larger firms dissipates at the news of bid withdrawal.

One might also argue that target management, considering financial bidders as corporate raider, successfully resist takeover threat and become more disciplined to improve operation performance Safieddine and Titman (1999). This does not relate to financial bidders' better know-how on corporate governance but can still affect investors' expectation. If it is unwanted bids that wake target management up and make them more engaged, this channel should be most prominent in bids that are hostile or rejected by target board of directors. In fact, financial bids on small firms rejected by directors are less than 10 percent and not associated with any significant positive revaluation, according to our investigation results on failure reasons reported in Table 2.7. Furthermore, when estimating financial bidders' revaluation effect on small firms in Table 2.4, we have already controlled for hostile deal attitude. The positive revaluation effect persists in all specifications. As a matter of fact, less than 15 percent of small firms targeted by financial bidders are hostile. In unreported column of Table 2.4, we drop those observations and check whether small firms targeted by financial bidders still obtain revaluation in friendly cash-only bids. Results show it is indeed the case.

To sum up, the cases pertaining to future operation improvement are shown to be thinly present in our sample. Evidence overall does not support this anticipation effect.

2.4.8. Can target undervaluation motive be generalized to completed bids?

Up to this point, we show our findings support undervaluation hypothesis and are not driven by unobservable factors associated simultaneously to failure and target price, or anticipation of future successful takeovers and operation improvement. Yet there is still another potential issue worth discussing. That is, using failed bids as main empirical specification would in fact hinder us from generalize our findings to completed deals, if bids driven by target undervaluation are only found in failed bids. In light of previous empirical evidences, this should not be a major concern. For example, Dong et al. (2006) show bids involving undervalued targets have a completion rate of 73.9 percent, only 8.7 percent lower than the other targets. More recently, Edmans et al. (2012) provide evidence that target undervaluation not only attracts both financial and strategic bidders, but also remains a significant driver when they only keep completed bids in their analysis.[53] In this regard, target undervaluation is neither restricted to a particular type of bidder nor exclusive to failed bids.

2.5. Conclusion

Acquisitions are attempts by biding firms to exploit not only synergies but other sources of potential gains. Specifically, we show that financial acquirers can exploit, among others, information advantages they may have regarding the true value of the target firm. When information is severely asymmetric, financial acquirers show an advantage over strategic acquirers and pocket a higher gain from mispriced targets.

As the next step, we are going to distinguish whether this differential revaluation effect between bidder types is due to financial bidders' better analytical skills or market segmentation, i.e. they are more specialized in the business of exploiting undervaluation opportunities than strategic bidders.

A caveat of our empirical strategy is that we rely on stock price information. That means we are unable to accommodate private target firms, which are probably subject to severe information asymmetry.

[53] See (Edmans et al., 2012) Internet Appendix Table IA6.

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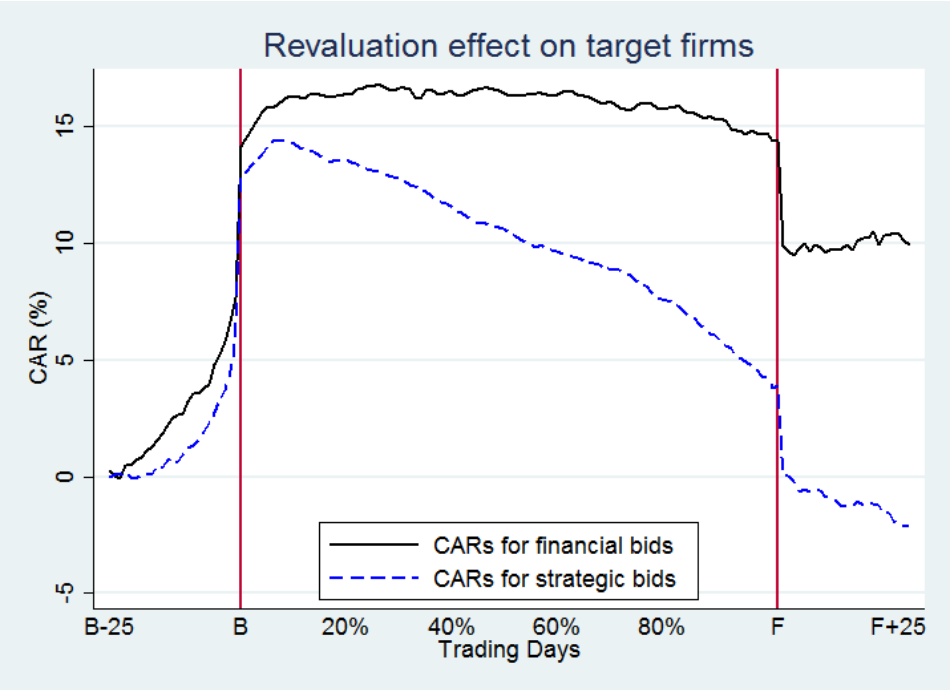

Figure 2.1: Revaluation effect on target firms

This figure depicts the evolution of cumulative abnormal returns(CARs) for 258 financial failed bids (solid line) and 531 strategic failed bids (dashed line), which constitute to our main sample. B is the date of bid announcement. F is the date of withdraw announcement. The first part, from B-25 to B along the x-axis, captures price run-ups prior to the bid announcement. The middle part, from B to F along the x-axis, depicts how the stock market incorporates deal-specific information as the bid progresses towards failure. For each bid, the length between B and F is standardized to 100 days. The third part, from F to F+25 along the x-axis, shows how the target price settles after the date of withdraw announcement and to what extent it reverts to its preannouncement level.

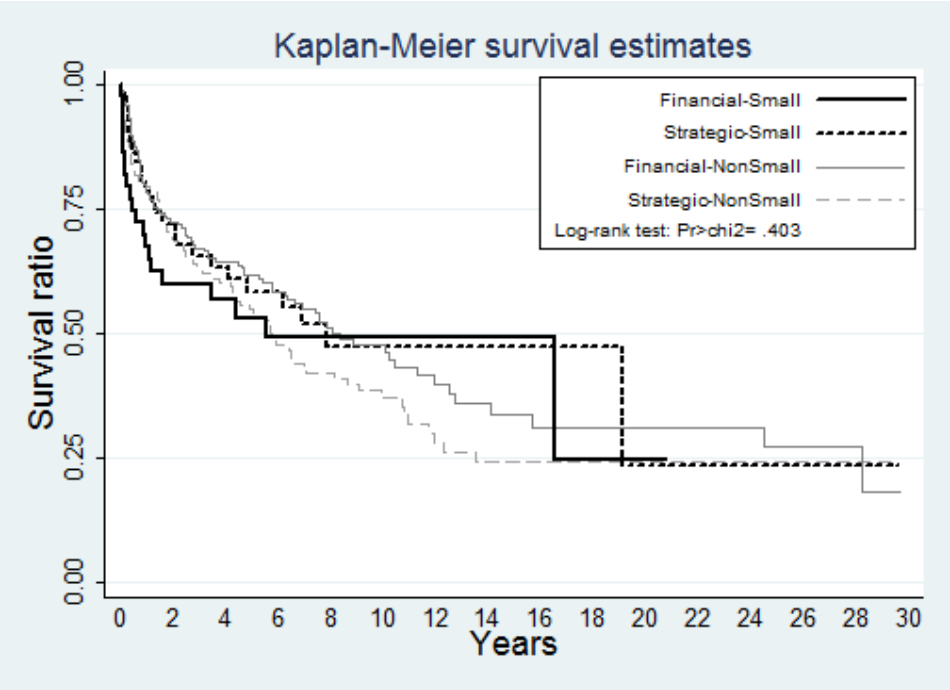

Figure 2.2: Kaplan-Meier survival estimates.

This figure plots the conditional probability of firms surviving takeover threat over time. The pool of observations starting at Years=0 is out cash-only sample (218 financial failed bids and 237 strategic failed bids). The estimates adjust for right censoring such as firms remaining listed and independent at the end of observation period and firms delisted for reasons other than takeover, e.g., bankruptcy or regulator enforcement.

Table 2.1: Deal summary statistics

(Continued)

This table reports key deal-level characteristics for the main sample as described in Section3. Panel A displays bids issued by financial acquirers. Panel B displays those by strategic acquirers. Panel C compares, in groups divided by completed and failed, the difference of deal characteristics between financial and strategic acquirers. Cash(Stock/Other) is the percent of deal value to be paid in cash (stock/total value minus value paid in cash and stock). Days is the number of trading days between announcement date and effective/withdraw date. DealValue (TargetSize) is the transaction value (target market capitalization four calendar weeks prior to bid announcement) in millions of 2014 dollar using Consumer Price Index Conversion Factors. MB is the market-to-book value of target's equity four calendar weeks prior to bid announcement. Premium is bidder's offer price divided by target's stock price four calendar weeks prior to bid announcement. It is truncated between zero and two. StakeSought is the percentage of common, or common equivalent, shares outstanding sought by the acquirer in this transaction. Toehold is the percent of common shares outstanding held by the acquirer as of the announcement date. Hostile, TenderOffer, TTerm and InvGroup equal one for bids in which the bidder's attitude is hostile, tender offer is involved, the target is liable to pay a termination fee to the acquirer upon withdrawal, or the acquirer is a group of multiple investors, respectively, and zero otherwise

Panel A: Acquisition bids issued by financial bidders

	Completed bids						Failed bids						Failed - Completed
	N	Mean	Median	Std.dev.	Min	Max	N	Mean	Median	Std.dev.	Min	Max	t-test p-value
Cash	1,310	91	100	25	0	100	258	93	100	21	0	100	0.43
Stock	1,310	4	0	19	0	100	258	4	0	17	0	100	0.64
Other	1,310	4	0	16	0	100	258	3	0	13	0	81	0.51
Days	1,310	94	78	63	0	357	258	100	80	80	0	365	0.21
DealValue	1,255	220	101	275	0	1,635	247	275	142	344	0	2,127	0.01
TargetSize	1,310	205	89	266	0	1,744	258	248	119	321	1	1,853	0.02
MB	730	2	1	2	0	12	173	2	1	5	0	48	0.2
Premium	1,310	41	32	32	0	125	258	40	31	34	0	200	0.57
Premium1d	1,303	46	26	243	-89	7,936	257	31	25	32	-50	249	0.35
Hostile	1,310	0	0	0	0	1	258	0	0	0	0	1	0
TenderOffer	1,310	1	1	0	0	1	258	0	0	1	0	1	0.04
StakeSought	1,309	87	100	22	0	100	255	89	100	18	3	100	0.12
Toehold	367	28	29	14	0	80	107	18	15	14	0	64	0
TTerm	1,310	0	0	0	0	1	258	0	0	0	0	1	0
InvGroup	1,310	0	0	0	0	1	258	0	0	0	0	1	0.04

Table 2.2: Deal summary statistics

(Continued)

Panel B: Acquisition bids issued by strategic bidders

	Completed bids						Failed bids						Failed - Completed
	N	Mean	Median	Std.dev.	Min	Max	N	Mean	Median	Std.dev.	Min	Max	t-test p-value
Cash	5,399	60	100	46	0	100	531	51	55	47	0	100	0
Stock	5,399	36	0	46	0	100	531	45	0	47	0	100	0
Other	5,399	4	0	13	0	100	531	4	0	15	0	100	0.85
Days	5,399	96	82	59	0	365	531	87	70	70	0	363	0
DealValue	5,097	253	125	305	0	1,906	509	210	106	271	0	2,094	0
TargetSize	5,399	223	101	287	0	1,968	531	192	79	275	0	2,155	0.02
MB	3,761	3	2	2	0	12	378	3	1	6	0	48	0
Premium	5,399	48	40	34	0	125	531	47	39	38	0	200	0.66
Premium1d	5,398	93	100	17	2	100	529	96	100	13	4	100	0
Hostile	924	24	25	16	0	88	84	16	14	12	1	52	0
TenderOffer	5,399	0	0	0	0	1	531	0	0	0	0	1	0
StakeSought	5,399	0	0	0	0	1	531	0	0	0	0	1	0
Toehold	5,399	0	0	0	0	1	531	0	0	0	0	1	0
TTerm	5,399	0	0	0	0	1	531	0	0	0	0	0	0.22
InvGroup	5,399	60	100	46	0	100	531	51	55	47	0	100	0

Table 2.1: Deal summary statistics

Panel C: Financial versus strategic bidders

	Completed bids		Failed bids	
	Financial - Strategic	t-test p-value	Financial - Strategic	t-test p-value
Cash	32	0	42	0
Stock	-32	0	-41	0
Other	0	0.29	0	0.71
Days	-2	0.18	13	0.02
DealValue	-34	0	65	0
TargetSize	-18	0.04	57	0.01
MB	-1	0	-1	0.2
Premium	-7	0	-8	0.01
StakeSought	-7	0	-7	0
Toehold	3	0	2	0.38
Hostile	0	0.66	0	0.48
TenderOffer	0	0	0	0
TTerm	0	0	0	0.13
InvGroup	0	0	0	0

Table 2.3: *Revaluation: summary statistics and univariate tests*

This table reports summary statistics and univariate tests on absolute cumulative abnormal returns (*CARs*) from 25 trading days before bid announcement to 25 trading days after failure announcement. Daily abnormal returns are computed using a market model with parameters estimated over 250 trading days ending 25 trading days prior to the bid announcement. *Financial bids (Strategic bids)* contain only bids initiated by a financial(strategic) bidder. *Paid in stock > 0* is the subsample in which a bid is to be paid, either partly or fully, in stock shares. *Pay in cash = 100%* is the subsample in which a bid is to be paid fully in cash. In *Q1, Q2, Q3,* and *Q4*, bids are ranked by target market capitalization and divided into four groups: in the lower quartile, between 25 percent to 50 percent, between 50 percent to 75 percent, and in the upper quartile. *Domestic(Crossborder)* is the subsample in which both acquirer and target are(not) domiciled in the same country. To test whether the cumulative absolute abnormal returns are significantly different from zero as well as between financial and strategic acquirers, we compute skewness-adjusted t-statistic (*t-test)* and z-statistic of Wilcoxon-Mann-Whitney rank-sum test (*Wilcoxon-test*). ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively

	Financial and Strategic bids			Financial bids			Strategic bids			Strategic - Financial	
	N	Mean	Median	N	Mean	Median	N	Mean	Median	t-test	Wilcoxon-test
Main sample	789	0.046**	0.036***	258	0.112***	0.059***	531	0.015	0.031	-2.56**	-1.76*
Pay in stock > 0	281	-0.044	-0.015	16	-0.039	0.069	265	-0.045	-0.018	-0.03	-0.71
Pay in cash = 100%	455	0.099***	0.076***	218	0.121***	0.050***	237	0.079**	0.097***	-0.94	-0.09
In the subsample of pay in cash =100%											
Q1	86	0.204***	0.156***	41	0.356***	0.243***	45	0.065	0.073	-2.26**	-2.28**
Q2	130	0.036	-0.002	130	0.036	-0.002	130	0.036	-0.002	-0.73	0.01
Q3	126	0.103***	0.100***	59	0.033	0.013	67	0.164***	0.168***	1.72*	1.87*
Q4	113	0.087**	0.064***	68	0.090*	0.020*	45	0.084	0.098*	-0.07	0.26
Domestic	321	0.125***	0.101***	164	0.138***	0.065***	157	0.112***	0.118***	-0.49	0.5
Domestic Q1	69	0.199***	0.155**	35	0.351***	0.233***	34	0.042	0.039	-2.25**	-2.21**
Cross border	134	0.035	0.015	54	0.069	0.007	80	0.013	0.015	-0.67	-0.64
Cross border Q1	17	0.225	0.303	6	0.388	0.493	11	0.135	0.162	-0.72	-1.01

Table 2.4: *Revaluation: baseline regression*

This table reports OLS regression on absolute cumulative abnormal returns (CARs) form 25 days before bid announcement to 25 days after failure announcement. Financial is a dummy variable equal to one for bids by financial acquirers and zero otherwise. Log(TargetSize) is the natural logarithm of target market capitalization in millions of 2014 dollar four calendar weeks prior to bid announcement. Cash is a dummy equal to one for bids that more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer are dummy variables indicating a bid in which the bidder's attitude is hostile and tender offer is involved, respectively. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(3) contain the main sample. Column (4)-(6) examine the subgroup of bids to be paid fully in cash. Heteroskedasticity-robust t statistics are in brackets. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	Main sample				Pay in cash = 100%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Financial	0.097**	0.078**	0.044	0.604**	0.042	0.028	0.046	0.825***
	[2.56]	[2.06]	[1.10]	[2.34]	[0.94]	[0.61]	[1.03]	[3.92]
Financial × Log(TargetSize)				-0.103***				-0.141***
				[-3.26]				[-3.68]
Financial × Cash				0.036				
				[0.25]				
Financial × Premium				-0.001				-0.002
				[-0.50]				[-1.10]
Financial × Hostile				0.044				0.177
				[0.47]				[1.61]
Financial × TenderOffer				-0.226***				-0.200**
				[-2.92]				[-2.37]
Log(TargetSize)			0.009	0.043**			0.002	0.074***
			[0.54]	[2.16]			[0.09]	[2.60]
Cash			0.090*	0.074				
			[1.93]	[1.48]				
Premium			0.002***	0.003***			0.002**	0.003**
			[3.19]	[3.20]			[2.25]	[2.26]
Hostile			0.130***	0.116**			0.116**	0.042
			[2.89]	[2.28]			[1.97]	[0.63]
TenderOffer			0.136***	0.215***			0.116**	0.201***
			[3.28]	[4.14]			[2.47]	[3.23]
Constant	0.015	0.028	-0.271*	-0.429***	0.079**	-0.031	-0.275	-0.659***
	[0.62]	[0.25]	[-1.95]	[-2.98]	[2.53]	[-0.19]	[-1.42]	[-3.38]
Observations	789	789	789	789	455	455	455	455
Adjusted R-squared	0.006	0.040	0.093	0.114	-0.000	0.041	0.080	0.124
Year dummies	N	Y	Y	Y	N	Y	Y	Y
Industry dummies	N	Y	Y	Y	N	Y	Y	Y

Table 2.5: *Revaluation: information asymmetry by target firm size*

This table reports OLS regression on absolute cumulative abnormal returns (CARs) form 25 days before bid announcement to 25 days after failure announcement. Financial(Strategic) equals one for bids initiated by financial(strategic) acquirers and zero otherwise. Q1 equals one for the subsample of bids in which the target market capitalization is ranked in the lower quartile. Log(TargetSize) is the natural logarithm of target market capitalization in millions of 2014 dollar four calendar weeks prior to bid announcement. Cash equals one for bids in which more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer equal one if the bidder's attitude is hostile or tender offer is involved, and zero otherwise. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(6) contain the main sample. Column (7)-(12) examine the subgroup of bids to be paid fully in cash. Heteroskedasticity-robust t statistics are in brackets. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	Main sample			Pay in cash = 100%		
	(1)	(2)	(3)	(4)	(5)	(6)
Financial	0.002	-0.018	0.426	0.001	-0.026	0.663***
	[0.06]	[-0.45]	[1.52]	[0.01]	[-0.55]	[2.69]
Financial × Q1	0.222**	0.293***	0.142	0.239**	0.334***	0.143
	[2.37]	[2.79]	[1.20]	[2.48]	[2.88]	[1.09]
Financial × Log(TargetSize)			-0.074**			-0.112**
			[-2.00]			[-2.47]
Financial × Cash			0.057			
			[0.38]			
Financial × Premium			-0.001			-0.002
			[-0.45]			[-1.07]
Financial × Hostile			0.038			0.170
			[0.40]			[1.53]
Financial × TenderOffer			-0.228***			-0.199**
			[-2.95]			[-2.35]
Log(TargetSize)		0.027	0.043**		0.036	0.074**
		[1.55]	[2.14]		[1.47]	[2.58]
Cash	0.093**	0.095**	0.074			
	[2.00]	[2.03]	[1.47]			
Premium	0.002***	0.002***	0.003***	0.002**	0.002**	0.003**
	[2.98]	[3.42]	[3.19]	[2.13]	[2.30]	[2.25]
Hostile	0.136***	0.122***	0.116**	0.118**	0.098*	0.041
	[3.13]	[2.76]	[2.28]	[2.10]	[1.70]	[0.62]
TenderOffer	0.129***	0.134***	0.215***	0.111**	0.112**	0.201***
	[3.15]	[3.27]	[4.14]	[2.38]	[2.43]	[3.23]
Constant	-0.208**	-0.345**	-0.432***	-0.231	-0.421**	-0.664***
	[-1.96]	[-2.50]	[-3.01]	[-1.52]	[-2.30]	[-3.43]
Observations	789	789	789	455	455	455
Adjusted R-squared	0.102	0.105	0.114	0.099	0.105	0.125
Year dummies	Y	Y	Y	Y	Y	Y
Industry dummies	Y	Y	Y	Y	Y	Y

Table 2.6: *Revaluation: robustness check on target firm size*

This table reports OLS regression on absolute cumulative abnormal returns (CARs) form 25 days before bid announcement to 25 days after failure announcement. Financial(Strategic) equals one for bids initiated by financial(strategic) acquirers and zero otherwise. Q1, Q2, and Q3 equal one respectively for the subsamples of bids in which the target market capitalization is ranked in the lower quartile, between 25th and 50th percentile, or be-tween 50th and 75th percentile, and zero otherwise. Cash equals one for bids in which more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer equal one if the bidder's attitude is hostile or tender offer is involved, and zero otherwise. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(4) contain the main sample. Column (5)-(8) examine the subgroup of bids to be paid fully in cash. Heteroskedasticity-robust t statistics are in brackets. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively

	Main sample				Pay in cash = 100%			
	Financial and strategic bids		Financial bids	Strategic bids	Financial and strategic bids		Financial bids	Strategic bids
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Financial	0.015				0.015			
	[0.31]				[0.27]			
Financial × Q1	0.209**		0.197*		0.225**		0.219**	
	[2.11]		[1.91]		[2.14]		[1.99]	
Financial × Q2	-0.037		-0.040		-0.024		0.004	
	[-0.46]		[-0.44]		[-0.27]		[0.04]	
Financial × Q3	-0.010		-0.003		-0.022		-0.009	
	[-0.17]		[-0.05]		[-0.32]		[-0.12]	
Strategic		-0.055				-0.037		
		[-1.03]				[-0.53]		
Strategic × Q1		-0.020		-0.012		-0.108		-0.069
		[-0.27]		[-0.17]		[-1.08]		[-0.67]
Strategic × Q2		-0.028		-0.026		-0.061		-0.073
		[-0.45]		[-0.40]		[-0.74]		[-0.83]
Strategic × Q3		0.078		0.082		0.107		0.131
		[1.34]		[1.39]		[1.20]		[1.35]
Control variables and constant variables are not reported here due to limitation of space.								
Observations	789	789	258	531	455	455	218	237
Adjusted R-squared	0.100	0.095	0.160	0.083	0.095	0.090	0.136	0.077
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y
Industry dummies	Y	Y	Y	Y	Y	Y	Y	Y

Table 2.7: *Revaluation information asymmetry by target-bidder geographic proximity*

This table reports OLS regression on absolute cumulative abnormal returns (CARs) form 25 days before bid announcement to 25 days after failure announcement. Financial(Strategic) equals one for bids initiated by financial(strategic) acquirers and zero otherwise. Q1 equals one for the subsample of bids in which the target market capitalization is ranked in the lower quartile. Domestic(Crossborder) is the subsample in which both acquirer and target are(not) domiciled in the same country Log(TargetSize) is the natural logarithm of target market capitalization in millions of 2014 dollar four calendar weeks prior to bid announcement. Cash equals one for bids in which more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer equal one if the bidder's attitude is hostile or tender offer is involved, and zero otherwise. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(6) contain the main sample. Column (7)-(12) examine the subgroup of bids to be paid fully in cash. Heteroskedasticity-robust t statistics are in brackets. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	Main sample					Pay in cash = 100%				
	(1)	(2)	(3)	(4)	(6)	(7)	(8)	(9)	(10)	(12)
Financial	0.580**	-0.017	0.033	0.021	-0.008	0.785***	0.035	0.071	0.095	0.044
	[2.20]	[-0.10]	[0.20]	[0.13]	[-0.04]	[3.54]	[0.34]	[0.67]	[1.11]	[0.42]
Financial × Domestic	0.023	0.039	0.007		0.036	0.036	0.066	0.036		0.064
	[0.35]	[0.59]	[0.10]		[0.53]	[0.48]	[0.89]	[0.47]		[0.86]
Financial × Q1		0.310***					0.339***			
		[2.95]					[2.85]			
Financial × Domestic × Q1			0.279**	0.296***	0.287**			0.288**	0.316***	0.301**
			[2.46]	[2.62]	[2.53]			[2.40]	[2.61]	[2.50]
Financial × Crossborder × Q1				0.262	0.287				0.273	0.316
				[1.09]	[1.16]				[0.86]	[0.97]
Financial × Log(TargetSize)	-0.102***					-0.138***				
	[-3.23]					[-3.62]				
Financial × Cash	0.036	0.076	0.059	0.073	0.073					
	[0.24]	[0.50]	[0.39]	[0.48]	[0.48]					
Financial × Premium	-0.001	-0.000	-0.000	-0.000	-0.000	-0.002	-0.001	-0.001	-0.001	-0.001
	[-0.51]	[-0.11]	[-0.01]	[-0.06]	[-0.09]	[-1.12]	[-0.61]	[-0.52]	[-0.54]	[-0.59]
Financial × Hostile	0.042	0.005	0.003	0.004	0.001	0.177	0.128	0.127	0.123	0.123
	[0.45]	[0.05]	[0.04]	[0.04]	[0.01]	[1.60]	[1.16]	[1.15]	[1.10]	[1.11]
Financial × TenderOffer	-0.221***	-0.211***	-0.209***	-0.215***	-0.207***	-0.192**	-0.179**	-0.178**	-0.190**	-0.177**
	[-2.80]	[-2.67]	[-2.61]	[-2.77]	[-2.59]	[-2.22]	[-2.05]	[-2.02]	[-2.22]	[-2.01]
Log(TargetSize)	0.043**	0.029*	0.024	0.027	0.028	0.074**	0.038	0.027	0.033	0.034
	[2.16]	[1.67]	[1.40]	[1.55]	[1.56]	[2.58]	[1.49]	[1.05]	[1.25]	[1.28]
Cash	0.074	0.074	0.073	0.073	0.074					
	[1.49]	[1.48]	[1.47]	[1.47]	[1.48]					
Premium	0.003***	0.002***	0.002***	0.002***	0.002***	0.003**	0.003**	0.003*	0.003*	0.003*
	[3.20]	[2.97]	[2.89]	[2.94]	[2.93]	[2.26]	[1.99]	[1.93]	[1.95]	[1.96]
Hostile	0.116**	0.123**	0.125**	0.124**	0.125**	0.042	0.061	0.066	0.064	0.065
	[2.29]	[2.42]	[2.45]	[2.44]	[2.44]	[0.64]	[0.93]	[0.99]	[0.96]	[0.97]
TenderOffer	0.215***	0.211***	0.210***	0.211***	0.211***	0.202***	0.202***	0.203***	0.203***	0.203***
	[4.14]	[4.06]	[4.04]	[4.05]	[4.05]	[3.23]	[3.25]	[3.26]	[3.24]	[3.24]
Constant	-0.430***	-0.385***	-0.362***	-0.373***	-0.375***	-0.658***	-0.515***	-0.464**	-0.486**	-0.488**
	[-2.98]	[-2.78]	[-2.61]	[-2.69]	[-2.70]	[-3.37]	[-2.72]	[-2.40]	[-2.52]	[-2.53]
Observations	789	789	789	789	789	455	455	455	455	455
Adjusted R-squared	0.112	0.109	0.105	0.107	0.106	0.122	0.109	0.099	0.102	0.101
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 2.8: *Revaluation: failure categories*

This table reports OLS regression on absolute cumulative abnormal returns (CARs) form 25 days before bid announcement to 25 days after failure announcement using the same specification as in the column of cash-only bids in Table 2.3 by failure categories for the subgroups of, Financial × Small, Other Financial, and Strategic after controlling for premium, target size, hostile and tender offer. N(% of N) reports the number(fraction) of observations for the corresponding subgroup falling into each failure category. Coefficient is the revaluation effect loading on each subgroup and left blank if observations are too few. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	Financial × Small			Other financial			Strategic		
	N	% of N	Coefficient	N	% of N	Coefficient	N	% of N	Coefficient
(1)No more information	17	38.64	0.38***	51	29.31	0.09	75	31.65	-0.19**
(2)Term mutually disagree	0	0		1	0.57		2	0.84	0
(3)Bidder no financing	0	0		3	1.72	0	0	0	
(4)Regulator reject	2	4.55	0.34	13	7.47	0.27	18	7.59	-0.27
(5)Bidder withdraw	1	2.27		8	4.6	-0.12	10	4.22	-0.02
(6)Target released news	0	0		0	0		1	0.42	
(7)Price too low	1	2.27		9	5.17	0.24*	14	5.91	-0.23*
(8)Tender failed	16	36.36	0.42***	61	35.06	-0.19**	53	22.36	0.01
(9)Board reject	4	9.09	0.11	22	12.64	-0.13	38	16.03	0.08
(10)Shareholder reject	3	6.82	0.59*	16	9.2	-0.25	18	7.59	0.06
Strict sample (1)-(4)	19	43.18	0.38***	67	38.51	0.1	95	40.08	-0.19***
Full sample (1)-(10)	44	100	0.36***	174	100	-0.04	237	100	-0.07

Table 2.9: *Frequency of future takeovers*

This table reports Cox proportional-hazard regression results. It estimates the marginal effect of variables on the rate of subsequent takeovers following a bid failure. Financial(Strategic) is a dummy variable equal to one for bids by financial(strategic) acquirers and zero otherwise. Small indicates the group of bids in which the target market capitalization is ranked in the lower quartile. CARs are the absolute cumulative abnormal returns form 25 days before bid announcement to 25 days after failure announcement. Log(TargetSize) is the natural logarithm of target market capitalization in millions of 2014 dollar four calendar weeks prior to bid announcement. Cash is a dummy equal to one for bids that more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer are dummy variables indicating a bid in which the bidder's attitude is hostile and tender offer is involved, respectively. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(4) contain the main sample. Column (5)-(6) examine the subgroup of bids to be paid fully in cash. z-statistics is in parentheses. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial × Small × Cars	0.041	0.207	-0.026	0.089	-0.116	-0.633	-0.276
	-0.16	-0.66	(-0.07)	-0.19	(-0.29)	(-1.34)	(-0.51)
Financial × Small			-0.018	-0.021		0.115	0.13
			(-0.06)	(-0.07)		-0.36	-0.41
CARs			0.316***			0.507***	
			-2.59			-2.76	
Financial × CARs				0.2			0.153
				-0.74			-0.49
Strategic × CARs				0.341**			0.661***
				-2.57			-3.14
Log(TargetSize)		0.066*	0.061	0.062	0.004	-0.015	-0.008
		-1.72	-1.5	-1.53	-0.07	(-0.24)	(-0.14)
Cash		0.276**	0.256**	0.259**			
		-2.38	-2.18	-2.21			
Premium		0.002	0.001	0.001	0.003	0.002	0.002
		-1.06	-0.56	-0.54	-1.24	-0.8	-0.68
Hostile		0.300**	0.274*	0.277*	0.261	0.221	0.24
		-1.99	-1.81	-1.83	-1.34	-1.12	-1.21
TenderOffer		-0.383***	-0.433***	-0.435***	-0.188	-0.26	-0.271*
		(-3.16)	(-3.53)	(-3.55)	(-1.20)	(-1.63)	(-1.70)
Observations	789	789	789	789	455	455	455
Year FE	N	Y	Y	Y	Y	Y	Y
Industry FE	N	Y	Y	Y	Y	Y	Y

Table 2.10: *Value of future takeovers*

This table reports OLS regression of the deal value (log in millions of 2014 US dollar) that a target firm eventually receives from a successful takeover after initial failed bid. Financial(Strategic) is a dummy variable equal to one for bids by financial(strategic) acquirers and zero otherwise. Small indicates the group of bids in which the target market capitalization is ranked in the lower quartile. CARs are the absolute cumulative abnormal returns form 25 days before bid announcement to 25 days after failure announcement. Cash is a dummy equal to one for bids that more than half of deal value is paid in cash. Premium is bidder's offer divided by target's market value of equity four calendar weeks prior to bid announcement and truncated between zero and two. Hostile and TenderOffer are dummy variables indicating a bid in which the bidder's attitude is hostile and tender offer is involved, respectively. Year fixed effect is based on the announcement year. Industry fixed effect is based on 1-digit SIC codes. Column(1)-(4) contain bids from main sample that are ultimately acquired. Column (5)-(6) examine bids from the subgroup of failed cash-only bids that are ultimately acquired. z-statistics is in parentheses. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively. ***, **, and * indicate 1 percent, 5 percent, and 10 percent significance respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial × Small × Cars	0.003	-0.673	-0.929	1.183	-1.342	-3.22	-1.354
	[0.00]	[-0.57]	[-0.81]	[0.59]	[-0.81]	[-1.46]	[-0.75]
Financial × Small			-0.228	-0.291		0.501	0.583
			[-0.15]	[-0.19]		[0.31]	[0.36]
CARs			0.583			1.842	
			[0.98]			[1.32]	
Financial × CARs				-1.485			0.016
				[-1.07]			[0.01]
Strategic × CARs				1.145			2.86
				[1.46]			[1.29]
Cash		-0.505	-0.561	-0.501			
		[-0.76]	[-0.85]	[-0.78]			
Premium		2.335***	2.184***	2.206***	2.694*	2.284*	2.256*
		[3.05]	[2.88]	[2.83]	[1.93]	[1.84]	[1.79]
Hostile		0.963	0.857	0.862	2.741	2.54	2.602
		[0.74]	[0.70]	[0.70]	[1.18]	[1.17]	[1.18]
TenderOffer		0.252	0.214	0.169	0.741	0.605	0.557
		[0.46]	[0.41]	[0.33]	[0.75]	[0.69]	[0.65]
Years between		0.379***	0.387***	0.383***	0.458**	0.479**	0.488**
		[2.75]	[2.75]	[2.78]	[2.18]	[2.16]	[2.15]
Constant	2.025***	-0.235	-0.209	-0.352	-2.3	-1.165	-0.678
	[5.96]	[-0.24]	[-0.21]	[-0.33]	[-0.73]	[-0.48]	[-0.32]
Observations	311	311	311	311	189	189	189
Adjusted R-squared)	-0.003	0.114	0.109	0.114	0.177	0.178	0.181
Year FE	N	Y	Y	Y	Y	Y	Y
Industry FE	N	Y	Y	Y	Y	Y	Y

Chapter 3: Is productivity a source of bargaining power in cross-border acquisitions?

3.1. Introduction

For the past 30 years, ownership of over USD10 trillion of assets has been transferred from local targets to foreign acquirers via cross-border acquisitions. While a growing literature uncovers when and why foreign acquirers are interested in buying ownership of local targets, surprisingly little is known about who has more bargaining power in the negotiation process. To this end, this study focuses on examining one plausible source of bargaining power: country-level productivity in a global value chain.

A global value chain is where different countries specialize in different stages of a sequential production process and each country contributes to the total value to a final product. Those who add more value are more productive. Local firms in those countries present higher country-level productivity for they have exclusive access to sources of productivity that spill over locally such as factor endowments, technology, institutional strength, and so on. To access those sources of productivity, foreign firms could buy local firms' ownership. But the latter can leverage their exclusive access and demand a high selling price because they know foreign firms do not have equally good outside options: they cannot find another target country that is equally productive in conducting same economic activity. As a result, foreign firms must either agree to the high price or – if they do not want to overpay – let local firms retain majority ownership. Accordingly, I argue that the likelihood of foreign corporate control decreases with local firms' country-level productivity in global value chains, all else being equal.

To test this hypothesis, I use a sample of 21,264 cross-border acquisitions between 2004 and 2010 with a total value of USD 2.20 trillion. I use the value-added-adjusted revealed comparative advantage index calculated in Koopman, Wang, and Wei (2014) to measure relative productivity among countries in a value chain. Conventionally, relative productivity is computed as the share of a sector of a country's total gross exports relative to the world average of the same sector in world exports. Given countries' participation in global value chains, using gross exports to compute revealed productivity is subject to double counting of value added embedded in intermediate goods along the production sequence. For this reason, Koopman, et al. decompose gross exports into (1) domestic value added ultimately consumed by foreign countries, (2) domestic value added eventually returning to home country, (3) foreign value added returning to foreign countries, (4) and pure double counting of intermediate goods that cross the border more than once. And country-sector productivity in a global value chain is based on Item (1): domestic value added in exports.

I find strong supporting evidence for the hypothesis. Foreign control is less common in target countries with higher relative productivity in global value chains. A one-unit increase in productivity decreases the probability of foreign control by approximately 2.5 percent. This mean marginal effect increases to 4.4 percent if target countries have an obvious advantage in their position of the value

chains. Results are consistent in both forward and backward vertical acquisitions. This negative relation between foreign control and target country relative productivity is robust after controlling for the listed status of targets and acquirers, the sector-level interdependence between acquirers and targets, ownership concentration and minority investor protection. The results also persist after controlling for drivers of cross-border acquisition flows, such as differences in market valuation and currency appreciation across countries.

Another reasonable concern is that unmeasurable acquirer heterogeneity induces a self-selection bias: some acquirers may only invest in a minority equity stake and crowd in target countries with high productivity for unknown reasons other than lower bargaining power. To control for this acquirer heterogeneity, I conduct an analysis on a refined sample composed of acquirers that have made both minority and majority acquisitions over the sample period. The findings remain unchanged. An alternative explanation for the negative relation between foreign control and target country-level productivity is that, to gain control over more product targets, acquirers must make a series of minority investments before takeover, whereas they can directly buy control rights of less productive targets. To address this concern, I narrow the sample to targets that have only been invested once by the same foreign acquirer over the sample period. Again, main findings remain the same. Furthermore, government protection can be an alternative driver for non-controlling acquisitions. Therefore, I re-examine the relation on the sample that contains only non-strategic sectors, which should be less prone to government protection. The negative effect of target country productivity on foreign control stays significant for non-strategic sectors. This indicates that non-controlling acquisition is not solely driven by government protection.

First, this work directly contributes to the literature on mergers and acquisitions. To my knowledge, this study is among the first to explore bargaining power and the negotiation outcome of cross-border acquisitions, whereas existing literature focuses on the drivers of acquisition flows across countries (Baker, Foley, and Wurgler, 2009; Huizinga and Voget, 2009; Rossi and Volpin, 2004; Bris and Cabolis, 2008; Chari, Ouimet, and Tesar, 2010; Erel, Liao, and Weisbach, 2012; Ahern, Daminelli, and Fracassi, 2015; Karolyi and Taboada, 2015; Frésard, Hege, and Phillips, 2017). This chapter also complements Ahern (2012). While Ahern identifies sector-level vertical relatedness as a source of bargaining power, this work provides supporting evidence that productivity in global value chains is another source of bargaining power. In terms of acquisition type and its link to product market, this study is closely related to Frésard, Hege, and Phillips (2017), which also explores how global product market characteristics affect firm decisions regarding global organization. They focus on horizontal acquisitions and horizontal specialization, whereby countries present different degrees of advantage in producing final products. My work complements theirs by investigating vertical acquisitions and accounting for a rising feature in the globalized product market: that of vertical specialization where countries have different advantages at different stages of a final product's production process. Furthermore, my work extends

Ouimet (2013) by uncovering cross-country determinants on the choice between full and partial integration at global scale, including variation in corporate governance among target countries as well as relative stock market valuation and temporary purchasing power between acquirer and target countries.

Second, this study builds on a growing research effort to assess the impact of vertical specialization. Yi (2003) focuses on vertical specialization's role in trade cost and ultimately, economic growth. Costinot, Vogel, and Wang (2013) examine how vertical specialization influences country interdependence. Several other studies investigate how vertical specialization alter conventional wisdom on international trade based on gross trade data (Bems, Johson, and Yi, 2011; Koopman, Wang, and Wei, 2014; Patel, Wang, and Wei, 2017). This work shows that the impact of vertical specialization goes beyond international trade, reaching out to the international market for corporate control by empowering local firms to stay independent from foreign capital flows if they are in countries that have greater advantage in adding value to global value chains.

Third, this work sheds new light on the literature on the distributional effects of globalization. Existing literature focus on the distribution of gain between government and firm shareholders (Col, Liao, and Zeume, 2016), between employees across different categories (Goldberg and Pavcnik, 2007; Feenstra, 2008; Bustos, 2011; Amiti and Davis, 2012; Bloom, Draca, and Van Reenen, 2016), between customers across different income levels (Fajgelbaum and Khandelwal, 2016), and between countries along global value chains (Timmer, Erumban, Los, Stehrer, and De Vries, 2014). This study focuses on a pertinent but relative unexplored pie-splitting mechanism – local firms versus foreign firms.

3.2. Related literature and hypothesis development

This work builds on a growing research effort to understand cross-border acquisitions. Existing literature focuses on identifying drivers of acquisition flows across borders, such as cross-country differences in taxation (Huizinga and Voget, 2009), in bilateral trade, stock market valuation and currency appreciation (Erel, Liao, and Weisbach, 2012), in regulation (Karolyi and Taboada, 2015), in culture (Ahern, Daminelli, and Fracassi, 2015), and in industry specialization (Frésard, Hege, and Gordon, 2017). While these studies uncover when and why foreign acquirers are interested in buying ownership of certain local targets, little is known about which party has more bargaining power.

3.2.1. Productivity in global value chain as a source of bargaining power

This study develops a hypothesis based on the idea that customer-supplier relations affect bargaining outcome of acquisitions. For example, Ahern (2012) shows that, for a sample of U.S. domestic acquisitions between local suppliers and customers, sector-level vertical relatedness affects the bargaining power. As for cross-border acquisitions, foreign acquirers and local targets are related as supplier and customer through global value chains where different countries specialize at different stages

of a final product's sequential production process (Hummels, Ishii, Yi, 2001; Yi, 2003; Johnson and Noguera, 2012). Whether a production stage is worth vertical integration depends on its relative position along the global value chain and its neighbor stages' relative contractibility, and the final product's elasticity of demand (Antràs and Chor, 2013; Alfaro, Antràs, Chor, and Conconi, 2017). After deciding which productions stage to integrate, foreign acquirers negotiate with potential target firms that participate at that particular production stage.

In negotiating with foreign acquirers, target firms would have more bargaining power if they are from countries that are more productive than other countries participating in that production stage. This is because country-level relative productivity is determined by factor endowments, technology, and institutional strengths that are only accessible to local firms (Chor, 2010; Costinot, 2009). With exclusive access to sources of high productivity, local firms induce foreign acquirers to compete for them. Competing foreign acquirers become local firms' valuable alternatives to reaching an agreement with any foreign acquirer. The existence of an outside option affects bargaining outcome only if opting out is credible, as predicted by bargaining theory (see for example Osborne and Rubinstein (1990)). Bargaining theory further predicts that, while the extent to which a credible outside option translates into a gain depends on features of bargaining procedures, the party with a credible outside option can gain at least as much as the value of his outside option. In line with this thinking, when local targets' productivity is relatively high, to opt out and terminate a negotiation is a credible threat to foreign acquirers, from which local targets can gain. This study focuses on examining the positive relation between local targets' productivity and bargaining power and leaves whether and how negotiation strategies and tactics adopted in bidding process affect bargaining outcome for future research.

3.2.2. Extent of ownership as a bargaining outcome

The extent of control reflects bargaining power between foreign acquirers and local targets. Williamson (1979) suggests that the optimal degree of control is a trade-off between acquirers' efficiency gain from acquisition and their commitment of resources to obtain such efficiency. More productive local targets can manifest their superior bargaining power by demanding a higher price for the transfer of control rights. Acquirers would not pay if the price is higher than the efficiency gain of acquisition; consequently, local targets retain majority ownership. Alternatively, Grossman and Hart (1986) predict that when two parties discuss the possibility of integration, control rights go to the party with higher proprietary content in relationship-specific investments to preserve investment incentives. More productive local targets pose a credible threat of productivity loss to foreign acquirers if the targets lose the incentive to invest after control rights are transferred to acquirers. To avoid such incentive distortion, foreign acquirers would choose not to buy majority ownership from local targets. Both are under the view that the observed degree of control reflects the bargaining power between acquirers and targets.

Ahern (2012) measures bargaining outcome with targets' share in total cumulative abnormal return around acquisition announcement and shows that the party with more bargaining power obtains the larger slice. Though conceptually consistent with the impact of credible outside option, this proxy might capture deals where acquirers overpay and obtain negative acquisition gain but exclude cases where acquirers refuse to overpay. In addition, this proxy requires sufficient stock price information for both acquirers and targets, while cross-border acquisitions mainly involve private targets which do not have stock price information. For those two reasons, this study chooses to use the extent of control to measure bargaining outcome.

Combining predictions on bargaining power and outcome, I propose the following testable hypothesis.

Baseline hypothesis: *The likelihood of foreign control decreases with the target countries' productivity in global value chains, all else being equal.*

3.3. Data and Methodology

3.3.1. Productivity in global value chains

I use the value-added-adjusted revealed comparative advantage index calculated in Koopman, Wang, and Wei (2014) to measure relative productivity among countries at each production stage of a value chain. Conventionally, relative productivity is computed as the share of a sector of a country's total gross exports relative to the world average of the same sector in world exports. When the ratio exceeds one, the country is considered to have a revealed comparative advantage in that economic activity (Balassa, 1965). Given countries' participation in global value chains, using gross exports to compute revealed productivity is subject to double counting of value added embedded in intermediate goods along the production sequence. For this reason, Koopman, Wang, and Wei (2014) decompose gross exports into (1) domestic value added ultimately consumed by foreign countries, (2) domestic value added eventually returning to home country, (3) foreign value added returning to foreign countries, (4) and pure double counting of intermediate goods that cross the border more than once. Koopman, Wang and Wei calculate country-sector revealed comparative advantage based on Item (1): domestic value added in exports.

The index calculation involves three main steps. First, Koopman et al. collect data on annual flows of goods and services at country-sector level for the entire world economy from the Global Trade Analysis Project database (GTAP, version 7). This dataset covers 129 countries and 41 sectors for the base year 2004. Second, they aggregate the 129 countries into 26 countries and regions. The full lists of sectors and countries/regions are reported in Appendices A. Finally, they use those data to construct a world input-output table. This describes the sale and purchase relationships between suppliers and customers in the world economy by showing flows of final and intermediate goods and services at the

country-sector level. Each of the 41 GTAP sectors represents one global value chain and each of the 26 countries/regions within that sector is a participant along the chain.

This 2004 revealed comparative advantage index is going to be used to analyze cross-border acquisitions from 2004 to 2010. I assume that country-sector ranking in the index does not change for the six-year period. This assumption is reasonable because sources of country-level relative productivity – technology, factor endowments and institutional strength – change slowly over time. In future versions of this work, I am going to expand the sample by using GTAP version 9. This dataset covers 129 countries and 57 sectors for three base years: 2004, 2007, and 2011. It will allow me to extend acquisition sample to 2016. Specifically, I am going to use country-level productivity in global value chains at year 2004 for cross-border transactions in the sub-periods of 2004 to 2006, year 2007 for 2007 to 2010, and year 2011 for 2011 to 2016.

[Insert Table 3.1 Here]

Table 3.1 Panel A presents descriptive statistics on country-level relative productivity in global value chains. Due to space limitations, I report the top 12 sectors ranked by total deal value (in U.S. dollars) of cross-border vertical acquisition. These nine sectors represent 75 percent of the total transaction value. I describe the data collection on acquisitions in the next section. For each GTAP sector, I report the top three and bottom three countries ranked by their productivity. For example, Hong Kong has high relative productivity in business services and trade but presents low productivity in sectors that require rich natural resources, such as oil and gas. Most importantly, there is not a single country that consistently dominates a top or bottom ranking across sectors. This feature is desirable because it ensures that the relation between productivity and extent of control is unlikely to be driven by a subset of countries.

Next, I perform an analysis to verify that country-level relative productivity is an important component of target firm relative productivity. Specifically, I collect from Worldscope the accounting measure earnings before interest expenses and taxes scaled by book value of total assets (*ROA*) as a proxy for firm productivity. Then, I aggregate this measure at the country-sector level, which are matched to GTAP country-sector pairs classified by Koopman et al. Next, I regress *ROA* on country-level productivity. Table 3.1 Panel B presents the estimation results. Clearly, for both target and acquirer countries, there is a positive and statistically significant association between the two variables. This association is robust after controlling for country and year fixed effects, meaning the association prevails across country-sector pairs.

3.3.2. Cross-border acquisitions

I collect mergers and acquisitions transactions from the Security Data Corporation (SDC) announced between 2004 and 2010. I drop transactions where acquirers have an SIC code between 6000 and 6999 but targets do not have an SIC code between 6000 and 6999 to ensure that transactions in my sample

are motivated by product market synergies. I keep transactions that are labeled as cross-border, mergers, acquisitions of majority interests, acquisitions of partial interests, and acquisitions of assets. Among these transactions, I keep those where acquirers do not own a stake equal or greater than 50 percent prior announcement. The extent of control is measured as the percentage of stake owned by acquirers after acquisitions. If it is less than 50 percent, the acquisition is labeled as a purchase of non-controlling interest. If it is greater than 50 percent, the acquisition is labeled as a purchase of controlling-interest. To better contrast bargaining power between acquirers and targets, I drop mergers of equal, i.e. acquirers owning exactly 50 percent after acquisition. Till this step, the sample contains 56,725 acquisitions.

I match the 41 GTAP sectors to three-digit standard industry classifications (SIC) provided by SDC for each target and acquirer. Then I match the 26 GTAP countries/regions to target and acquirer countries provided by SDC. SDC record target and acquirer countries according to their location of operation. For example, if the U.S. branch of Nestlé acquirers an Italian branch of Danone, the acquirer country will be marked as U.S. and that of target marked as Italy. Therefore, the match of country correctly reflects country-level productivity and value chain relations between acquirers and targets. I retain transactions where both acquirer and target have non-missing measures of country-level productivity. At this step, the sample has 52,547 acquisitions.

To identify the vertical relatedness between acquirers and targets, I follow Acemoglu, Johnson and Mitton (2009) and use the vertical relatedness index computed by Fan and Lang (2000). The computation of vertical relation is based on the U.S. input-output table reported in 1992 by U.S. Bureau of Economic Analysis (BEA). The input–output table classifies the U.S. economy into 498 six-digit input-output code levels (IO code) and report the dollar value of input used to produce the output of 498 sectors. To use U.S. sector vertical relatedness to describe world economy, I assume that the U.S. input-output tables provide a standardized measure of input requirements internationally. As noted by Acemoglu et al., the U.S. input-output tables should be informative on input flows across sectors to the extent that these are determined by technology.

To identify vertical acquisition, I follow Ahern (2012) and drop input-output sector pairs that share the same IO code. Then I keep pairs where vertical relatedness is non-missing and non-zero. As the final step, I match IO codes to four-digit SIC codes using the concordance table provided by the BEA. Till now, my final sample contains 21,264 vertical cross-border acquisitions.

[Insert Table 3.2 Here]

Table 3.2 reports the summary statistics of cross-border acquisitions for the period between 2004 and 2010. In Panel A, the column – Total– includes all acquisitions, whereas the columns under the tag – Vertical – contains the relevant sample for this study. The column – Control – (– Non-control–) consists with vertical acquisitions in which greater (less) than 50 percent is transferred from targets to

foreign acquirers. The first row indicates the number of transactions. In 40 percent of acquisitions from 2004 to 2010, acquirers and targets have unambiguous vertical relation in international product market. While foreign acquirers typically seek controlling stake, there are 20 percent of transactions in which local targets retain control rights. In unreported table, I find this 20-80 difference a stable pattern throughout the sample period. The second row indicates the deal value in trillion US dollars after adjusting for inflation based on 2015 Consumer Prices Index. It appears that economic significance of non-controlling acquisition decreases to 14 percent, compared to that of controlling acquisitions. However, it is worth noting that purchases of non-controlling stake are often through private negotiation. Also, news on minority investment catches less attention. For those two reasons, I cannot exclude the possibility that this seemingly drop in economic significance is due to less deal value information on minority acquisition than on controlling acquisitions.

Panel B of Table 3.2 presents summary statistics of the sector-level supplier-customer relatedness between acquirers and targets for the sample of vertical acquisitions. Acquisitions are characterized as forward acquisitions if acquirers sell more to targets more than buy from targets, and as backward if acquirers buy more from targets than sell to targets. Following this identification rule, I identify 10,194 forward acquisitions and 11,070 backward acquisitions. As shown in the Column – Mean – , in forward acquisitions, targets use averagely 3.46 cents worth of goods from acquirers to produce one-dollar worth of goods, but their goods are seldom used by acquirers for production, indicating by a mean as low as 0.27. The opposite relations hold for backward acquisitions.

Panel C of Table 3.2 lists the 15 most active GTAP sector pairs for cross-border acquisitions between 2004 and 2010 and experienced 60 percent of the full sample. Approximately 20 percent of vertical acquisitions take place in the same GTAP sectors mainly because GTAP sectors are highly aggregated. Though such aggregation limits a more precise ranking on country-level productivity beneath GTAP sector level, it should not pose any major bias in subsequent analysis for two reasons. First, I have already reduced the presence of horizontal acquisitions at six-digit IO code level. Second, relative productivity should be positively correlated among sub-sectors under each GTAP sector, to the extent that sources of productivity are transferrable among them. Nevertheless, to ensure robustness, I exclude acquisitions where acquirers and targets share the same GTAP sector, replicate the empirical analysis in Section 3.4., and reported replication results in Appendix 3.2-4. In fact, replicated results are not only consistent with but even stronger than main analysis results listed in Section 3.4..

3.3.3. Model

I use the following probit regression to test the hypothesis:

$$Pr\left(Y_{c,i,t}=1\middle|P_{c,i,s},P_{c',j',s},X_{c,i,t},X_{c',i't},\boldsymbol{X_{c,c',t}},\boldsymbol{V_{i,i'}}\right)=\Phi\left(\alpha+\beta P_{c,i,s}+\theta P_{c',j',s}+\gamma X_{c,i,t}+\delta X_{c',i',t}+\boldsymbol{\eta' X_{c,c',t}}+\boldsymbol{V_{i,i'}}\right), \quad (3.1)$$

where *Pr* denotes the probability and $\Phi(\cdot)$ is the cumulative distribution function of the standard normal distribution. The dependent variable $Y_{c,i,t}$ is a binary variable equal to one (zero) if the average transfer of ownership from local targets in country *c*, sector *i*, and year *t* to foreign acquirers is greater than (lower than) 50 percent.[54] It means I test the choice between majority and minority acquisitions. Minority acquisitions can be an optimal bargaining outcome when the value of local targets' outside option is lower than staying fully independent of foreign acquirers but no lower than retaining majority interests. For example, a financially constrained target would be better off accepting a minority acquisition by foreign acquirers so that foreign acquirers can certify and facilitate future capital raise of targets (Liao, 2014). I do not test the likelihood of majority acquisitions against that of firms not being targeted by any foreign acquirers because this approach cannot distinguish greenfield investment from no integration. Since the analysis of this work is to be conducted at country-sector level, the pair of targeted firms versus non-targeted firms is an inaccurate proxy for integration decision unless there is complete information on greenfield investment in the same destination country-sector pair. On the contrary, examining the choice between majority and minority acquisition circumvents the trouble to account for all factors that affect the choice of entry mode.

The variable of interest, $P_{c,i,s}$, measures the productivity of target country *c* in sector *i* and year *s*, where *s* indicates the most recent year with non-missing observation of *P* (i.e. $s \leq t$). In other words, *s* indicates 2004, and *t* indicates years from 2004 to 2010. As explained in Section 3.3.1., sector *i* represents one value chain. Given my hypothesis, β should be negative and indicate to what extent higher productivity deter foreign control.

The rest of the variables on the left-hand side of Model (3.1) are control variables that may affect the likelihood of foreign acquirers buying majority ownership. First, I control for acquirers' country-level relative productivity. This is because more productive acquirers are more likely to afford high price of control stake for they have higher capacity to amortize fixed costs of full integration (Alfaro et al., 2017). Then, I consider the stock-listing status of targets and include in the vector $X_{c,i,t}$ the average percentage of listed targets in country *c*, sector *i*, and year *t*. Unlike unlisted targets, listed targets can learn about themselves from information reflected in their stock market prices (Foucault and Frésard, 2012). As a result, it is more difficult for foreign acquirers to persuade listed targets to agree to a lower offer. Also, as Ouimet (2013) suggests, acquirers avoid buying majority ownership in targets to preserve the incentive of target managers if target firms are listed and managers are compensated by stock options prior to acquisition.

Furthermore, higher ownership concentration and poorer minority investor protection could deter acquisitions of non-controlling stake and lead to an over-presence of controlling stake purchases in

[54] Mergers of equal or transactions where the transfer of ownership is equal to 50 percent are dropped from my sample.

certain target country-sector pairs. To attenuate this potential bias, I use the percentage of closely-held shares to proxy ownership concentration and anti-director rights index to measure minority investor protection. Closely-held shares are extracted from Datastream and then aggregated from firm level to country-sector level. Anti-director rights index is taken from Spamann (2010). These two governance-related control variables are also in the vector $X_{c,i,t}$.

Next, I control for the stock-listing status of acquirers because they have easier access to external financing and hence higher capability to make a top offer for target majority ownership. $X_{c',i',t}$ is the average percentage of listed acquirers in country *c'*, sector *i'*, and year *t*.

Additionally, Erel, Liao and Weisbach (2012) show that appreciation in acquirer countries' wealth relative to that of target countries facilitates acquirers' cross-border deals. Similarly, such relative purchasing power could enable acquirers to make a better offer for target majority ownership. Thus, I control for differences in stock market valuation and currency appreciation between target and acquirer countries. Those two variables are in the vector $\boldsymbol{X_{c,c',t}}$. Specifically, I download stock prices, exchange rates, and consumer price indices from Datastream database to compute differences between target and acquirer countries in buy-and-hold inflation-adjusted stock market returns and exchange rate returns from 275 trading days to 25 trading days prior to the announcement.

The vector $\boldsymbol{V_{i,i'}}$ includes dummies of GTAP sector pairs between targets and acquirers. As discussed in Section 3.2.1., previous studies show characteristics of production stages relations affect extent of control. By holding vertically related sector pairs constant, I remove influence of production stages on choice between majority and minority acquisition. In doing so, I ensure that $P_{\mathrm{c,i,s}}$ is a clean rank of country productivity within the same vertical integration relations.

3.4. Empirical results

3.4.1. Univariate results

Table 3.3 reports summary statistics of key variables and their mean differences between controlling and non-controlling acquisitions. Controlling acquisition commonly involves an ownership stake over 90 percent. Non-controlling acquisitions tend to involve the transfer of a sizeable stake in the target firm, with a mean of 25.2 percent and a median of 21.2 percent.

The next ten rows report differences across country-sector pairs. Target-country relative productivity ($P_{c.i}$) is lower in controlling acquisitions, on average, than that of non-controlling acquisitions. This observation aligns with the prediction in the baseline hypothesis. Compared to the target side, the difference of acquirer-country relative productivity ($P_{c'.i'}$) is smaller between controlling acquisitions and non-controlling acquisitions. Listed targets are rarely involved in control acquisitions, indicated by a presence as low as five percent (*Listed Target*). Its presence is higher in non-controlling acquisitions

but still significantly less frequent than unlisted firms. On the contrary, listed companies account for most acquirers in both controlling and non-controlling acquisitions (*Listed Acquirer*). As expected, non-controlling acquisitions are more common in countries with better investor protection ($ADRI_c$), though the difference to controlling acquisition is small. Non-controlling acquisitions is associated with a lower degree of shareholder concentration ($CHS_{c,j}$), which alleviates the potential bias of closely held firms crowded in target country-sector pairs with high relative productivity. Non-controlling acquisitions are more common when the target country stock market is valued higher than the acquirer country stock market ($MktRet_{c,c'}$) and when the acquirer country experiences a larger local currency appreciation ($FX_{c,c'}$). Non-controlling acquisitions are more common when it is suppliers acquiring customers (*Forward*) and less common when it is customers acquiring suppliers (*Backward*).

Table 3.3 also presents two deal-level variables. Relative size is estimated as deal value relative to the acquirer's market capitalization 25 trading days prior to the deal announcement. Non-controlling acquisitions are associated with smaller mean relative size ratios than are majority acquisitions. Premium is the offer price scaled by target stock price four weeks prior to the announcement date. Though smaller than controlling acquisitions, acquirers tend to pay a considerable premium for a non-controlling stake. As mentioned above, cross-border vertical acquisitions involve mainly unlisted target firms. That is why deal information based on stock price is scarce and the sample size shrinks dramatically.

[Insert Table 3.3 Here]

3.4.2. Multivariate results

Table 3.4 presents baseline estimation results based on Model (3.1). Coefficients are marginal effects on the likelihood of controlling acquisition. Column 1 complements the univariate results and shows that target country-level relative productivity ($P_{c,j}$) has a negative effect on the choice to buy controlling stake. And this effect is economically and statistically significant (z-statistics of 8.14). A one-unit increase in productivity decreases the probability of takeover by approximately 2.5 percent. Column 2 and Column 3 introduces year and GTAP sector pair dummies and a set of control variables. The signs and significance of target country productivity are largely unchanged. Some of coefficients on control variables are consistent with previous studies. Target firms being listed reduces the chance of foreign control by 26 percent, whereas acquirers' listed status does not seem to matter. High ownership concentration does not affect foreign acquirers' choice, but a better protection for minority investors does encourage foreign acquirers to buy non-controlling stake. Notably, coefficients on stock market valuation and purchasing power reveal new insights. When foreign acquirers experience a relative higher valuation and currency appreciation than local targets, foreign acquirers tend to buy non-controlling stake. This suggests that, while to take advantage of financial arbitrage generally promotes cross-border acquisition flows (Erel, Liao, and Weisbach, 2012), it is the minority interest acquisition that benefits the most. Furthermore, acquirers' country-level relative productivity appears to have no effect on the

bargaining outcome between a controlling and non-controlling stake. This is not necessarily against the notion that productive firms are more likely to integrate global production (Alfaro et al., 2017). It could be that highly productive firms are overall more likely to internalize production at global scale, but to have higher capacity to amortize high integration costs might not matter much for the choice between full and partial integration. Alternatively, it is possible that the true effect of acquirer productivity is masked by the mixed directions of vertical acquisitions. I examine in the next subsection these two alternative explanations by separating forward and backward acquisitions.

Next, I narrow the sample to those where target countries have an advantage in each production stage, i.e. relative productivity is no less than one, and report results in Columns 4-6. I expect that targets would be in an even better negotiation position when their countries have established an obvious advantage. Consistent with this line of thinking, the difficulty for foreign acquirers to get a control on targets with obvious productive advantage is now doubled. Changes on coefficients of control variables are also worth mentioning. Compared to results on main sample, acquirers being listed starts to matter for getting control rights on highly productive targets. This is consistent with my expectation that acquirers with easier external financing have a higher chance to afford high purchasing price for control rights. Coefficients of capital market valuation and currency appreciation continue to have the same sign but lose statistical significance here.

To reinforce the intuition of estimated coefficients, Figure 3.1 plots predictive marginal effect of target country productivity based on the specification in Column 3 of Table 3.4. The curve shows how the likelihood of foreign control is going to change if target relative productivity increases by 0.5 at a step from one to eight while other covariates are held fixed. The vertical lines on each dot indicates the 95 percent confidence interval. The plot indicates that, for example, a country that is eight times as advantageous as another country in providing crops, firms located in the former country have 50 percent less chance (0.9-0.4=0.5) that those located in the latter country of being fully integrated along value chains, all else being equal.

[Insert Table 3.4 Here]

[Insert Figure 3.1 Here]

3.4.3. Do vertical and backward acquisitions receive different effects?

Table 3.5 presents estimates of Model (3.1) for forward and backward vertical acquisitions separately. Now that acquisition directions are explicit and separated, I introduce vertical relatedness as an additional control. In this way, I isolate the role of target country-level relative productivity in capturing sources of superior productivity – technology, institutional strength, labor, etc. – from its role

in capturing interdependence between acquirers and targets. The effect of target productivity on foreign control is largely consistent between forward and backward acquisitions. And its effect appears to be slightly more pronounced in forward acquisitions.

More interestingly, I obtain additional new insights on the control variables from this analysis. First, acquirer country-level productivity now becomes statistically significant and has opposite effect on forward and backward acquisitions. It settles the puzzle mentioned in previous section: acquirer productivity does matter for the extent of control, but the sign and magnitude of its effect depends on the direction of vertical acquisitions. While its effect in forward acquisitions is consistent with the prediction of Alfaro et al. (2017), that in backward acquisitions lacks theoretical prediction.

Second, there is also a marked difference in the impact of vertical relatedness on the bargaining outcome. The relatedness between acquirers and targets does not seem to matter in forward acquisitions but significantly and negatively affects acquirers' bargaining position in backward acquisitions. More specifically, the more important targets function as supplier to foreign acquirer in international product market, the less likely foreign acquirers would be able to obtain control rights over targets. This result complements that of Ahern (2012), which finds that relative gain of targets in backward acquisitions is unrelated to product market relations. My findings suggest that product market relations matter for bargaining in backward acquisitions, but their effects are reflected in the extent of control instead of in the division of acquisition gains. Results here reinforce the view of this work that the observed extent of control is a valid proxy for bargaining outcome and complements the division of total acquisition gain in scenarios when stock price information is unavailable or acquirers are unwilling to strive for overpriced control rights and settle for a minority stake.

Finally, the slightly positive effect of listed acquirers found in previous section turns out to be stronger and solely from backward acquisitions. The other relevant control variables continue to have consistent signs between forward and backward acquisitions, though their economic and statistical significances are largely higher in forward than in backward acquisitions.

Taken as a whole, the baseline hypothesis of this work receives consistent and supporting evidence. But there is more to learn about why effects of certain control variables change when the direction of vertical acquisition changes. Future versions of this work are going to investigate those questions in depth.

[Insert Table 3.5 Here]

3.4.4. Robustness tests

So far, I have used control variables to ensure that it is target country-level productivity that reduces foreign control. However, results can still be undermined by unmeasurable acquirer heterogeneity. More

specifically, some acquirers might only invest in minority equity stakes and others only conduct takeovers. If the former type of acquirers has a disproportionately high presence in target countries with higher relative productivity, it would be unclear whether the effect on foreign control comes from target country productivity or acquirer heterogeneity. To address this issue, I conduct an analysis on a refined sample composed of acquirers that have made between 2004 and 2010 both controlling and non-controlling acquisitions overseas. Results are reported in Table 3.6 Column 1 and 2. The findings remain unchanged.

An alternative explanation for the negative relation between foreign control and target country-level productivity is that, to gain control over more product targets, acquirers must make a series of minority investments before takeover, whereas they can directly buy control rights of less productive targets. To reduce the impacts of serial acquisitions, I narrow the sample to targets that have only been invested once by the same foreign acquirer over the sample period.[55] Table 3.6 Column 3 and 4 present robustness test results. Again, main findings remain the same.

To further check for robustness, I examine whether results are biased by regulatory protection against foreign ownership. Dinc and Erel (2013) provide evidence that, for mergers across EU countries, local governments take measures to prevent foreign acquirer from taking control of 'national champions'. In the current version, I address this issue by looking into GTAP sectors that are not commonly regarded as strategic ones. I therefore drop coal, oil and gas, minerals, petroleum and coal products, mineral products, electricity, gas manufacture and distribution, water, air transportation, communication (including telecommunication), financial services, public admin, public defense, education, and health. Estimates on the rest of GTAP sectors are reported in Table 3.6 Column 5 and 6. Notably, coefficient of target country-level productivity loses its significance in the main sample. That means strategic sectors do have less foreign control. However, once I restrict the sample to targets that have an obvious advantage, the negative effect of target country productivity on foreign control is restored for non-strategic sectors. That suggest that the negative relation is not solely driven by government protection but also driven by target country-level productivity.

This approach does not account for the fact that sectors of national interest may vary across countries and over time. Also, government intervention could deter foreign acquirers in the future. Thus, in future version of this work, I construct a more refined measure of propensity for government intervention than a country-year invariant dummy on strategic sectors. To do this, I follow Dinc and Erel (2013) and search news about government intervention in the top ten cross-border acquisitions by deal value per target country per calendar year from 2003 to 2016. Then I sort target countries according to their propensity for government intervention a year before the acquisition announcement. If coefficient of target country-level productivity stays negative and statistically significant in subsamples less prone to

[55] I use both 2004-2010 and 2014-2016 periods to detect subsequent acquisitions by the same acquirer. Results stay the same.

government intervention, results should reinforce findings documented here and support the hypothesis of bargaining power.

[Insert Table 3.6 Here]

3.5. Conclusion

This chapter explores the role of target productivity in vertical cross-border acquisitions. I find foreign control is less common in targets countries with higher productivity in global value chains. This finding suggests that target country productivity in global value chains is an important determinant of the extent of control transferred from local firms to foreign firms. I interpret it as supporting evidence that one source of bargaining power for targets vis-à-vis foreign acquirers in the international market of corporate control is a country's relative position in international product market.

This work also obtains new insights on how determinants of cross-border acquisitions flows documented in previous studies can further affect the choice between controlling and non-controlling stake size. And such effects may have opposite signs in forward and backward vertical acquisitions. To better understand reasons behind such opposite effects, it is worth investigating further into the structure and characteristics of international product market.

A caveat in this version is the sample period is short and the aggregation of countries is high. Because of such limitations, I am unable to generalize findings beyond the year of 2010 or test my hypothesis on cross-border acquisitions within EU. Therefore, an expansion on dataset awaits in future versions of this work.

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Table 3.1: Country-level relative productivity

This table reports descriptive statistics of country relative productivity in global value chains. Panel A. reports top three and bottom three countries ranked by productivity for the most significant sectors in terms of total deal value in 2015 U.S. dollar. Panel B. presents the association between country productivity as explanatory variable for the proxy for fir-level productivity (operating income over total assets *ROA*). *ROA* is aggregated at country-sector level. Data on operating income and total assets are from Worldscope. *Coef*, *t-stat*, *N*, *Controls*, and *Adj-R^2* are estimated coefficients on country relative productivity, its t-statistics, sample size, the dummies on year and country, adjusted R-squared. *, **, and *** indicate coefficients significantly different from zero at the 0.1, 0.05 and 0.01 levels respectively.

Panel A.

Business Services			Financial Services			Chemical, Rubber& Plastic		
1	SGP	1.86	1	BRA	2.21	1	SGP	2.19
2	E15	1.62	2	TWN	1.94	2	TWN	1.36
3	HKG	1.57	3	USA	1.64	3	USA	1.32
24	PHL	0.19	24	ROW	0.45	24	SSA	0.27
25	MET	0.08	25	PHL	0.32	25	XEA	0.23
26	IDN	0.06	26	MET	0.11	26	HKG	0.19
Electronic Equipment			Machinery & Equipment			Motor Vehicles and Parts		
1	PHL	7.03	1	KOR	1.89	1	MET	2.81
2	MYS	5.29	2	JPN	1.60	2	JPN	2.60
3	MET	3.88	3	USA	1.45	3	CAN	2.22
24	ZAF	0.56	24	HKG	0.19	24	HKG	0.14
25	RUS	0.09	25	XEA	0.18	25	SGP	0.12
26	SSA	0.03	26	SSA	0.06	26	SSA	0.04
Minerals			Transportation Equipment			Communication		
1	AUS	5.88	1	USA	2.24	1	ZAF	1.74
2	IDN	4.52	2	BRA	1.97	2	AUS	1.64
3	ZAF	4.03	3	KOR	1.65	3	E12	1.33
24	MET	0.15	24	RUS	0.25	24	THA	0.71
25	JPN	0.10	25	HKG	0.14	25	RUS	0.37
26	SGP	0.04	26	ZAF	0.13	26	MET	0.33
Trade			Paper Products & Publishing			Construction		
1	HKG	3.85	1	CAN	2.42	1	RUS	2.20
2	ZAF	1.54	2	MET	1.61	2	USA	1.83
3	SSA	1.49	3	USA	1.37	3	E12	1.65
24	KOR	0.42	24	VNM	0.28	24	XEA	0.24
25	MET	0.20	25	SSA	0.24	25	PHL	0.21
26	VNM	0.17	26	PHL	0.22	26	CHT	0.17

Panel B.

	Coef.	t-stat	N	Controls	Adj-R^2
Target ROA	0.007***	7.37	12,238	Country-Year	0.13
Acquirer ROA	0.065***	4.56	14,170	Country-Year	0.02

Table 3.2: Summary statistics of cross-border vertical acquisitions

This table reports summary statistics of cross-border acquisitions between 2004 and 2010. I include all cross-border mergers and acquisitions that are labeled as mergers, acquisitions of majority interests, acquisitions of partial interests, and acquisitions of assets. Among these transactions, I keep those where acquirers do not own a stake equal or greater than 50 percent prior announcement. Panel A reports the proportions of transactions across cross-border, vertical, control, and non-control deals in the whole sample in terms of the number of deals and the dollar value adjusted for inflation. Panel B presents summary statistics of vertical acquisitions. Vertical relatedness is provided by Fan and Lang (2000) based on BEA 1992 input-output table and is the fraction the input industry contributes in added-value to the output industry. Vertical acquisitions (forward, backward) are those in which the acquirer and target do not share the same BEA IO industry code and either the acquirer sell (more, less) to the target than the target sell to the acquirer and the target buys (more, less) inputs from the acquirer than the acquirer buys from the target. Panel C reports the 15 vertically related GTAP sector pairs with the most cross-border acquisitions during the sample period in descending order. GTAP sectors are matched to three-digit SIC codes. BEA IO codes are matched to four-digit SIC codes.

Panel A.

	Total	Vertical					
		Sub-total		Control		Non-control	
			% of total		% of sub-total		% of sub-total
Number of deals	52,547	21,264	40%	17,106	80%	4,158	20%
Deal value (in trillion USD)	5.934	2.196	37%	1.893	86%	0.303	14%

Panel B.

				Percentile				
	N	Mean	Std. Dev.	10th	25th	50th	75th	90th
Forward acquisition								
Forward relatedness(%)	10,194	3.46	7.05	0.08	0.35	1.27	3.28	7.28
Backward relatedness(%)	10,194	0.27	0.50	0.00	0.00	0.01	0.36	0.95
Backward acquisition								
Forward relatedness(%)	11,070	0.26	0.52	0.00	0.00	0.02	0.19	0.95
Backward relatedness(%)	11,070	3.69	7.03	0.09	0.34	1.62	3.82	8.15

Acquirer GTAP sector	Target GTAP sector	All vertical acquisitions		Forward		Backward	
		N	%	N	%	N	%
Business Services	Business Services	2,027	10%	1,199	41%	828	59%
Financial Services	Financial Services	1,738	8%	687	60%	1,051	40%
Financial Services	Business Services	1,646	8%	554	66%	1,092	34%
Communication	Business Services	1,041	5%	635	39%	406	61%
Electronic Equipment	Business Services	946	4%	466	51%	480	49%
Trade	Chemical, Rubber & Plastic	833	4%	376	55%	457	45%
Trade	Electronic Equipment	633	3%	263	58%	370	42%
Machinery & Equipment	Business Services	562	3%	211	62%	351	38%
Machinery & Equipment	Machinery & Equipment	552	3%	265	52%	287	48%
Trade	Business Services	546	3%	241	56%	305	44%
Trade	Motor Vehicles & Parts	498	2%	136	73%	362	27%
Minerals	Minerals	477	2%	289	39%	188	61%
Water Transportation	Transportation Equipment	460	2%	130	72%	330	28%
Communication	Communication	426	2%	225	47%	201	53%
Chemical, Rubber & Plastic	Chemical, Rubber & Plastic	422	2%	243	42%	179	58%
Total 15 GTAP pairs		12,807	60%	5,920		6,887	
Total sample		21,264	100%	10,194		11,070	

Table 3.3: Univariate results

This table reports summary statistics of key variables used in empirical analysis. Column 2-5 (6-9) are controlling acquisitions (non-controlling acquisitions), defined as stake owned after acquisition is greater (less) than 50 percent. The last column reports p-value of t-test between the controlling and non-controlling subsamples. *Stake size (%)* is the percentage of targets' stock shares that acquirers owns after acquisition. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *Relative size* is estimated as deal value relative to the acquirer's market capitalization 25 trading days prior to the deal announcement. *Premium* is the offer price over target stock price four weeks before deal announcement date.

	Acquisition of controlling stake				Acquisition of non-controlling stake				Difference of group mean
	N	Mean	Median	Std.dev.	N	Mean	Median	Std.dev.	p-value
Stake size (%)	16,695	94.7	100	13.52	3,388	25.2	21.2	17.07	0.00
$P_{c,i}$	17,106	1.19	1.15	0.77	4,158	1.33	0.99	1.28	0.00
$P_{c',i'}$	17,106	1.17	1.18	0.65	4,158	1.14	0.97	0.75	0.02
Listed target	17,106	0.05	0	0.23	4,158	0.32	0	0.47	0.00
Listed acquirer	17,106	0.57	1	0.49	4,158	0.51	1	0.5	0.00
$ADRI_c$	13,966	3.87	4	1.1	2,888	4.06	4	0.94	0.00
$CHS_{c,i}$ *(%)*	12,290	8.77	3.15	15.03	2,884	7.42	2.32	12.93	0.00
$MktRet_{c,c'}$ *(%)*	16,261	1.59	0.9	18.04	3,844	3.08	2.2	22.14	0.00
$FX_{c,c'}$ *(%)*	14,177	-6.69	-0.48	64.48	2,979	-9.29	-1.26	50.88	0.04
Forward	17,106	0.47	0	0.5	4,158	0.51	1	0.5	0.00
Backward	17,106	0.53	1	0.5	4,158	0.49	0	0.5	0.00
Relative size	3,050	0.02	0	0.07	751	0	0	0.01	0.00
Premium (%)	413	42.4	32.79	34.92	308	28.8	17.3	33.51	0.00

Table 3.4: Multivariate results

This table reports marginal effects of probit regression, where dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1 0.05, and 0.01 levels respectively.

	Main sample			$P_{c,i} \geq 1$		
	(1)	(2)	(3)	(4)	(5)	(6)
$P_{c,i}$	-0.025***	-0.021***	-0.025***	-0.061***	-0.044***	-0.044***
	[-8.14]	[-6.97]	[-6.76]	[-19.61]	[-11.98]	[-9.52]
$P_{c',i'}$			0.002			0.007
			[0.26]			[1.01]
Listed target			-0.260***			-0.226***
			[-28.65]			[-21.10]
Listed acquirer			0.011			0.013*
			[1.64]			[1.77]
$CHS_{c,i}$			0.000			0.000
			[1.64]			[0.50]
$ADRI_c$			-0.018***			-0.012***
			[-4.66]			[-2.62]
$MktRet_{c,c'}$			-0.069***			-0.042
			[-2.89]			[-1.49]
$FX_{c,c'}$			-0.016**			-0.011
			[-2.36]			[-1.43]
N	21,264	21,264	11,268	11,941	11,941	7,432
Pseudo R^2	0.004	0.064	0.181	0.036	0.104	0.216
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	Y	Y	Y	Y	Y	Y

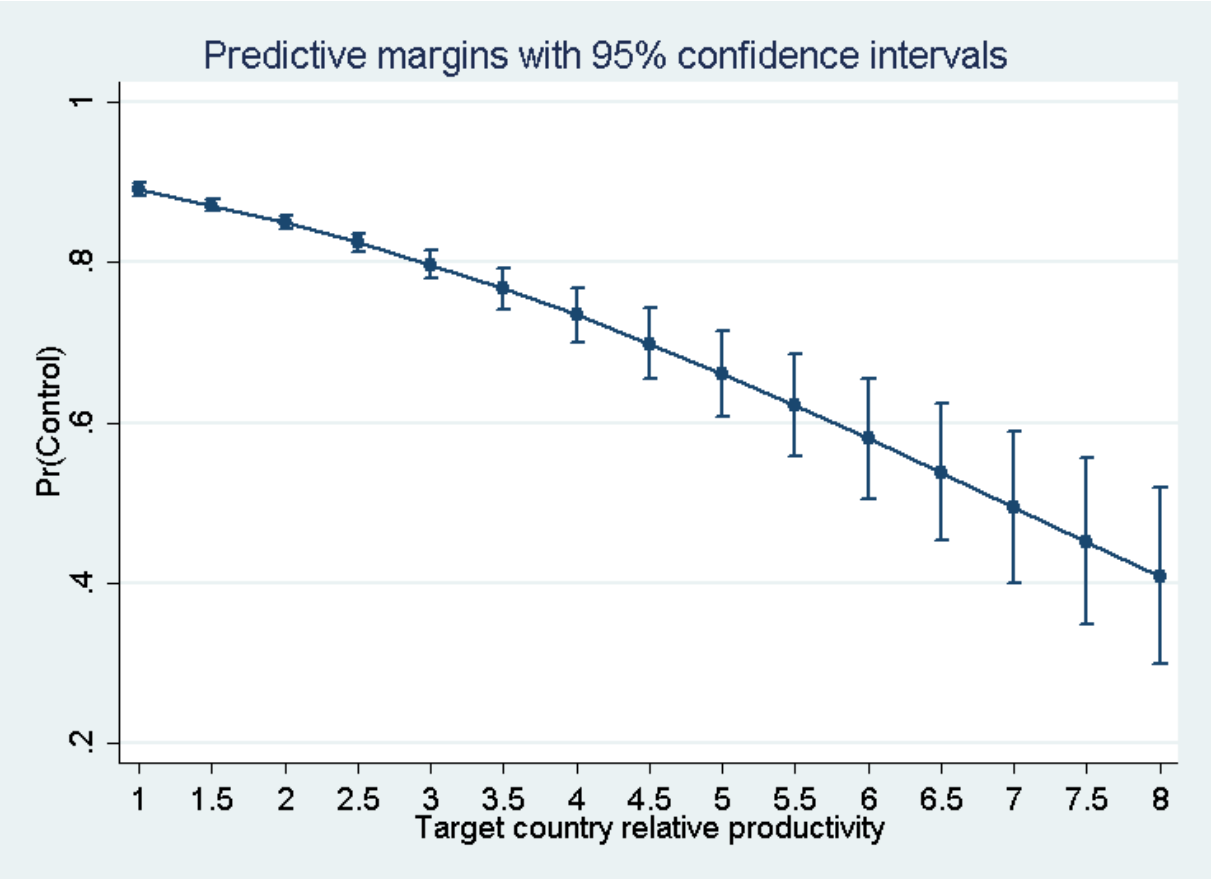

Figure 3.1:Predictive marginal effect of target country productivity on the likelihood of foreign corporate control

This figure plots the marginal effect of target country relative productivity on the likelihood of controlling acquisitions by foreign acquirers, estimated based on probit regression. The sample size is composed by cross-border vertical acquisitions from 2004 to 2010. In the probit regression, the dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling stake. The blue dots plot the evolution of mean marginal effect of target country-level relative productivity if it increases from one to eight with 0.5 unit per time. The vertical lines on each dot indicates the 95 percent confidence interval.

Table 3.5: Results by subsamples of forward and backward acquisitions

This table reports marginal effects of probit regression for forward and backward acquisitions separately. The dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Vertical relatedness* is provided by Fan and Lang (2000) based on BEA 1992 input-output table and is the fraction the input industry contributes in added-value to the output industry. *Forward* (*backward*) indicates the direction of selling to (buying from) between two sectors. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1, 0.05, and 0.01 levels respectively.

	Forward acquisition			Backward acquisition		
	Main sample	Main sample		Main sample	Main sample	
	(1)	(2)	$P_{c,i} \geq 1$ (3)	(3)	(4)	$P_{c,i} \geq 1$ (5)
$P_{c,i}$	-0.032***	-0.026***	-0.055***	-0.024***	-0.018***	-0.046***
	[-6.71]	[-5.30]	[-10.11]	[-4.48]	[-2.96]	[-8.25]
$P_{c',i'}$	0.029***	0.023**	0.029***	-0.015*	-0.015*	-0.006
	[3.08]	[2.56]	[2.70]	[-1.92]	[-1.82]	[-0.62]
Forward relatedness	-0.106	0.126	-0.038			
	[-1.45]	[1.47]	[-0.46]			
Backward relatedness				-0.314***	-0.260***	-0.269***
				[-5.27]	[-3.26]	[-3.87]
Listed target	-0.301***	-0.271***	-0.265***	-0.262***	-0.244***	-0.227***
	[-22.61]	[-20.12]	[-16.68]	[-21.49]	[-19.46]	[-15.93]
Listed acquirer	0.006	0.001	0.010	0.023***	0.021**	0.025**
	[0.64]	[0.07]	[0.84]	[2.68]	[2.33]	[2.54]
$CHS_{c,i}$	0.000	0.000	-0.000	0.000	0.000	-0.000
	[0.67]	[0.44]	[-0.20]	[0.69]	[1.20]	[-0.48]
$ADRI_c$	-0.027***	-0.025***	-0.025***	-0.017***	-0.015***	-0.001
	[-4.44]	[-4.17]	[-3.46]	[-3.22]	[-2.90]	[-0.24]
$MktRet_{c,c'}$	-0.098***	-0.081**	-0.076*	-0.073**	-0.068**	-0.035
	[-2.68]	[-2.30]	[-1.76]	[-2.21]	[-2.09]	[-0.92]
$FX_{c,c'}$	-0.028***	-0.022**	-0.028**	-0.019**	-0.016*	-0.013
	[-2.78]	[-2.15]	[-2.26]	[-2.19]	[-1.76]	[-1.21]
N	5,341	5,326	3,475	5,940	5,843	3,981
Pseudo R^2	0.172	0.220	0.225	0.129	0.175	0.146
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	N	Y	N	N	Y	N

Table 3.6: Robustness tests

This table reports marginal effects of probit regression for subsamples. *Dual-acquirer* indicates the subsample of acquisitions where acquirers have conducted both controlling and non-controlling acquisitions between 2004 and 2010. *Non-serial acquisitions* indicated acquisitions in which the same targets only received one investment from the same acquirer between 2004 and 2010. *Non-strategic sectors* indicate acquisitions take place in GTAP sectors other than coal, oil and gas, minerals, petroleum and coal products, mineral products, electricity, gas manufacture and distribution, water, air transportation, communication (including telecommunication), financial services, public admin, public defense, education, and health. The dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1, 0.05, and 0.01 levels respectively

	Dual-acquirer		Non-serial acquisitions		Non-strategic sectors	
	Main sample	$P_{c,i} \geq 1$	Main sample	$P_{c,i} \geq 1$	Main sample	$P_{c,i} \geq 1$
	(1)	(2)	(3)	(4)	(5)	(6)
$P_{c,i}$	-0.045***	-0.063***	-0.021***	-0.039***	0.006	-0.029***
	[-4.10]	[-4.09]	[-5.85]	[-8.71]	[0.91]	[-3.80]
$P_{c',i'}$	0.023	0.029	-0.003	-0.000	0.002	0.001
	[1.15]	[1.06]	[-0.56]	[-0.03]	[0.22]	[0.09]
Listed target	-0.299***	-0.290***	-0.235***	-0.200***	-0.215***	-0.176***
	[-11.12]	[-8.60]	[-25.95]	[-18.95]	[-22.95]	[-16.15]
Listed acquirer	0.030	0.020	0.013**	0.017**	0.010	0.012
	[1.15]	[0.61]	[1.98]	[2.35]	[1.53]	[1.61]
$CHS_{c,i}$	0.001	0.000	0.000*	0.000	0.000**	0.000*
	[0.83]	[0.06]	[1.94]	[0.50]	[2.01]	[1.75]
$ADRI_c$	-0.006	-0.003	-0.017***	-0.012***	-0.020***	-0.011***
	[-0.47]	[-0.20]	[-4.55]	[-2.60]	[-5.05]	[-2.64]
$MktRet_{c,c'}$	-0.246***	-0.200**	-0.061***	-0.035	-0.054**	-0.026
	[-3.39]	[-2.07]	[-2.62]	[-1.31]	[-2.12]	[-0.90]
$FX_{c,c'}$	-0.020	-0.038	-0.014**	-0.010	-0.018***	-0.015*
	[-0.87]	[-1.26]	[-2.15]	[-1.29]	[-2.74]	[-1.96]
N	2,176	1,401	10,791	7,118	8,361	5,979
Pseudo R^2	0.123	0.148	0.172	0.207	0.146	0.125
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	Y	Y	Y	Y	Y	Y

Appendix 3.1:Global Trade Analysis Project (GTAP2004) country and sector lists

Country code	Description	Country code	Description
AUS	Australia	PHL	Philippines
BRA	Brazil	ROA	Rest of Americas
CAN	Canada	ROH	Iceland,Liechtenstein,Norway,Switzerland
CHT	China	ROW	Rest of the world
E12	EU-12(May.2004 - Jun.2013)	RUS	Russia
E15	EU-15(Nov.1993 - Apr.2004)	SGP	Singapore
HKG	Hong Kong	SSA	Rest of South Asia
IDN	Indonesia	THA	Thailand
IND	India	TWN	Taiwan
JPN	Japan	USA	United States
KOR	South Korea	VNM	Vietnam
MET	Mexico	XEA	Rest of East Asia
MYS	Malaysia	ZAF	South Africa

Sector code	Description	Sector code	Description
1	Crop Production	22	Motor Vehicles and Parts
2	Animal Husbandry	23	Transportation Equipment
3	Forestry	24	Electronic Equipment
4	Fishing	25	Machinery and Equipment
5	Coal	26	Manufactures
6	Oil and Gas	27	Electricity
7	Minerals	28	Gas Manufacture and Distribution
8	Meat and Dairy	29	Water
9	Food Products	30	Construction
10	Beverages and Tobacco	31	Trade
11	Textiles	32	OtherTransportation
12	Wearing Apparel	33	Water Transportation
13	Leather Products	34	Air Transportation
14	Wood Products	35	Communication
15	Paper Products and Publishing	36	Financial Services
16	Petroleum and Coal Products	37	Insurance
17	Chemical, Rubber and Plastic Products	38	Business Services
18	Mineral Products	39	Recreational and Other Services
19	Ferrous Metals	40	Public Admin and Defense, Education and Health
20	Non-Ferrous Metals	41	Dwellings
21	Finished Metal Products		

Appendix 3.2: Baseline results – without acquisitions in the same GTAP sector

This table reports marginal effects of probit regression, where dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1 0.05, and 0.01 levels respectively.

	Main sample			$P_{c,i} \geq 1$		
	(1)	(2)	(3)	(4)	(5)	(6)
$P_{c,i}$	-0.038***	-0.023***	-0.026***	-0.070***	-0.050***	-0.050***
	[-11.47]	[-6.62]	[-5.71]	[-19.87]	[-11.63]	[-8.36]
$P_{c',i'}$			-0.008			0.005
			[-1.05]			[0.52]
Listed target			-0.284***			-0.239***
			[-24.78]			[-17.53]
Listed acquirer			0.005			0.006
			[0.59]			[0.65]
$CHS_{c,i}$			0.000			-0.000
			[0.62]			[-0.18]
$ADRI_c$			-0.020***			-0.015**
			[-4.09]			[-2.53]
$MktRet_{c,c'}$			-0.070**			-0.041
			[-2.31]			[-1.13]
$FX_{c,c'}$			-0.025***			-0.021**
			[-2.98]			[-2.10]
N	14244	14236	7702	8246	8224	5126
Pseudo R^2	0.004	0.064	0.181	0.036	0.104	0.216
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	Y	Y	Y	Y	Y	Y

Appendix 3.3: Results by subsamples of forward and backward acquisitions – without acquisitions in the same GTAP sector

This table reports marginal effects of probit regression for forward and backward acquisitions separately. The dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Vertical relatedness* is provided by Fan and Lang (2000) based on BEA 1992 input-output table and is the fraction the input industry contributes in added-value to the output industry. *Forward* (*backward*) indicates the direction of selling to (buying from) between two sectors. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1, 0.05, and 0.01 levels respectively.

	Forward acquisition			Backward acquisition		
	Main sample	Main sample		Main sample	Main sample	
	(1)	(2)	$P_{c,i} \geq 1$ (3)	(3)	(4)	$P_{c,i} \geq 1$ (5)
$P_{c,i}$	-0.043***	-0.033***	-0.069***	-0.026***	-0.024***	-0.048***
	[-7.18]	[-5.07]	[-9.42]	[-4.21]	[-3.11]	[-7.44]
$P_{c',i'}$	0.020	0.029**	0.021	-0.028***	-0.029***	-0.012
	[1.62]	[2.35]	[1.36]	[-3.17]	[-3.17]	[-1.12]
Forward relatedness	-0.176	-0.087	-0.255			
	[-1.31]	[-0.41]	[-1.17]			
Backward relatedness				-0.357***	-0.295**	-0.265**
				[-4.22]	[-2.09]	[-2.23]
Listed target	-0.327***	-0.314***	-0.281***	-0.263***	-0.249***	-0.214***
	[-18.55]	[-17.29]	[-13.07]	[-17.88]	[-16.76]	[-12.61]
Listed acquirer	0.005	-0.004	0.006	0.012	0.015	0.014
	[0.38]	[-0.31]	[0.37]	[1.21]	[1.48]	[1.26]
$CHS_{c,i}$	0.000	0.000	-0.000	-0.000	0.000	-0.000
	[0.68]	[0.13]	[-0.25]	[-0.65]	[0.09]	[-0.90]
$ADRI_c$	-0.035***	-0.033***	-0.038***	-0.016***	-0.015**	-0.004
	[-4.24]	[-4.01]	[-3.72]	[-2.75]	[-2.40]	[-0.61]
$MktRet_{c,c'}$	-0.147***	-0.106**	-0.068	-0.048	-0.043	-0.047
	[-2.97]	[-2.23]	[-1.12]	[-1.23]	[-1.14]	[-1.05]
$FX_{c,c'}$	-0.047***	-0.039***	-0.040**	-0.025**	-0.017*	-0.018
	[-3.50]	[-2.92]	[-2.32]	[-2.41]	[-1.68]	[-1.48]
N	3405	3389	2166	4312	4252	2986
Pseudo R^2	0.186	0.237	0.241	0.135	0.183	0.150
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	N	Y	N	N	Y	N

Appendix 3.4: Robustness tests – without acquisitions in the same GTAP sector

This table reports marginal effects of probit regression for subsamples. *Dual-acquirer* indicates the subsample of acquisitions where acquirers have conducted both controlling and non-controlling acquisitions between 2004 and 2010. *Non-serial acquisitions* indicated acquisitions in which the same targets only received one investment from the same acquirer between 2004 and 2010. *Non-strategic sectors* indicate acquisitions take place in GTAP sectors other than coal, oil and gas, minerals, petroleum and coal products, mineral products, electricity, gas manufacture and distribution, water, air transportation, communication (including telecommunication), financial services, public admin, public defense, education, and health. The dependent variable equals one if the acquisition is for a controlling stake, and zero if the acquisition is for a non-controlling equity stake. The sample is vertical cross-border acquisitions between 2004 and 2010. $P_{c,i}$ ($P_{c,i}$) is target (acquirer) country-level relative productivity. *Listed target* (*Listed acquirer*) are dummies equal to one if the target (acquirer) is listed in stock exchange, and zero otherwise. $ADRI_c$ is the Antidirector Rights Index. $CHS_{c,i}$ *(%)* is closely held shares in a firm aggregated at country-industry level. $MktRet_{c,c'}$ is the target-acquirer country pair differences in buy-and-hold inflation-adjusted stock market return from 275 trading days to 25 trading days before announcement. $FX_{c,c'}$ is the difference of annual inflation-adjusted bilateral U.S. dollar exchange rate return of the target country and acquirer country from 275 trading days to 25 trading days. *, **, and *** indicate coefficients significantly different from zero at the 0.1, 0.05, and 0.01 levels respectively

	Dual-acquirer		Non-serial acquisitions		Non-strategic sectors	
	Main sample	$P_{c,i} \geq 1$	Main sample	$P_{c,i} \geq 1$	Main sample	$P_{c,i} \geq 1$
	(1)	(2)	(3)	(4)	(5)	(6)
$P_{c,i}$	-0.054***	-0.071***	-0.023***	-0.044***	0.003	-0.042***
	[-3.75]	[-3.54]	[-5.00]	[-7.70]	[0.35]	[-4.24]
$P_{c',i'}$	0.006	0.021	-0.010	-0.003	-0.003	-0.005
	[0.22]	[0.58]	[-1.44]	[-0.31]	[-0.40]	[-0.49]
Listed target	-0.285***	-0.275***	-0.263***	-0.215***	-0.250***	-0.199***
	[-7.71]	[-6.01]	[-22.70]	[-16.03]	[-20.90]	[-14.38]
Listed acquirer	-0.012	-0.012	0.008	0.011	0.011	0.013
	[-0.32]	[-0.25]	[1.03]	[1.19]	[1.35]	[1.42]
$CHS_{c,i}$	-0.000	-0.001	0.000	-0.000	0.000	0.000
	[-0.16]	[-0.58]	[1.19]	[-0.07]	[1.62]	[1.62]
$ADRI_c$	-0.014	-0.032	-0.020***	-0.016***	-0.023***	-0.015***
	[-0.77]	[-1.40]	[-4.31]	[-2.82]	[-4.66]	[-2.76]
$MktRet_{c,c'}$	-0.309***	-0.235*	-0.073**	-0.044	-0.063**	-0.032
	[-3.03]	[-1.77]	[-2.47]	[-1.27]	[-2.02]	[-0.91]
$FX_{c,c'}$	-0.023	-0.052	-0.025***	-0.023**	-0.025***	-0.025***
	[-0.69]	[-1.25]	[-3.15]	[-2.37]	[-3.01]	[-2.61]
N	1248	829	7387	4915	5837	4145
Pseudo R^2	0.151	0.170	0.184	0.224	0.156	0.133
Year dummies	Y	Y	Y	Y	Y	Y
Sector-pair dummies	Y	Y	Y	Y	Y	Y