Essays in Empirical Corporate Finance

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Today Now! Host Jim Haggerty: *"I'm sure that you—"*
Dr. Franklin Kearns: *"—You know, don't apologize. I'm the one who wasted my life on this."*
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Although it may sound strange now, there were many points at which I felt I would not be able to finish this PhD degree. I am grateful to everyone above for making it possible for me. Luckily, finance is more fun (and less lonely) than studying anteaters.

Markus Lithell

Contents

SUMMARY

This dissertation consists of three essays in empirical corporate finance.

The first, titled "Do Acquirers Pay Less for Unlisted Targets? Evidence from OTC Markets" introduces a novel sample of mergers & acquisitions (M&A) to provide new evidence on an old puzzle. While it is widely known that bidder announcement returns are higher in M&A deals with unlisted targets (i.e., those not traded on a stock exchange) than listed, the source of these gains – either because acquirers pay less or because deal value creation is greater – remains elusive due to data limitations. First, most unlisted target deals, such as those involving private and subsidiary targets, do not allow the econometrician to observe the target's standalone stock price prior to the acquisition. Second, the targets of these takeovers often do not publicly disclose information about their financials or performance.

To circumvent these limitations, I introduce a sample of deals to the M&A literature with a new unlisted target type: firms with equity traded over the counter (OTC). This sample allows me to use stock prices to directly measure offer premiums and expected synergy gains in unlisted target deals for the first time. I show that (1) contrary to the conventional wisdom, premiums are higher – not lower – for OTC targets, (2) these high premiums originate from shared synergy gains rather than bidder overpayment, (3) the synergy gains are consistent with improvements to OTC targets' access to capital, with a larger portion of synergies going to OTC target shareholders due to stronger bargaining, and (4) both premiums *and* synergies are higher for OTC targets that are closer to private firms (low stock liquidity) than listed firms (high stock liquidity).

The second essay, "Merger-driven Listing Dynamics", coauthored with Espen Eckbo, continues the M&A theme but expands the scope to include all transactions with listed acquirers around the world. Both regulators and academics have expressed concern over the declining number of listed firms in the US since 1996, from a peak of 7,325 to 3,633 by the end of 2020. The main concern that has been raised is whether this decline is attributable to a reduction in the net benefit of being listed in the US, i.e., whether US stock exchanges have become a less attractive destination for firms, particularly compared to foreign stock exchanges.

We address this debate by emphasizing the role and significance of M&A in listing count changes, or listing dynamics. Specifically, takeovers have two salient properties: First, they result in the complete transfer of a firm's components (assets, employees, patents, etc.) from one owner to another but are poorly represented by the listing count, and second, they are motivated by expected synergy gains rather than changes in net listing benefits. To expand on the former, consider a merger between two listed firms. While the listing count declines by one, the *de facto* corporate assets present on the stock exchange remain unchanged. Similarly, if a listed firm buys an unlisted one, that target transitions to being publicly owned without increasing the listing count. Thus, if the shrinking number of listed US firms can be largely attributed to merger activity, then the conclusion that US net listing benefits have declined may be premature.

With this in mind, we construct a merger-adjusted listing count that accounts for M&A activity to more accurately track changes in the firms under public ownership. We first show that our merger adjustment eliminates the post-1996 listing decline, meaning that the composition of US stock exchanges has changed far less than suggested by the unadjusted listing count. Next, we document that listing peaks, much as in the US, are in fact the global norm: As much as four-fifths of countries have fewer listed firms than in the past. However, the US peak differs from others because it primarily reflects the merger-driven reshuffling of firms on the exchange, rather than firm net outflows as in foreign countries. This points to a merger-driven US listing *advantage* with regards to attracting and retaining firms relative other countries.

The final essay is titled "Debt and Equity Crowdfunding in the Financial Growth Cycle" and is coauthored with Matteo Pirovano, Davide Sinno, and Trang Vu. Since 2016, Regulation CF of the JOBS Act allows small businesses in the US to offer securities to the investing public via online crowdfunding platforms. We investigate firms' choice between issuing crowdfunded debt and equity and how this decision relates to their stage in the financial growth cycle and access to bank financing. We find that firms that are less profitable, are in an earlier developmental stage, and have stronger ties to the banking system are more likely to issue crowdfunded equity than debt. Successful crowdfunding is associated with increases in firm size, revenue, and profitability for early-stage firms, but not for late-stage firms. Our findings suggest that crowdfunding can alleviate capital constraints and stimulate growth for early-stage startups, but has a negligible impact on established firms that are already profitable.

CHAPTER 1

Do Acquirers Pay Less for Unlisted Targets? Evidence from OTC Markets

Do Acquirers Pay Less for Unlisted Targets? Evidence from OTC Markets*

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Abtract

It is widely known that bidder announcement returns are higher when targets are unlisted (i.e., not traded on a stock exchange) than listed. However, the source of these gains – either because acquirers pay less or because deal value creation is greater – remains elusive due to data limitations. I introduce a set of deals to the M&A literature with a novel unlisted target type: firms with equity traded over the counter (OTC). This sample allows me to directly measure offer premiums and synergies in unlisted target deals for the first time. I show that (1) contrary to the conventional wisdom, premiums are higher – not lower – for OTC targets, (2) these high premiums originate from shared synergy gains rather than bidder overpayment, (3) the synergy gains are consistent with improvements to OTC targets' access to capital, with a larger portion of synergies going to OTC target shareholders due to stronger bargaining, and (4) both premiums *and* synergies are higher for OTC targets that are closer to private firms (low stock liquidity) than listed firms (high stock liquidity).

JEL classification: G30, G32, G34

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1 Introduction

Mergers and acquisitions (M&A) of unlisted targets – firms not traded on a stock exchange – account for two thirds of US takeovers and represent a total deal value of $3.5 trillion, 1980-2020. Despite their prevalence, relatively little is known about these deals due to limited data on the unlisted targets themselves, which rarely file public disclosure or have observable stock prices. Instead, previous studies typically rely on information that can be inferred from the stock price of listed acquirers. Since at least Chang (1998), it is known that these acquirers experience higher cumulative abnormal returns (CAR) when announcing takeovers of unlisted targets than listed targets. However, it remains unclear whether this return differential is attributable to higher synergy gains or better deal terms for the bidder (i.e., paying less). Most recently, Jaffe, Jindra, Pedersen, and Voetmann (2015) test a battery of hypotheses but do not find strong support for either channel, concluding that the return differential remains an unsolved puzzle.

In this paper, I provide new evidence on an important question about the market for corporate control: Do acquirers pay less for unlisted targets than listed targets? I introduce a set of deals to the M&A literature with a novel unlisted target type: firms with equity traded over the counter (OTC). OTC targets present an ideal test case because they have observable stock prices while still diverging from listed targets in terms of stock liquidity, information disclosure, and ownership concentration. Importantly, these characteristics also vary considerably within the OTC target sample. This allows me to run econometric tests evaluating potential economic channels related to offer premiums and synergies, in a way that is difficult or impossible with private target deals. For example, practically all private targets have completely illiquid stock – making it difficult to separate effects related to stock liquidity and private status more broadly. In contrast, OTC target deals allow me to clearly observe whether unlisted-listed premium and synergy differentials are larger for unlisted targets with low versus high liquidity.

Two prior papers address the question of how much acquirers pay for unlisted versus listed targets. Officer (2007) compares deal valuation multiples for unlisted private and subsidiary targets with those of listed targets and estimates that unlisted targets sell at a 15-30% discount relative listed targets. Jaffe, Jindra, Pedersen, and Voetmann (2019) also use multiples as in Officer (2007) but with an updated methodology that corrects for biases related to one-sided sample truncation and Jensen's inequality. Their findings suggest that bidders pay neither less nor more for unlisted targets than listed targets. By introducing OTC target deals, I am able to observe unlisted target stock prices and directly measure offer premiums for the first time in the literature. Doing so is beneficial because stock prices incorporate all public information and investors' expectations about future cash flows when estimating the standalone value of the target firm. In comparison, multiples are limited to relatively simple proxies like EBITDA or book value of equity, with the implicit assumption that the target has the same future growth rate and discount rate as its comparable firms. Perhaps even more importantly, stock prices represent the firm's *de facto* value to shareholders.

I start by testing whether offer premiums are different for OTC targets and listed targets. I run cross-sectional regressions for a sample of 735 OTC and 7,923 listed target deals and control for deal, acquirer, and target characteristics. My results are surprising and contrary to expectations from the previous literature: I estimate that OTC target shareholders receive a statistically and economically significant 26 percentage point (pp) *higher* premium compared to listed target shareholders.

Are OTC target premiums higher due to deal synergies or bidder overpaying? To investigate, I estimate acquirer CAR around deal announcement for the subsample of deals with listed bidders. I also estimate combined market-value-weighted acquirer-target CAR to proxy for expected deal synergies (as in Bradley, Desai, and Kim 1988 and Dessaint, Eckbo, and Golubov 2023). I find strong support for higher deal synergies driving OTC premiums: Bidders experience 1.1pp higher returns when announcing OTC target deals than listed target deals – despite paying higher premiums – and expected combined synergy gains are 1.5pp higher. This result is robust to controlling for both acquirer and target runup, indicating that the difference in expected gains is not driven by deal anticipation.

Both acquirer and target shareholders are better off in OTC target deals than listed target deals, meaning the additional synergies are shared. To measure how synergy gains are allocated, I estimate the fraction captured by target shareholders in negotiations by dividing target dollar CAR by combined dollar CAR. I find that OTC target shareholders receive around a one-fifth (21pp) larger fraction of total synergy gains than do listed target shareholders, suggesting stronger bargaining on the part of unlisted target management and owners.

I proceed by evaluating several possible economic channels to explain the premium and synergy differences between OTC and listed target deals. The first is target stock liquidity. OTC target shares are much less liquid than those of listed targets, with two-thirds trading at liquidity levels below the least liquid listed target. Low stock liquidity can inhibit project financing by raising the cost of equity issuance (Amihud and Mendelson, 1986; Butler, Grullon, and Weston, 2005; Hanselaar, Stulz, and Van Dijk, 2019) and cost of capital (Amihud and Levi, 2023; Bolton and Von Thadden, 1998; Brav, 2009; Eckbo and Norli, 2005). As such, it is feasible that a bidder can enhance OTC target value by alleviating financial frictions.[1]

My findings are consistent with financial synergy gains via the stock liquidity channel. I show a negative and monotonic relationship between OTC target liquidity and offer premium: Premiums are highest for the least liquid OTC targets and lowest for the most liquid. Furthermore, controlling for target liquidity is sufficient to explain most (but not all) of the OTC-specific premium and all of the additional acquirer CAR and estimated synergies. Notably, the fraction of synergies captured by target shareholders is unrelated to liquidity – suggesting that OTC targets have a stronger negotiating position that is independent from liquidity. The results are consistent with higher premiums and synergies for targets that are closer to private firms (illiquid stock) than listed firms (liquid stock).

The second channel I consider is public information disclosure. Roughly half of OTC targets do not file a 10-K filing prior to being acquired. Much like stock liquidity, disclosure is known to improve cost of capital (Bailey, Karolyi, and Salva, 2006; Easley and O'Hara, 2004; Leuz and Verrecchia, 2000), which may in turn influence premiums or synergies. Moreover, non-disclosure may also signal about a target's characteristics. For example, managers could

[1]Relatedly, Almeida, Campello, and Hackbarth (2011) and Erel, Jang, and Weisbach (2015) show that takeovers can relieve financial frictions for cash-constrained targets. Boucly, Sraer, and Thesmar (2011) also find that leveraged buyout targets often issue additional debt to finance investments post-buyout, particularly when these targets are private.

be trying to conceal innovation or strategic activity from competitors (Leuz and Wysocki, 2016) or hide poor performance from the public (Leuz, Triantis, and Wang, 2008). Either case allows for synergy gains by bringing previously undisclosed innovations to market (Gao, Ritter, and Zhu, 2013) or professionalizing target management. I test by running cross-sectional regressions controlling for disclosure using several different proxies, in a similar manner as for stock liquidity. However, I do not find any evidence of disclosure-related synergy gains; None of the proxies are related to OTC-specific premiums or synergies.

Third, I consider the role of concentrated ownership. It is difficult to locate consistent data on OTC target owners, which presents a challenge when testing the implications of ownership directly. However, prior literature and anecdotal evidence indicates that OTC stocks are more closely held than listed firms (Marosi and Massoud, 2007) with little institutional ownership (Ang, Shtauber, and Tetlock, 2013). Theoretical and empirical evidence also provide clear guidance about what to expect with regards to sources of synergy gains related to concentrated ownership. One can reasonably assume that closely-held target firms will be better-run than firms with dispersed ownership owing to more management monitoring, long-run growth orientation, and risk-taking (Aghion, Van Reenen, and Zingales, 2013; Edmans, 2009). Thus, there are unlikely to be OTC-specific synergy gains from ownership-related underperformance. On the other hand, Chang (1998) suggests that listed acquirers may benefit from improved governance by integrating a private target blockholder into their ownership structure. However, this only applies when the method of payment is stock, not cash. I test this hypothesis using regression analysis, but do not find any evidence of higher synergies in OTC target deals when stock payment is used. As such, there is also little to suggest that improvement in acquirer governance is a source of OTC-specific synergies.

While concentrated ownership of OTC targets is unlikely to yield higher synergies, it is expected to give targets more bargaining power when dividing up synergy gains (Ang and Kohers, 2001; Ghosh and Ruland, 1998). Concentrated owners are less willing to give up control (Amihud, Lev, and Travlos, 1990; Stulz, 1988), may be more bullish about the firm's future prospects, and may receive private benefits from ownership in the form of sentimental value for founders or family. My evidence is consistent with the interpretation that concentrated ownership strengthens OTC target bargaining but is not a source of synergy gains. As shown above, I find that accounting for stock liquidity is sufficient to explain all of the OTC-specific acquirer gains and synergies, part of the target offer premiums, and none of the distribution of synergies. In other words, while synergies are associated with stock illiquidity, how those synergies are shared between acquirer and target is not – in line with expectations about concentrated ownership based on prior theory and empirical evidence.

Fourth, I consider whether underperformance could be a source of OTC-specific synergy gains. Are OTC firms poorly run compared to listed firms? I find this interpretation unlikely. For one, my evidence thus far (no association between disclosure and synergies, expectations about concentrated ownership) is inconsistent with worse management of OTC targets than listed targets. I also observe that OTC targets have higher average (median) monthly returns in the ten months prior to measuring standalone value (i.e., the start of the runup period) than listed targets: 4.7% (2.3%) versus 1.2% (1.2%). Moreover, OTC targets that were previously listed (so-called "fallen angels"), which account for one-third of my sample and were in almost all cases involuntarily delisted due to poor performance – and could thus be expected to be worse-run than targets that were never listed – have lower average (median) synergies than never-listed targets at 1.7% (0.1%) versus 3.1% (2.7%), respectively.

Finally, I consider whether my results could be driven by OTC target mispricing. Unlike listed markets, most trading of OTC equities is conducted by (potentially uninformed) retail investors (White, 2016). If OTC targets are undervalued when standalone value is estimated, offer premiums will appear larger than they should. I argue that, to the extent that there is any mispricing in my sample, it works *against* – not in favor of – my results. Due to brokerage restrictions, search costs, and limited supply, short selling of OTC equities is difficult, expensive, and rare (Ang, Shtauber, and Tetlock, 2013; Eraker and Ready, 2015). It is known since at least Miller (1977) that in scenarios where investors hold heterogeneous beliefs (as with OTC retail traders) and there are constraints to short-selling, prices will be inflated. Indeed, Ang, Shtauber, and Tetlock (2013) find that the OTC market return is negative (-0.8% per month), "implying widespread overpricing of OTC stocks" (p. 2987). Moreover, as mentioned above, OTC targets have relatively high monthly pre-runup returns, making it unlikely that they are underpriced at the time that standalone value is measured. All in all, there is little to suggest that this paper's results are biased by mispricing.[2]

Related literature. This paper contributes to several strands of literature. Foremost is the M&A literature, particularly studies on takeovers involving unlisted targets. The two papers closest to this one are Officer (2007) and Jaffe, Jindra, Pedersen, and Voetmann (2019). They estimate acquisition discounts for unlisted versus listed targets using deal valuation multiples and find significant unlisted target discounts and no significant discounts, respectively. My findings are surprising because they differ from the expectations set by these previous papers. This differential is primarily because my study measures offer premiums directly (and incorporates stock price information about the target's standalone value) rather than relying on multiples (which are noisy and contain limited information).

I also contribute new evidence on the source of bidder gains in unlisted target deals. Multiple studies, as early as Chang (1998), document that bidders experience higher announcement CAR when acquiring unlisted firms than when acquiring listed firms. However, limited target data makes this return differential difficult to explain. Chang (1998) concludes that acquirers benefit from improved governance by adding unlisted target blockholders when paying with stock. Fuller, Netter, and Stegemoller (2002), Officer (2007), and Cooney, Moeller, and Stegemoller (2009) find evidence that the return differential is related to better deal terms for the buyer. Faccio, McConnell, and Stolin (2006) and Jaffe, Jindra, Pedersen, and Voetmann (2015) revisit and test these previous hypotheses as well as new ones but do not find evidence in support of any particular channel. I show that for OTC deals, the return differential is consistent with synergy gains rather than better deal terms. Moreover, this relationship becomes stronger the closer the OTC target is to private status than listed status in terms of stock liquidity, with both higher premiums and synergies for the former.

More broadly, my findings add to the extensive literature on M&A deal offer premiums. Previous studies examine the relationship between premiums and deal initiation (Masulis and

[2] A similar intuition holds for any sort of market price manipulation. Since short-selling is expensive, manipulators are only incentivized to inflate prices, such as in a "pump-and-dump" scheme. Moreover, since my sample consists of *bona fide* deals where the acquiring firm launches a takeover bid after accessing the target's data room, it is unlikely that they would be biased by market manipulation schemes (or still be willing to extend an offer should they observe such a scheme).

Simsir, 2018), managerial hubris (Aktas, De Bodt, Bollaert, and Roll, 2016; Roll, 1986), rival bidders (Aktas, De Bodt, and Roll, 2010), size (Alexandridis, Fuller, Terhaar, and Travlos, 2013; Moeller, Schlingemann, and Stulz, 2004), target stock price runup (Betton, Eckbo, Thompson, and Thorburn, 2014; Eaton, Liu, and Officer, 2021; Schwert, 1996), termination fees (Officer, 2003), toeholds (Betton, Eckbo, and Thorburn, 2009), as well as many others. I contribute by showing that premiums are also related to target listing status: OTC target shareholders receive higher premiums than owners of listed firms, consistent with both greater value creation and stronger bargaining.

Beyond the M&A literature, I also contribute to the body of papers on OTC-traded firms. Earlier papers on OTC equities focused on asset prices (Ang, Shtauber, and Tetlock, 2013; Bollen and Christie, 2009; Bushee and Leuz, 2005; Eraker and Ready, 2015). More recently, several papers look at OTC firms in the corporate finance context. Brüggemann, Kaul, Leuz, and Werner (2018) document institutional details of the OTC market and examine the trade-off between regulation and market quality (crash risk and liquidity). Cole, Floros, and Ivanov (2019) show that initial public offering (IPO) underpricing is lower for firms that trade OTC before listing on a stock exchange than firms that list directly from private ownership.[3] Cole, Liang, and Zhang (2020) use OTC firms to investigate the relationship between debt financing and the financial growth cycle proposed by Berger and Udell (1998). Most recently, Jiang, Wang, and Yang (2022) measure returns for firms that trade OTC or on stock exchanges after bankruptcy reorganization. I add to this growing field with the first evidence on takeovers involving OTC firms.

2 Data and empirical methods

2.1 Sample Construction

OTC target deals. I construct my main sample of M&A OTC target deals from Refinitiv SDC Platinum and the FactSet Mergerstat/BVR Control Premium Study (Mergerstat henceforth).

[3] See also Eckbo and Lithell (2023), who document that uplists from OTC markets account for as much as 28% of all new US stock exchange listings during 1980-2020.

I identify all deals announced between 1985-2020 where the target firm exchange is OTC or Pink Sheets. I keep control bids, defined as when the buyer holds less than 50% of target shares before the deal is announced and seeks to own at least 50%. The deal form must be either "merger" ("M") or "acquisition of majority interest" ("AM"). Deal value must be known and at least $1 million. I exclude deals in which the target is a utilities firm (SIC 49) or a REIT, trust, or investment unit (SIC 6722, 6726, 6798, or 6799).[4] I limit the sample to initial bids, in which target has not been the target in any other deal in the last 18 months. After applying these filters, I have a sample 2,966 deals, of which 544 are recorded in both SDC and Mergerstat, 516 are found in Mergerstat only, and 1,906 are in SDC only.

Next, I filter out any deals where the target firm was listed at any point in the 12 months prior to the deal announcement. I do so by linking targets to CRSP after keeping CRSP observations with US-domiciled common stock (share code 10 and 11) on NYSE, Amex, or NASDAQ (exchange code 1-3 or 31-33) that have an active trading status, non-missing price, and positive trading volume. I set a 12-month minimum to ensure that the target firm is not listed at any point during the estimation window, which covers 12 to 2 months before the deal announcement date and is further discussed in Section 2.2. Doing so eliminates 1,131 deals. This suggests that a sizeable fraction of the deals labelled as OTC target deals by SDC and MS are actually listed and incorrectly categorized.

I locate stock price data for unlisted OTC target firms from three sources: WRDS OTC Markets, Compustat Daily, and Refinitiv Eikon. WRDS OTC Markets records end-of-day pricing data directly from OTC Markets Group (formerly Pink Sheets). While it is the most detailed and comprehensive of these three data sources, the data only start in September 2011. For deals that are announced in November 2012 or later, I prioritize data from WRDS OTC Markets to allow for a full estimation window. For deals announced before November 2012, I prioritize data from Compustat Daily, then Eikon. Where indicators are available, I

[4] Many M&A studies also exclude deals in which the target is a financial industry firm. In this paper, I opt to keep these deals for two reasons. First, a large fraction (51%) of the OTC targets in my final sample are financial industry firms, mostly banks. Retaining these in the sample is important to avoid losing too much statistical power. Second, it is not clear that deal offer premiums or synergies should materially differ for financial industry targets than targets from other industries. Moreover, I control for target industry fixed effects throughout the analysis. While not tabulated here, my findings also hold when run exclusively on the subsample of non-financial industry target firms.

require observations to be from when a firm has an "active" status designation and where the security is common stock or ordinary shares (there are basically no prices recorded for preferred shares). Following Schwert (1996), I measure acquirer and target standalone value at the start of the runup period 42 trading days (2 months) before the deal announcement. I require non-missing stock prices (either fresh prices or bid-ask midpoints) to be observed 42 trading days (2 months) before the deal announcement and at least one day with trading activity in the event window (-2, +2). With these criteria, I find stock price information for 908 of the remaining 1,835 deals.

Finally, I set a minimum offer price to avoid measurement error in case stock prices are rounded (Ince & Porter 2006). First, I keep deals where the deal offer price per share is known, to allow for estimation of the deal premium, resulting in 830 remaining deals. Next, I require the minimum offer price to be at least $0.10, after which 801 observations remain. I maintain a low minimum price to maximize the number of OTC target deals in my sample. Additionally, many firms trade OTC precisely because they are so-called "penny stocks" (with a share price of less than $5) and are ineligible for listing, making these firms interesting objects of study. For robustness, I rerun my analysis using higher minimum prices ($1, $5, $10, and even $50) in untabulated results and find that my main results hold.[5] My final sample of OTC takeovers consists of 735 deals.

Listed and private target deals. I assemble a sample of listed target deals by selecting all control bids from SDC with the same initial filters as OTC firms except for keeping only targets that trade on NYSE, Amex, and NASDAQ. This gives me a starting sample of 9,553 listed target deals. Next, I link targets to CRSP using the same filters as above. After linking and requiring observations on event day -42 and (-2, +2), I am left with 8,344 deals. Finally, I require deal offer price to be known and at least $0.10 for a final sample of 7,925 listed target deals.

I also select private target deals from SDC and using the same initial filters as above for a starting sample of 13,252. As with OTC targets, I filter out any targets that were listed within

[5] Ang, Shtauber, and Tetlock (2013) also find that setting a minimum stock price of $0.10 gives them similar results as using $1 when estimating OTC return premiums.

1 year prior to deal announcement by linking to CRSP (reducing the sample to 13,191). Since it is not possible to calculate premiums for private targets, I do not filter on deal offer price (which is anyway rarely recorded in SDC for these deals).

Other data sources and cleaning. I download additional firm accounting data from Compustat Annual Fundamentals, using observations from the year before the merger announcement. For information on 10-K and 10-Q filings I use the Loughran-McDonald SEC/EDGAR 10-X Summaries File (Loughran & McDonald 2016), which I link to target firms via CIK and company name. This file contains summary data gathered via textual analysis for all 10-K and 10-Q forms filed with the SEC from 1993-2021, although the number of filings on record prior to 1996 is relatively small since companies were not required to file via electronically EDGAR until that year.

I winsorize all continuous variables at the 5% tails by target type (listed, OTC, or private). I winsorize by type since the sample mean and standard deviation vary significantly by type (as shown next in Section 2.3), which can result in large one-sided winsorization if done on the deal sample as a whole. To filter out any potentially misrecorded returns from the estimation window, I replace one-day returns below -62.3% or above 149.6% with missing values. These thresholds respectively correspond to the 0.001st and 99.999th percentiles of listed target estimation window returns, which applies to 0.1% of the estimation period OTC return observations. My results are not sensitive to the level of winsorization or to filtering out extreme returns.

2.2 Constructing key variables

In this section, I describe how I construct my four outcome variables, as well as eight different control variables to proxy for deal anticipation, target stock liquidity, and target information disclosure.

Dependent variables. I measure deal offer premiums as in Betton, Eckbo, Thompson, and Thorburn (2014), who compare the offer price to the target's standalone value at the start of the runup period 42 trading days (approximately two months) before the deal is announced.

To I calculate acquirer announcement CAR, I use a Carhart four-factor model (Carhart, 1997) and estimate factor loadings using the estimation window (-252, -42), corresponding to the 10 months before the start of the runup period. I cumulate abnormal returns over a five-day window around the announcement date, corresponding to event trading days (-2, +2).

Following Bradley, Desai, and Kim (1988) and Dessaint, Eckbo, and Golubov (2023), I estimate expected deal synergy gains by calculating the estimated dollar value of synergies (combined market-value-weighted acquirer-target CAR) and dividing this by the sum of the acquirer and target's standalone values. This measure can be interpreted as the percent increase in value that the merging firms can achieve together by merging instead of remaining separate.

Finally, I estimate which fraction of dollar synergy gains is allocated to target shareholders. This measure proxies for target management bargaining: The higher the fraction of synergy gains that are captured by target shareholders, the stronger their negotiation outcome. The fraction of synergy gains is also calculated following Bradley, Desai, and Kim (1988).

Independent variables. I construct eight additional explanatory variables for use in my analysis. The first two are acquirer and target runup, which serve as proxies for deal anticipation. In deals with more market anticipation and higher expected value, target runups are expected to be larger (Betton, Eckbo, Thompson, and Thorburn, 2014). The relationship between acquirer runup and expectations is less clear, and more sensitive to the deal terms negotiated by the acquirer. To calculate target and acquirer runup, I calculate factor loadings in the same manner as for announcement CAR above and cumulate abnormal returns through event trading days (-42, -3).

I also construct three proxies for target liquidity, which are measured during the ten-month estimation window defined above to avoid any bias related to deal anticipation. The first is the fraction of trading days with trading activity (positive trade volume), similar to the primary OTC illiquidity measure used by Ang, Shtauber, and Tetlock (2013). As shown next in Section 2.3, the number of days with trading varies considerably among OTC targets; Half trade every other day or less frequently.

The second is an Amihud liquidity measure based on Amihud (2002). This measure captures how sensitive a stock's price is to trading – the price of an illiquid stock will move more in response to small amounts of trading than a liquid stock. I construct my Amihud liquidity measure for each target firm in three steps: (1) per day, divide the absolute value of the return by the dollar trading volume, (2) take the daily average across the estimation period and rescale by 10^6 as in Amihud (2002) to get the Amihud illiquidity measure, (3) add 1 and take the natural logarithm to reduce skewness, and (4) multiply by -1 to convert this illiquidity measure into a liquidity measure to align it with the other liquidity indicators used here.

The third liquidity proxy I use is the average daily dollar trading volume, expressed as a natural logarithm to reduce skewness. OTC stocks often have low free float and little trading activity. As such, the overall dollar amount of trading is a useful tool for gauging how much stock it is possible for investors to transact.[6]

Next, I construct three proxies for how much information the target discloses to the public, based on information from the 10-X Summaries File (Loughran and McDonald, 2016) and measured in the two-year period before the announcement date. The measures are (1) a dummy equal to one if the target filed a 10-K, (2) the log of the total number of filings (10-Ks and 10-Qs), and (3) the log of the total word count in all filings. Since EDGAR's coverage is incomplete prior to 1996, I measure disclosure only for deals announced in 1998 and onward to allow for two full years of data. The disclosure measures are assigned a missing value if the deal is announced prior to 1998.

2.3 Descriptive statistics

This section summarizes the variables used in this analysis. Table 1 summarizes continuous variables for listed and OTC targets. Private target deals are also included for comparison in

[6]Two other well-known liquidity measures are bid-ask spreads and turnover. I am unable to produce the former due to data limitations. However, (Lesmond, 2005) shows that the Amihud measure is closely correlated to bid-ask spreads, making it unnecessary to include both in this analysis. I exclude turnover, defined as the number of shares traded divided by shares outstanding. Turnover can simultaneously proxy for liquidity and difference in investor opinion and is thus considered less accurate (Lesmond, 2005), with many studies ignoring turnover entirely (Goyenko, Holden, and Trzcinka, 2009).

Panel A, but not in Panel B, which contains variables that are either unobservable or not relevant for private target deals. Table 2, which is discussed further below, describes categorical (dummy) variables. Finally, for illustrative purposes, Appendix Table 1 also presents the ten largest OTC target deals alongside additional hand-collected information.

Starting with Table 1, Panel A summarizes deal value, relative deal size, target leverage, and acquirer CAR for listed, OTC, and private target deals. It is noteworthy how closely OTC target deals resemble private target deals in the cross-section, in particular when compared to listed targets. Opening with deal value, both OTC and private target deals are close in size with mean (median) values of \$78m (\$35m) and \$88m (\$29m) respectively.[7] Listed target deals tend to be considerably larger, averaging \$1.35 billion and with a median of \$349 million. The ratio of deal value over acquirer market cap (limited to the subsample of deals with listed US acquirers), is similar for OTC firms (mean/median 0.21/0.12) and private firms (mean/median 0.20/0.08). Listed targets are generally closer in size to their respective buyers (mean/median 0.41/0.22).

Both OTC and private targets tend to have higher leverage than listed targets. I collect target debt ratios from SDC and bound them to be between 0 and 1 if nonmissing. The mean (median) debt ratio is 0.72 (0.9) for OTC targets and 0.73 (0.8) for private targets. In comparison, listed targets only have a mean (median) debt ratio of 0.56 (0.6). This differential is consistent with higher equity issuance costs for unlisted firms, and also suggests that it may be difficult for these firms to issue additional debt prior to being acquired because of their high leverage.

Next, I summarize the four key dependent variables used in this analysis. The first is acquirer deal announcement CAR, which is recorded for the subsample of deals with US listed acquirers. Consistent with the prior literature following Chang (1998), investors react more favorably to deals with private targets than listed targets, with respective CAR of 1.6% (0.5%) versus -1.6% (-1.2%) on average (median). OTC target deals appear to fall in the middle, with mean (median) acquirer CAR of 0.0% (-0.1%).

[7]OTC target deal value is slightly larger than the mean (median) market cap for the OTC population at \$64m (\$21m), as calculated for 2001-2010 by Brüggemann, Kaul, Leuz, and Werner (2018) (and converted to 2020 USD here).

Panel B of Table 2 further summarizes continuous variables for listed and OTC targets but leaves out private targets, for which these variables are either unmeasurable or irrelevant. I start by showing deal offer premiums, which are the main focus of my analysis. Consistent with the prior literature (see e.g., Eckbo, Malenko, and Thorburn 2020), listed target shareholders receive an average unconditional premium of 43% and a median premium of 37%. In comparison, OTC target shareholders receive even higher unconditional premiums: 63% on average and 46% at the median.

Combined bidder-target announcement CAR, the third key outcome variable presented here, proxies for expected synergy gains. OTC target deals yield larger unconditional synergies at 2.7% (2.2%) versus 1.9% (1.2%) for listed target deals on average (median). The fourth key outcome variable documents what fraction of these synergy gains go to target shareholders. In OTC target takeovers, target shareholders are able to negotiate for a larger fraction of the value created in the deal, with a mean (median) share of 55% (41%) versus only 36% (27%) for listed target shareholders.

Sections 3 and 4 are dedicated to examining these four key outcome variables in detail. In particular, I check to see if the unconditional differences observed here still hold after controlling for salient factors such as size and payment type. I also test several economic channels that may account for these differences.

Table 2 Panel B continues with three measures of stock liquidity. All measures indicate that OTC targets generally have much lower stock liquidity than listed targets. Listed targets generally trade every day (mean/median 96/100%), while OTC targets tend to only have trading activity every other day (mean/median 52/46% of trading days). Amihud liquidity indicates that OTC target share prices are more sensitive to trading; More negative values correspond to lower liquidity, while values closer to zero indicate higher liquidity. On average (median), a listed target has $7,283,000 ($941,000) in daily trading while an OTC target only has $34,000 ($8,000). Across these three liquidity measures, the least liquid listed target has higher liquidity than roughly two-thirds of the OTC target sample.

To round out Table 1, I show that roughly one-third of the OTC deals feature a target that was at some point listed (237 of 735 deals). Among those that previously traded on a stock exchange, the mean (median) number of years between the target's delisting date and the takeover announcement date is 5.6 (4.2) years. While not shown here, CRSP delisting codes indicate that 85% of these delistings are due to cause and 15% voluntary. In other words, most OTC target firms that were previously listed were taken off the exchange for failing to uphold listing requirements (e.g., the stock price became too low, target did not file timely reports with the SEC, or firm failed to uphold governance standards or financial performance).

I also present unadjusted stock returns, expressed in monthly terms, for listed and OTC targets during the estimation period. Listed targets experience 1.2.% (1.2%) monthly returns on mean (median) in the 10 months prior to the runup period. Comparatively, OTC targets experience higher returns, with a mean (median) monthly return of 4.7% (2.3%). This suggests that OTC targets generally tend to be performing well prior to acquisition, which is noteworthy since OTC stocks have been shown to provide negative returns to investors on average at -0.04% per month (Ang, Shtauber, and Tetlock, 2013).

Table 2 proceeds by summarizing categorical (dummy) variables. Panel A tabulates deal characteristics. Compared to listed target deals, OTC/private target deals, respectively, are more likely to be completed (88/88% vs 79%) but less likely to be hostile (0.4/0.2% vs 5%), be tender offers (5/0.4% vs 22%), or feature lockup provisions (2/1% vs 11%). These results are consistent with unlisted firms having lower free float and more concentrated ownership; Hostile and tender bids are difficult or even impossible to execute if too few shares are floated for the bidder to acquire a controlling position, and announced deals are more likely to have received approval from target owners and management prior to the deal being made public.

OTC target deals are somewhat more likely to be horizontal mergers than other deals (61% vs 53/54% for listed/private targets). The distribution of payment type (all cash, mixed, or all stock) is roughly similar between OTC and listed target deals. Private deals are more likely to feature a mix of stock and cash or some other type of payment, although this could potentially be due to less precise payment method data.

Panel B of Table 2 shows acquirer characteristics. Acquirers of OTC firms are slightly more likely to be strategic buyers (88%) than in listed target deals (83%) and slightly less than in private target deals (93%). OTC target deals feature a larger fraction of deals with buyers that are financial firms (62%) versus listed/private target deals (40/28%). In terms of buyer public status (listed, OTC, private, subsidiary, or other) and nation (US or foreign), the distribution varies but is overall fairly similar across target types.

Table 2 Panel C summarizes target characteristics. Interestingly, I observe that in 53% (375 of 735) of the OTC target deals, the target files at least one 10-K filing in the two years preceding the deal announcement. In comparison, 96% of listed targets file, while only 5% of private targets do. While not tabulated, the correlation between previous listing status and 10-K filing among OTC target deals is fairly weak – only around 25%. In other words, roughly half OTC targets do not file any 10-K filings before they are acquired, and this decision appears mostly unrelated to prior listing status. I also show that a larger proportion of OTC deals feature targets that are financial firms (51%) than in deals with other target types, at 21% for both listed and private targets (see also Footnote 4).

Finally, for illustrative purposes, Appendix Table 1 presents detailed information on the ten largest OTC target deals in my sample. All ten deals have transaction values above $1 billion and are spread across several industries and years, with the earliest deal in 1992 and most recent in 2018. Half of the targets were previously listed while the other half had never traded on a stock exchange. I manually identify the largest target owners from web searches and newspaper clippings where possible. For deals where I can identify the largest owners, I observe that they tend to own a large fraction of the target shares prior to the acquisition; For example, in the largest deal (Belk Inc at $2.9 billion), 70% of the shares were family-owned before the sale, while five other deals had private equity, hedge fund, and former senior lender ownership ranging from 40% to 90% of shares.

2.4 Empirical methodology

In the remainder of the paper, I use multivariate regression analysis to investigate the relationship between target listing status and four different dependent variables: offer premium, acquirer CAR, deal synergies, and division of synergies. I run a set of cross-sectional deal-level regressions for listed and OTC targets using the following base specification:

$$Y_d = \alpha + \beta_1 OTC_d + \lambda X_{i,t} + \theta Z_d + \mu_t + \nu_j + \epsilon_{d,t} \tag{1}$$

where Y_d is one of the four dependent variables listed above. OTC_d is a dummy taking a value of one if the deal target trades OTC and zero otherwise. The following four terms are vectors: $X_{i,t}$ for acquirer characteristics, Z_d for deal characteristics, μ_t for year fixed effects (FE), and ν_j for industry FE. $\epsilon_{d,t}$ is the error term. Standard errors are clustered by industry, which is measured at the target SIC-2 level.

The acquirer characteristics include a listed acquirer dummy, OTC acquirer dummy, and strategic bidder dummy. For regressions in which the outcome variables is related to acquirer CAR or synergies, the listed and OTC dummies are automatically dropped since the sample is limited to deals with listed acquirers.

Deal characteristics consist of dummies for deal completion, all-stock payment, hybrid stock-cash payment, hostility, tender offer, and lockup provisions. I also include a size control that corresponds to the outcome variable: For offer premiums, I use log deal value, while for other outcome variables I use the ratio of deal size over acquirer standalone value (market capitalization at the start of the runup period) to capture relative deal size alongside dummy if the deal value is above median. Since relative deal size is a ratio with deal value in the numerator, I am unable to use it alongside the deal value control at the same time. Relative deal size is widely recognized as being important for acquirer CARs, since deals involving smaller targets will have a lesser impact on acquirer stock price *ceteris paribus*. As CAR and synergy regressions only involve listed-acquirer deals, I can consistently measure relative deal size using acquirer market capitalization. However, for offer premium regressions, I include all acquirer types, which necessitates the use of a size control variable that can be consistently recorded regardless of acquirer type. Additionally, the relationship between offer premiums and relative size is ex-ante more ambiguous than for acquirer CAR. For this reason, I control for deal size instead of relative deal size when the dependent variable is offer premium.[8]

[8] To validate this decision, in Table 4, I show that replacing deal size with relative deal size (scaled by acquirer market value) has a negligible impact on the other coefficient estimates. While not tabulated, doing the same using a relative size measure scaled by total assets (which is available for a small subset of the unlisted acquirers as well as listed acquirers) yields the same result.

3 Main results

3.1 Estimating OTC target premiums

Do buyers pay less when buying OTC targets than listed targets? In Table 3, I run a set of cross-sectional regressions based on the model specified in Equation 1. The primary independent variable of interest is a dummy indicating that the target is an unlisted OTC firm. I vary the fixed effects by column to check whether the OTC-target coefficient estimate is sensitive to unobserved time-, industry-, and even acquirer-invariant characteristics. Column (1) excludes FE, while the remaining columns include (2) year FE, (3) year and industry FE, (4) year-times-industry FE, (5) year, industry, and acquirer FE, and (6) year-times-industry and acquirer FE.

In all six specifications, the coefficient estimate for the OTC target dummy is highly statistically significant at the 1% level as well as economically significant, with estimated OTC premiums ranging between 20pp and 29pp. Results for my main specification, which uses year and industry FE as in Equation 1, are shown in Column (3). This model estimates that OTC target shareholders receive 26.1pp higher offer premiums than listed target shareholders. For comparison purposes, listed target shareholders receive an unconditional 43% premium on average. As discussed in the introduction, this result is both novel and surprising since it contradicts expectations set by the prior literature, which predicts that buyers pay less (Officer, 2007) or the same (Jaffe, Jindra, Pedersen, and Voetmann, 2019) when buying unlisted targets as listed ones.

In Column (4), I replace year and industry FE with a year-times-industry FE, which captures the relationship between offer premiums and industry-specific merger waves (Mitchell & Mulherin 1996; Harford 2005). The OTC target deal coefficient remains unchanged at 25.9pp, suggesting that the OTC-specific premium is unrelated to merger wave activity. In Columns (4)-(6), the number of sampled OTC target deals shrinks due to more granular fixed effects, down to only 271 OTC target deals in Column (6) from the starting sample of 735 as in Column (3) (and reducing the overall deal count from 8,658 to 4,429). Despite the loss in sample size and more stringent controls, the coefficient of interest remains remarkably stable with an estimated value of 25.8pp in Column (6). In other words, even after accounting for acquirer fixed effects and unobserved year-industry characteristics, the OTC-specific offer premium remains large and significant.

Among the other control variables in my regressions, I estimate that premiums are higher when the acquirer is a strategic buyer and when the bidder is more aggressive (the deal is hostile or a tender offer). Deal completion is also positively related to offer premiums, which can intuitively be explained since target shareholders are more likely to accept a bid with more generous terms. Stock payment is associated with lower premiums, particularly for all-stock bids. In Columns (5)-(6), many of these coefficient estimates become insignificant since they may be consistent over time for many acquirers or due to model overspecification. Interestingly, the premium does not appear related to the acquirer's listing status (whether listed or OTC versus the base case of a private bidder).

Next, I consider whether my main results are significantly impacted by omitted variable bias. The challenge with unlisted target deals, including OTC deals, is that data on firm characteristics are missing or unobservable for many targets (e.g., those without 10-K filings). Additionally, half of OTC target deals involve bidders that are not US listed. To isolate the potential impact of excluded variables from changes in sample size, I run regressions in pairs where the variable I evaluate is non-missing in both, but only included in the second specification. I evaluate four control variables that are not included in my main specification and pay particular attention to whether the OTC target coefficient changes when the control variable is included.

Table 4 presents my findings. In Columns (1)-(2), I test for a deal termination agreement dummy (Officer, 2003); in (3)-(4), a deal relative size variable scaled by acquirer market value; in (5)-(6), the target debt ratio; and in (7)-(8), the target sales growth in the five years prior to the announcement. In each case, the OTC target coefficient estimate in even-numbered columns including the control variable is largely unchanged from the odd-numbered columns without it. This holds even when the added control variable is significant as in Columns (2), (4), and (6). Thus, it appears unlikely that the relationship between target OTC status and high premiums is significantly biased because of some correlation between OTC target deals and unobserved acquirer, deal, or target characteristics. Moreover, it is worth noting again that despite the large variation in the sample size between specifications – from a maximum of 573 OTC target deals (8,491 deals in total) in Columns (1)-(2) to a minimum of only 164 deals (4,936 deals total) in Columns (7)-(8), or only a fifth of the original sample of OTC 735 deals, the OTC target dummy remains consistently significant at the 1% level and relatively stable, ranging from 18.1pp to 23.9pp.

3.2 Are high OTC premiums due to synergies or bidder overpayment?

The surprising result that OTC target shareholders receive higher premiums than listed target shareholder begs the question: Are premiums higher because synergy gains are larger or because the buyer overpays? To test, I use acquirer announcement CAR and expected combined synergies. If high OTC premiums are due to bidder overpayment, we expect acquirer CAR to be lower for OTC target deals than listed target deals. If, on the other hand, the high offer premiums are due to unlisted-target-specific synergies, we expect to see one of two outcomes: Either (1) synergies are higher but acquirers have similar CAR when acquiring OTC targets as when acquiring listed targets or (2) synergies are higher and acquirers simultaneously see more positive CAR. In the former, there are additional synergy gains but target shareholders capture their entire value when negotiating deal terms. In the latter, these synergy gains are instead shared – a "win-win" scenario for both the bidder and target.

In Table 5, I put these hypotheses to the test using the regression model specified in Equation 1 with three different outcome variables: acquirer announcement CAR in Columns (1)-(2), expected synergies in Columns (3)-(4), and the fraction of synergy gains allocated to target shareholders in Columns (5)-(6). The results are inconsistent with bidder overpayment and instead indicate that OTC offer premiums are higher due to OTC-specific synergy gains that are shared by the buyer and target. First, Column (1) shows that acquirer CAR is higher when the deal involves an OTC target instead of a listed target, with the differential estimated to be 1.1pp and statistically significant at the 1% level. Moreover, Column (3) also shows that combined synergies are higher by 1.5pp (also significant at the 1% level). Finally, Column (5) estimates that OTC target shareholders capture around one-fifth more of synergy gains than listed target shareholders (21pp). In other words, Table 5 shows that both acquirers and targets are better off in OTC target deals, despite the buyer paying higher premiums and target shareholders successfully bargaining for a larger fraction of the synergy gains.

In the even-numbered Columns (2), (4), and (6) of Table 5, I add a pair of additional control variables to the model: acquirer runup and target runup. Since announcement CAR is measured using the five-day event window (-2, +2), it is possible that my results could be influenced by differences in deal anticipation between OTC and listed target deals. If the market is better at predicting listed target deals than OTC target deals (for example, due to more public information, analyst attention, or rumors and leaks), a larger fraction of the expected synergy gains may already be factored into the acquirer and target stock price by the time the deal is announced. If the differences between OTC and target deals above are due to differences in deal anticipation, we expect that controlling for runups should have a significant impact on the coefficient estimate of the OTC target deal dummy.

My results indicate that concerns about deal anticipation and measurement error are unfounded. For all three dependent variables, the OTC target coefficient remains identical (acquirer CAR and combined synergies) or barely changes (target fraction of synergies). This holds even when the runup variable itself is significant; Higher target runup is associated with lower measured synergies and a lower fraction of the synergies going to the target − which is consistent with more of the target's gains being anticipated by the market and thus not measured within the (-2, +2) window. Overall, Table 5 shows that despite paying higher offer premiums in OTC target deals, bidders do not overpay but instead pay more because of higher expected synergy gains. In Section 4, I proceed by investigating several economic channels that could be the source of these synergies.

4 Evaluating economic channels

In this section, I consider four channels that could plausibly explain the differences shown above between OTC and listed target deals in terms of offer premiums, acquirer CAR, combined synergy gains, and division of synergies. I start with three channels for each of the main characteristics distinguishing listed from unlisted firms: stock liquidity, public information disclosure, and ownership concentration. The fourth channel I consider is target underperformance.

4.1 Stock liquidity

A key difference between listed and unlisted firms is stock liquidity. While listed firms tend to have a large fraction of their shares freely floated on highly liquid stock exchanges, unlisted firms may have a relatively small fraction of shares floated on less liquid marketplaces (OTC firms) or essentially be completely illiquid (private firms). Stock illiquidity has been shown to increase equity issuance costs (Amihud and Mendelson, 1986; Butler, Grullon, and Weston, 2005; Hanselaar, Stulz, and Van Dijk, 2019) and cost of capital (Amihud and Levi, 2023; Bolton and Von Thadden, 1998; Brav, 2009; Eckbo and Norli, 2005), both of which can inhibit firms from making value-increasing investments. Thus, one explanation for why synergy gains are higher in OTC target deals than listed target deals could be that the former allows the target to take on profitable projects that would otherwise be restricted by financing constraints. Indeed, prior research has also shown that mergers can ease financial frictions for target firms with low cash reserves (Almeida, Campello, and Hackbarth, 2011; Erel, Jang, and Weisbach, 2015), although to the best of my knowledge a similar effect has not yet been documented for targets with low stock liquidity.

If synergy gains and correspondingly, higher offer premiums, are related to stock liquidity, we should expect to see larger synergy gains and premiums for less liquid OTC targets. One advantage of my setting is the considerable variation among OTC targets in stock liquidity prior to being acquired. For example, on average, the bottom quartile of OTC targets in terms of stock liquidity has trading activity on 16% of days and daily trading volume of $1,200, while the top quartile trades 93% of days with $115,100 in daily trading. Two-thirds of the OTC targets are less liquid than the least liquid listed target. As such, OTC targets present an ideal test case to isolate variation in stock liquidity and link this to deal outcomes.

I first examine the relationship between offer premiums and stock liquidity, starting with within-OTC variation in liquidity. In Table 6 Columns (1)-(3), I run the offer premium regression defined in Equation 1 but split the OTC target dummy into four separate dummies corresponding to OTC stock liquidity quartiles. In Column (1), the liquidity measure used is the fraction of days with trading, while (2) and (3) use Amihud liquidity and dollar volume respectively. Regardless of which liquidity measure is used, the results show a monotonic and negative relationship between OTC target liquidity and premiums. The first-quartile OTC target deals with the lowest liquidity have the highest premiums – between 41.7pp and 55.6pp more relative listed target deals for the first quartile, depending on the liquidity measure. In contrast, the fourth-quartile highest-liquidity OTC target deals have premiums that are closer to listed target premiums (Column 1 estimates 15.5pp larger premiums) or even statistically indifferent from them (as in Columns 2-3). For all three liquidity measures, Wald F-tests confirm that the coefficient estimates for first and fourth quartile OTC target deals are statistically different from each other.

In Table 6 Columns (4)-(6), I consider the relationship between offer premiums and liquidity more broadly using the same three liquidity proxies as in (1)-(3). Do the high OTC-specific premiums observed in Tables 3 and 4 persist after controlling for variation in liquidity between and within OTC and listed target deals? I run cross-sectional offer premium regressions as per Equation 1 and add an additional control variable for target liquidity in all deals (OTC and listed). The results show that liquidity is negatively associated with offer premiums at the 1% significance level. Moreover, accounting for liquidity reduces the magnitude of the OTC target dummy coefficient from 26.1pp to between 6.6pp and 12.7pp, or a reduction of around 50-75%. This suggests that some – but not all – of the high OTC premiums may be related to differences in liquidity between OTC and listed targets, which we saw was the case within OTC target deals in Columns (1)-(3).

Next, Table 7 considers the relationship between target stock liquidity and acquirer CAR in Columns (1)-(3), expected synergy gains in Columns (4)-(6), and the division of synergies in Columns (7)-(9). The liquidity measures are the same as in Table 6. Since acquirer CAR is required to be known, the sample is limited to the subset of deals with listed US acquirers.

Table 7 shows that acquirer CAR and synergies are strongly associated with target stock liquidity. In fact, controlling for liquidity causes the OTC target dummy coefficient to become insignificant in all specifications, Columns (1)-(6). In other words, the results show that the OTC-specific synergy gains – including those captured by the acquirer – are related to target stock illiquidity. These findings are consistent with the hypothesis that M&A activity can increase the value of an unlisted target by lowering its barriers to issuing equity as well as its hurdle rate for new projects.

While not tabulated here, additional evidence supports this conjecture. OTC targets tend to have higher leverage than listed targets, and this relationship is correlated with stock liquidity. Specifically, the bottom quartile of OTC targets by liquidity has an average debt ratio of 0.76, while the top quartile (most liquid) OTC targets have a mean of 0.68. By comparison, the bottom liquidity quartile of listed targets has a mean debt ratio of 0.62, while the top quartile measures 0.53. In other words, illiquid targets appear to be more reliant on debt financing than liquid targets, which is consistent with equity issuance costs as well as limits to taking on more debt. This appears to be most pronounced for OTC targets.

Interestingly, Columns (7)-(9) of Table 7 shows that target stock liquidity is unrelated to the division of synergy gains, with the OTC target dummy coefficient remaining large and statistically significant. While the high acquirer CAR and synergy gains in OTC target deals appear consistent with reductions in financial frictions due to stock illiquidity, some other explanation is needed for why target management is able to secure a larger fraction of synergies for shareholders in OTC target deal negotiations than listed target deals. This result is also consistent with the findings from Table 6 Columns (4)-(6), which showed that variation in stock liquidity was unable to account for all of the high OTC target premiums, leaving some 25-50% of the high OTC premiums unexplained. In Section 4.3, I argue that the division of synergies and the unexplained premium component could be consistent with concentrated ownership.

4.2 Information disclosure

Another important difference between listed and unlisted firms is how much information they disclose to the public. Listed firms are required by the SEC to regularly disclose information including financial statements in 10-K and 10-Q filings. In contrast, very few unlisted firms are required to do so. Indeed, as shown in Section 2.3, the target files a 10-K filing in the two years prior to the takeover announcement in 96% of listed target deals, while the same applies to only 5% of private target deals. OTC target deals fall somewhere in the middle, with 53% of targets filing a 10-K form prior to being acquired.

As with target stock liquidity, there is reason to believe that low disclosure may be a source of high OTC-specific offer premiums and synergy gains. Disclosure has been shown to improve cost of capital (Bailey, Karolyi, and Salva, 2006; Easley and O'Hara, 2004; Leuz and Verrecchia, 2000), so takeovers may create value by reducing financial frictions faced by non-disclosing OTC targets (much as in the case of stock illiquidity). Moreover, disclosure may signal potential sources of synergy gains even if disclosure-related cost of capital is not itself a value creation channel. Specifically, non-disclosure may contain information about the target's characteristics. For example, managers could be trying to conceal innovation or strategic activity from competitors (Leuz and Wysocki, 2016) or hide poor performance from the public (Leuz, Triantis, and Wang, 2008). Either case allows for synergy gains by bringing previously undisclosed innovations to market (Gao, Ritter, and Zhu, 2013) or professionalizing target management.

In Table 8, I replicate the regressions from Table 6 Columns (4)-(6) and Table 7, but control for target disclosure instead of stock liquidity. I use three different disclosure proxies measured in the two years prior to the takeover announcement: a dummy if the target files a 10-K, the log total number of filings (10-K and 10-Q), and the log total word count in those filings. Regardless of which dependent variable or disclosure proxy is used, the coefficient estimate for the disclosure variable remains insignificant and the OTC target dummy coefficient remains significant. In other words, I find no evidence that the OTC-specific premiums or synergy gains are related to differences in disclosure.

4.3 Concentrated ownership

The evidence from Sections 4.1 and 4.2 is consistent with OTC-target deal value creation related to stock illiquidity rather than information non-disclosure. While the former seems to account for all of the high acquirer CAR and synergy gains in OTC target deals, it does not account for 25-50% of the OTC target premiums or any of the division of synergy gains during the negotiation process. What explains the remaining OTC-specific premiums?

To address this question, I turn to the third major characteristic separating listed from unlisted firms: concentrated ownership. Prior literature and anecdotal evidence indicate that OTC stocks are more closely held than listed firms (Marosi and Massoud, 2007) and have little institutional ownership (Ang, Shtauber, and Tetlock, 2013). Although it is challenging to test the implications of ownership directly due to data limitations, prior theory and empirical evidence provide clear guidance about what to expect. One can reasonably assume that closely-held target firms will be better-run than firms with dispersed ownership owing to more management monitoring, long-run growth orientation, and risk-taking (Aghion, Van Reenen, and Zingales, 2013; Edmans, 2009). Thus, there are unlikely to be OTC-specific synergy gains from ownership-related underperformance.

Chang (1998) suggests an alternative source of value creation in unlisted-target deals. He hypothesizes that listed acquirers may benefit from improved governance by integrating a private target blockholder into their ownership structure. This only applies when the method of payment is stock. In Table 9, I put this hypothesis to the test by adding an interaction variable for OTC target times all stock payment to the regression specification in Equation 1. I find that the added interaction variable yields insignificant coefficient estimates in all Columns (1)-(4), corresponding to the four different outcome variables used above. Overall, there is no indication that there are OTC-specific synergies due to blockholder governance benefits for listed acquirers.

While concentrated ownership of OTC targets is thus unlikely to yield higher synergies, it is expected to give targets more bargaining power when dividing up synergy gains (Ang and Kohers, 2001; Ghosh and Ruland, 1998). Concentrated owners are less willing to give up control (Amihud, Lev, and Travlos, 1990; Stulz, 1988), may be more bullish about the firm's future prospects, and may receive private benefits from ownership in the form of sentimental value for a founder or family. My evidence is consistent with the interpretation that concentrated ownership strengthens OTC target bargaining but is not a source of synergy gains. As documented in Sections 4.1 and 4.2, I find that accounting for stock liquidity is sufficient to explain all of the OTC-specific acquirer gains and synergies, part of the target offer premiums, and none of the distribution of synergies. In other words, while synergies are associated with stock illiquidity, how those synergies are shared between the acquirer and target is not – in line with expectations about concentrated ownership based on prior theory and empirical evidence.

4.4 Poor performance

Finally, I consider whether underperformance could be a source of OTC-specific synergy gains. If OTC targets are poorly run compared to listed targets prior to the acquisition, there may be synergy gains by professionalizing target management. However, I find this interpretation unlikely. For one, the evidence presented thus far is inconsistent with subpar management for OTC targets. In Section 4.2, no relationship is observed between non-disclosure (potentially to conceal poor performance) and premiums or synergies. Section 4.3 also does not provide any reason to expect that concentrated ownership is likely to be correlated with poorer management.

Moreover, additional empirical evidence contradicts the interpretation that OTC targets are mismanaged. First, I observe that OTC targets have higher average (median) monthly returns in the ten months prior to measuring standalone value (i.e., the start of the runup period) than listed targets: 4.7% (2.3%) versus 1.2% (1.1%). Second, OTC targets that were previously listed (so-called "fallen angels"), which account for one-third of my sample and were in almost all cases involuntarily delisted due to poor performance – and could thus be expected to be worse-run than targets that were never listed – have lower average (median) synergies than never-listed targets at 1.7% (0.1%) versus 3.1% (2.7%), respectively. While not tested directly here, I do not find it likely that OTC-specific premiums and synergies are related to poor target performance.

5 Potential concerns

5.1 Mispricing

Since OTC equities are new to the M&A literature, it is important to consider if there are any data issues that could bias my results. In particular, I consider whether my results could be driven by OTC target mispricing. Unlike listed markets, most trading of OTC equities is conducted by (potentially uninformed) retail investors (White 2016). If OTC targets are undervalued when standalone value is estimated, offer premiums will appear larger than they should.

I argue that, to the extent that there is any mispricing in my sample, it works *against* – not in favor of – my results. Due to brokerage restrictions, search costs, and limited supply, short-selling of OTC equities is difficult, expensive, and rare (Ang, Shtauber, and Tetlock, 2013; Eraker and Ready, 2015). It is known since at least Miller (1977) that in scenarios where investors hold heterogeneous beliefs (as with OTC retail traders) and there are constraints to short selling, prices will be inflated. Indeed, Ang, Shtauber, and Tetlock (2013) find that the OTC market return is negative (-0.8% per month), "implying widespread overpricing of OTC stocks" (p. 2987). Moreover, as noted above in Section 4.3, OTC targets have relatively high monthly pre-runup returns, making it unlikely that they would be underpriced at the time that their standalone value is measured.

Similarly, it is worth considering whether low information disclosure could bias prices downward and thus inflate offer premiums. In particular, one might be concerned that investors would be more cautious when investing in firms with limited available information, resulting in prices that are too low. Again, I find it unlikely that this would be the case. First, we know from Section 4.2 that there is no discernable relationship between disclosure and offer premiums. Second, because less information exacerbates investor disagreement, we expect to see similar upward price pressure due to market restrictions on short selling restrictions as in Miller (1977) or Jarrow (1980). Ang, Shtauber, and Tetlock (2013) find theoretical and empirical support that this is the case in OTC markets when information is disclosed.

Finally, a similar intuition holds for any sort of market price manipulation. Since short selling is expensive, manipulators are only incentivized to inflate prices, such as in "pump-and-dump" schemes. Moreover, since my sample consists of *bona fide* merger deals with (friendly) acquirers launching takeover bids only after accessing the target's data room, it is unlikely that they would be biased by market manipulation schemes (or still be willing to extend an offer should they observe such a scheme). All in all, there is little to suggest that this paper's results are biased by mispricing.

6 Conclusion

In this paper, I introduce a new type of target firm to the M&A literature: unlisted over-the-counter (OTC) firms. Bringing in this new target type allows me to provide the first direct evidence on how much bidders pay when acquiring unlisted targets versus listed targets. Moreover, it allows me to provide new evidence on a twenty-five year old puzzle, first introduced by Chang (1998): Why are acquirer announcement cumulative abnormal returns (CAR) higher in deals with unlisted targets than listed targets?

I find that deal offer premiums are significantly higher for OTC target shareholders than listed target shareholders. This finding is surprising because it contradicts the expectations set by prior papers (Jaffe, Jindra, Pedersen, and Voetmann, 2019; Officer, 2007), which indirectly estimate whether buyers pay less for unlisted targets using deal valuation multiples. I also provide clear evidence showing that the high OTC target premiums are motivated by higher deal synergies rather than overpaying: Acquirer CAR are higher when announcing OTC target deals than listed target deals, despite paying more in the former than the latter. This is also consistent with the prior evidence documenting higher acquirer CAR in unlisted target (specifically, private and subsidiary) deals (Chang 1998; many others). Moreover, combined expected synergy gains are also larger in OTC target deals and OTC target shareholders capture a larger fraction of the value from these synergies during deal negotiations.

Finally, I evaluate several potential economic channels that could explain high OTC target premiums and synergies. My evidence suggests that OTC-specific target synergies are strongly related to differences in stock liquidity, with less liquid targets benefiting more from the market for corporate control than more liquid targets. This is also reflected in higher offer premiums for OTC targets that are closer to private firms (low stock liquidity) than listed firms (high stock liquidity). While stock liquidity can explain most of the high OTC premium, it cannot explain all of it. I propose that the remainder of the premium is consistent with stronger target bargaining due to more concentrated ownership. In contrast, I do not find any evidence that target information disclosure or mismanagement are related to premiums or synergies.

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Table 1: Summary statistics for continuous variables

This table present summary statistics for the continuous variables used in this paper. In Panel A, variables are presented for listed, OTC, and private target deals. Panel B omits private targets since it contains variables that cannot be calculated for, or are not relevant to, private targets. Variables are winsorized at the 5% tails by target type (OTC, listed, or private). Observations are at the deal level.

Variable	Target	N	Mean	Median	Std dev	Min	Max
Panel A: Listed, OTC, and private target deals							
Deal value (2020 USDm)	Listed	7,925	1,349	349	2,278	24	8,848
	OTC	735	78	35	110	4	447
	Private	13,191	88	29	137	2	528
Deal relative size (over acq market cap)	Listed	4,344	0.41	0.22	0.46	0.01	1.65
	OTC	345	0.21	0.12	0.25	0.00	0.93
	Private	7,006	0.20	0.08	0.31	0.01	1.21
Target debt ratio (debt/assets)	Listed	7,683	0.56	0.60	0.26	0.1	0.9
	OTC	387	0.72	0.90	0.27	0.2	1
	Private	2,189	0.73	0.80	0.27	0.2	1
Acquirer announcement CAR (-2, +2)	Listed	4,374	-1.6%	-1.2%	6.7%	-15.9%	11.5%
	OTC	345	0.0%	-0.1%	5.4%	-10.7%	13.2%
	Private	7,116	1.6%	0.5%	7.8%	-12.6%	20.3%
Panel B: Listed and OTC target deals							
Deal offer premiums	Listed	7,925	43%	37%	34%	-11%	124%
	OTC	735	63%	46%	69%	-33%	261%
Combined announcement CAR (-2, +2)	Listed	4,215	1.9%	1.2%	6.6%	-10.7%	16.3%
	OTC	329	2.7%	2.2%	5.8%	-7.9%	16.5%
Target fraction of synergies	Listed	4,215	36%	27%	125%	-251%	339%
	OTC	329	55%	41%	121%	-163%	391%
Target liq: Fraction of days w/ trading	Listed	7,925	96%	100%	8%	70%	100%
	OTC	735	52%	46%	30%	8%	100%
Target liq: (-)log Amihud illiquidity	Listed	7,925	-0.49	-0.08	0.76	-2.60	0.00
	OTC	732	-3.01	-2.77	1.80	-6.82	-0.41
Target liq: Daily trade volume (2020 USDk)	Listed	7,925	7,283	941	14,021	24	54,032
	OTC	735	34	8	64	0.4	262
Target years bef deal ann since last listed	Listed	7,925	0	0	0	0	0
	OTC	237	5.6	4.2	4.1	0.9	14.4
Target est window (t-252, t-43) monthly ret	Listed	7,847	1.2%	1.2%	3.5%	-6.0%	8.2%
	OTC	726	4.7%	2.3%	7.6%	-5.0%	25.9%

Table 2: Summary statistics for categorical variables

This table summarizes dummy variables used in this paper. Each cell shows what fraction of deals meet that column's condition, i.e., have a value of one. Panels A, B, and C present deal, acquirer and target characteristics respectively. Observations are at the deal level.

Panel A: Deal characteristics

						Payment type		
Target type	Complete	Hostile	Tender	Lockup	Horizontal	All cash	Mixed pay	All stock
Listed	79%	5%	22%	11%	53%	41%	28%	31%
OTC	88%	0.4%	5%	2%	61%	48%	28%	24%
Private	88%	0.2%	0.4%	1%	54%	18%	51%	31%

Panel B: Acquirer characteristics

		Industry				Public status					
Target type	Strategic	Finance	High tech	Non-HT	Utilities	US listed	US OTC	US priv	US subs	US misc	Foreign
Listed	83%	40%	35%	25%	0.4%	57%	1%	17%	11%	1%	13%
OTC	88%	62%	21%	17%	0.0%	49%	8%	16%	11%	10%	6%
Private	93%	28%	41%	30%	0.8%	58%	12%	6%	8%	2%	16%

Panel C: Target characteristics

		Industry			
Target type	10-K filing	Finance	High tech	Non-HT	Utilities
Listed	96%	21%	43%	36%	0.0%
OTC	53%	51%	26%	23%	0.0%
Private	5%	21%	43%	36%	0.0%

Table 3: Deal offer premiums by target listing status

This table presents cross-sectional regression results using the specification outlined in Equation 1, with variation in the choice of fixed effects as indicated at the bottom of the table. The dependent variable is deal offer premium and the sample consists of OTC and listed target M&A deals, 1985-2020. The sample size shrinks with later columns as more granular fixed effects force singletons to drop out of the regression. A constant is included but not displayed. All continuous variables are winsorized at the 5% tails by target type (OTC or listed). T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Premium					
	(1)	(2)	(3)	(4)	(5)	(6)
OTC target	0.200***	0.245***	0.261***	0.259***	0.286***	0.258***
	(5.15)	(7.11)	(7.45)	(6.88)	(3.86)	(4.20)
Acquirer listed	0.009	0.020	0.015	0.016	0.050	0.101*
	(0.73)	(1.42)	(1.05)	(1.05)	(0.86)	(1.72)
Acquirer OTC	-0.013	0.006	0.008	-0.029	0.001	-0.011
	(0.23)	(0.11)	(0.14)	(0.60)	(0.03)	(0.18)
Acquirer strategic	0.058***	0.043***	0.048***	0.050***	0.016	0.019
	(4.48)	(3.17)	(3.66)	(4.03)	(0.47)	(0.53)
Deal complete	0.021	0.034**	0.038**	0.039**	0.046	0.042
	(1.25)	(2.21)	(2.54)	(2.42)	(1.60)	(1.13)
Deal payment all-stock	-0.032**	-0.064***	-0.058***	-0.050***	-0.044***	-0.023
	(2.36)	(5.98)	(5.40)	(3.92)	(2.75)	(1.02)
Deal payment mixed	-0.016	-0.038***	-0.028**	-0.025	-0.024	-0.004
	(1.51)	(2.92)	(2.24)	(1.63)	(1.17)	(0.15)
Deal hostile	0.093***	0.083***	0.090***	0.080***	0.087**	0.061
	(4.38)	(3.95)	(4.24)	(3.88)	(2.37)	(1.16)
Deal horizontal	0.003	0.010	0.019*	0.019	0.029	0.049***
	(0.18)	(0.72)	(1.68)	(1.47)	(1.58)	(3.17)
Deal tender offer	0.101***	0.076***	0.059***	0.068***	0.037*	0.028
	(5.03)	(3.53)	(3.09)	(3.12)	(1.97)	(1.20)
Deal lockup agreement	0.041***	0.011	0.020	0.008	0.023	0.022
	(3.22)	(0.78)	(1.39)	(0.46)	(1.02)	(0.80)
Deal log-value	-0.008*	0.001	-0.000	0.001	-0.009	-0.011
	(1.93)	(0.17)	(0.03)	(0.18)	(0.99)	(0.95)
Observations	8,658	8,658	8,655	8,174	4,429	3,767
...of which OTC target deals	735	735	735	688	297	271
Adjusted R-squared	0.040	0.076	0.085	0.104	0.171	0.190
Year FE		Y	Y		Y	
Industry FE			Y		Y	
Year-industry FE				Y		Y
Acquirer FE					Y	Y

Table 4: Deal offer premiums by target listing status, alternative controls

This table presents cross-sectional regression results using the specification in Equation 1. The dependent variable is deal offer premium and the sample consists of OTC and listed target M&A deals, 1985-2020. To isolate the potential impact of excluded variables from Column (3) of Table 3, I run regressions in pairs where the variable I am evaluating is nonmissing in both, but only included in alternating specifications (odd-numbered columns). Acquirer and deal controls which are included but not shown are the same as in Table 3 Column (3). The sample size varies depending on which control variable is being evaluated, since these variables are missing for many deals. A constant is included but not displayed. All continuous variables are winsorized at the 5% tails by target type (OTC or listed). T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Premium							
Nonmissing control variable:	Termination agreement		Relative size (market cap)		Target debt ratio		Target sales growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OTC target	0.237***	0.239***	0.219***	0.226***	0.236***	0.227***	0.182***	0.181***
	(7.98)	(7.99)	(5.54)	(5.64)	(7.44)	(7.55)	(3.75)	(3.74)
Deal termination agreement		0.026***						
		(2.66)						
Deal relative size (market cap)				-0.041**				
				(2.52)				
Target debt ratio						0.079**		
						(2.31)		
Target 5-year sales growth								0.006
								(1.27)
Deal log-value	-0.001	-0.002	-0.006		-0.002	-0.002	-0.005	-0.005
	(0.20)	(0.42)	(0.88)		(0.41)	(0.54)	(1.27)	(1.30)
Observations	8,491	8,491	4,684	4,684	8,067	8,067	4,936	4,936
... of which OTC target deals	573	573	345	345	387	387	164	164
Adjusted R-squared	0.079	0.080	0.082	0.084	0.079	0.081	0.089	0.090
Acquirer controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal controls	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y

Table 5: **Acquirer CAR, synergy gains, and division of synergies by target listing status**

This table presents cross-sectional regression results using the specification in Equation 1. The dependent variable varies by column: acquirer CAR in (1)-(2), combined CAR indicating expected synergy gains in (3)-(4), and the fraction of the combined CAR going to target shareholders. The sample consists of OTC and listed target M&A deals, 1985-2020. To measure acquirer CAR, bidders are restricted to US listed firms. Both the bidder and target must have at least 40 return observations during the estimation window. A constant is included but not displayed. All continuous variables are winsorized at the 5% tails by target type (OTC or listed). T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Acquirer CAR		Combined CAR		Target % of synergies	
	(1)	(2)	(3)	(4)	(5)	(6)
OTC target	0.011***	0.011***	0.015***	0.015***	0.210***	0.203***
	(3.24)	(3.29)	(4.31)	(4.06)	(5.09)	(5.17)
Target runup		0.008*		-0.021***		-0.264***
		(1.70)		(5.13)		(3.40)
Acquirer runup		-0.007		-0.003		-0.056
		(0.88)		(0.35)		(0.73)
Acquirer strategic	-0.003	-0.002	-0.001	-0.001	0.006	0.001
	(0.57)	(0.54)	(0.19)	(0.30)	(0.07)	(0.01)
Deal complete	0.002	0.002	0.006*	0.006**	0.062	0.065
	(0.58)	(0.56)	(1.95)	(2.05)	(1.25)	(1.32)
Deal payment all-stock	-0.019***	-0.019***	-0.017***	-0.017***	-0.142***	-0.143***
	(5.02)	(5.05)	(8.43)	(8.24)	(2.94)	(2.93)
Deal payment mixed	-0.013***	-0.014***	-0.010***	-0.009***	0.028	0.029
	(4.38)	(4.34)	(3.39)	(3.43)	(0.53)	(0.56)
Deal hostile	0.002	0.002	0.026***	0.026***	0.131	0.123
	(0.35)	(0.35)	(4.09)	(4.09)	(1.11)	(1.04)
Deal horizontal	0.002	0.002	0.006***	0.006***	0.051	0.052
	(0.55)	(0.55)	(2.71)	(2.73)	(1.40)	(1.41)
Deal tender offer	0.013***	0.012***	0.019***	0.019***	0.106**	0.112**
	(4.97)	(4.89)	(8.40)	(8.37)	(2.00)	(2.05)
Deal lockup agreement	-0.010***	-0.010***	-0.007**	-0.007**	0.013	0.008
	(3.50)	(3.46)	(2.53)	(2.65)	(0.20)	(0.12)
Deal relative size	-0.007**	-0.007**	0.039***	0.039***	0.495***	0.496***
	(2.45)	(2.28)	(13.65)	(12.83)	(12.68)	(12.86)
Observations	4,538	4,538	4,538	4,538	4,538	4,538
...of which OTC target deals	328	328	328	328	328	328
Adjusted R-squared	0.056	0.056	0.123	0.127	0.048	0.050
Year FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y

Table 6: Deal offer premiums by target stock liquidity

This table presents cross-sectional regression results based on specification in Equation 1. The dependent variable is deal offer premium. In Columns (1)-(3), the OTC target dummy is split into four parts, each corresponding to an OTC target stock liquidity quartile. In Columns (4)-(6), the OTC target dummy is kept as is but a continuous stock liquidity control variable is added instead for all deals. Liquidity is measured in the ten months before the start of the runup period. The liquidity proxy varies by column, as indicated in the second row. Sample of OTC and listed target deals, 1985-2020. Constant included but not shown. Continuous variables winsorized at 5% tails by target type. T-stats in parentheses, standard errors clustered at industry level. *, **, and *** indicate 10%, 5%, and 1% significance.

Dependent variable:	Premium					
Liquidity measure:	% days trade (1)	(-)Amihud (2)	$ volume (3)	% days trade (4)	(-)Amihud (5)	$ volume (6)
OTC target liq Q1 (lowest)	0.417***	0.510***	0.556***			
	(3.37)	(7.75)	(4.97)			
OTC target liq Q2	0.307***	0.331***	0.303***			
	(3.46)	(8.51)	(6.27)			
OTC target liq Q3	0.210***	0.209***	0.212***			
	(5.21)	(5.12)	(4.49)			
OTC target liq Q4 (highest)	0.155***	0.029	0.053			
	(3.36)	(0.63)	(0.96)			
OTC target				0.127***	0.067**	0.066**
				(3.49)	(2.41)	(2.34)
Target liquidity				-0.373***	-0.107***	-0.089***
				(3.64)	(11.30)	(21.74)
Acquirer listed	0.016	0.017	0.017	0.018	0.022*	0.028**
	(1.07)	(1.15)	(1.20)	(1.34)	(1.83)	(2.33)
Acquirer OTC	-0.007	-0.005	-0.019	-0.011	-0.001	0.019
	(0.13)	(0.09)	(0.30)	(0.18)	(0.02)	(0.32)
Acquirer strategic	0.047***	0.045***	0.044***	0.046***	0.038***	0.049***
	(3.59)	(3.60)	(3.47)	(3.72)	(3.34)	(4.37)
Deal complete	0.038**	0.039**	0.038**	0.036**	0.035**	0.018
	(2.54)	(2.64)	(2.51)	(2.45)	(2.57)	(1.26)
Deal payment all-stock	-0.058***	-0.058***	-0.058***	-0.054***	-0.057***	-0.045***
	(5.47)	(5.31)	(5.54)	(5.15)	(5.37)	(3.17)
Deal payment mixed	-0.029**	-0.028**	-0.028**	-0.029**	-0.034***	-0.040***
	(2.35)	(2.27)	(2.42)	(2.36)	(2.83)	(3.62)
Deal hostile	0.088***	0.086***	0.085***	0.088***	0.087***	0.092***
	(4.09)	(4.14)	(3.98)	(4.05)	(4.14)	(4.25)
Deal horizontal	0.019*	0.018	0.019*	0.019*	0.021*	0.017
	(1.69)	(1.66)	(1.69)	(1.73)	(1.99)	(1.66)
Deal tender offer	0.058***	0.062***	0.059***	0.060***	0.067***	0.055***
	(3.02)	(3.22)	(3.12)	(3.19)	(3.56)	(2.83)
Deal lockup agreement	0.023	0.013	0.020	0.026*	0.018	0.026*
	(1.64)	(0.92)	(1.37)	(1.92)	(1.31)	(1.73)
Deal log-value	0.001	0.004	0.003	0.008	0.033***	0.092***
	(0.14)	(0.88)	(0.77)	(1.64)	(6.82)	(14.11)
Observations	8,652	8,652	8,652	8,652	8,652	8,652
...of which OTC target deals	732	732	732	732	732	732
Adjusted R-squared	0.090	0.101	0.102	0.094	0.122	0.141
Year FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Wald F-test OTC liq Q1=Q4	0.054*	0.000***	0.000***			

Table 7: Acquirer CAR and synergies by target stock liquidity

This table presents cross-sectional regression results based on the specification in Equation 1, but with the addition of a target stock liquidity variable as in Table 6 Columns (4)-(6). The dependent variable varies by column as indicated in the first row. The sample consists of OTC and listed target M&A deals, 1985-2020. To measure acquirer CAR, bidders are restricted to US listed firms. Both the bidder and target must have at least 40 return observations during the estimation window. A constant is included but not displayed. All continuous variables are winsorized at the 5% tails by target type (OTC or listed). T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Dependent variable:	Acquirer CAR			Combined CAR			Target % of synergies		
Liquidity measure:	% days trade (1)	(-)Amihud (2)	$ volume (3)	% days trade (4)	(-)Amihud (5)	$ volume (6)	% days trade (7)	(-)Amihud (8)	$ volume (9)
OTC target	0.001	0.000	-0.002	0.003	0.006	0.002	0.285***	0.266***	0.165***
	(0.32)	(0.08)	(0.45)	(0.67)	(1.02)	(0.51)	(5.08)	(4.65)	(3.14)
Target liquidity	-0.022***	-0.005***	-0.003***	-0.031***	-0.004***	-0.003***	0.184	0.025*	-0.010
	(3.20)	(3.10)	(3.89)	(5.33)	(3.06)	(4.92)	(1.64)	(1.72)	(1.23)
Acquirer strategic	-0.002	-0.002	-0.002	-0.001	-0.000	-0.000	0.005	0.004	0.008
	(0.53)	(0.47)	(0.46)	(0.13)	(0.10)	(0.08)	(0.06)	(0.05)	(0.09)
Deal complete	0.002	0.002	0.002	0.006**	0.006**	0.007**	0.061	0.059	0.063
	(0.62)	(0.75)	(0.74)	(2.03)	(2.16)	(2.13)	(1.23)	(1.20)	(1.28)
Deal payment all-stock	-0.018***	-0.018***	-0.018***	-0.016***	-0.016***	-0.016***	-0.144***	-0.143***	-0.139***
	(4.83)	(4.98)	(4.99)	(8.06)	(8.36)	(8.14)	(3.04)	(2.94)	(2.85)
Deal payment mixed	-0.013***	-0.013***	-0.013***	-0.009***	-0.009***	-0.009***	0.026	0.026	0.029
	(4.38)	(4.36)	(4.20)	(3.41)	(3.35)	(3.21)	(0.52)	(0.50)	(0.57)
Deal hostile	0.003	0.003	0.005	0.027***	0.027***	0.029***	0.128	0.125	0.139
	(0.40)	(0.50)	(0.75)	(4.12)	(4.28)	(4.70)	(1.08)	(1.07)	(1.22)
Deal horizontal	0.001	0.002	0.001	0.006***	0.006***	0.006**	0.052	0.051	0.051
	(0.53)	(0.56)	(0.48)	(2.66)	(2.71)	(2.56)	(1.40)	(1.36)	(1.39)
Deal tender offer	0.013***	0.013***	0.013***	0.019***	0.019***	0.019***	0.105**	0.103*	0.107**
	(4.98)	(5.22)	(5.02)	(8.45)	(8.79)	(8.67)	(2.00)	(1.94)	(2.03)
Deal lockup agreement	-0.009***	-0.009***	-0.007**	-0.006**	-0.006**	-0.004	0.008	0.007	0.023
	(3.32)	(3.06)	(2.37)	(2.25)	(2.11)	(1.47)	(0.11)	(0.10)	(0.35)
Deal relative size	-0.007**	-0.006*	-0.005	0.039***	0.040***	0.041***	0.491***	0.488***	0.504***
	(2.25)	(1.96)	(1.41)	(13.33)	(14.06)	(13.74)	(13.08)	(12.47)	(11.50)
Observations	4,537	4,537	4,537	4,537	4,537	4,537	4,537	4,537	4,537
...of which OTC target deals	327	327	327	327	327	327	327	327	327
Adjusted R-squared	0.057	0.059	0.062	0.126	0.126	0.130	0.048	0.048	0.048
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 8: **Premiums, acquirer CAR, and synergies by target information disclosure**

This table presents cross-sectional regression results based on the specification in Equation 1, with an added target information disclosure variable. Dependent variable varies by column as indicated in first row. Sample consists of OTC and listed target M&A deals, 1998-2020, since SEC EDGAR filing coverage is incomplete prior to 1996. In Columns (4)-(12), bidders are restricted to US listed firms to allow for measurement of acquirer CAR and both bidder and target must have at least 40 return observations during estimation window. Similarly restricting the sample in Columns (1)-(3) yields similar results to those shown. Disclosure is measured in two years before the deal announcement date. Disclosure proxy varies by column, as indicated in second row: a dummy if the target filed any 10-K, target's total 10-Q/10-K filing count, or total word count in filings. Acquirer controls as in Table 3 and a constant term are included but not shown. Continuous variables winsorized at 5% tails by target type. T-stats in parentheses, standard errors clustered at industry level. *, **, and *** indicate 10%, 5%, and 1% significance.

Dependent variable:	Premium			Acquirer CAR			Combined CAR			Target % of synergies		
Disclosure measure:	10-K (1)	Files (2)	Words (3)	10-K (4)	Files (5)	Words (6)	10-K (7)	Files (8)	Words (9)	10-K (10)	Files (11)	Words (12)
OTC target	0.269***	0.269***	0.267***	0.009*	0.008*	0.008*	0.015***	0.014***	0.013***	0.162**	0.192***	0.177***
	(8.36)	(8.02)	(7.99)	(1.94)	(1.89)	(1.71)	(3.35)	(3.13)	(2.97)	(2.64)	(3.20)	(2.75)
Target disclosure	0.034	0.016	0.002	-0.001	-0.001	-0.000	0.002	-0.000	-0.000	-0.010	0.028	0.002
	(1.25)	(1.35)	(1.20)	(0.34)	(0.80)	(1.41)	(0.42)	(0.14)	(0.42)	(0.13)	(0.88)	(0.30)
Deal complete	0.046**	0.045**	0.046**	0.003	0.003	0.003	0.007	0.007	0.007	0.011	0.010	0.011
	(2.17)	(2.17)	(2.17)	(0.80)	(0.81)	(0.82)	(1.62)	(1.63)	(1.64)	(0.19)	(0.17)	(0.18)
Deal pay all-stock	-0.092***	-0.093***	-0.093***	-0.028***	-0.028***	-0.028***	-0.028***	-0.028***	-0.028***	-0.179***	-0.178***	-0.179***
	(6.83)	(6.83)	(6.82)	(6.62)	(6.57)	(6.56)	(10.19)	(10.19)	(10.19)	(3.43)	(3.41)	(3.41)
Deal pay mixed	-0.027**	-0.027**	-0.027**	-0.022***	-0.022***	-0.022***	-0.016***	-0.016***	-0.016***	0.025	0.026	0.025
	(2.25)	(2.25)	(2.24)	(5.66)	(5.67)	(5.67)	(4.49)	(4.50)	(4.51)	(0.48)	(0.50)	(0.48)
Deal hostile	0.060*	0.060*	0.060*	-0.029**	-0.029**	-0.029**	0.007	0.006	0.006	0.135	0.140	0.137
	(1.86)	(1.86)	(1.85)	(2.50)	(2.50)	(2.49)	(0.50)	(0.48)	(0.47)	(0.73)	(0.76)	(0.75)
Deal horizontal	0.025*	0.025*	0.025*	0.004	0.004	0.004	0.006**	0.006**	0.006**	0.046	0.047	0.047
	(1.80)	(1.81)	(1.79)	(1.10)	(1.09)	(1.09)	(2.61)	(2.58)	(2.57)	(1.09)	(1.12)	(1.10)
Deal tender offer	0.051*	0.051*	0.051*	0.007*	0.007*	0.007*	0.009***	0.009***	0.009***	0.052	0.053	0.052
	(1.79)	(1.79)	(1.79)	(1.96)	(1.95)	(1.95)	(3.03)	(3.02)	(3.01)	(0.78)	(0.78)	(0.77)
Deal lockup	0.054**	0.053**	0.054**	-0.005	-0.005	-0.005	-0.001	-0.001	-0.001	-0.005	-0.008	-0.007
	(2.12)	(2.14)	(2.14)	(0.65)	(0.64)	(0.63)	(0.14)	(0.13)	(0.12)	(0.05)	(0.07)	(0.06)
Deal log-value	-0.006	-0.005	-0.006									
	(1.10)	(1.08)	(1.14)									
Deal relative size				-0.009**	-0.009**	-0.009**	0.039***	0.039***	0.039***	0.450***	0.450***	0.450***
				(2.25)	(2.24)	(2.23)	(11.77)	(11.66)	(11.56)	(11.10)	(11.17)	(11.09)
Observations	5,855	5,855	5,855	2,976	2,976	2,976	2,976	2,976	2,976	2,976	2,976	2,976
...of which OTC	711	711	711	318	318	318	318	318	318	318	318	318
Adj R-squared	0.099	0.099	0.099	0.072	0.072	0.072	0.105	0.105	0.105	0.043	0.043	0.043
Acquirer controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 9: **Do listed acquirers benefit by integrating OTC target blockholders?**

This table presents cross-sectional regression results based on the specification in Equation 1, with an added interaction term between the OTC target and all-stock payment dummy variables. Dependent variable varies by column as indicated in first row. Sample consists of OTC and listed target M&A deals, 1985-2020. Bidders are restricted to US listed firms since these are the acquirers that are expected to experience governance benefits by acquiring an unlisted target with concentrated ownership, as hypothesized by **?**. Both bidder and target must have at least 40 return observations during estimation window. Constant included but not shown. Continuous variables winsorized at 5% tails by target type. T-stats in parentheses, standard errors clustered at industry level. *, **, and *** indicate 10%, 5%, and 1% significance.

Dependent variable:	Premium (1)	Acquirer CAR (2)	Combo CAR (3)	Tar % synergies (4)
OTC target	0.247***	0.013***	0.016***	0.219***
	(5.02)	(3.54)	(3.70)	(3.99)
OTC target X Deal payment all-stock	-0.086	-0.006	-0.001	-0.026
	(1.62)	(1.32)	(0.21)	(0.33)
Acquirer strategic	-0.017	-0.003	-0.001	0.006
	(0.52)	(0.57)	(0.19)	(0.07)
Deal complete	0.037*	0.002	0.006*	0.061
	(1.88)	(0.57)	(1.94)	(1.23)
Deal payment all-stock	-0.055***	-0.018***	-0.017***	-0.140***
	(3.94)	(4.56)	(8.63)	(2.72)
Deal payment mixed	-0.031*	-0.013***	-0.010***	0.028
	(1.91)	(4.38)	(3.39)	(0.53)
Deal hostile	0.092***	0.002	0.026***	0.130
	(2.76)	(0.34)	(4.07)	(1.10)
Deal horizontal	0.015	0.002	0.006***	0.051
	(1.29)	(0.55)	(2.72)	(1.39)
Deal tender offer	0.077***	0.013***	0.019***	0.106*
	(3.87)	(5.02)	(8.40)	(1.98)
Deal lockup agreement	0.006	-0.010***	-0.007**	0.013
	(0.38)	(3.50)	(2.55)	(0.19)
Deal log-value	-0.006			
	(1.03)			
Deal relative size		-0.007**	0.039***	0.495***
		(2.42)	(13.66)	(12.62)
Constant	0.453***	-0.001	0.001	0.093
	(8.05)	(0.22)	(0.26)	(0.85)
Observations	4,538	4,538	4,538	4,538
...of which OTC target deals	328	328	328	328
Adjusted R-squared	0.085	0.056	0.123	0.048
Year FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y

A Appendix

Appendix A: Additional descriptive statistics

In Appendix Table 1, I describe the ten largest OTC M&A deals in my sample, all of which have a deal value of over $1 billion (2020 USD). The targets operate in a variety of industries and the deals are announced in various years between 1992 and 2018. Half of the targets were previously listed. I identify the largest target owner and their fraction of shares outstanding prior to the takeover via manual web searches of press releases and news articles. I am unable to identify the largest owner in three of the ten deals. For six of the remaining deals, the largest owners hold between 40% and 90% of target shares. Many of these are a mix of private equity funds, hedge funds, former senior lenders (for firms target that went into bankruptcy, with their original shareholders getting wiped out), and company insiders.

Table 1: Top 10 largest OTC target M&A deals (2020 USD billion), 1985-2020

This table provides descriptive information on the ten largest OTC target deals in my sample. The sample consists of US target control bids, 1985-2020.

Rank (1)	Deal value (2)	Target (3)	Acquirer (4)	Industry (5)	Ann year (6)	Prev listed (7)	Largest owner(s) (8)
1	$2.9	Belk Inc	Sycamore Partners LLC	Department stores	2015		70% family
2	$2.5	Citadel Broadcasting Corp	Cumulus Media Inc	Radio broadcasting	2010	Y	90% PE & senior lenders
3	$2.4	MNC Financial Inc	NationsBank Corp	Banking	1992	Y	10% former chairman
4	$1.6	MPM Holding Inc	MOM Holding Co	Silicones and advanced materials	2018		40% PE
5	$1.4	Mariner Health Care Inc	National Senior Care Inc	Nursing homes	2004		
6	$1.4	Samsonite Corp	CVC Capital Partners Ltd	Luggage	2007	Y	85% PE
7	$1.3	Alliance Imaging Inc	Kohlberg Kravis Roberts & Co (KKR)	MRI systems and services	1999	Y	83% PE
8	$1.2	Seventy Seven Energy Inc	Patterson-UTI Energy Inc	Onshore oil/gas drilling	2016		50% PE
9	$1.1	Trans Financial Inc	Star Banc Corp	Banking	1998	Y	
10	$1.1	National Community Banks Inc	Bank of New York Co Inc	Banking	1993		

CHAPTER 2

Merger-driven listing dynamics

Merger-driven listing dynamics*

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Abtract

Stock-market effectiveness in attracting and retaining firms under public ownership depends not only on stand-alone firms' net listing benefits but also on gains from merging with a public acquirer. Using a novel merger-adjusted listing count, we show that the dramatic (≈50%) post-1996 U.S. listing decline—previously attributed to declining listing benefits—is reversed as the 'missing' firms de facto continue existing inside their public acquirers. Our merger adjustment also eliminates the U.S. listing gap, pointing instead to a distinct U.S. listing *advantage*: providing access to a well-functioning market for complex merger transactions.

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1 Introduction

The dramatic (≈50%) post-1996 decrease in the number of firms listed on the three major U.S. stock exchanges, shown here in Panel A of Figure 1, has prompted substantial interest in the major drivers of listing dynamics. Naturally, much attention has been given to the similar-sized reduction in initial public offerings (IPOs), the majority of which took place on the Nasdaq exchange throughout the 1990s (Eckbo and Norli, 2005; Fama and French, 2004). Gao, Ritter, and Zhu (2013) carefully consider several potential drivers of this reduction, including increased costs of investment-banking services and the 2002 Sarbanes Oxley Act (SOX). While they conclude that these cost-increases are unlikely explanations, they suggest that many high-technology startups may have chosen to rapidly scale up through a sellout rather than undertaking an IPO, as the latter mechanism poses greater risk of publicly disclosing valuable private information.[1] Moreover, Doidge, Karolyi, and Stulz (2017) point to a positive trend in aggregate international listings—illustrated here in Panel B of Figure 1 (extended to 2020)—before estimating a significant 'U.S. listing gap'. They conclude that the listing gap not only exists but "is consistent with a decrease in the net benefits of a listing for U.S. firms" (abstract).

In this paper, we make several contributions to our understanding of listing dynamics. We begin by pointing to the fact that, while listing dynamics is affected by changes in listing benefits (such as access to public debt and equity, 'acquisition currency', and improved managerial incentives through stock-based compensation, as well as listing costs), it is also directly affected by expected gains from merger transactions, which can be substantial in magnitude.[2] Hence, we argue that, before drawing inferences about changes in listing benefits one must

[1]Also, two decades of increased funding from private equity and other financial institutions has enabled young firms to delay going public and hence increased the age firms undertaking IPOs. In our data (shown in Section 2.2 below), the median age since incorporation has increased from 8 to 12 years by 2020. For analyses of the decision to go public, see, e.g., Poulsen and Stegemoller (2008), Da Rin, Hellman, and Puri (2013), Doidge, Karolyi, and Stulz (2013), Dambra, Casares Field, and Gustafson (2015), Ewens and Farre-Mensa (2020), Kwon, Lowry, and Qian (2020), and Dathan and Xiong (2022).

[2]Alexandridis, Antypas, and Travlos (2017) and Dessaint, Eckbo, and Golubov (2022) document positive average bidder and target abnormal returns over the past four decades. Target offer premiums in deals where both the bidder and the target are public firms average 40% (Betton, Eckbo, Thompson, and Thorburn, 2014). We return to the issue of time-series changes in synergy gains, in particular during the merger wave of the 1990s, in Section 5.2 below.

account for listing changes caused by mergers. While Doidge, Karolyi, and Stulz (2017) also discuss merger activity involving public firms, our analysis is the first to causally link the merger channel directly to the listing dynamics at the firm level.

By integrating merger activity directly into the listing dynamics, we are in effect refocusing the listing debate on a broader issue that cannot be addressed by the actual listing count itself: The ability of stock markets around the world to attract and retain firms under public ownership—arguably a fundamental objective of any public market. By tracking the number of stand-alone listed firms only, the listing count does not accurately gauge this ability. Specifically, because targets give up their stand-alone status, they are either ignored by the actual listing count (when private) or, in the case of public targets, even treated as having *left* the stock exchange. This despite the fact that these former stand-alone companies continue under public ownership—likely deriving some of the parent company's listing benefits. We solve this measurement problem by simply treating a stand-alone listed company as a nexus of the firm and its de facto consolidated targets since going public. Our merger adjustments therefore amounts to adding the targets of listed acquirers to the actual listing count since going public.[3]

We present the main findings of our analysis of the merger-driven listing dynamics in five steps. We begin by documenting that mergers involving U.S. public acquirers are nearly as important as IPOs in impacting listing dynamics—both in number and value. More specifically, over the period 1980–2020, U.S. listed companies on average acquire one public or private target firm, bringing the annual average number of companies from 5,108 to 10,907 after adding the targets. Moreover, while IPOs brought in 10,567 firms valued at \$6 trillion over the same period, the total transaction value of the acquisition targets was nearly \$13 trillion—twice that of the IPOs. This evidence helps to illustrates how much the listing count itself underestimates the actual flow of firms into the three major U.S. stock exchanges.[4]

Second, we show that our merger adjustment reverses almost the entire post-1996 listing

[3] For internal consistency, when a listed firms leaves the exchange, this merger adjustment requires lowering the listing count by one plus the sum of its targets. Further detailed in Section 2 below.

[4] As we document, the private targets included in our study are bona fide 'listable' firms as their annual median deal value averages nearly half of the size of the yearly median listed firm in the same industry.

decline—there is no merger-adjusted listing peak. That is, accounting for acquisitions by public firms of other public companies, and of private targets—some of which might otherwise have chosen to go public themselves—is sufficient to eliminate the listing decline. Third, turning to the two international trend lines in Panel B of Figure 1, we discover that the smoothly rising trends in fact hide a large number of dramatic country-specific listing peaks that occur at different point in time over the sample period. Surprisingly, as much as four-fifths of the 74 countries represented in that figure experience a listing peak followed by a total decline that averages nearly 50%—much like in the U.S. after 1996.

Our evidence that a 'U.S. style' listing peak is the rule rather than the exception internationally raises the possibility that the post-1996 listing decline in the U.S. is driven by forces that are common across countries—including the merger channel. For example, as new technological innovations occur randomly across countries, the merger-driven dynamics is also expected to differ in timing across stock markets. We use our merger-adjusted listing series to test whether the merger channel, during the post-peak period of listing decline, works to retain targets under public ownership more strongly in the U.S. than elsewhere. We find that this is indeed the case. Specifically, relative to the U.S., public firms on foreign exchanges more often *exit* their respective stock markets instead of being retained under public ownership by a public acquirer. This evidence is important as it points to a merger-driven U.S. listing *advantage*: Providing access to a legal and regulatory system that promotes relatively cost-efficient complex corporate control transactions involving public companies.[5]

Fourth, we use our merger-adjusted listing series to revisit the U.S. listing gap estimated by Doidge, Karolyi, and Stulz (2017). With 1990 as their base year, they find that the U.S. listing count per capita falls significantly below an international trend line, 1996–2012. In our replication of their econometric analysis (detailed in our Appendix A), as many as 3,289 U.S. listed firms are "missing" in year 2012 (their last period). However, when we replace their dependent variable with our merger-adjusted listing series—which adds actual target firms

[5]See, e.g., Coffee (1984), Bebchuk, Cohen, and Ferrell (2009), and Coates (2018) for discussions legal rules and regulations governing U.S. transactions in the market for corporate control. Levine (1997), La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997), and Demirguc-Kunt and Maksimovic (1998) present evidence of the high degree of minority shareholder protection afforded by the U.S. legal system. Eckbo, Malenko, and Thorburn (2020) discuss the resolution of complex merger transactions under information asymmetries.

purchased by listed acquirers—the listing-gap estimates becomes statistically insignificant in all years, 1991–2020.

Again, Doidge, Karolyi, and Stulz (2017) interpret their listing gap as pointing to a relative decrease in the net benefit of being listed in the U.S. As changes in net listing benefits are unobservable to the econometrician, this interpretation cannot of course be ruled out. However, since we show that the listing gap is merger-driven, the gap is much more likely to reflect the quantifiable merger gains than a response to lower net listing benefits. This observation is particularly important for the listing debate as the extraordinary ability of the U.S. stock market to retain firms under public ownership through mergers pints to a relative listing advantage.

Finally, since our interpretation of the merger channel as a relative U.S. listing advantage begs the question of the likely value and productivity of this channel, we round off our analysis by providing new and supporting evidence. We first show that the net firm-value inflow—inflows minus outflows generated by the full anatomy of U.S. listing changes—is *higher* in the post-peak period than between 1980 and 1996 ($1.7 trillion versus $1.2 trillion, respectively). Second, estimating what John, Kadyrzhanova, and Lee (2022) label a 'synergy wave', which is based on the frequency of merger transactions with a positive combined bidder and target wealth effect, we find that the merger activity that drove much of the post-1996 listing decline was predominantly value increasing. Third, presumably with the help of their respective targets, firms that remain listed after 1996 have maintained or even improved on the pre-1996 contribution to aggregate employment and GDP and expanded R&D and patenting activity. This evidence further supports our argument that accounting for the merger channel is necessary to understand the forces driving U.S. listing dynamics.

2 Merger-driven listing dynamics in the U.S.

In this section, we first explain and then apply our merger-adjustment procedure to U.S. listed companies. As stated above, our procedure implements the simple view of a public stand-alone company as a nexus of the initial firm itself (at the time of the IPO) and its subsequently consolidated 'listable' targets. As explained below, while public targets are, of course, all 'listable' firms, we impose a minimum size threshold based on actually listed firms for private targets to also be counted in this nexus. All variable definitions are summarized in Table 1. Our data sources for the full U.S. listing anatomy, which includes both foreign and domestic target firms, are from CRSP and Refinitiv's SDC Platinum M&A database (SDC). These data sources, as well as other sources used to identify listing dynamics of foreign stock exchanges, are fully described in Appendix B.

2.1 The merger-adjustment procedure

Let ΔL denote the annual net change in the actual listing count, i.e., new lists minus delists of stand-alone companies. The following components describe ΔL:

$$\Delta L = \begin{cases} Newlists: & IPO + Spin + Misc_{New} \\ Delists: & Merge_{Public-to-Public} + Merge_{Public-to-Private} + Misc_{Del} \end{cases} \tag{2}$$

New lists arise from initial public offerings (IPO), public-company divisional spinoffs into new public companies ($Spin$), and miscellaneous new listings ($Misc_{New}$). The latter includes new lists without raising capital (in particular uplists from smaller exchanges and over-the-counter markets), relistings following leveraged buyouts and emergence from bankruptcy, and firms that change status from foreign-domiciled to U.S.-domiciled.

Delists arise from public-to-public and public-to-private mergers, where the subscript indicates the direction of the flow of the target firm, and miscellaneous other reasons. In $Merge_{Public-to-Public}$ a public target is acquired by another public company, while in $Merge_{Public-to-Private}$ the public target is acquired by a private firm. The private acquirer may be U.S.-domiciled or a foreign company.[6] The miscellaneous other delistings $Misc_{Del}$ include delistings that are voluntary, for cause, or for unknown reasons. A delisting for cause occurs when a firm fails to uphold certain exchange-listing requirements, such as when the firm files for bankruptcy or

[6] We designate the acquirer as 'private' even if it trades over-the-counter or on a minor exchange in the U.S. or on a public exchange in a foreign country.

its stock falls below a minimum price.

Turning to the merger-driven listing changes, let ΔL_A denote the net change in the merger-adjusted listing count. It is the sum of the following six components:

$$\Delta L_A = \begin{cases} Newlists_A: & IPO + Merge_{Private-to-Public} + Misc^N_{New} \\ Delists_A: & Merge^N_{Public-to-Private} + Divest_{Subsidiary-to-Private} + Misc^N_{Del} \end{cases} \tag{3}$$

While $Newlists_A$ is affected by IPO in the same way as $Newlists$, it adds $Merge_{Private-to-Public}$ and excludes $Spin$. In $Merge_{Private-to-Public}$, which is also not part of $Newlists$, a public company is acquiring a non-public (private or foreign) firm. $Spin$ is excluded since a divisional spinoff into a separate public firm does not change corporate resources under public ownership. Comparing the actual and adjusted delists, $Delists_A$ is not lowered by $Merge_{Public-to-Public}$. However, $Divest_{Subsidiary-to-Private}$ now subtracts from the listing count when the subsidiary of a public parent is sold to a private firm.

The superscript N in Eq. (3), refers to the acquisition tracking index N_{it} in Eq. (4) below. For internal consistency, as we continually add the targets of public acquirer i to ΔL_A, we must also lower the merger-adjusted count by the same number of targets whenever firm i leaves the stock exchange for reasons other than being acquired by another public company. Beginning in 1980, N_{it} is updated by one if target j is a private firm and by $N_{j,t-1}+1$ if target j is a public company:

$$N_{it} = \begin{cases} N_{i,t-1}+1 & \text{if target } j \text{ acquired in period } t \text{ is a private firm} \\ N_{i,t-1}+1+N_{j,t-1} & \text{if target } j \text{ acquired in period } t \text{ is a public firm} \end{cases} \tag{4}$$

where $N_{j,t-1}+1$ is the value of the public target's acquisition index. We reiterate that N_{it} is only used to adjust ΔL_A for public companies, and primarily when a public company leaves the stock exchange for reasons other than being acquired by another public company. The one exception is when a firm with $N_{it} > 0$ relists after having exited the exchange, as covered by $Misc^N_{New}$.

2.2 Size-threshold and age of private targets

Throughout our empirical analysis, we impose a minimum size-threshold for a private target (and subsidiary) to be included in the acquisition index N_{it}. The threshold is the year-end 1^{st} percentile of the market capitalization of all publicly listed firms in the target's respective Fama-French-12 industry.[7] Panel A of Figure 2 shows the relative size of the private targets produced by this threshold. Specifically, for each Fama-French-12 industry, we each year compute the ratio of the median target deal value to the median listed-firm value, and then report the average value of this ratio over the period 1980–2020. This setup allows us to compare private targets to the outstanding listed firms in its industry and year of acquisition specifically. As shown, this ratio varies between 18% (for Utilities) and as much as 163% (for Consumer durables), with six of the 12 industries having a relative-size ratio of 50% or more. In other words, from a pure size-perspective, N_{it} records what might be described as 'listable' private targets inside the public acquirer's own portfolio of consolidated companies.

To further support this intuition, Panel A of Figure 2 also plots industry ratios constructed using firm age since incorporation (birth) instead of value. Year of incorporation is found using data from Capital IQ as well as the Field-Ritter dataset of company founding dates.[8] As with value, the median private target of a public company is usually not too much younger than the median listed firm in its industry: The average ratio across industries is 67%, with the largest difference between targets and listed firms found in utilities (39%) and the smallest in telecommunications (108%). In other words, both in terms of age and value, our private-to-public targets tend to be smaller than the median listed firm—as is to be expected—but not by a large margin.

In Panel B of Figure 2, we also plot the median age of private-to-public targets and firms doing an IPO, without filtering by industry. This plot provides reveals two interesting

[7]To avoid a downward bias due to financial distress, we also require the firms used to identify this size threshold to be listed also in year $t + 1$. Panel A of Appendix Figure A.1 plots the annual distribution of this size threshold (solid black line) as well as the same threshold without a one-year survivorship requirement (dotted black line). As shown, eliminating the one-year survivorship requirement has a negligible impact on the size threshold.

[8]As used in Field and Karpoff (2002) and Loughran and Ritter (2004). Available at Jay Ritter's website: https://site.warrington.ufl.edu/ritter/ipo-data/. See also Ritter (2022).

patterns. First, private targets tend to be older than IPOs, but not by very much: on median (average), 12 (23) versus 8 (16) years, respectively, or around 50% older. Second, firms going public via IPO tend to be older after the listing peak than before it, with the annual median listing age averaging 7 in years 1981–1996 and 10 in years 1997–2020. However, this trend has reversed since the peak of 15 years median IPO age in 2009, and was back down to only 7 years old in 2020—the same level as at the listing peak in 1996.

Panel B of Appendix Figure A.1 shows the large number of post-1996 merger transactions that qualify as drivers of the wedge between the actual and merger-adjusted U.S. listing counts L and L_A. Of these, the most numerous are $Merge_{Private-to-Public}$ and $Merge_{Public-to-Public}$. Also shown are the total outflows (net of relistings) from the acquisition index N_{it} when public firms leave the exchange. The dark shaded area restricts N_{it} to public targets only, while the lighter shaded area also includes private targets. As shown, N_{it} is substantial and, naturally, lags both $Merge_{Private-to-Public}$ and $Merge_{Public-to-Public}$.

In the following, we proceed by first singling out the effect of public targets on the listing dynamics in a public-to-public merger-adjusted listing count. This involves adjusting Eq. (3) by excluding $Merge_{Private-to-Public}$ from the new lists and $Divest_{Subsidiary-to-Private}$ from the delists, and using N_{it} to track public targets only. The purpose of this separation is to highlight the impact of mergers between listed firms alone, without involving private targets. We then report results with the full merger-adjustment in Eq. (3)—also referred to as the all-merger-adjusted listing count.

2.3 Absence of a merger-adjusted listing peak

Figure 3 shows the actual listing count (the lowest of the three curves), the public-to-public merger-adjusted count (the middle curve), and the full merger-adjusted listing count (top curve), 1980-2020. Table 2 summarizes the total number of transactions driving ΔL and ΔL_A over both the total sample period and the post-peak period (1996–2020), with the annual counts of the different transaction types tabulated in Appendix tables A.1 and A.2.

Focusing first on the actual listing series in Table 2, over the 1980–2020 period, the values of

Newlists and *Delists* sum to 17,837 and 18,919, respectively, for a net decline ΔL(1980-2020) of -1,083 listed firms. This net decline is the result of the 10,567 IPOs (59% of *Newlists*) and the 6,792 miscellaneous additional new listings being offset by 18,919 delistings. The delistings are due to 10,063 acquisitions of public targets (of which roughly two-thirds involve public acquirers) plus 8,856 other delistings, of which 7,063 or 70% are due to cause. Over the post-1996 period, *Newlists* amounts to 7,004 and *Delists* to 10,696, which results in a much larger net decline ΔL(1996-2020) of -3,692 listed firms by 2020. This decline is primarily caused by a reduction in IPOs to 4,190 over the post-peak period, as well as the continued high merger activity involving public targets (3,734 public-to-public and 2,511 public-to-private transactions).[9]

Turning to the merger-adjusted series in Table 2, ΔL_A(1980-2020) totals 7,479 listed firms. This increase, which contrasts with the decline ΔL(1980-2020) of -1,083 companies, is the difference between $Newlists_A$ (28,021 firms) and $Delists_A$ (20,542 firms). For $Newlists_A$, the main addition comes from 9,481 private-to-public mergers—amounting to as much as 90% of the number of IPOs. In the post-1996 period, the merger-adjustment almost entirely eliminates the 1996 listing peak: ΔL_A(1996-2020) amounts to -84 firms only. In other words, while the actual listing in 2020 is down by 50% from the 1996-level, the adjusted count is down by less than one percent.

The elimination of the listing peak caused by the merger-adjustment has two main components. First, backfilling public targets in 3,734 public-to-public mergers after 1996, while tracking public targets only in the adjustment via the acquisition index (N_{it}), restores as much as two-thirds of the post-peak decline. The remaining third comes from the inflows of private targets net of subsidiary divestitures (with N_{it} including private targets as well).

Yet another perspective on the magnitude of the merger adjustment is seen by inspecting year 2020 in Figure 3 and Appendix tables A.1 and A.2. In 2020, the total merger-adjusted listing count is 12,195, while the actual count is 3,633. The difference of 8,562 firms are targets of public acquirers that operate under the ownership of their respective acquirers. Of these

[9] A little noticed fact: As much as 28% of *Newlists* are uplists from minor exchanges and over-the-counter (OTC) markets. Of the public-to-private transactions where the acquirer is a U.S. private firm, leveraged buyouts account for roughly one-third of the transactions, 1980–2020.

targets, about half were publicly traded before the merger. While all of these 8,519 firms have de facto entered into or remained under public ownership through the merger channel, none are included in the actual listing count.

In sum, while the actual listing count is a useful metric for examining changes in the size of stand-alone listed companies, it substantially underestimates the actual number of firms that flow into and are retained by public acquirers.

3 International merger-driven listing dynamics

In this section, we begin by providing evidence of a surprisingly high frequency of international listing peaks in calendar time. Conditional on observing a listing peak, we then examine how merger activity affects the speed of decline during the five years following the peak. This five-year period typically covers the bulk of the post-peak decline across countries. Finally, we examine whether merger activity affects the post-peak rate of decline differently in the U.S. than in foreign stock markets.

3.1 Country selection and data sources

As detailed in Appendix B.3, we start the country selection process with the 100 countries and territories with highest GDP as of 2020 per the IMF. Of these 100, 26 are excluded due to insufficient data, leaving a final sample of 74 countries. Using the IMF's classification, 33 of these 74 countries are advanced economies, representing 59% of global GDP. The remaining 41 countries are classified as developing and emerging economies, and represent 37% of world GDP.

The non-U.S. listing counts are identified from the World Bank's World Development Indicators (WDI), World Federation of Exchanges (WFE), ISI Emerging Market Group's CEIC database (CEIC), and individual stock exchange home pages. We count the number of listings on a country's major stock exchanges and only count cross-listed firms once (in the country where they are incorporated). Finally, we identify public-to-public and private-to-public (including cross-border) mergers for each country using SDC. To maximize SDC's data coverage of international mergers, we limit the sample to 1990–2020 when applying our merger adjustment.

While the above data sources track a country's aggregate listing counts and the number of mergers, it does not provide information on the identity of each listed company. Hence, when a foreign listing count decreases by one for reasons other than a public-to-public acquisition, that country's merger-adjusted listing count is also lowered by one ($N_{it} = 0$), while it is lowered by $1 + N_{it} \geq 1$ when a U.S. listed firm exits. By setting $N_{it} = 0$ across foreign stock markets, we overstate foreign merger-adjusted listing counts in the comparison with the U.S. below. We later illustrate the magnitude of this difference, which implies a relative U.S. listing penalty, after estimating the U.S. listing gap in Section 4.

3.2 Listing peaks in calendar time

In our definition, a listing peak occurs if the country's unadjusted listing count is lower in 2020 than in a previous year during our sample period, where the listing-peak year is the year with the highest listing count. Figure 4 plots the number of countries that experience a listing peak in each year from 1975–2019. It shows that listing peaks are not only numerous, but also distributed throughout the sample period—a pattern common to both advanced and developing/emerging economies.

Figure 5 further details these peaks by showing how the listing count has decreased from peak until 2020 for each of the 74 countries. In Table 3 we also order countries according to listing-peak year and divide the sample into four non-overlapping categories: advanced/non-advanced countries with/without a peak. Columns (2) and (3) if this table show the number of listed firms at peak and the listing count in 2020, while Column (4) shows the total percent change in the listing count between the peak year and 2020, with the average annual percent change in Column (5). As discussed next, this international listing-peak information yields five important and surprising facts.

First, experiencing a listing peak is the norm rather than the exception: Among the 33 advanced economies alone, as much as 82% (27 economies) exhibit a listing peak—five before the U.S. and another 21 in 1996 or later.[10] A similar proportion of developing and emerging countries also experience a listing peak: 31 of 41 (76%). In sum, more than three-quarters (58 of 74) of all sampled countries have fewer listed firms in 2020 than in the past.

Second, the total number of listing peaks is widely distributed across the period 1985–2019, with the greatest number of peaks in 1998. The average peak year for the advanced countries is 2000 with a standard deviation of 8 years. For the developing and emerging economies, the average peak year is 2001 with a standard deviation of 10 years. The substantial international variation in the year of the listing peak is interesting as it suggests that these peaks are largely driven by country-specific factors rather than global macroeconomic shocks common to all countries. While identifying these factors goes beyond the purpose of this paper, we examine certain country-level macroeconomic variables in Section 3.5 below.

Third, just as the U.S. experiences a 50% post-peak decline in the listing count, the average decline across all advanced economies with a listing peak is 49%, with fifteen advanced countries experiencing an even greater overall decline than in the U.S. Fourth, while the annual percent decline in the number of lists since the peak year is 2.1% for the U.S., the average rate of decline for advanced economies is slightly higher: 2.5%. More than half (16 of 27) of advanced countries experiencing a higher rate of decline than the U.S. Similar results hold for developing and emerging economies, with an average decline of 33% at an annual rate of 2.2%. Fifth, the earlier in the sample period that a country peaks, the lower is the 2020 listing count relative to the peak count. The correlation between number of years passed since the peak and the percent decline is 65%, which suggests that the post-peak listing decline tends to persist over time.

3.3 Listing peaks in event time

Conditional on experiencing a listing peak, Panel A of Figure 6 (enumerated in the Internet Appendix) shows the average listing pattern over the eleven-year event period (-5,5) centered

[10] The six advanced economies that have not peaked by 2020 are Hong Kong, Italy, Japan, South Korea, Sweden, and Taiwan. The earliest advanced economies to peak are Denmark and New Zealand in 1986 and the most recent is Australia in 2017. Among developing economies, the first country to peak is Argentina in 1975, while Sri Lanka peaks last in 2018.

on the peak year (year 0). It reveals that the shapes of the three U.S., non-U.S. advanced, and developing/emerging listing patterns are surprisingly similar both in terms of the pre-peak incline and post-peak decline. Focusing first on the pre-peak runup period for advanced countries, the U.S. experiences a 24% runup over the (-10,0) period and a 29% runup over the shorter (-5,0) event period. For other advanced (developing/emerging) economies, the runup averages 65% (87%) over the (-10,0) period and 51% (40%) for the (-5,0) period. This shows that, as in the U.S., these pre-peak runups are on average large and concentrated in the (-5,0) event period for advanced and developing/emerging economies alike.

Turning to the post-peak event period, the actual U.S. listing count declines -24% over the (0,5) period and -37% over the longer (0,10). For advanced (developing/emerging) economies, the decline over these two event periods average -24% (-22%) and -32% (-30%) and for the 11-year and 21-year event periods, respectively. This shows that the average annual rate of listing decline is also similar across the U.S. and other countries, and that the bulk of the decline occurs quickly—within the event period (0,5) for four-fifths of the countries. In sum, the (-5,5) event period catches the bulk of the listing runups and declines around the peaks. Next, we present a cross-country analysis of the impact of mergers on the rate of post-peak listing decline that focuses on the (0,5) event window.

3.4 Merger-propensities and merger-adjusted listing counts

We begin by illustrating international differences in merger propensities. Panel A of Figure 7 shows the international average annual merger rate per listed firm where at least one of the two parties to the transaction is a public company, while Panel B further restricts the mergers to deals between two public firms. In Panel A, a U.S. public firm has a 10.2% chance of being involved in an M&A transaction in average year 1990-2020, while this equivalent number is only 2.9% for non-U.S. advanced economies and 1.0% for developing and emerging economies.[11] For the public-to-public merger deals in Panel B, the annual U.S. merger propensity is 2.7% versus 0.3% (0.2%) in non-U.S. advanced (developing/emerging) economies. In sum, the U.S.

[11] This evidence is consistent with Doidge, Karolyi, and Stulz (2017), who show that the U.S. merger delist rate is higher than for an aggregate of non-U.S. countries.

likelihood of a merger is noticeably higher than the likelihood in any other country in our sample. Moreover, this difference is even more pronounced for the public-to-public mergers in Panel B. This also suggests that the effect of mergers on listing dynamics will be stronger in the U.S. than in other countries, which our analysis below confirms.

In Figure 8, we plot the public-to-public merger-adjusted (Panel A) and all-merger-adjusted (Panel B) event-time average listing patterns with the window (-5, 5) around the peak year. Panel A shows that the public-to-public merger-adjusted listing count on average declines by 22% for non-U.S.-advanced and by 21% for developing and emerging economies in the five years following the listing peak. This contrasts with the U.S. public-to-public merger-adjusted series, which declines by 5% only. In other words, while the U.S. post-peak listing decline is to a great extent driven by a reallocation of corporate resources among public firms, declines elsewhere are far less attenuated by public-to-pubic mergers. Instead, these declines represent outflows of listed firms from public markets.

The all-merger-adjusted series in Panel B of Figure 8 also includes private-to-public mergers. This incremental adjustment reduces the decline in the non-U.S. advanced (developing/emerging) economies from an average of 22% to 10% (21% to 18%). This means that, internationally, targets entering public markets via private-to-public mergers significantly outnumber targets retained via public-to-public mergers. In the U.S., the addition of private-to-public mergers changes the adjusted listing count from a 5% decrease to a 13% increase. As Figure 7 suggests as well, this shows that the marginal impact of private-to-public mergers on the listing dynamics is also greater in the U.S. than elsewhere.

3.5 Determinants of the post-peak rate of listing decline

To examine the U.S.-specific effect on the post-peak decline speed, let $Decline_{Ti}$ denote the average annual rate of decline (in percent) in the number listed firms for country i in the $T = 5$ years (alternatively, $T = 3$) after that country's listing peak. $Decline_{Ti}$ is either the unadjusted listing count, the public-to-public merger-adjusted listing count, or the full merger-adjusted count. We run the following cross-sectional regression:

$$Decline_{Ti} = \alpha + \beta D_{US} + \lambda Z_{Ti} + \epsilon_{Ti}, \quad i = 1, ..., N, \tag{5}$$

where D_{US} is a dummy taking a value of one if the country is the U.S. and zero otherwise. The vector Z_{Ti} is a set of pre-peak country-specific control variables using data from the World Bank and IMF. Each variable is computed as the annual T-period average prior to the listing-peak year of country i. The pre-peak growth variables are *Listing count runup* (the percent growth in the unadjusted listing count) and *GDP growth*. The GDP-scaled variables are *Trade* (the sum of exports and imports) and *FDI net inflows* (foreign direct investment). Finally, population-scaled variables are *Patent applications* and *GDP*. The patent applications are restricted to those filed by domestic firms and residents. We use patents to measure innovation activity because they are more consistently recorded across countries than are data on R&D expenditures.

The regression results are reported in Table 4. Odd-numbered columns use all available countries, while the even-numbered columns are based on advanced economies only. In columns (1)–(4), the dependent variable is the rate of decline of the unadjusted listing count. Note first that D_{US} is insignificant in Column (1) (all countries) and in Column (2) (advanced economies). This implies that the U.S.-specific five-year average annual rate of post-peak decline is statistically indistinguishable from other countries. The same holds for columns (3) and (4), in the three-year post-peak period.

Columns (5)–(8) of Table 4 show the regression results when $Decline_{Ti}$ is the post-peak annual average rate of decline of the public-to-public merger-adjusted listing series. Most important, D_{US} now receives a negative and statistically significant coefficient estimate—implying a significantly slower rate of post-peak decline in the merger-adjusted listing series. The coefficient on D_{US} is estimated at -2.2 to -2.6 percentage points for the five-year event window and from -4.2 to -4.9 for the three-year window. Importantly, the fact that the merger adjustment *lowers* the coefficient estimate of D_{US} when going from columns (1)–(4), means that there is a U.S.-specific effect of public-to-public mergers that reduces the speed at which listed firms leave the stock exchange. Between columns (1)–(4) and columns (5)–(8), the U.S.-specific effect of public-to-public merger activity decelerates the speed of decline by 3.5 pps, relative to other countries.

It is worth reemphasizing the above interpretation of the coefficient estimates on D_{US}. They show that U.S. public-to-public merger activity reallocates target firms *within* the stock exchange to a greater extent than in other countries. This interpretation follows because, when going from, say, columns (1) to (5), we are *only* changing the dependent variable $Decline_{Ti}$. As a result, the significant decline in the coefficient estimate on D_{US} means that public-to-public merger activity slows down the post-peak rate of decline relative to other countries.

In columns (9)–(12), $Decline_{Ti}$ is measured using the full merger-adjusted listing count series. Again focusing on D_{US} and the total sample of countries, recall that the full merger adjustment adds private-to-public acquisitions to the listing count. The marginal decline in the coefficient estimate for D_{US} by 1.4 pps to 2.2 pps when going from columns (5)–(8) to (9)–(12) is evidence that the U.S.-specific effect of private-to-public acquisitions is smaller than the case is for public-to-public mergers. Furthermore, it confirms that what distinguishes the post-peak U.S. merger activity is less an inflow of private targets than the effective retention of listed targets through public-to-public mergers. This result is also noticeable by comparing Panels A and B of Figure 8, which shows a somewhat similar private-to-public effect on US and non-US advanced, but a noticeably different public-to-public effect.

Finally, we test whether role of post-peak merger activity documented above for the U.S. is unique. In Table 5, we estimate country-by-country regressions where we replace the U.S. dummy D_{US} in Eq. (5) with a dummy for each respective non-U.S. country. In the sample of advanced economies, this replacement fails to produce a significantly negative country dummy when using the merger-adjusted listing series (columns 5–12) for all non-U.S. countries with insignificant or positive unadjusted dummy estimates (columns 1–4). This reinforces the notion that the significant effect of merger activity on the rate of post-peak listing decline is uniquely strong in the U.S.—primarily due to public-to-public mergers.

4 Merger-adjusted U.S. listing gap estimation

As shown by Doidge, Karolyi, and Stulz (2017), the actual U.S. listing count has developed a listing gap relative to an international listing trend line estimated from 1990. In this section, we revisit their listing gap estimation using our merger-adjusted listing series. Our evidence above suggests that inferences about a relative U.S. listing gap may well differ when adjusted for merger activity. To address this issue, we replace the actual listing count for all countries with our merger-adjusted count as the dependent variable in the list-gap estimation. Rather than correlate aggregate merger activity with the actual listing dynamics, this replacement allows us to draw causal inferences about the impact of merger activity. We first describe the econometric specification of our listing-gap regression, and then present the gap-parameter estimates.

4.1 Econometric specification

The U.S. listing gap in year t is defined as the difference between two conditional expected listing counts. The first difference is the expected number of U.S. listings in year t relative to the base year 1990. Let D_{US} denote a dummy variable with a value of one if the country is the U.S. and zero otherwise. The first difference is then

$$E(Y_{it} \mid D_{US} = 1, year = t) - E(Y_{it} \mid D_{US} = 1, year = 1990). \tag{6}$$

The second difference is between the expected number of listings in a non-U.S. country in year t and that in 1990:

$$E(Y_{it} \mid D_{US} = 0, year = t) - E(Y_{it} \mid D_{US} = 0, year = 1990). \tag{7}$$

We estimate the listing gap parameter (the two differences in conditional means) across a total of 30 years and N countries using the following panel regression:

$$ln(Y_{it}) = \alpha + \delta_i + \tau_t + \beta D_{US} + \Gamma(D_{US} \times \tau_t) + \lambda X_{it} + \epsilon_{it}, \quad t = 1990, .., 2020, \quad i = 1, .., N. \tag{8}$$

The dependent variable Y_{it} is country i's listing count (L) per capita (Pop) or per GDP in year t, and δ_i and τ_t are country and year fixed effects, respectively. X_{it} is a vector of three country-specific control variables: country i's anti-self-dealing index (Djankov, Porta, Lopez-de-Silanes, and Shleifer, 2008), log(GDP/Pop) and annual GDP growth.

Hence, ignoring the country-specific parameters λ_i and δ_i (since these cancel out in the difference below), the gap-parameter in year t is:

$$\begin{aligned}
[E(Y_{it} \mid D_{US}=1, year=t) &- E(Y_{it} \mid D_{US}=1, year=1990)] \\
-[E(Y_{it} \mid D_{US}=0, year=t) &- E(Y_{it} \mid D_{US}=0, year=1990)] \\
&= [(\alpha+\tau_t+\beta+\gamma_t)-(\alpha+\beta)]-[(\alpha+\tau_t)-\alpha] \\
&= \gamma_t, \qquad (9)
\end{aligned}$$

where γ_t—the annual parameter in the vector Γ—captures the U.S.-specific residual in year t. For a given γ_t, we then compute the U.S. listing gap in year t (expressed as the number of firms) as follows:

$$\text{US gap computation, year } t\text{:} \quad \begin{cases} Y_{US,1990} \times Pop_{US,t} \times (e^{\gamma_t}-1) \text{ for } L \text{ scaled by population} \\ Y_{US,1990} \times GDP_{US,t} \times (e^{\gamma_t}-1) \text{ for } L \text{ scaled by GDP} \end{cases} \qquad (10)$$

In other words, computing the U.S. listing gap for year t in terms of the total number of firms involves multiplying three items: the U.S. listing count per capita or GDP in 1990, the corresponding population or GDP scaling variable in year t, and the antilogarithm of γ_t minus one.[12]

To show clearly the marginal impact of our novel listing count adjustment, we fix the

[12]Our econometric specification of the U.S. listing gap differs somewhat from that of Doidge, Karolyi, and Stulz (2017). We provide a detailed explanation of this econometric differences in the Internet Appendix.

right-hand-side of Eq. (8) and gradually develop the following three listing gaps:

$$\text{Gap} \begin{cases} \text{G1:} \quad Y_{it} \ is \text{ unadjusted (the actual listing gap).} \\ \text{G2:} \quad Y_{it} \ is \text{ public-to-public merger-adjusted only, with } N_{it} = 0 \text{ for non-U.S. countries.} \\ \text{G3:} \quad Y_{it} \ is \text{ merger-adjusted, with } N_{it} = 0 \text{ for non-U.S. countries.} \end{cases} \tag{11}$$

In G1, the numerator of the dependent variable Y_{it} is the actual (unadjusted) listing count for all countries. For the U.S., G2 adjusts the actual listing count for public-to-public mergers and spinoffs and, therefore, the acquisition index N_{it} tracks public targets only. Moreover, for the U.S., G3 fully tracks inflows and outflows of all firms—both public and private—to and from U.S. public markets using the full Eq. (3) and an acquisition index N_{it} in Eq. (4) that tracks both public and private targets.

4.2 Listing gap estimates

Figure 9 plots the annual U.S. listing gap estimates for all three gap definitions G1–G3 in Eq. (11) using the full set of 74 countries. A complete set of annual coefficient estimates for the gaps, each with four different regression specifications, is listed in Table 6. In the discussion below, we primarily focus on the regression specification with the listing count scaled by population and including country fixed effects (columns 2, 6, and 10). Table 6 also reports three alternative regression specifications: (i) the dependent variable scaled by population and without country fixed effects, (ii) the dependent variable scaled by GDP and with country fixed effects, and (iii) the dependent variable scaled by GDP but without country fixed effects (the GDP-based listing gap estimates with country fixed effects are further illustrated in the Internet Appendix).

We begin with the U.S. unadjusted listing gap (G1), which is shown as the solid black line in Panel A of Figure 9. The gray shaded area is the 90% confidence interval around the annual gap estimates (with standard errors clustered by country). The coefficient estimates corresponding to the black line are shown in Column (2) of Table 6, where $ln(Y_{it})$ is natural logarithm of the actual listing count scaled by population and including country fixed effects. Using Eq. (10), the estimate of γ_t in Column (2) of Table 6, and population data from the IMF, the estimated G1-gap in year 2020 is $Y_{US,1990} \times Pop_{US,2020} \times (e^{\gamma_t} - 1) = 22.78 \times 330.01 \times (e^{-0.636} - 1) = -3,538$ listed companies. In 2012, which is the final sample year in Doidge, Karolyi, and Stulz (2017), $G1 = Y_{US,1990} \times Pop_{US,2012} \times (e^{\gamma_t} - 1) = 22.57 \times 314.12 \times (e^{-0.631} - 1) = -3,348$ listed companies.

Doidge, Karolyi, and Stulz (2017) instead report a listing-gap estimate of -5,436 listed firms for 2012. In terms of the regression parameters in our Eq. (8), their regression specification is equivalent to using $\gamma_t + \tau_t$ to estimate the listing gap G1 (see Internet Appendix for proof). In other words, the difference between our G1-gap for 2012 of 2,088 listed firms and the larger number reported by Doidge, Karolyi, and Stulz (2017) emerges primarily because we subtract out the common component (the time trend τ_t) in the listing dynamic before computing G1. By netting out the time trend in the panel estimation, our gap estimate is restricted to the portion of the international time trend that is unique to the U.S. As shown in the Internet Appendix, the time trend parameter estimates of τ_t become negative and statistically significant after 2009, hence causing the gap-estimates in Doidge, Karolyi, and Stulz (2017) to have larger negative values.

Panel A of Figure 9 also shows the full merger-adjusted listing gap (G3), which is again computed using our main regression specification, this time with the γ_t coefficient estimates shown in Column 10 of Table 6). Adjusting for both public-to-public and private-to-public merger activity causes G3 to be positive and statistically significant in years 1993–1999, and insignificant in all sample years thereafter. In year 2020, the estimated G3-gap is $Y_{US,1990} \times Pop_{US,2020} \times (e^{\gamma_t} - 1) = 22.78 \times 330.01 \times (e^{0.005} - 1) = +38$ listed companies (a statistically insignificant listing surplus). The absence of a listing gap 1991–2020 holds across the three alternative regression specifications for G3.

The broken line in Panel B of Figure 9 shows G2, the public-to-public merger-adjusted listing gap, from 1991–2020. This broken line is based on the γ_t coefficient estimates shown in Column (6) of Table 6. Recall that, while all countries are adjusted for public-to-public mergers, the acquisition index N_{it} (which, in G2, accumulates public targets only) is applied exclusively to U.S.-listed firms when these firms leave the exchange, which lowers the merger-adjusted U.S. listing count relative to other countries. Nevertheless, the estimates of G2 are statistically insignificant at conventional levels in all sample years 1991–2020. In year 2020, the estimated G2-gap is $Y_{US,1990} \times Pop_{US,2020} \times (e^{\gamma_t} - 1) = 22.78 \times 330.01 \times (e^{-0.138} - 1) = -966$ listed companies. Also important, G2 is statistically insignificant at conventional levels in all years, and across almost all years of the three alternative regression specifications in columns (5), (7), and (8) of Table 6.

In sum, we have shown that the merger-adjusted listing gap is statistically insignificant for both gap definitions G2 and G3. Importantly, since a public-to-public merger does not rely on the supply of private equity capital, it is not necessary to appeal to the contemporaneous growth in private equity funding or decline in IPOs to explain the actual U.S. listing gap G1. Rather, our evidence is consistent with the notion that the extraordinary propensity of U.S. stock exchanges to effectuate large merger transactions between public companies is sufficient to explain G1. Since these transactions require a high level of capital market functionality in terms of contracting technology and legal protection of minority shareholders, they may provide U.S. listed firms with a comparative advantage in terms of realizing scale economies through external growth strategies.

4.3 Robustness issues

In this section, we examine several robustness issues. The first is whether the statistical insignificance shown for the merger-adjusted listing gap (G2 and G3) also holds for the subsample of 28 advanced economies. Table 7 shows the parameter estimates restricted to this subsample. Note first that the unadjusted gap G1 is now somewhat larger in size and remains significant at the 1% level or higher. Moreover, the merger-adjusted gaps G2 and G3 are also larger (more negative) than for the full sample of 74 countries. Most important, G2 and G3 remain insignificantly different from zero in nearly all years up through 2020. In other words, the merger-adjusted U.S. listing gap is statistically insignificant also when measured relative to the subgroup of other advanced economies, which contain the most internationally competitive stock exchanges.

Second, we address SDC as a source of merger data, which may be more comprehensive for the U.S. than for some foreign exchanges. While not tabulated, we re-estimate Eq. (8) after artificially multiplying the annual number of public-to-public mergers outside of the U.S. The result of this experiment is that most estimates of G2 and G3 remain statistically insignificant even after *quintupling* non-U.S. public-to-public mergers. Furthermore, when we in addition nearly triple the foreign private-to-public acquisitions (which include cross-border mergers), the all-merger-adjusted gap G3 continues to be similarly insignificant. We conclude from this that our main finding of a statistically insignificant merger-adjusted U.S. listing gap is robust to any reasonable level of missing data on foreign mergers in SDC.

Third, recall from Section 3.1 that, since our data sources on the international listing counts do not track the names of the listed firms, we necessarily set the acquisition tracking index to zero ($N_{it} = 0$) for non-U.S. countries. It is worth pointing out that this differential treatment of N_{it} substantially penalizes the U.S. merger adjustment. Specifically, for U.S. listed firms that exit the stock exchange over the period 1991–2020, the tracking index amounts to $\sum_{i=1}^{N} \sum_{t=1991}^{2020} N_{it} = 4,459$ additional delists.[13] With 1990 as base year, this penalty lowers the 2020 merger-adjusted U.S. listing count by as much as 42% (from 10,700 firms when $N_{it} = 0$ to 6,241 firms). Our finding of a statistically insignificant merger-adjusted listing gap withstands this U.S.-specific penalty.

5 U.S. transaction values and firm performance

The above evidence suggests that the strong U.S. merger channel represents a relative listing advantage. In this section, we present evidence confirming the likely value and productivity of the this channel. We first ask whether net firm-value inflow—inflows minus outflows generated by the full anatomy of U.S. listing changes—is higher in the post-peak period than between 1980 and 1996. Second, we ask whether the merger activity that drove much of the post-1996

[13]Breaking the total of 4,459 firms into public and private targets, respectively, this treatment effectively cancels out as much as 21% (1,286 of 6,144) of public-to-public mergers and 33% (3,173 of 9,481) of private-to-public mergers.

listing decline was predominantly value increasing. Finally, we examine whether firms that remain listed after 1996 have been able to maintain or improve on the pre-1996 contribution to several measures of aggregate economic activity—presumably with the help of their respective targets.

5.1 Transaction values of inflows and outflows

Figure 10 shows the contribution of each of the listing channels in terms of the annual transaction net value inflow to public markets, ΔV_A (inflation-adjusted to 2020). Since the market value of a public firm that delists directly accounts for any value-implications of the firm's acquisition history, ΔV_A is constructed using $Merge_{Public-to-Private}$ and not $Merge^N_{Public-to-Private}$. Over the period 1980–2020, total inflow amounts to $Newlists_A = \$11.1$ trillion, while total outflow is $Delists_A = \$8.2$ trillion. The difference of \$2.9 trillion is also shown in the left-side vertical axis for the solid curve in Figure 10. \$1.2 trillion of the net inflow is added between 1980–1996 and the remaining \$1.7 trillion is added *after* the listing peak.

While we noted above that the number of private-to-public acquisitions number as much as 90% of the number of IPOs, switching to dollar values changes this picture because the average private-to-public target is smaller than the average IPO firm. In terms of dollar values, $Merge_{Private-to-Public}$ constitutes 28% of $IPO + Merge_{Private-to-Public}$ (\$2.5/8.7 trillion). Also interesting, on the delist side, $Merge_{Public-to-Private}$ accounts for as much as 80% (\$6.6/8.2 trillion) of the total transaction value of delisting outflows. Moreover, while not shown, the value of $Merge_{Public-to-Public}$—which reflects the reshuffling of assets already on the exchange—is 1.6 times that of $Merge_{Public-to-Private}$ (\$10.7 trillion versus \$6.6 trillion).

Beyond the substantial (\$10.7 trillion) transaction value of public-to-public mergers, it is also interesting to note that the \$2.9 trillion net transaction-value inflow shown in Figure 10 represents no more than 8% of the total market-value increase of \$34.9 trillion on NYSE, AMEX, and Nasdaq from 1980–2020. In other words, as much as 92% of the total market-value increase during this period is generated on the stock exchange: a combination of organic growth (internal investments and revaluation of assets in place) and synergies generated by public-to-public merger activity. To our knowledge, this evidence is also new to the literature, and made possible by our measurement of the complete anatomy of transactions causing listing changes.[14]

5.2 Post-peak economic activity of listed firms

In this section, we address three questions of relevance for how to interpret the underlying economic relevance of our U.S. merger-adjustment: What triggered the merger wave of the 1990s? Did this merger wave increase shareholder value? Did the post-1996 listing decline slow economic activity of listed firms? As to the first question, the most powerful answer in the literature is given by Harford (2005). He shows that six of eleven industry-specific deregulatory events between 1981 and 1996 took place after 1990. The resulting increase in product market competition appears to have triggered several rival firms to merge with the objective of lowering operating costs. Also important, the evidence in Harford (2005) and other studies rejects the alternative notion that the merger wave of the 1990s was 'market driven' (bidder opportunism) in the vernacular of Shleifer and Vishny (2003).[15]

Panel A of Figure 11 addresses the question concerning shareholder wealth effects of the merger wave. Focusing on the Fama-French-49 industries it addresses whether the industry-specific merger waves involving public-to-public mergers were 'synergistic' in the sense of increasing the combined market values of bidder and target firms. We follow John, Kadyrzhanova, and Lee (2022) and classify an industry-year as experiencing a 'synergy wave' if the number of deals with positive combined bidder and target wealth effect (CWE) is one standard deviation above the time-series industry median. We restrict the sample to mergers between listed firms and calculate CWE as the value-weighted average of the bidder and target's seven-day cumulative abnormal return, CAR(-3,3), where day zero is the first public announcement of

[14]Internet Appendix Figure 1 breaks down net listing value inflows by industry. The figure shows that the net firm value inflow over the total sample period 1981–2020 is largest in the high-tech industries. Moreover, roughly half of the net high-tech inflow occurs in business services and electronics, while the industry with the largest net outflow is chemicals and allied products (mostly pharmaceuticals).

[15]See also Rhodes-Kropf, Robinson, and Viswanathan (2005), Phillips and Zhdanov (2013) and Eckbo, Makaew, and Thorburn (2018) for evidence on how U.S. merger waves correlate with the relative market-to-book ratios (M/B) of bidder and target firms.

the merger given by SDC.[16] As Panel A shows, synergistic merger waves occur to a higher degree during the second half of the 1990s than during any other period, 1980–2020. This evidence supports the hypothesis that the merger activity that drove much of the post-1996 U.S. listing decline predominantly increased the combined value of the merging firms.

Panel B of Figure 11 addresses the third question concerning the post-1996 economic activity of listed firms. It shows the time series from 1982 through 2018 of the annual percent contribution of U.S. domestic listed firms to aggregate labor employment, GDP, R&D spending, and patents. As detailed in Appendix B.2, we generate the figure using data from the Bureau of Economic Analysis, Bureau of Labor Statistics, Compustat, IMF, OECD, University of Virginia Darden Global Corporate Patent Dataset, and U.S. Patent and Trademark Office. We follow Schlingemann and Stulz (2022) and measure GDP (employment) as the sum of value added (employment) generated both domestically and by majority-owned foreign affiliates. While they do not study patents and R&D, we adjust R&D for foreign affiliates in a similar fashion.[17]

As shown in Panel B, notwithstanding the post-1996 drop in the actual listing count, there is little evidence that the remaining listed firms contribute less to the macroeconomic time series. Specifically, in the post-1996 period, the ratio of U.S. workers employed by public firms is 25.5% in 1996 and 23.8% in 2018 (the last year of information on foreign affiliates in BEA), while the value added by public firms to U.S. GDP is 26.7% in 1996 and 28.5% in 2018. Also important, there is a substantial increase in innovation activity of U.S. listed firms as a fraction of all U.S. entities (public and private firms, governmental agencies, universities, and individuals): R&D spending increases from 54.5% to 68.7% (1996–2018), while granted patents relative to all entities increases from 40.8% to 49.7% (1996–2016). We conclude from Panel B that the post-1996 merger-driven listing decline in important ways has increased rather than decreased the contribution of listed firms to the U.S. economy.

[16]CAR is the difference between the realized and the value-weighted market returns from CRSP. The pre-announcement market value of the bidder and the target is measured one month before the deal announcements. Due to missing data, the sample consists of 3,923 public-to-public mergers, or around two-thirds of all of the public-to-public mergers in our sample.

[17]With a sample period that starts in 1973, Schlingemann and Stulz (2022) show that the proportion of U.S. employment and GDP attributable to listed firms declines prior to the early 1990s for then to increase. The late-period increase i GDP is confirmed below as well.

6 Conclusion

Listing dynamics, which results from stock-markets attracting and retaining firms under public ownership, depends not only on changes in the net listing benefits of stand-alone entities but also on gains from merging with public acquirers. While extant research primarily attributes the dramatic post-1996 U.S. listing decline to a reduction in net listing benefits, we instead focus on the merger channel. Our empirical methodology is novel in that it directly adjusts, at the firm level, for the targets of public acquirers—creating a causal relationship that we label merger-driven listing dynamics. While actual listing series count stand-alone firms only, our merger-adjustment also recognizes that a delisted firm may continue inside its public acquirer, and that a private firm may choose a sellout to a public acquirer rather than going public. Our merger-adjusted listing count explicitly accounts for this external growth in firm size, which the actual listing count does not.

Using the full anatomy of U.S. lists and delists over the period 1980–2020, we show that targets of public acquirers exceed stock market entries via IPOs both in number and transaction value. Moreover, accounting for these targets eliminates the post-1996 decline in the U.S. Furthermore, our international evidence on merger-driven listing dynamics uncovers a unique ability of U.S. stock markets to attract and retain firms under public ownership. This inference is based on three additional findings. First, we discover that as much as four-fifths of countries experience listing peaks followed by a 'U.S.-style' decline, with their peaks distributed widely over the past four decades. Second, exploiting this cross-country variation reveals that, internationally, the merger channel plays a significantly weaker role than in the U.S. in explaining the post-peak rates of listing decline. Rather, in other countries, post-peak listing declines tend to reflect outflows of firms from public markets rather than retentions within public acquirers.

Third, and equally important, our merger adjustment eliminates the so-called 'U.S. listing gap', which the extant literature suggest is caused by a relative decline in U.S. net listing benefits. Instead, our evidence points to a distinct U.S. listing *advantage* by providing access to a well-functioning market for complex merger transactions. While the efficiency of U.S.

merger transactions is shown in extant research, we further support this notion through our evidence that the net transaction value (inflows net of outflows) increased after 1996, and that the contribution of the remaining listed firms to employment and GDP did not fall between 1996 and 2020. Moreover, listed firms' share of R&D and patents has increased substantially.

Finally, the surprisingly high frequency of international listing peaks—and their surprising similarity to the U.S. 1996-peak in terms of the rates of incline and decline—raises questions of what constitutes the fundamental drivers of listing dynamics. While our analysis controls for mergers as well as country-level differences in factors such as macroeconomic growth, trade, and innovation activity, additional research is required to identify the timing of the peaks and, by extension, why some countries have yet to experience such peaks.

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Table 1: Definition of variables representing actual and merger-adjusted new lists and delists

Definition	Data sources (further details in Appendix B.1)
A: New lists	
IPO Initial public offering on NYSE, AMEX, or Nasdaq.	Matched to IPO data from SDC and Jay Ritter's webpage, counting U.S. operating companies only.
$Spin$ Divisional spin-off from a U.S. public company.	Identified in CRSP (distribution code 3763) and SDC (acquirer name 'shareholders'). Spin-off parent is confirmed as U.S. public using CRSP. Includes equity carve-outs (for cash).
$Misc_{New}$ Relist, uplist, CRSP reorganization (when a merger of equals results in the creation of a new firm), CRSP form change (to U.S. common stock and/or U.S. incorporation, and also when a SPAC acquisition is completed), or unidentified new list.	Relists, reorganizations, and form changes are identified in CRSP. Remaining new lists are classified as uplists, and verified when possible using OTC data from WRDS, SDC (by identifying 'follow-on' listings that occur simultaneously with a new listing), and manual web searches.
$Merge_{Private-to-Public}$ Private-to-public merger: acquisition in which a U.S. public company acquires a non-public corporation (foreign, private, or OTC firm). Does not include SPAC acquisitions, since SPACs (with other investment vehicles) are not counted as 'public'.	Mergers are completed transactions that are identified in SDC using the deal forms 'merger', 'acquisition', and 'acquisition of remaining-, partial- and majority interest', and result in 100% ownership. Targets must have a greater market value than the first percentile of same-industry (using Fama-French 12 industry definitions) public firms that remain listed one year later. Percentiles are determined using data from CRSP.
B: Delists	
$Merge_{Public-to-Public}$ Public-to-public merger: a merger between two publicly listed U.S. companies.	Merger delistings are identified in CRSP using acquiring PERMCO and PERMNO (delisting codes 200-399). Acquirer identity is found in SDC, CRSP, and manually with web searches.
$Merge_{Public-to-Private}$ Public-to-private merger: merger in which a U.S. public firm is acquired by a foreign, private, or OTC firm.	Same as above.
$Misc_{Del}$ Delist due to cause, voluntarily, or for unknown reasons.	Cause delists are identified in CRSP using delisting codes 400-569 and 574-999, and voluntary delists with codes 570-573. Unknown delistings are not marked in CRSP by a delisting code, but occur when the firm leaves the CRSP sample of U.S. public firms for more than two weeks for reasons other than trading suspensions.
$Divest_{Subsidiary-to-Private}$ Subsidiary-to-private divestiture: acquisition of a U.S. public-owned subsidiary by a private, foreign, or OTC firm.	Takeovers are identified in SDC (excludes deals with acquirer name 'shareholders'). Minimum target size threshold is calculated using CRSP and is the same as that of $Merge_{Private-to-Public}$. Subsidiary parent is confirmed as U.S. public using CRSP. The subsidiary itself must not be publicly listed.

Table 2: Summary of actual and merger-adjusted U.S. listing counts, 1980–2020

Listing counts are given by equations (2) and (3) in the text and are replicated in the expressions shown below. The merger-adjusted listing count also uses the cumulative acquisition index in Eq. (4). ΔL is the change in the actual listing count, ΔL_A is the change in the merger-adjusted listing count, IPO counts initial public offerings, $Spin$ counts spinoffs, $Misc_{New}$ counts miscellaneous new listings, and $Misc_{Del}$ counts miscellaneous delists. The subscript in $Merge$ indicates the direction of the change in the target's public/private status. Thus, in $Merge_{Public-to-Public}$ and $Merge_{Public-to-Private}$ a public target merges with a public or a private acquirer, respectively, while a private target merges with a public acquirer in $Merge_{Private-to-Public}$. In Panel B, the acquisition index N tracks all public and private targets. See also Table 1 for variable definitions. The annual distribution of all variables in this table is found in Appendix Table A.1 for Panel A and Appenix Table A.2 for Panel B.

A: Actual listing count

A.1 Total sample period (12/31/1980–12/31/2020)

$$\Delta L = -1,083 \begin{cases} 17,837\ Newlists = 10,587\ IPO + 458\ Spin + 6,792\ Misc_{New} \\ 18,919\ Delists = 6,144\ Merge_{Public-to-Public} + 3,919\ Merge_{Public-to-Private} + 8,856\ Misc_{Del} \end{cases}$$

A.2 Post-peak sample period (12/31/1996–12/31/2020)

$$\Delta L = -3,692 \begin{cases} 7,004\ Newlists = 4,190\ IPO + 293\ Spin + 2,521\ Misc_{New} \\ 10,696\ Delists = 3,734\ Merge_{Public-to-Public} + 2,511\ Merge_{Public-to-Private} + 4,451\ Misc_{Del} \end{cases}$$

B: Merger-adjusted listing count

B.1 Total sample period (12/31/1980–12/31/2020)

$$\Delta L_A = +7,479 \begin{cases} 28,021\ Newlists_A = 10,587\ IPO + 9,481\ Merge_{Private-to-Public} + 7,953\ Misc^N_{New} \\ 20,542\ Delists_A = 7,900\ Merge^N_{Public-to-Private} + 613\ Divest_{Subsidiary-to-Private} + 12,029\ Misc^N_{Del} \end{cases}$$

B.2 Post-peak sample period (12/31/1996–12/31/2020)

$$\Delta L_A = -84 \begin{cases} 13,369\ Newlists_A = 4,190\ IPO + 5,756\ Merge_{Private-to-Public} + 3,423\ Misc^N_{New} \\ 13,453\ Delists_A = 5,955\ Merge^N_{Public-to-Private} + 392\ Divest_{Subsidiary-to-Private} + 7,106\ Misc^N_{Del} \end{cases}$$

Table 3: International listing counts and peak years

This table provides an overview of country-specific listing peaks, sorted by year of peak. A country's listing-peak year is defined as the year with the highest listing count between 1975–2019. Columns (4) and (5) show each country's change in listing count from the peak year to 2020. Advanced and developing/emerging economies are defined by the IMF. Data are from CRSP, WDI, WFE, CEIC, and stock exchange homepages.

Country	Peak listing year (1)	Listing count at peak (2)	2020 listing count (3)	Change since peak (4)	Annual change (5)
A: Advanced countries that have peaked					
Denmark	1986	274	127	-54%	-1.6%
New Zealand	1986	339	122	-64%	-1.9%
Luxembourg	1987	347	27	-92%	-2.8%
Portugal	1988	158	37	-77%	-2.4%
Austria	1992	112	68	-39%	-1.4%
Ireland	1996	93	38	-59%	-2.5%
United States	1996	7,325	3,633	-50%	-2.1%
Canada	1998	1,991	764	-62%	-2.8%
Czech Republic	1998	92	20	-78%	-3.6%
Estonia	1998	25	18	-28%	-1.3%
Latvia	1998	67	18	-73%	-3.3%
Lithuania	1998	60	25	-58%	-2.7%
Belgium	1999	278	110	-60%	-2.9%
Finland	2000	158	126	-20%	-1.0%
France	2000	1,185	417	-65%	-3.2%
Israel	2000	664	429	-35%	-1.8%
Netherlands	2000	392	98	-75%	-3.8%
Slovenia	2001	151	29	-81%	-4.3%
Greece	2003	339	167	-51%	-3.0%
Switzerland	2003	289	220	-24%	-1.4%
Singapore	2005	564	458	-19%	-1.3%
United Kingdom	2006	2,913	1,601	-45%	-3.2%
Germany	2007	761	438	-42%	-3.3%
Norway	2008	209	174	-17%	-1.4%
Slovakia	2009	16	12	-25%	-2.3%
Spain	2015	3,623	2,695	-26%	-5.1%
Australia	2017	2,013	1,901	-6%	-1.9%
Average (N = 27)	2000	905	510	-49%	-2.5%
B: Advanced countries that have not peaked by 2020					
Hong Kong	–	–	2,360	–	–
Italy	–	–	374	–	–
Japan	–	–	2,808	–	–
South Korea	–	–	2,323	–	–
Sweden	–	–	335	–	–
Taiwan	–	–	948	–	–
Average (N = 6)	–	–	1,525	–	–

Continued on next page

Table 3: Continued (page 2 of 2)

Country	Peak listing year (1)	Listing count at peak (2)	2020 listing count (3)	Change since peak (4)	Annual change (5)
C: Developing/emerging countries that have peaked					
Argentina	1975	321	91	-72%	-1.6%
South Africa	1988	754	259	-66%	-2.1%
Brazil	1989	592	345	-42%	-1.3%
Mexico	1990	390	140	-64%	-2.1%
Costa Rica	1994	31	10	-68%	-2.6%
India	1996	5,999	5,579	-7%	-0.3%
Pakistan	1996	782	540	-31%	-1.3%
Chile	1997	294	207	-30%	-1.3%
Colombia	1997	128	65	-49%	-2.1%
Peru	1998	246	199	-19%	-0.9%
Romania	1998	126	81	-36%	-1.6%
Hungary	1999	64	45	-30%	-1.4%
Panama	2000	151	33	-78%	-3.9%
Egypt	2002	1,150	238	-79%	-4.4%
Iran	2005	408	368	-10%	-0.7%
Oman	2005	235	111	-53%	-3.5%
Malaysia	2006	1,021	925	-9%	-0.7%
Croatia	2007	359	107	-70%	-5.4%
Bahrain	2008	45	42	-7%	-0.6%
Bulgaria	2008	404	259	-36%	-3.0%
Morocco	2008	77	75	-3%	-0.2%
Jordan	2010	277	180	-35%	-3.5%
Nigeria	2010	215	177	-18%	-1.8%
Kuwait	2011	215	171	-20%	-2.3%
Russia	2012	292	213	-27%	-3.4%
Poland	2015	872	784	-10%	-2.0%
Turkey	2015	392	366	-7%	-1.3%
Ghana	2016	37	31	-16%	-4.1%
Kenya	2016	65	60	-8%	-1.9%
Tunisia	2017	82	80	-2%	-0.8%
Sri Lanka	2018	297	265	-11%	-5.4%
Average (N = 31)	2003	526	389	-33%	-2.2%
D: Developing/emerging countries that have not peaked by 2020					
Bangladesh	–	–	628	–	–
China	–	–	4,186	–	–
Indonesia	–	–	716	–	–
Kazakhstan	–	–	97	–	–
Philippines	–	–	268	–	–
Qatar	–	–	48	–	–
Saudi Arabia	–	–	207	–	–
Thailand	–	–	744	–	–
United Arab Emirates	–	–	74	–	–
Vietnam	–	–	751	–	–
Average (N = 10)	–	–	772	–	–

Table 4: Determinants of post-peak listing count rate of decline

This table shows coefficient estimates from the following regression specification:

$$Decline_i = \alpha + \beta D_{US} + \lambda Z_i + \epsilon_i, \quad i = 1, ..., N,$$

where $Decline_i$ is the average annual rate (percent) of decline in listed firms for country i in the five years (columns 1–2, 5–6, 9–10) or three years (columns 3–4, 7–8, 11–12) after that country's listing peak. $Decline_i$ is calculated from the unadjusted listing count in columns (1)–(4), the public-to-public merger-adjusted listing count in columns (5)–(8), and the full merger-adjusted listing count in columns (9)–(12). D_{US} is a dummy taking a value of one if the country is the U.S. and zero otherwise. Z_i is a set of pre-peak country-specific control variables. Each is an annual average value from the five or three years (depending on the sample) before the listing peak in country i. Pre-peak growth variables are *Listing count runup* (percent growth in unadjusted listing count) and *GDP growth*. GDP-scaled variables are *Trade* (exports plus imports) and *FDI net inflows* (foreign direct investment). Finally, population-scaled variables are *Patent applications* (filed by domestic firms and residents) and *GDP*. The sample starts with the full list of countries that experience a peak between 1975 and 29 (45 countries). Several countries are dropped due to missing data. Additionally, Croatia, Czech Republic, Luxembourg, and Portugal are excluded due to outliers. Odd-numbered columns use all available countries and even-numbered columns only sample advanced economies. U.S. listing count data are from CRSP, foreign listing count data are from WDI, WFE, CEIC, and exchange homepages, and merger data are from SDC. Control variables are from the World Bank and IMF. Advanced economies are classified by the IMF. Parentheses display robust standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	$Decline_i$: **Unadjusted listing count**				$Decline_i$: **Public-to-public merger-adj. listing count**				$Decline_i$: **All-merger-adj. listing count**			
Event time:	±5 years		±3 years		±5 years		±3 years		±5 years		±3 years	
Sampled countries:	All	Adv.	All	Adv.	All	Adv.	All	Adv.	All	Adv.	All	Adv.
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant	0.037**	0.066***	0.048***	0.084***	0.035**	0.061***	0.045***	0.076***	0.039**	0.058**	0.047***	0.073***
	(0.014)	(0.016)	(0.015)	(0.025)	(0.014)	(0.014)	(0.015)	(0.021)	(0.015)	(0.019)	(0.014)	(0.018)
D_{US}	0.013	0.009	-0.014	-0.007	-0.022**	-0.026**	-0.049***	-0.042***	-0.036***	-0.041**	-0.068***	-0.064***
	(0.009)	(0.009)	(0.011)	(0.012)	(0.009)	(0.009)	(0.010)	(0.012)	(0.012)	(0.013)	(0.010)	(0.011)
Pre-peak growth variables												
Listing count runup	0.054	0.081	-0.003	-0.055	0.052	0.075	0.000	-0.043	0.031	0.034	-0.001	-0.028
	(0.046)	(0.051)	(0.033)	(0.048)	(0.045)	(0.049)	(0.033)	(0.046)	(0.056)	(0.068)	(0.029)	(0.038)
GDP growth	-0.001	-0.001	0.000	-0.000	-0.001	-0.001	0.000	-0.000	-0.001	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
GDP-scaled variables												
Trade	0.000	-0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
FDI net inflows	0.001	0.007	-0.001	0.005	0.001	0.005	-0.001	0.003	0.001	-0.000	-0.001	-0.000
	(0.001)	(0.005)	(0.002)	(0.007)	(0.001)	(0.004)	(0.002)	(0.006)	(0.002)	(0.005)	(0.002)	(0.005)
Population-scaled variables												
Patent applications	-42.484	-56.537	-25.328	-41.370	-42.241	-56.212	-28.794	-42.922	-64.222	-78.406	-50.286	-63.089
	(26.046)	(38.165)	(45.646)	(55.951)	(27.253)	(41.474)	(42.428)	(53.336)	(41.504)	(50.980)	(41.629)	(48.308)
GDP	0.000	-0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R^2	0.079	0.468	0.025	0.134	0.103	0.570	0.076	0.275	0.175	0.607	0.212	0.521
N	30	15	35	17	30	15	35	17	30	15	35	17

Table 5: Country-specific effects on post-peak listing count rate of decline

This table shows β coefficient estimates from the regression specification:

$$Decline_i = \alpha + \beta D_{country} + \lambda Z_i + \epsilon_i, \quad i = 1, ..., N,$$

where variable definitions are as in Table 4 except for $D_{country}$, which replaces D_{US}. Each row shows the β coefficient estimates that results from setting $D_{country}$ to equal one if country i is the country indicated in the first column. Columns (1)–(12) and data sources are as in Table 4. Regression standard errors are robust but not shown in the table. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	$Decline_i$: **Unadjusted listing count**				$Decline_i$: **Public-to-public merger-adj. count**				$Decline_i$: **All-merger-adj. listing count**			
Event time:	±5 years		±3 years		±5 years		±3 years		±5 years		±3 years	
Sampled countries:	All	Adv.	All	Adv.	All	Adv.	All	Adv.	All	Adv.	All	Adv.
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Advanced economies												
Australia			-0.052*	-0.075**			-0.053*	-0.074**			-0.049*	-0.064*
Canada	0.032***	0.040***	0.070***	0.073***	0.023**	0.029*	0.058***	0.059***	0.007	0.002	0.039***	0.034**
Finland	-0.017	-0.031*	-0.008	-0.033	-0.017	-0.029	-0.010	-0.032	-0.042*	-0.050*	-0.020	-0.030
France	0.026***	0.019*	0.062***	0.063***	0.029***	0.024**	0.064***	0.066***	0.035***	0.034**	0.066***	0.066***
Germany	0.003	0.027	-0.019	0.003	0.026	0.058**	0.004	0.027	0.055*	0.077*	0.030	0.045
Greece	-0.020	-0.033*	-0.002	-0.020	-0.021*	-0.034**	-0.004	-0.021	-0.024*	-0.048***	-0.004	-0.028
Israel	-0.009	-0.013	0.000	-0.015	-0.000	-0.003	0.010	-0.003	0.014	0.007	0.029**	0.017
Latvia			-0.096	-0.189*			-0.094	-0.172*			-0.065	-0.111
Netherlands	0.024**	0.018	0.067***	0.062***	0.024**	0.021	0.065***	0.061***	0.021	0.027	0.057***	0.057***
Norway	-0.034	-0.040	-0.094*	-0.074	-0.021	-0.023	-0.077*	-0.047	0.005	-0.000	-0.053	-0.007
Singapore	-0.067	0.011	0.050	-0.025	-0.081	-0.057	0.033	-0.083	-0.092	-0.114	0.014	-0.141
Slovakia	0.014	0.002	0.008	-0.018	0.017	-0.002	0.012	-0.004	0.028	0.011	0.029	0.027
Slovenia	0.004	0.004	-0.025*	-0.037	0.008	0.016	-0.021*	-0.032	0.041	0.054	-0.008	-0.029
Spain	0.000	-0.012	0.009	-0.013	0.007	-0.002	0.013	-0.006	0.014	0.012	0.020	0.003
Switzerland	-0.033***	-0.029**	-0.015	-0.019	-0.025*	-0.019*	-0.009	-0.013	-0.022*	-0.014	-0.003	-0.005
UK	0.028***	0.014	0.038***	0.040	0.026***	0.014	0.033**	0.037	0.009	0.012	0.008	0.009
U.S.	0.013	0.009	-0.014	-0.007	-0.022**	-0.026**	-0.049***	-0.042***	-0.036***	-0.041**	-0.068***	-0.064***
Developing/emerging economies												
Bulgaria	-0.048*		-0.091*		-0.046*		-0.088*		-0.042		-0.078*	
Chile	0.002		-0.014		0.003		-0.012		-0.001		-0.016	
Colombia	-0.007		0.103***		-0.014		0.100***		-0.013		0.099***	
Costa Rica			0.040*				0.042**				0.047**	
Egypt	0.091***		0.075***		0.093***		0.078***		0.095***		0.078***	
Hungary	0.008		0.046***		0.011		0.048***		0.016		0.052***	
India	-0.057***		-0.042**		-0.055***		-0.040**		-0.051**		-0.040***	
Iran	-0.033*		-0.000		-0.029*		0.001		-0.028*		0.002	
Jordan	-0.007		-0.000		-0.008		-0.000		-0.005		0.004	
Kenya			-0.021				-0.024				-0.023	
Malaysia	-0.035		-0.041**		-0.038		-0.042**		-0.052		-0.055***	
Morocco	-0.034**		-0.043***		-0.035**		-0.045***		-0.041***		-0.052***	
Pakistan	-0.032*		-0.046***		-0.030*		-0.043***		-0.030*		-0.043***	
Panama	0.131***				0.130***				0.134***			
Peru	0.011		0.011		0.008		0.007				0.002	
Poland	-0.025**		-0.031***		-0.022*		-0.028**		-0.022*		-0.028**	
Russia			-0.003				-0.002				-0.012	
Tunisia			-0.044***				-0.043***				-0.040***	
Turkey	-0.029**		-0.038***		-0.031***		-0.037***		-0.032**		-0.038***	
Average R^2	0.112	0.508	0.057	0.215	0.128	0.532	0.065	0.268	0.186	0.535	0.166	0.413
N	30	15	35	17	30	15	35	17	30	15	35	17

Table 6: **Estimates of U.S. unadjusted and merger-adjusted listing gaps, all countries 1990–2020**

The table reports coefficient estimates from the following regression specification:

$$ln(Y_{it}) = \alpha + \delta_i + \tau_t + \beta D_{US} + \Gamma(D_{US} \times \tau_t) + \lambda X_{it} + \epsilon_{it}, \quad t = 1990, .., 2020, \quad i = 1, .., N,$$

where the dependent variable for country i in year t (Y_{it}) varies by column: actual listing count (G1) per capita (1–2) or per GDP (3–4), public-to-public merger-adjusted listing count (G2) per capita (5–6) or per GDP (7–8), or all-merger-adjusted listing count (G3) per capita (9–10) or per GDP (11–12). G1, G2, and G3 are defined in Eq. (11). δ_i and τ_t are country and year fixed effects, respectively. Country fixed effects are only included in even-numbered columns below. D_{US} is a dummy variable taking a value of one if the country is the U.S. and zero otherwise, and X_{it} is a set of country-specific control variables (anti-self-dealing index, log(GDP/capita) and GDP growth) in year t. For each year t after 1990, the size of the U.S. listing gap is computed as $Y_{US,1990} \times Pop_{US,t} \times \ (e^{\gamma_t} - 1)$ or $Y_{US,1990} \times GDP_{US,t} \times \ (e^{\gamma_t} - 1)$ (depending on the Y_{it} scaling variable), where γ_t is the annual parameter in the vector Γ. The regressions are run on the full sample of 74 countries. U.S. listing count data are from CRSP, foreign listing count data are from WDI and exchange homepages, and merger data are from SDC. Parentheses display country-clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Y_{it}: **Unadjusted listing count (G1)**				Y_{it}: **Public-to-public merger-adjusted listing count (G2)**				Y_{it}: **All-merger-adjusted listing count (G3)**			
	Per capita		Per GDP		Per capita		Per GDP		Per capita		Per GDP	
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant	0.176	1.571***	-0.812**	-0.031	0.109	1.592***	-0.835**	-0.045	-0.086	1.838***	-0.853***	-0.107
	(0.362)	(0.310)	(0.330)	(0.102)	(0.360)	(0.310)	(0.327)	(0.103)	(0.359)	(0.327)	(0.312)	(0.109)
Anti-self-dealing index	1.375***		1.180**		1.425***		1.242**		1.489***		1.340***	
	(0.479)		(0.510)		(0.472)		(0.501)		(0.454)		(0.472)	
Log(GDP/capita)	0.634***	0.299**			0.652***	0.282**			0.717***	0.146		
	(0.085)	(0.135)			(0.084)	(0.135)			(0.084)	(0.143)		
GDP growth	-0.003	-0.001	0.004	-0.004***	-0.004	-0.001	0.003	-0.004***	-0.005	0.000	0.000	-0.004***
	(0.003)	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)
U.S. dummy	-0.401**		-0.695***		-0.434**		-0.712***		-0.533***		-0.758***	
	(0.181)		(0.187)		(0.179)		(0.185)		(0.173)		(0.179)	
U.S. 1991 dummy	0.043	0.012	-0.027	0.074	0.059	0.019	-0.009	0.083	0.084	0.015	0.029	0.091
	(0.054)	(0.050)	(0.060)	(0.060)	(0.053)	(0.048)	(0.058)	(0.059)	(0.051)	(0.046)	(0.055)	(0.057)
U.S. 1992 dummy	0.068	0.002	0.009	0.060	0.098*	0.025	0.042	0.084*	0.151***	0.047	0.105*	0.118**
	(0.058)	(0.049)	(0.062)	(0.050)	(0.057)	(0.048)	(0.061)	(0.049)	(0.056)	(0.048)	(0.059)	(0.050)
U.S. 1993 dummy	0.162*	0.080	0.010	0.118	0.209**	0.114	0.061	0.154*	0.297***	0.168*	0.177*	0.215***
	(0.092)	(0.085)	(0.107)	(0.079)	(0.092)	(0.084)	(0.105)	(0.079)	(0.090)	(0.084)	(0.099)	(0.079)
U.S. 1994 dummy	0.034	0.063	-0.143	0.072	0.103	0.117	-0.074	0.129	0.196**	0.177**	0.052	0.191**
	(0.099)	(0.088)	(0.116)	(0.091)	(0.096)	(0.086)	(0.115)	(0.090)	(0.097)	(0.086)	(0.114)	(0.092)
U.S. 1995 dummy	0.069	0.069	-0.035	0.127	0.151	0.144	0.052	0.205**	0.274***	0.228**	0.193*	0.299***
	(0.097)	(0.091)	(0.111)	(0.092)	(0.098)	(0.090)	(0.111)	(0.092)	(0.100)	(0.090)	(0.112)	(0.095)
U.S. 1996 dummy	0.182	0.076	-0.008	0.144	0.296**	0.173*	0.113	0.243**	0.457***	0.278***	0.308**	0.361***
	(0.116)	(0.095)	(0.136)	(0.094)	(0.117)	(0.094)	(0.136)	(0.095)	(0.118)	(0.096)	(0.135)	(0.099)
U.S. 1997 dummy	0.086	-0.009	-0.183	0.040	0.237*	0.122	-0.023	0.173*	0.432***	0.260***	0.222	0.321***
	(0.131)	(0.094)	(0.162)	(0.097)	(0.132)	(0.094)	(0.162)	(0.098)	(0.134)	(0.095)	(0.159)	(0.103)
U.S. 1998 dummy	-0.047	-0.131	-0.364**	-0.151	0.150	0.046	-0.155	0.028	0.380***	0.235**	0.133	0.214*
	(0.135)	(0.095)	(0.163)	(0.105)	(0.136)	(0.094)	(0.163)	(0.106)	(0.139)	(0.096)	(0.162)	(0.111)
U.S. 1999 dummy	-0.232*	-0.277***	-0.562***	-0.343***	0.027	-0.040	-0.303*	-0.115	0.273*	0.183*	0.005	0.093
	(0.138)	(0.093)	(0.168)	(0.106)	(0.142)	(0.093)	(0.169)	(0.107)	(0.146)	(0.095)	(0.167)	(0.113)

Continued on next page

Table 6: Continued (page 2 of 2)

	Y_{it}: **Unadjusted listing count (G1)**				Y_{it}: **Public-to-public merger-adjusted listing count (G2)**				Y_{it}: **All-merger-adjusted listing count (G3)**			
	Per capita		Per GDP		Per capita		Per GDP		Per capita		Per GDP	
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
U.S. 2000 dummy	-0.355**	-0.369***	-0.697***	-0.458***	-0.050	-0.093	-0.400**	-0.200*	0.208	0.153	-0.076	0.026
	(0.141)	(0.094)	(0.174)	(0.105)	(0.144)	(0.094)	(0.172)	(0.107)	(0.149)	(0.096)	(0.170)	(0.113)
U.S. 2001 dummy	-0.502***	-0.456***	-0.844***	-0.576***	-0.147	-0.128	-0.494***	-0.264**	0.114	0.132	-0.167	-0.030
	(0.139)	(0.096)	(0.170)	(0.108)	(0.142)	(0.096)	(0.170)	(0.108)	(0.148)	(0.097)	(0.169)	(0.115)
U.S. 2002 dummy	-0.556***	-0.504***	-0.851***	-0.639***	-0.198	-0.165*	-0.491***	-0.303***	0.067	0.107	-0.170	-0.057
	(0.131)	(0.100)	(0.152)	(0.109)	(0.132)	(0.099)	(0.152)	(0.109)	(0.138)	(0.101)	(0.152)	(0.115)
U.S. 2003 dummy	-0.580***	-0.547***	-0.769***	-0.632***	-0.194	-0.176*	-0.387***	-0.265**	0.073	0.093	-0.084	-0.013
	(0.123)	(0.103)	(0.138)	(0.109)	(0.123)	(0.102)	(0.138)	(0.109)	(0.128)	(0.104)	(0.139)	(0.114)
U.S. 2004 dummy	-0.485***	-0.532***	-0.676***	-0.549***	-0.116	-0.146	-0.300**	-0.164	0.170	0.120	0.021	0.098
	(0.127)	(0.105)	(0.146)	(0.110)	(0.128)	(0.105)	(0.146)	(0.111)	(0.134)	(0.107)	(0.147)	(0.116)
U.S. 2005 dummy	-0.452***	-0.511***	-0.657***	-0.482***	-0.041	-0.113	-0.237	-0.081	0.258*	0.149	0.098	0.186
	(0.135)	(0.110)	(0.158)	(0.114)	(0.136)	(0.109)	(0.158)	(0.114)	(0.142)	(0.111)	(0.159)	(0.119)
U.S. 2006 dummy	-0.421***	-0.491***	-0.595***	-0.423***	0.003	-0.081	-0.163	-0.008	0.295**	0.166	0.160	0.252**
	(0.137)	(0.112)	(0.159)	(0.116)	(0.139)	(0.112)	(0.159)	(0.116)	(0.146)	(0.114)	(0.162)	(0.123)
U.S. 2007 dummy	-0.433***	-0.506***	-0.513***	-0.376***	0.001	-0.086	-0.075	0.051	0.290**	0.147	0.228	0.309**
	(0.136)	(0.117)	(0.151)	(0.119)	(0.137)	(0.116)	(0.152)	(0.119)	(0.143)	(0.119)	(0.155)	(0.125)
U.S. 2008 dummy	-0.421***	-0.529***	-0.479***	-0.321***	0.037	-0.091	-0.020	0.127	0.334**	0.126	0.289*	0.385***
	(0.138)	(0.122)	(0.152)	(0.119)	(0.139)	(0.121)	(0.153)	(0.120)	(0.145)	(0.125)	(0.158)	(0.127)
U.S. 2009 dummy	-0.429***	-0.569***	-0.620***	-0.351***	0.054	-0.116	-0.131	0.111	0.368**	0.096	0.217	0.365***
	(0.158)	(0.126)	(0.185)	(0.122)	(0.161)	(0.125)	(0.186)	(0.122)	(0.168)	(0.129)	(0.190)	(0.131)
U.S. 2010 dummy	-0.451***	-0.585***	-0.542***	-0.365***	0.041	-0.116	-0.047	0.111	0.334**	0.087	0.262	0.358***
	(0.144)	(0.126)	(0.164)	(0.123)	(0.145)	(0.125)	(0.165)	(0.124)	(0.153)	(0.129)	(0.169)	(0.132)
U.S. 2011 dummy	-0.447***	-0.617***	-0.499***	-0.342***	0.055	-0.137	0.005	0.146	0.345**	0.053	0.305*	0.389***
	(0.144)	(0.130)	(0.162)	(0.126)	(0.146)	(0.130)	(0.163)	(0.126)	(0.152)	(0.133)	(0.167)	(0.135)
U.S. 2012 dummy	-0.448***	-0.631***	-0.594***	-0.343***	0.073	-0.139	-0.068	0.158	0.373**	0.046	0.258	0.398***
	(0.158)	(0.134)	(0.187)	(0.128)	(0.160)	(0.132)	(0.188)	(0.129)	(0.168)	(0.136)	(0.192)	(0.138)
U.S. 2013 dummy	-0.436***	-0.611***	-0.547***	-0.332**	0.088	-0.113	-0.020	0.175	0.370**	0.063	0.283	0.404***
	(0.155)	(0.135)	(0.177)	(0.131)	(0.156)	(0.134)	(0.179)	(0.131)	(0.164)	(0.136)	(0.183)	(0.139)
U.S. 2014 dummy	-0.387**	-0.577***	-0.528***	-0.307**	0.094	-0.093	-0.046	0.186	0.372**	0.077	0.258	0.409***
	(0.156)	(0.136)	(0.180)	(0.131)	(0.162)	(0.134)	(0.187)	(0.131)	(0.170)	(0.137)	(0.191)	(0.140)
U.S. 2015 dummy	-0.464***	-0.638***	-0.718***	-0.431***	0.029	-0.146	-0.222	0.069	0.306	0.032	0.102	0.287**
	(0.171)	(0.134)	(0.205)	(0.134)	(0.178)	(0.132)	(0.213)	(0.134)	(0.186)	(0.134)	(0.215)	(0.144)
U.S. 2016 dummy	-0.567***	-0.686***	-0.760***	-0.527***	-0.028	-0.162	-0.215	0.002	0.224	0.017	0.072	0.212
	(0.152)	(0.131)	(0.174)	(0.133)	(0.160)	(0.128)	(0.183)	(0.134)	(0.169)	(0.130)	(0.188)	(0.143)
U.S. 2017 dummy	-0.531***	-0.672***	-0.681***	-0.511***	-0.017	-0.147	-0.165	0.021	0.221	0.023	0.101	0.223
	(0.149)	(0.131)	(0.171)	(0.133)	(0.154)	(0.129)	(0.177)	(0.134)	(0.162)	(0.131)	(0.181)	(0.143)
U.S. 2018 dummy	-0.511***	-0.667***	-0.674***	-0.495***	0.007	-0.140	-0.153	0.041	0.239	0.021	0.109	0.235
	(0.153)	(0.134)	(0.175)	(0.135)	(0.158)	(0.132)	(0.181)	(0.136)	(0.166)	(0.134)	(0.185)	(0.145)
U.S. 2019 dummy	-0.530***	-0.657***	-0.742***	-0.493***	0.015	-0.133	-0.189	0.036	0.239	0.019	0.074	0.221
	(0.162)	(0.136)	(0.189)	(0.137)	(0.165)	(0.133)	(0.190)	(0.137)	(0.173)	(0.135)	(0.194)	(0.146)
U.S. 2020 dummy	-0.506***	-0.636***	-0.706***	-0.497***	0.015	-0.135	-0.178	0.009	0.219	0.007	0.063	0.178
	(0.163)	(0.135)	(0.189)	(0.136)	(0.165)	(0.133)	(0.191)	(0.137)	(0.174)	(0.134)	(0.195)	(0.146)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.490	0.933	0.151	0.892	0.503	0.935	0.140	0.888	0.553	0.938	0.133	0.867
N	1,775	2,057	1,775	2,057	1,791	2,079	1,791	2,079	1,791	2,079	1,791	2,079

Table 7: Estimates of U.S. unadjusted and merger-adjusted listing gaps, advanced economies 1990–2020

The table reports coefficient estimates from the following regression specification:

$$ln(Y_{it}) = \alpha + \delta_i + \tau_t + \beta D_{US} + \Gamma(D_{US} \times \tau_t) + \lambda X_{it} + \epsilon_{it}, \quad t = 1990, .., 2020, \quad i = 1, .., N,$$

where the dependent variable for country i in year t (Y_{it}) varies by column: actual listing count (G1) per capita (1–2) or per GDP (3–4), public-to-public merger-adjusted listing count (G2) per capita (5–6) or per GDP (7–8), or all-merger-adjusted listing count (G3) per capita (9–10) or per GDP (11–12). G1, G2, and G3 are defined in Eq. (11). δ_i and τ_t are country and year fixed effects, respectively. Country fixed effects are only included in even-numbered columns below. D_{US} is a dummy variable taking a value of one if the country is the U.S. and zero otherwise, and X_{it} is a set of country-specific control variables (anti-self-dealing index, log(GDP/capita) and GDP growth) in year t. For each year t after 1990, the size of the U.S. listing gap is computed as $Y_{US,1990} \times Pop_{US,t} \times \ (e^{\gamma_t} - 1)$ or $Y_{US,1990} \times GDP_{US,t} \times \ (e^{\gamma_t} - 1)$ (depending on the Y_{it} scaling variable), where γ_t is the annual parameter in the vector Γ. The regressions are run on the subsample of 33 advanced economies. U.S. listing count data are from CRSP, foreign listing count data are from WDI and exchange homepages, and merger data are from SDC. Parentheses display country-clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels.

	Y_{it}: **Unadjusted listing count (G1)**				Y_{it}: **Public-to-public merger-adjusted listing count (G2)**				Y_{it}: **All-merger-adjusted listing count (G3)**			
	Per capita		Per GDP		Per capita		Per GDP		Per capita		Per GDP	
Regressors	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant	-0.284	3.273***	-1.564***	-0.477***	-0.372	3.475***	-1.567***	-0.496***	-0.845	3.985***	-1.558***	-0.542***
	(1.000)	(0.587)	(0.378)	(0.109)	(1.000)	(0.608)	(0.374)	(0.108)	(0.975)	(0.615)	(0.352)	(0.115)
Anti-self-dealing index	2.063***		1.997***		2.089***		2.024***		2.128***		2.089***	
	(0.549)		(0.561)		(0.534)		(0.549)		(0.484)		(0.497)	
Log(GDP/capita)	0.644**	-0.107			0.668**	-0.173			0.802***	-0.338*		
	(0.262)	(0.174)			(0.259)	(0.180)			(0.249)	(0.180)		
GDP growth	0.003	0.001	0.006	-0.002	0.002	0.002	0.005	-0.002	0.000	0.003*	0.002	-0.001
	(0.007)	(0.002)	(0.008)	(0.002)	(0.007)	(0.002)	(0.008)	(0.002)	(0.007)	(0.002)	(0.007)	(0.002)
U.S dummy	-0.431**		-0.478**		-0.452**		-0.493**		-0.518**		-0.543***	
	(0.209)		(0.206)		(0.209)		(0.206)		(0.206)		(0.197)	
U.S 1991 dummy	-0.068	-0.073	-0.090	-0.010	-0.050	-0.072	-0.072	-0.005	-0.014	-0.079	-0.028	-0.003
	(0.086)	(0.046)	(0.091)	(0.039)	(0.085)	(0.046)	(0.092)	(0.039)	(0.080)	(0.048)	(0.086)	(0.041)
U.S 1992 dummy	-0.030	-0.051	-0.047	0.040	0.003	-0.038	-0.014	0.058	0.066	-0.027	0.056	0.083*
	(0.092)	(0.056)	(0.097)	(0.045)	(0.092)	(0.055)	(0.099)	(0.045)	(0.087)	(0.055)	(0.093)	(0.047)
U.S 1993 dummy	-0.072	-0.011	-0.153	0.017	-0.020	0.014	-0.099	0.043	0.081	0.053	0.034	0.087
	(0.167)	(0.068)	(0.180)	(0.068)	(0.166)	(0.068)	(0.183)	(0.069)	(0.154)	(0.068)	(0.170)	(0.076)
U.S 1994 dummy	-0.027	0.013	-0.084	0.005	0.038	0.063	-0.017	0.054	0.143	0.135	0.110	0.126
	(0.104)	(0.086)	(0.104)	(0.088)	(0.103)	(0.085)	(0.105)	(0.088)	(0.096)	(0.082)	(0.100)	(0.093)
U.S 1995 dummy	0.097	0.034	0.102	0.122	0.181**	0.105	0.185**	0.198**	0.314***	0.201**	0.316***	0.306***
	(0.081)	(0.087)	(0.081)	(0.092)	(0.081)	(0.086)	(0.081)	(0.094)	(0.081)	(0.084)	(0.083)	(0.098)
U.S 1996 dummy	0.056	-0.028	-0.028	0.019	0.172	0.058	0.091	0.108	0.334**	0.159	0.286	0.216
	(0.149)	(0.101)	(0.182)	(0.120)	(0.150)	(0.101)	(0.185)	(0.123)	(0.150)	(0.101)	(0.179)	(0.135)
U.S 1997 dummy	-0.060	-0.084	-0.199	-0.107	0.094	0.036	-0.040	0.011	0.286	0.167	0.206	0.138
	(0.214)	(0.104)	(0.247)	(0.126)	(0.215)	(0.104)	(0.252)	(0.130)	(0.209)	(0.107)	(0.243)	(0.144)
U.S 1998 dummy	-0.214	-0.200*	-0.368	-0.296**	-0.017	-0.026	-0.163	-0.129	0.199	0.155	0.112	0.038
	(0.197)	(0.099)	(0.230)	(0.123)	(0.199)	(0.099)	(0.235)	(0.126)	(0.197)	(0.105)	(0.229)	(0.139)
U.S 1999 dummy	-0.346*	-0.302***	-0.519**	-0.476***	-0.112	-0.083	-0.276	-0.268**	0.106	0.126	0.008	-0.086
	(0.188)	(0.100)	(0.231)	(0.124)	(0.189)	(0.100)	(0.235)	(0.127)	(0.187)	(0.106)	(0.226)	(0.141)

Continued on next page

Table 7: Continued (page 2 of 2)

Regressors	Y_{it}: **Unadjusted listing count (G1)** Per capita (1)	(2)	Per GDP (3)	(4)	Y_{it}: **Public-to-public merger-adjusted listing count (G2)** Per capita (5)	(6)	Per GDP (7)	(8)	Y_{it}: **All-merger-adjusted listing count (G3)** Per capita (9)	(10)	Per GDP (11)	(12)
U.S 2000 dummy	-0.521**	-0.411***	-0.733***	-0.654***	-0.247	-0.152	-0.448*	-0.410***	-0.023	0.072	-0.143	-0.222
	(0.222)	(0.104)	(0.257)	(0.117)	(0.223)	(0.103)	(0.263)	(0.119)	(0.218)	(0.110)	(0.255)	(0.136)
U.S 2001 dummy	-0.643***	-0.517***	-0.850***	-0.800***	-0.325	-0.205*	-0.521**	-0.505***	-0.111	0.031	-0.228	-0.311**
	(0.194)	(0.110)	(0.228)	(0.117)	(0.195)	(0.109)	(0.232)	(0.117)	(0.194)	(0.114)	(0.228)	(0.129)
U.S 2002 dummy	-0.645***	-0.572***	-0.801***	-0.812***	-0.307**	-0.232*	-0.454**	-0.486***	-0.096	0.012	-0.184	-0.277**
	(0.141)	(0.116)	(0.180)	(0.121)	(0.143)	(0.115)	(0.182)	(0.121)	(0.151)	(0.117)	(0.181)	(0.129)
U.S 2003 dummy	-0.589***	-0.639***	-0.665***	-0.747***	-0.225*	-0.273**	-0.296*	-0.386***	-0.012	-0.036	-0.054	-0.165
	(0.117)	(0.120)	(0.148)	(0.126)	(0.117)	(0.118)	(0.146)	(0.125)	(0.127)	(0.118)	(0.148)	(0.129)
U.S 2004 dummy	-0.492***	-0.667***	-0.524***	-0.632***	-0.170	-0.290**	-0.233	-0.279**	0.072	-0.072	0.034	-0.059
	(0.147)	(0.122)	(0.154)	(0.128)	(0.144)	(0.119)	(0.175)	(0.128)	(0.152)	(0.118)	(0.177)	(0.133)
U.S 2005 dummy	-0.521***	-0.624***	-0.609***	-0.574***	-0.116	-0.246*	-0.200	-0.193	0.138	-0.044	0.088	0.017
	(0.177)	(0.132)	(0.201)	(0.134)	(0.180)	(0.128)	(0.204)	(0.133)	(0.187)	(0.127)	(0.207)	(0.139)
U.S 2006 dummy	-0.488**	-0.615***	-0.562***	-0.537***	-0.073	-0.227*	-0.144	-0.144	0.167	-0.044	0.125	0.051
	(0.180)	(0.132)	(0.200)	(0.139)	(0.183)	(0.129)	(0.204)	(0.138)	(0.192)	(0.127)	(0.210)	(0.144)
U.S 2007 dummy	-0.465***	-0.689***	-0.466**	-0.509***	-0.043	-0.292**	-0.045	-0.100	0.184	-0.123	0.183	0.096
	(0.169)	(0.129)	(0.172)	(0.140)	(0.171)	(0.125)	(0.175)	(0.140)	(0.179)	(0.121)	(0.182)	(0.144)
U.S 2008 dummy	-0.482**	-0.756***	-0.471**	-0.476***	-0.038	-0.349**	-0.029	-0.051	0.200	-0.206	0.206	0.134
	(0.186)	(0.133)	(0.185)	(0.140)	(0.189)	(0.129)	(0.189)	(0.139)	(0.199)	(0.124)	(0.198)	(0.145)
U.S 2009 dummy	-0.617**	-0.797***	-0.686**	-0.562***	-0.147	-0.379***	-0.215	-0.130	0.097	-0.259*	0.056	0.026
	(0.237)	(0.137)	(0.268)	(0.141)	(0.243)	(0.132)	(0.276)	(0.140)	(0.251)	(0.128)	(0.278)	(0.148)
U.S 2010 dummy	-0.592***	-0.793***	-0.629***	-0.578***	-0.117	-0.357**	-0.153	-0.128	0.098	-0.238*	0.077	0.022
	(0.200)	(0.138)	(0.223)	(0.143)	(0.205)	(0.133)	(0.228)	(0.142)	(0.217)	(0.127)	(0.234)	(0.149)
U.S 2011 dummy	-0.556***	-0.814***	-0.552***	-0.544***	-0.075	-0.370**	-0.071	-0.083	0.129	-0.261*	0.131	0.067
	(0.190)	(0.143)	(0.192)	(0.146)	(0.194)	(0.138)	(0.196)	(0.145)	(0.204)	(0.130)	(0.205)	(0.151)
U.S 2012 dummy	-0.649**	-0.829***	-0.715**	-0.598***	-0.144	-0.375**	-0.210	-0.130	0.066	-0.284**	0.026	-0.005
	(0.236)	(0.147)	(0.265)	(0.147)	(0.242)	(0.142)	(0.272)	(0.145)	(0.250)	(0.136)	(0.275)	(0.154)
U.S 2013 dummy	-0.594***	-0.789***	-0.635***	-0.575***	-0.093	-0.327**	-0.134	-0.101	0.086	-0.243*	0.062	0.015
	(0.207)	(0.152)	(0.228)	(0.154)	(0.212)	(0.146)	(0.233)	(0.152)	(0.221)	(0.138)	(0.239)	(0.158)
U.S 2014 dummy	-0.581**	-0.755***	-0.635**	-0.553***	-0.091	-0.306**	-0.144	-0.091	0.081	-0.231	0.049	0.014
	(0.217)	(0.154)	(0.241)	(0.158)	(0.222)	(0.148)	(0.247)	(0.156)	(0.232)	(0.139)	(0.253)	(0.162)
U.S 2015 dummy	-0.731**	-0.768***	-0.880***	-0.695***	-0.228	-0.310**	-0.371	-0.232	-0.062	-0.237	-0.147	-0.149
	(0.277)	(0.156)	(0.316)	(0.158)	(0.282)	(0.149)	(0.325)	(0.155)	(0.285)	(0.143)	(0.324)	(0.165)
U.S 2016 dummy	-0.782***	-0.789***	-0.878***	-0.763***	-0.207	-0.288*	-0.306	-0.257	-0.082	-0.208	-0.141	-0.173
	(0.194)	(0.156)	(0.223)	(0.158)	(0.208)	(0.149)	(0.239)	(0.156)	(0.218)	(0.144)	(0.246)	(0.163)
U.S 2017 dummy	-0.689***	-0.769***	-0.776***	-0.721***	-0.171	-0.273*	-0.254	-0.222	-0.061	-0.206	-0.111	-0.147
	(0.198)	(0.159)	(0.225)	(0.161)	(0.202)	(0.153)	(0.230)	(0.160)	(0.213)	(0.146)	(0.237)	(0.167)
U.S 2018 dummy	-0.673***	-0.764***	-0.754***	-0.690***	-0.153	-0.268	-0.230	-0.190	-0.053	-0.216	-0.099	-0.127
	(0.202)	(0.165)	(0.227)	(0.169)	(0.206)	(0.159)	(0.232)	(0.167)	(0.216)	(0.151)	(0.239)	(0.173)
U.S 2019 dummy	-0.705***	-0.747***	-0.827***	-0.706***	-0.187	-0.259	-0.304	-0.215	-0.095	-0.225	-0.165	-0.174
	(0.238)	(0.168)	(0.267)	(0.172)	(0.243)	(0.161)	(0.274)	(0.170)	(0.250)	(0.153)	(0.279)	(0.177)
U.S 2020 dummy	-0.657***	-0.698***	-0.771***	-0.681***	-0.167	-0.231	-0.275	-0.213	-0.103	-0.207	-0.168	-0.186
	(0.219)	(0.169)	(0.246)	(0.173)	(0.224)	(0.162)	(0.252)	(0.171)	(0.233)	(0.154)	(0.258)	(0.177)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.390	0.912	0.318	0.874	0.396	0.915	0.304	0.868	0.450	0.921	0.309	0.846
N	927	975	927	975	930	979	930	979	930	979	930	979

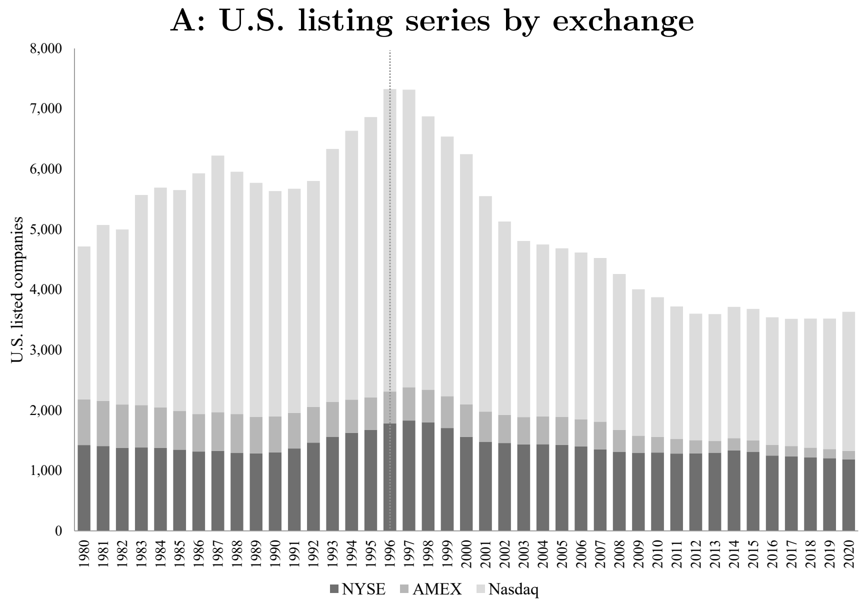
Table 8: Listed firms' employment, GDP, R&D spending, and patents granted, 1982–2018

This table shows the total annual amount of employment (in millions of people), value added (in USD trillion), research and development spending (in USD billion), and patents granted (in thousands) for U.S. public firms, all U.S. organizations or entities (public and private firms, government, universities, and individuals), and majority-owned foreign affiliates (MOFAs). To calculate the series shown in Figure 11, U.S. public firm output is divided by the sum of output from all U.S. firms and all MOFAs (except for patents). All monetary values are expressed in 2020 USD. MOFA R&D spending prior to 1989 is estimated and marked with * below. Data are from the BEA, BLS, Compustat, GCPD, IMF, OECD, and USPTO. Details in Appendix B.2.

	Employees (m)			Gross product (USD tn)			R&D spending (USD bn)			Patents granted (k)	
Year (1)	U.S. pub. firms (2)	All U.S. org. (3)	All MOFA (4)	U.S. pub. firms (5)	All U.S. org. (6)	All MOFA (7)	U.S. pub. firms (8)	All U.S. org. (9)	All MOFA (10)	U.S. pub. firms (11)	All U.S. ent. (12)
1982	26.9	89.4	5.0	2.7	8.9	0.6	95.8	216.4	13.1*	12.5	33.9
1983	27.0	92.9	4.9	2.7	9.4	0.6	102.9	233.5	12.3*	12.3	32.9
1984	28.0	96.8	4.8	2.9	10.0	0.5	114.7	254.7	12.0*	14.5	38.4
1985	28.0	99.4	4.8	2.9	10.4	0.5	118.1	275.5	11.6*	14.8	39.6
1986	27.4	101.3	4.7	2.8	10.7	0.5	123.4	282.9	12.0*	13.5	38.1
1987	27.7	104.5	4.7	2.9	11.0	0.6	126.0	286.8	13.5*	15.3	43.5
1988	27.5	107.7	4.8	3.1	11.4	0.6	133.1	291.9	14.3*	14.3	40.5
1989	27.3	109.7	5.1	3.0	11.7	0.7	137.0	295.1	14.6	17.3	50.2
1990	27.4	110.0	5.4	2.9	11.7	0.7	138.6	300.0	20.1	16.3	47.4
1991	27.5	109.1	5.4	2.8	11.6	0.7	142.3	304.8	17.7	18.2	51.2
1992	28.1	110.3	5.3	2.9	12.0	0.7	149.9	304.0	20.3	19.5	52.3
1993	28.6	113.1	5.2	3.1	12.2	0.6	153.2	295.9	19.5	20.8	53.2
1994	29.5	117.0	5.7	3.3	12.6	0.7	157.8	294.4	20.6	21.9	56.1
1995	30.7	119.1	5.9	3.6	12.9	0.8	179.2	310.7	21.2	22.2	55.7
1996	32.7	122.0	6.1	3.8	13.2	0.8	189.4	324.4	23.0	24.9	61.1
1997	34.6	125.4	6.5	4.1	13.7	0.8	215.4	340.9	23.4	26.1	61.7
1998	35.6	128.4	6.8	4.1	14.3	0.8	229.0	358.1	23.1	34.4	80.3
1999	36.3	131.6	7.8	4.4	14.9	0.9	227.2	379.2	28.0	35.4	83.9
2000	36.8	133.5	8.2	4.5	15.3	0.9	255.1	402.6	30.6	37.5	85.1
2001	36.1	131.8	8.2	4.1	15.4	0.9	259.7	407.1	28.6	40.0	87.6
2002	35.5	131.2	8.3	4.0	15.6	0.9	243.3	400.3	30.1	40.8	87.0
2003	35.2	131.4	8.2	4.2	16.0	1.0	242.1	410.9	31.9	42.7	87.9
2004	36.3	133.4	8.7	4.5	16.6	1.1	252.9	416.3	35.2	42.5	84.3
2005	36.6	136.0	9.1	4.7	17.2	1.2	255.5	432.2	36.4	37.8	74.6
2006	37.5	138.1	9.6	5.3	17.6	1.3	282.6	450.9	37.7	44.9	89.8
2007	37.1	139.3	10.0	5.4	17.9	1.4	288.9	471.8	42.7	39.5	79.5
2008	36.1	135.7	10.0	4.6	17.6	1.4	290.1	486.6	49.8	40.2	77.5
2009	34.1	130.7	10.8	4.2	17.3	1.4	247.9	473.4	47.0	41.9	82.4
2010	35.1	131.6	11.3	4.9	17.7	1.5	269.4	465.7	47.1	54.3	107.8
2011	36.3	133.7	11.9	5.2	17.8	1.6	283.1	472.9	51.1	55.6	108.6
2012	36.8	135.9	12.1	5.2	18.1	1.6	295.6	466.8	50.4	62.0	121.0
2013	37.3	138.3	12.4	5.3	18.5	1.5	304.6	479.8	54.4	70.0	133.6
2014	38.2	141.3	14.1	5.8	19.0	1.6	326.0	491.6	60.1	76.6	144.6
2015	39.0	144.0	14.1	5.8	19.8	1.5	341.0	510.4	60.9	71.3	141.0
2016	38.1	146.3	14.3	5.8	20.1	1.4	355.0	521.4	58.2	71.4	143.7
2017	38.5	148.5	14.4	6.1	20.5	1.5	377.7	535.1	60.7	–	151.0
2018	39.2	150.8	14.4	6.4	21.1	1.5	420.5	552.3	59.7	–	144.4
Avg.	33.3	124.3	8.3	4.2	14.9	1.0	225.0	383.7	32.2	35.0	79.8

Figure 1: **Listing count by stock exchange and around the world, 1980–2020**

Panel A shows the number of firms listed on each of the three major U.S. stock exchanges. Panel B shows the total number of domestic listed firms in 74 of the 100 countries with highest GDP in 2020 according to the IMF, with 33 classified as advanced economies and 41 as developing or emerging economies. U.S. data are from CRSP. Non-U.S. listing counts are from WDI, WFE, CEIC, and individual stock exchange home-pages. See Appendix B for further details on the data selection. The vertical dotted line in 1996 marks the year of the U.S. listing peak.



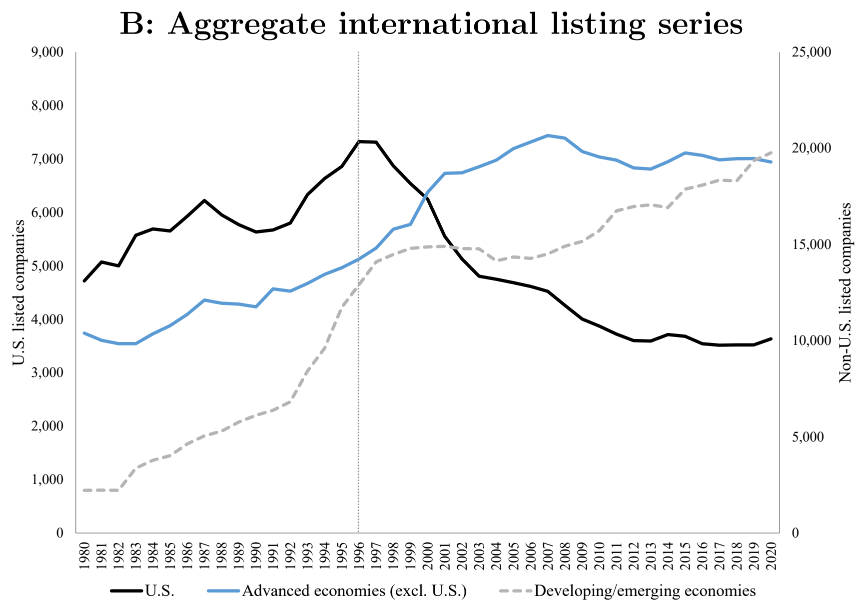
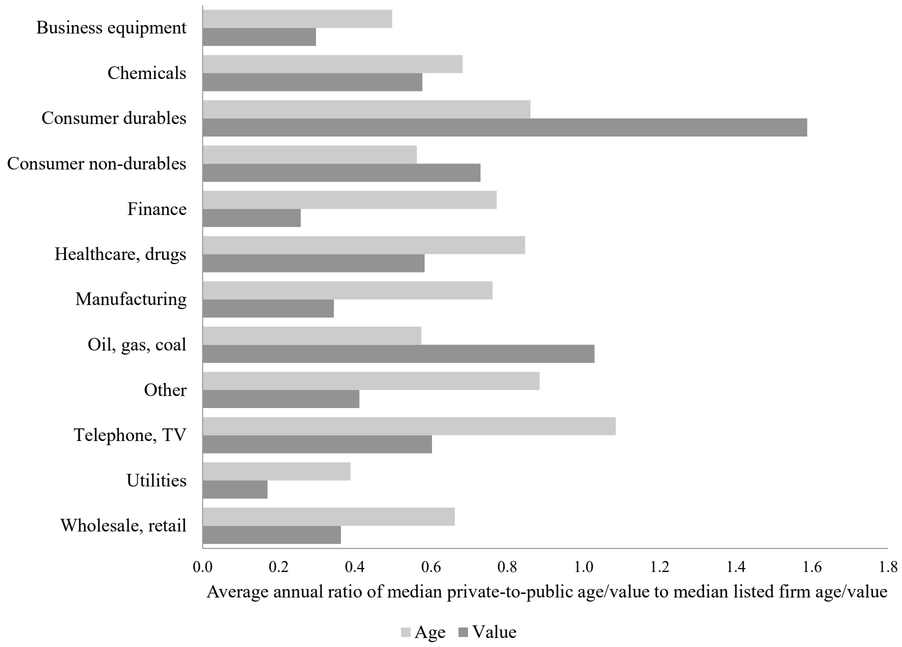


Figure 2: Relative size and age of private targets of public acquirers

This figure shows the relative size and age of private-to-public targets compared to listed firms. In Panel A, the median size of private-to-public targets (measured by deal value) and age are divided by the median size and age, respectively, of listed firms within Fama-French 12 industries and years, after which this ratio is averaged across years and plotted here by industry. Panel B plots the annual median age (since incorporation) of private-to-public targets at acquisition and IPO firms at listing. The vertical dotted line in 1996 marks the year of the U.S. listing peak. Sample period is 12/31/1980–12/31/2020. Data are from CRSP, SDC, Jay Ritter, and Capital IQ.

A: Relative size and age of private targets to listed firms by industry



B: Age of private targets and IPO firms by year

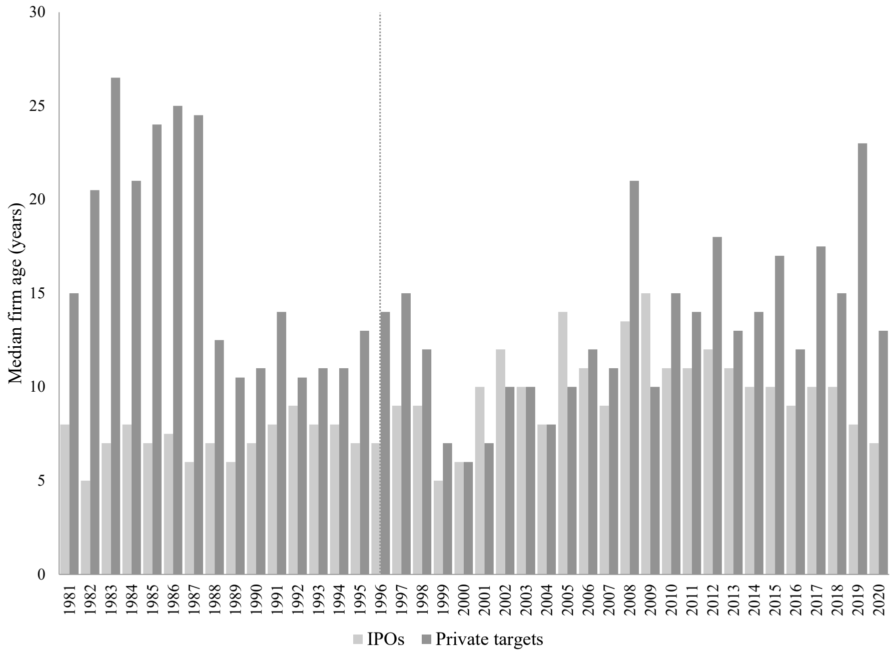


Figure 3: Actual and merger-adjusted U.S. listing counts, 1980–2020

This figure plots the (monthly) U.S. actual and merger-adjusted counts of listed firms on NYSE, AMEX, and Nasdaq from 12/31/1980-12/31/2020. The change in the actual (ΔL) and all-merger-adjusted (ΔL_A) listing counts are as follows:

$$\Delta L = \begin{cases} Newlists: & IPO + Spin + Misc_{New} \\ Delists: & Merge_{Public-to-Public} + Merge_{Public-to-Private} + Misc_{Del} \end{cases}$$

$$\Delta L_A = \begin{cases} Newlists_A: & IPO + Merge_{Private-to-Public} + Misc^N_{New} \\ Delists_A: & Merge^N_{Public-to-Private} + Divest_{Subsidiary-to-Private} + Misc^N_{Del} \end{cases}$$

The dotted curve in the middle of this figure is the merger-adjusted listing count when adjusting for mergers involving public targets only. All variables defined in Table 1. Data are from CRSP and SDC.

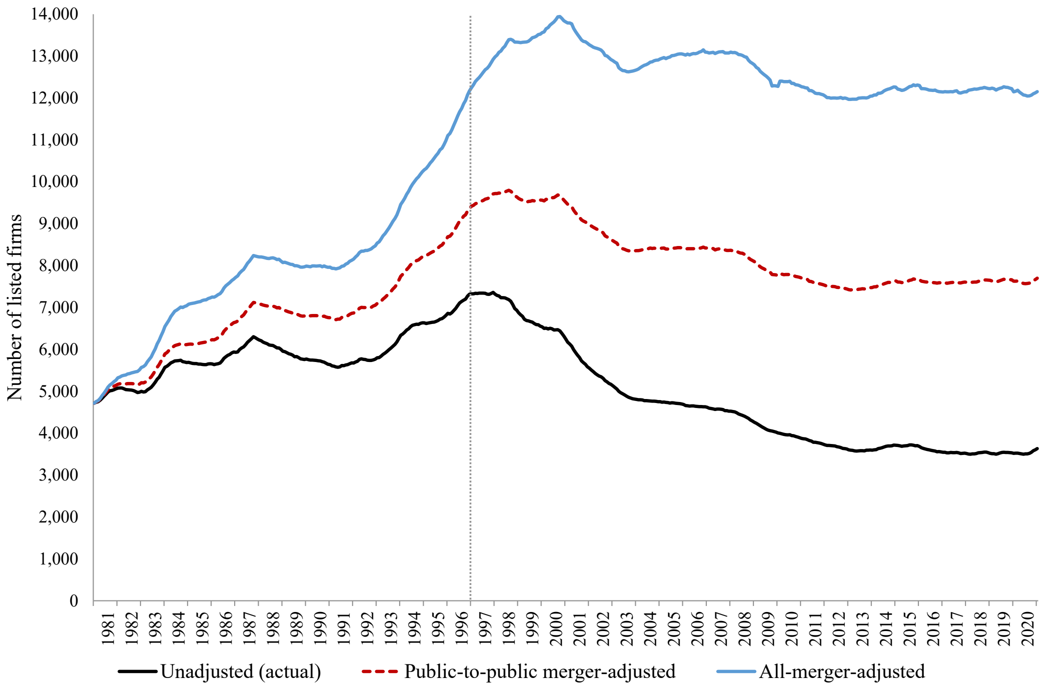


Figure 4: **Annual number of global listing peaks, 1975–2019**

This figure shows the annual number of listing peaks (economies with fewer listed firms in 2020 than earlier, at peak) around the world. The peak in 1975 is Argentina. Blue bars designate advanced economies and grey bars designate developing and emerging economies. 57 of 74 sampled countries and territories are represented in the figure. The U.S. listing count is from CRSP and consists of firms with common stock listed on NYSE, AMEX, or Nasdaq. Non-U.S. listing counts are found using data from WDI, the WFE, CEIC, and individual stock exchange home-pages. Investment companies, mutual funds, real estate investment trusts, and other collective investment vehicles are excluded. See Appendix B.3 for further details on data selection. The vertical dotted line in 1996 marks the year of the U.S. listing peak.

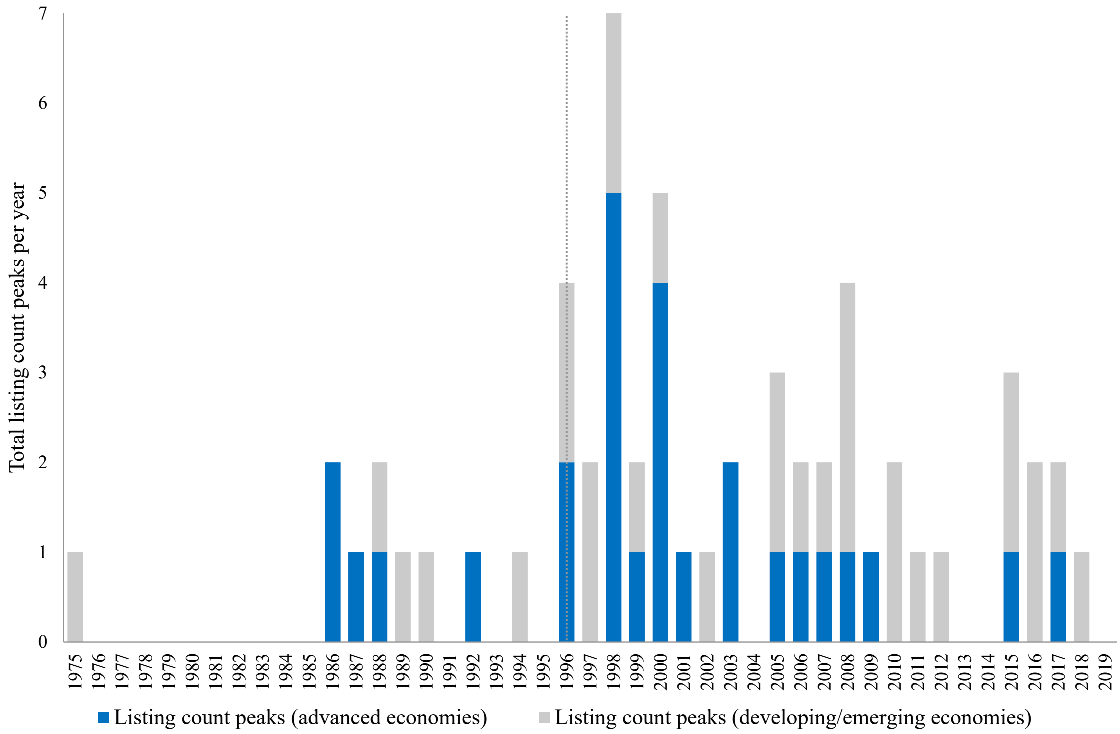
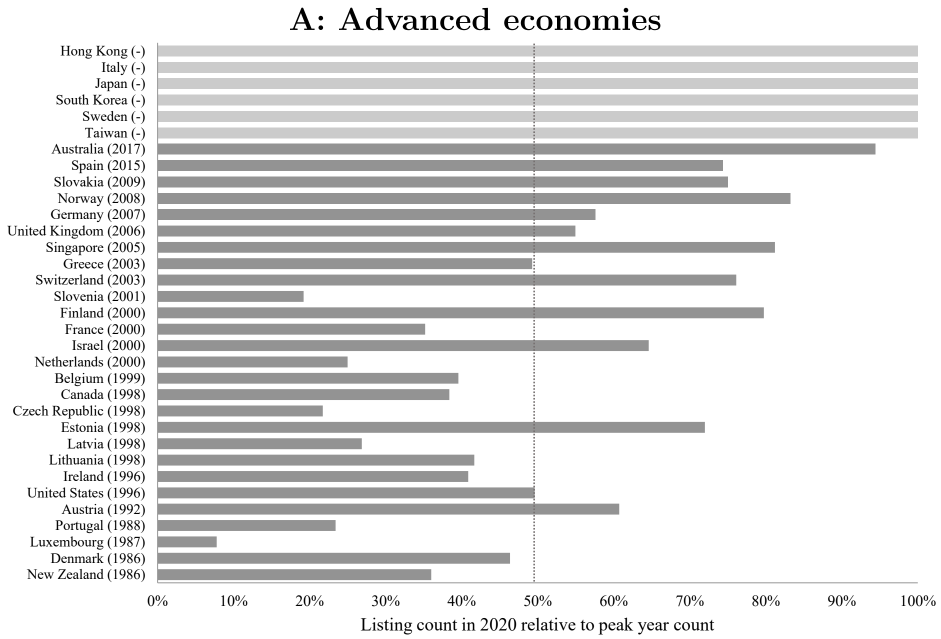


Figure 5: **Country-specific listing peak years and subsequent listing decline, 1975–2020**

This figure shows the decline in the number of listed firms from the listing peak year to 2020. Light bars are countries that have not experienced a peak, and dark bars indicate countries that have peaked (have fewer listed firms in 2020 than at peak). The listing peak year is shown in parentheses. 74 countries are sampled: 33 advanced (Panel A) and 41 developing/emerging (Panel B). Data are from CRSP, WDI, WFE, CEIC, and stock exchange homepages. Advanced and developing/emerging economies are classified by the IMF. The vertical dotted line shows the U.S. decline of 50% from 1996 to 2020.



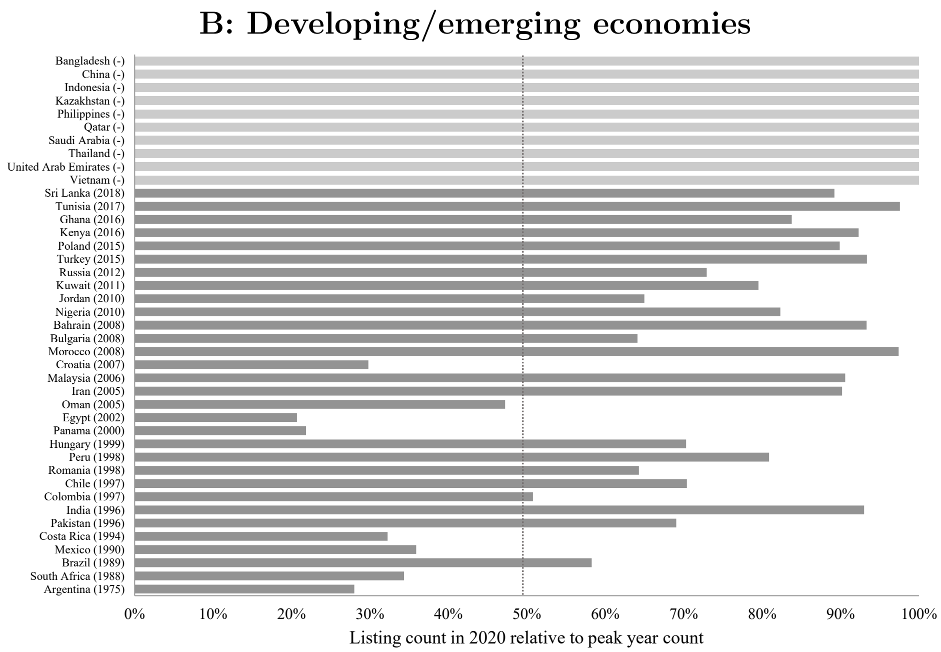
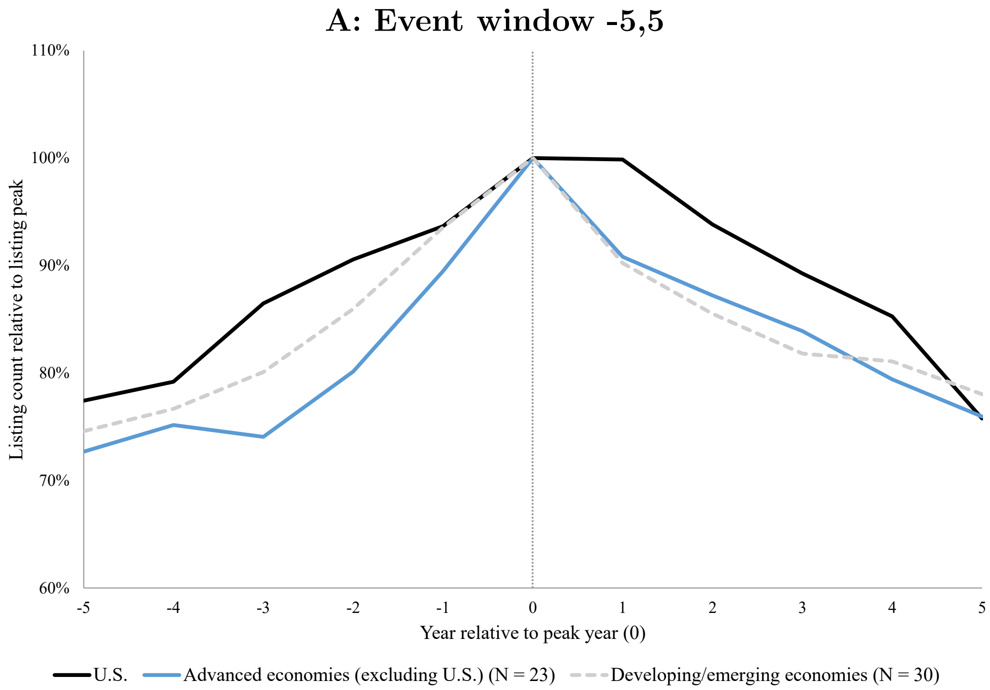


Figure 6: Listing peaks in event time, 1975–2020

Conditional on experiencing a listing peak, this figure plots the percent change in listing count over the eleven-year event window (-5,5) centered on the peak year (year 0) in Panel A, and 21-year window (-10,10) in Panel B. Countries with listing peaks are drawn from the period 1975–2020. The percent change is relative to the country's listing count in year 0. The portfolios of 23 non-U.S. advanced and 30 developing/emerging economies are equal-weighted. Four countries are excluded due to outliers: Croatia, Czech Republic, Luxembourg, and Portugal. Economic development is classified by the IMF. Data are from CRSP, WDI, WFE, CEIC, and stock exchange home pages.



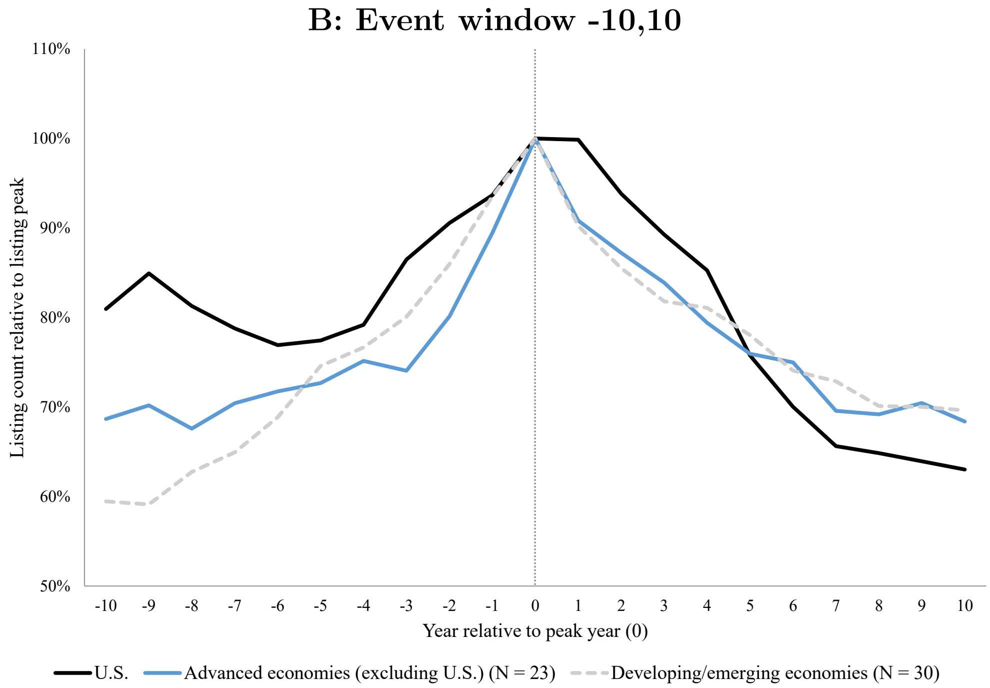
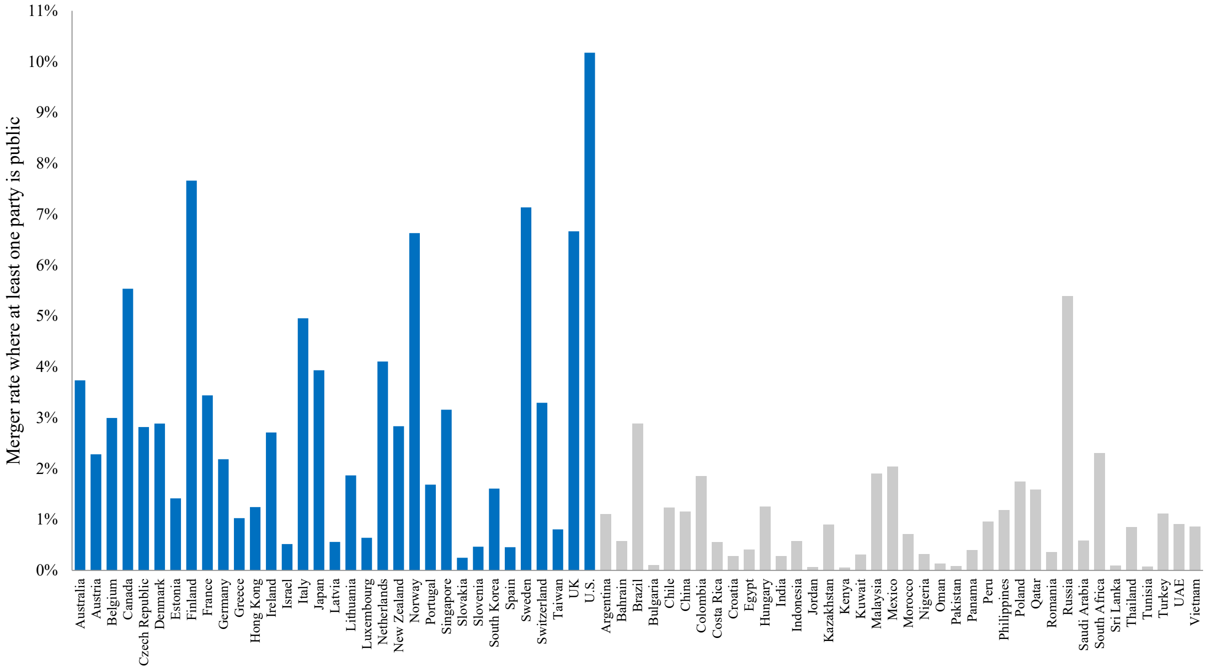


Figure 7: International merger rates, 1990–2020

This figure shows the average annual merger likelihood for listed companies by country or territory. Panel A shows the likelihood for a listed company to be the target or acquirer in a completed merger. Panel B shows the likelihood for a listed company to be acquired by another domestic listed firm. Blue bars indicate advanced economies and grey bars indicate developing/emerging economies. Merger data are from SDC, listing counts are from CRSP, WDI, WFE, CEIC, and stock exchanges, and economic development status is classified by the IMF.

A: All mergers where at least one party is a public firm



B: Public-to-public mergers only

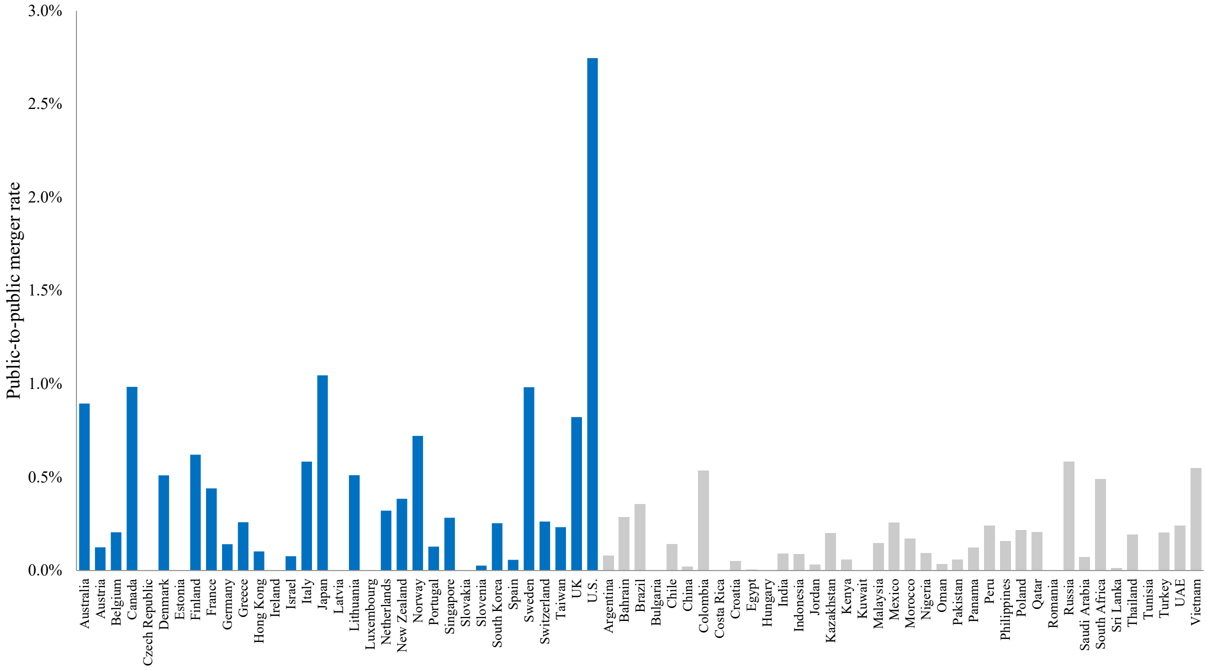
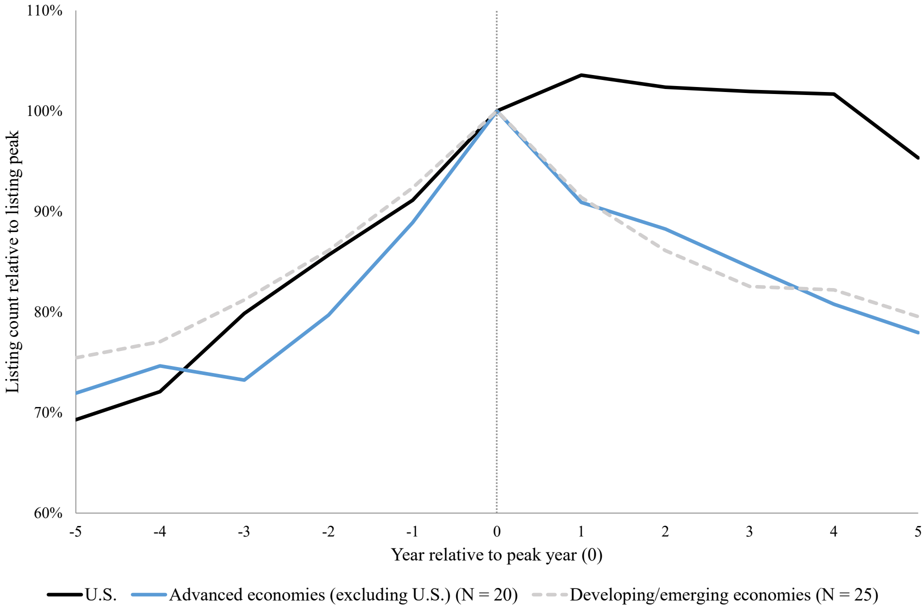


Figure 8: Merger-adjusted peaks in event time, 1990–2020

For countries with a listing peak, Panel A plots the percent change in public-to-public merger-adjusted listing count over the eleven-year event window (-5,5) centered on the peak year (year 0). Panel B plots the all-merger-adjusted listing count during the same event window. The countries in this event-period sample are required to have a peak in 1995 or later to allow for full event-period data coverage. Croatia and Czech Republic are excluded due to outliers. The percent change is relative to the country's adjusted listing count in year 0.

A: Public-to-public merger-adjusted listing counts in event time



B: All-merger-adjusted listing counts in event time

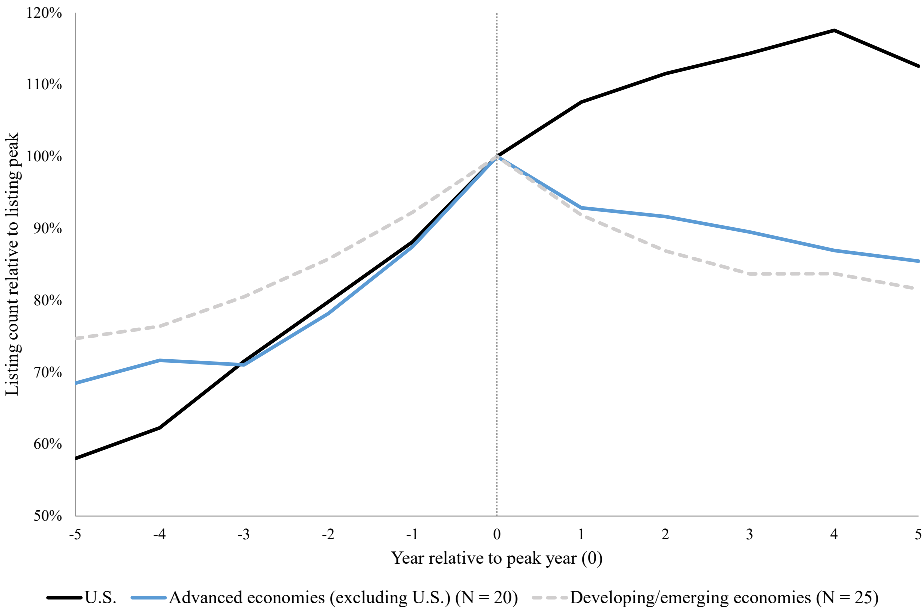


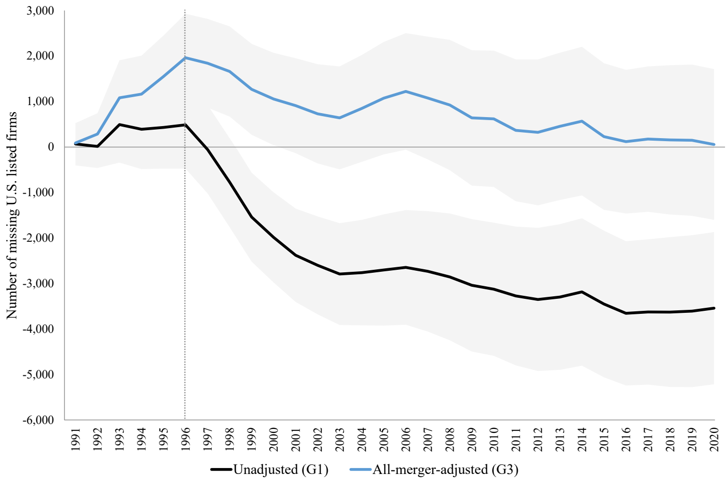
Figure 9: Population-scaled unadjusted and merger-adjusted U.S. listing gaps

This figure shows the unadjusted (G1, black line) and two merger-adjusted U.S. listing gaps, estimated as follows:

$$ln(L/Pop_{it}) = \alpha + \delta_i + \tau_t + \beta D_{US} + \Gamma(D_{US} \times \tau_t) + \lambda X_{it} + \epsilon_{it}, \quad t = 1990, .., 2020, \quad i = 1, .., N.$$

$ln(L/Pop_{it})$ is the natural logarithm of the unadjusted or merger-adjusted listing count of country i in year t, scaled per capita and specified as follows. In Panel A, the listing count is adjusted by adding one to the listing count for each public- and minimum-sized private-to-public merger (G3, blue line). In Panel B, the listing count is adjusted by adding back one for each domestic public-to-public merger (G2, broken red line). Additionally, the U.S. merger-adjusted listing series tracks net firm outflows via the acquisition index N_{it}, as well as spinoffs and subsidiary divestitures. Listing gaps G1, G2, and G3 are defined in Eq. (11). δ_i and τ_t are country and year fixed effects, respectively. D_{US} is a dummy variable that takes a value of one if country i is the U.S. and zero otherwise, and X_{it} is a vector of three country-specific control variables: country i's anti-self-dealing index, log(GDP/capita) and GDP growth. Standard errors are clustered at the country-level. The U.S. listing gap in year t is computed as $L/Pop_{US,1990} \times GDP_{US,t} \times \ (e^{\gamma_t} - 1)$, where γ_t is the annual parameter in the vector Γ. The sample consists of 74 countries and covers 1990–2020. U.S. listing data are from CRSP, non-U.S. listing data are from WDI, WFE, CEIC, and exchange homepages, and merger data are from SDC. The vertical dotted line indicates the year of the U.S. listing peak. The shaded grey area displays 90% confidence intervals.

A: Unadjusted and merger-adjusted listing gaps (G1, G3)



B: Public-to-public merger-adjusted listing gap (G2)

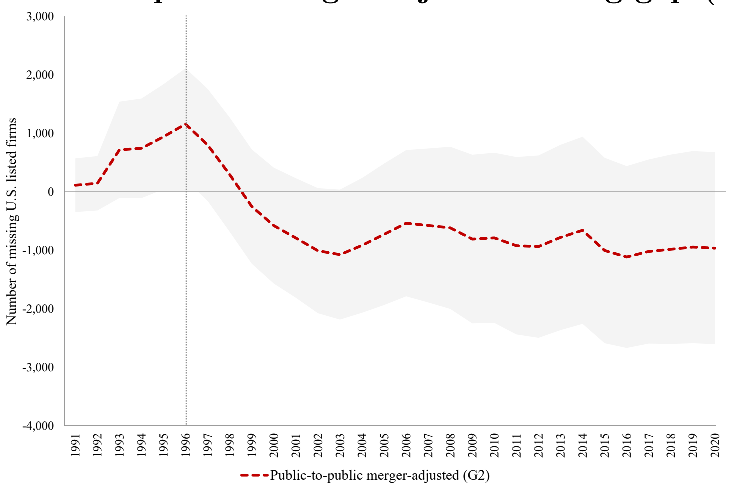


Figure 10: **Inflows and outflows of firm value classified by (de)listing channel**

The figure shows the annual values (V_A) of firm inflows (merger-adjusted new lists) and outflows (merger-adjusted delists) in U.S. public markets from 12/31/1980 to 12/31/2020. The annual change in V_A (ΔV_A) is measured using individual transaction values as follows:

$$\Delta V_A = \begin{cases} Newlists_A : & IPO + Merge_{Private-to-Public} + Misc_{New} \\ Delists_A : & Merge_{Public-to-Private} + Divest_{Subsidiary-to-Private} + Misc_{Del} \end{cases}$$

The right axis shows annual values for each channel in 2020 USD billion (bars), while the left axis shows the cumulative net new listing value in 2020 USD trillion (line). The new lists and delists in Table 1 that have an effect on the actual, but not merger-adjusted, listing count are not included. The vertical dotted line indicates the date of the U.S. listing peak. Variable definitions are as in Figure 3 except that, in this figure, transactions are measured by market value. Data from CRSP and SDC.

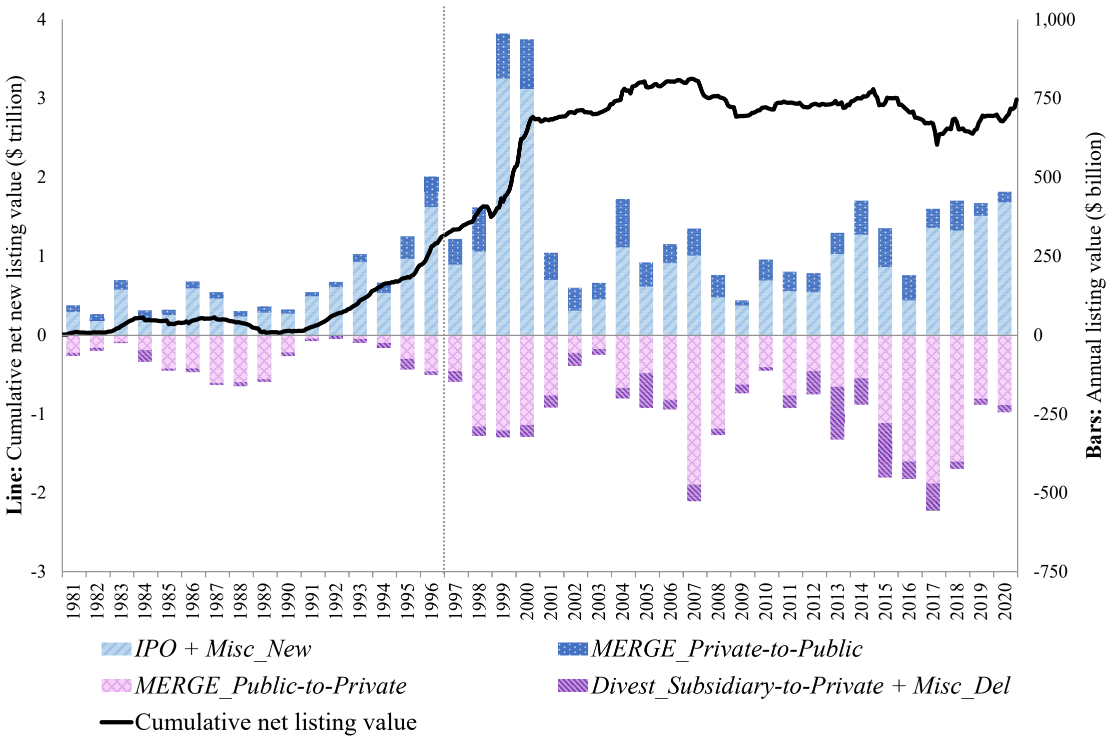
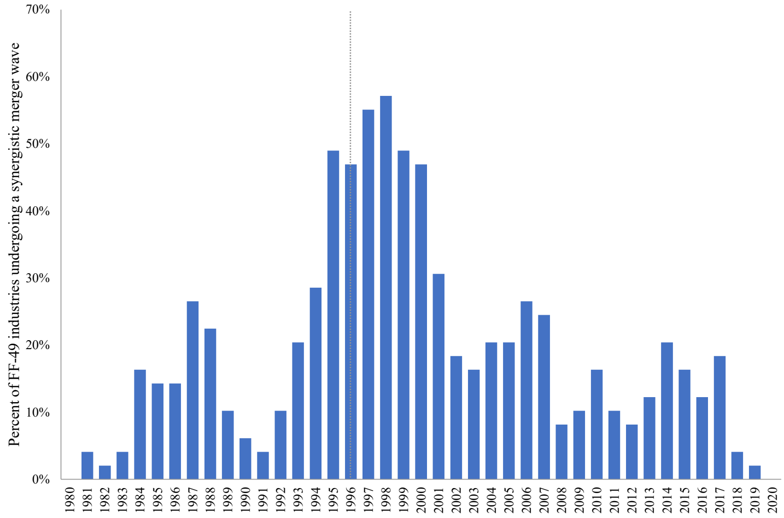


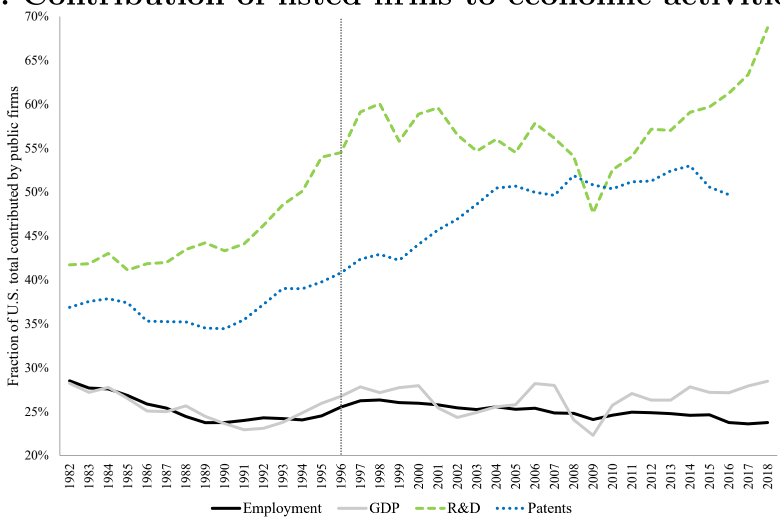
Figure 11: **'Synergistic' merger waves and economic contribution of listed firms**

Panel A shows the share of industry-years undergoing a synergistic merger wave for our sample of public-to-public mergers, 1980–2020, using the Fama-French 49 industries. Following **?**, industry-years are considered to undergo a synergy wave if the number of deals with positive bidder and target combined wealth effect (CWE) in that year is one standard deviation above the industry time-series median. CWE is the value-weighted average CAR for the event period (-3,3), where (0) is the announcement date. CARs are calculated as the difference between the realized and value-weighted market return. Pre-announcement market value of the bidder and target is measured one month before the deal announcement. Both acquirer and target must be U.S. public firms, with the bidder holding less than 50% of target shares before announcement and seeking to hold at least 50% after the transaction. Panel B shows the time series of public firms' percent contribution to aggregate U.S. employment, GDP, R&D spending, and patents, with data from the BEA, BLS, Compustat, GCPD, IMF, OECD, and USPTO. Construction and data series are detailed in Appendix B.2.

A: Share of industry-years with synergistic public-to-public merger waves



B: Contribution of listed firms to economic activities



A Further on U.S. listing gap econometrics

In this section, we provide a detailed comparison of alternative ways to estimate the U.S. listing gap. While we use the parameter γ_t to compute the listing gap, Doidge, Karolyi, and Stulz (2017) instead employ a non-U.S. dummy in their basic listing-gap regressions and use the year fixed effect to compute the gap. In our vernacular, this alternative approach is equivalent to using $\gamma_t + \tau_t$ to compute the gap. To see why, consider the regression model in Doidge, Karolyi, and Stulz (2017):

$$ln(Y_{it}) = \alpha' + \tau_t' + \beta' D_{non-US} + \Gamma'(D_{non-US} \times \tau_t') + \lambda' X_{it} + \epsilon_{it}, \quad t = 1990, ..., 2012, \quad i = 1, .., N. \tag{12}$$

Their gap-parameter in year t is therefore

$$\begin{aligned} E(Y_{it} \mid D_{non-US} = 0, year = t) \quad &- \quad E(Y_{it} \mid D_{non-US} = 0, year = 1990) \\ &= \quad (\alpha' + \tau_t') - \alpha' \\ &= \quad \tau_t'. \end{aligned} \tag{13}$$

If we switch the country dummy back to our D_{US}, and noting that $E(Y_{it} \mid D_{non-US} = 0) = E(Y_{it} \mid D_{US} = 1)$, it follows that

$$\begin{aligned} \tau_t' = E(Y_{it} \mid D_{US} = 1, year = t) \quad &- \quad E(Y_{it} \mid D_{US} = 1, year = 1990) \\ &= \quad (\alpha + \tau_t + \beta + \gamma_t) - (\alpha + \beta) \\ &= \quad \gamma_t + \tau_t. \end{aligned} \tag{14}$$

Hence, the year fixed effect (τ_t') estimated in Doidge, Karolyi, and Stulz (2017) equals the sum of the year fixed effect τ_t and the gap-parameter in this paper γ_t, where τ_t is the portion of the U.S. listing trend that is common to the U.S. and all other countries.

The estimates provided in Internet Appendix Table 3 illustrate the impact of the two different econometric parameterizations of the U.S. listing gap—here and in Doidge, Karolyi, and Stulz (2017). This table shows estimates of the listing-gap parameters γ_t, τ_t, and τ_t' when we use a U.S. dummy (columns 1 and 3, as in our analysis) and a non-U.S. dummy (columns 2 and 4, as in the earlier paper), respectively. This information allows us to isolate the impact on the U.S. listing-gap computation of the inclusion of τ_t. Columns (1) and (2), which exclude the country fixed effect δ_i in the estimation, show that $(\tau_{2020} + \gamma_{2020})/\gamma_{2020} = \tau_{2020}'/\gamma_{2020} = (-0.915)/(-0.506) = 1.81$. In columns (3) and (4), where country fixed effects are included in the regression, the corresponding ratio is smaller: 1.27. In other words, in our analysis, including the global common trend in the listing gap computation (which we do not do) would have increased the size of the gap by 27% at minimum and 81% at maximum. Finally, note that using $-\gamma_t$ as the listing-gap parameter in a regression with a non-U.S. dummy produces exactly the same listing gap estimate as using γ_t with a U.S. dummy.

The above analysis provides a basis for directly comparing the actual (not merger-adjusted) U.S. listing gaps reported by Doidge, Karolyi, and Stulz (2017) and this paper. For year 2012—the last year in the sample period of the earlier paper—the two gaps are -5,436 and -3,289 (both significant at the 1% level), respectively. The above difference in the two listing gap estimates is primarily driven by the earlier paper's inclusion of the common listing trend τ_t in their estimate. However, the two estimates also differ because we adjust for the growth in the dependent-variable scaling factor and take the antilog of γ_t (as per Eq. 10). Other differences arise because of our inclusion of country fixed effects, somewhat different data sources for the listing count, a slightly different set of sampled countries, and a longer sample period (1990–2020 instead of 1990–2012).

Lattanzio, Megginson, and Sanati (2023) also report listing-gap estimates, but with the unscaled listing count $ln(L_{it})$ as the dependent variable—moving the scaling factor $ln(Pop)$ to the right-hand side as a regressor. As Doidge, Karolyi, and Stulz (2017), they use the equivalent of our parameter τ_t' to compute the listing gap, and hence also do not filter out the listing trend that is common across countries. Moreover, their model adds country-level regressors aggregating stock market valuation, private equity volume, and merger activity. They show that this alternative regression specification substantially lowers the listing gap. From 1991–2019, their regression renders the U.S. listing-gap estimate statistically insignificant for the years 1992–1993 and 2011–2012. In 2019, their gap-estimate is -1,974 firms, which is statistically significant at the 5% level. Internet Appendix Table 4 shows that replacing our dependent variable with $ln(L_{it})$ and using the scaling factor as a regressor does not alter our main conclusion using either the full sample of 74 countries or the subsample of 33 advanced economies.

Finally, we plot our estimates of GDP-scaled U.S. listing gaps in Internet Appendix Figure 2. This figure corresponds to Figure 9 in the paper, except that it scales the dependent regression variable by GDP instead of by population. The three gaps (G1, G2, and G3) of Internet Appendix Figure 2 are generated using the U.S.-year dummy coefficient estimates from columns (4), (8), and (12) of Table 6 in the paper.

B Data sources and additional listing information

B.1 Data on U.S. listing anatomy

In the paper, we define U.S. public firms in CRSP and require them to be domestic companies with common stock (share codes 10 or 11) that are listed on the NYSE, AMEX, or Nasdaq (exchange codes 1, 2, 3, 31, 32, and 33). We further exclude investment funds and trusts (SIC codes 6722, 6726, and 6798–6799). We also exclude firms that are listed for only one day.

Appendix Figure A.1 Panel A shows the number of U.S. public firms listed on each individual stock exchange from 1980–2020.

New lists are recorded when a firm first appears in the sample of CRSP public firms, or when it is relisted after at least two weeks off public markets (thus excluding SEC trading suspensions of a listed firm, which may last no more than ten days). To categorize new lists, we first identify IPOs using data from SDC and Jay Ritter's website.[18] Spinoffs are identified either in CRSP, with distribution code 3763 (Vijh, 1994), or SDC, using acquirer name "shareholders" or spinoff, splitoff, and carve-out dummies. For each spinoff new list, we match the parent company to a U.S. public firm at the time of listing. Relistings occur after a U.S. public firm has been delisted for at least two weeks (not including suspension periods). Reorganizations are cases in which a merger between two public companies results in the creation of a new firm and removal of the old firms (as defined by PERMCO). We identify form changes when a firm that already exists in CRSP but did not meet the U.S. public criteria does so.[19]

Delists are recorded when a firm ceases to be publicly listed for at least two weeks. To classify delists, we follow Fama and French (2004) and use CRSP delisting codes: merger (delisting codes 200–399), cause (codes 400–569 and 574–999), and voluntary (codes 570–573). In CRSP, every PERMNO has one and only one delisting code observation (if a PERMNO has never been delisted, it will have a delisting code of 100 on the last day of available CRSP data). This means that if a firm is delisted and later relisted, no CRSP delisting code is provided for the first delisting. Furthermore, no delisting code is provided if a PERMNO fails to uphold the public-firm criteria listed above but still remains in CRSP. If no CRSP delisting code is available, we classify the delisting reason as unknown.[20] Finally, for CRSP merger delistings we identify the acquiring firm using SDC, CRSP variables 'acquiring PERMNO' and 'acquiring PERMCO', or by hand using web searches.

The value of a new listing is the CRSP market cap on the day of the listing. If this value is unavailable, we use the earliest available market value within two weeks. To estimate the value of a firm at delisting, we use the CRSP variable 'amount after delisting'. If this is missing or equal to zero, we use CRSP delisting price instead. If the delist is not marked in CRSP (i.e., an unspecified delist), or if both amount after delisting and delisting price are missing, we use market cap on the day of delisting. If no market cap data are available on that day, we use the closest available data no more than two weeks before the delisting. If a firm (PERMCO)

[18]https://site.warrington.ufl.edu/ritter/ipo-data/

[19]Examples of form changes include when a company relocates from another country to the U.S., changes the form of its listed equity to common stock, or a SPAC completes an acquisition and changes SIC code from investment vehicle to operating company.

[20]We manually exclude one unknown delisting and relisting: JPMorgan Chase, which changes SIC to 6726 between Sep 9, 2009 and Jan 28, 2010 in CRSP, causing it to disappear from the sample of U.S. public firms during this 4-month period. While this adjustment does not impact our analysis, it removes what otherwise appears as a large value outflow-inflow in this period, despite the firm remaining active and listed on NYSE.

has two or more U.S. public PERMNOs (usually different share classes) simultaneously, we sum the value of these when calculating market cap.

B.2 Data on economic contribution of listed firms

Table 8 shows the annual amount of employment, gross product, R&D spending, and patents generated by U.S. public firms, the U.S. economy as a whole, and majority-owned foreign affiliates (MOFAs), explained below. To calculate the contribution of public firms to U.S. employment, we follow the methodology of Schlingemann and Stulz (2022). For U.S. public firms, we collect the Employees (EMP) variable from CRSP/Compustat Merged Fundamentals Annual (CCM) database from WRDS. We only keep firms that can be matched to our CRSP sample of end-of-year public firms described above. If a firm is missing EMP in one year but not in adjacent years before and after, we replace the missing value with the average of the adjacent values. To find U.S. aggregate employment, we use non-farm employment in December of each year (not seasonally adjusted) as reported by the Bureau of Labor Statistics (BLS) (series ID: CEU0000000001). Since Compustat does not distinguish between the employment and gross product generated by U.S. multinational corporations (MNCs) in the U.S. versus abroad, it is necessary to adjust aggregate U.S. employment to also include output generated by MOFAs of U.S. MNCs. We therefore add MOFA employment from the Bureau of Economic Analysis (BEA) to U.S. employment reported by the BLS.

Schlingemann and Stulz (2022) also provide the methodology that we use to calculate the fraction of U.S. gross product (value added) attributable to public firms. Firm-level gross product is found by summing Operating Income Before Depreciation (OIBDP) and Staff Expense Total (XLR). To fill in missing values of XLR, we find the median ratio of XLR to EMP for industries with at least 20 non-missing observations (firms) in each year. For firms with missing XLR but non-missing EMP, EMP is multiplied with this median ratio to estimate labor expenses. Four industry classifications are used, in order of descending preference: Fama-French 17, Fama-French 12, 2-digit SIC, and finally BLS Supersectors. At the aggregate U.S. level, GDP is from the IMF and MOFA gross product is from the BEA.

To analyze the role of U.S. public firms in innovation, we look at both research and development (R&D) expenditure and patents. Firm-level R&D spending is found in CCM using the Research and Development Expense (XRD) variable. U.S. aggregate R&D spending is reported by the OECD (series name: GERD-SOF) and includes the source of funding. We include all sectors with funding from domestic sources. We also add MOFA R&D spending to the U.S. aggregate with data from the BEA. The BEA does not report MOFA R&D prior to 1989, so we estimate these values by assuming that the ratio of MOFA R&D to value added is the same in 1982–1988 as in 1989. Firm-level patents are from the University of Virginia Darden School of Business Global Corporate Patent Dataset (GCPD) (Bena, Ferreira, Matos, and Pires, 2017). The GCPD reports the annual number of utility patents granted by the U.S. Patent and Trademark Office (USPTO) to publicly listed firms around the world, with complete coverage from 1980–2016. After matching GCPD data to our CRSP sample of public firms and aggregating patent grants by year, we divide by the annual count of USPTO utility patent grants of U.S. origin.

B.3 Data on non-U.S. listings and mergers

To select which countries are included in our international sample, we start with the top 100 countries and territories by GDP per the IMF and as of 2020. For each country, we require listing count data to be available from WDI, WFE, CEIC, or stock exchange homepages. We also require the 2020 listing count to be reported and the country to have at least 10 years of listing count observations. The full list of countries and territories included in each step of the sample selection procedure is available in the Internet Appendix.

U.S. listing data are from CRSP as per above. For non-U.S. countries, the number of listed firms is sourced from WDI and supplemented when necessary with data from the WFE, CEIC, and foreign stock exchange homepages themselves. Data from the following stock exchange's homepages are used: Borsa Italiana, Boursa Kuwait, Bratislava Stock Exchange, Cambodia Securities Exchange, Central Africa Securities Stock Exchange (BVMAC), Euronext, Ghana Stock Exchange, Japan Exchange Group, Nairobi Securities Exchange, Nasdaq Baltic, Nasdaq Nordic, Pakistan Stock Exchange, Prague Stock Exchange, and TMX Group. In some cases, older versions of a stock exchanges homepage are accessed via The Wayback Machine.

The WDI data source raises some issues due to the merging of smaller local stock exchanges within a country. To account for this, we use the data sources listed above to record a consistent set of stock exchanges for each sampled country.[21] As in the U.S., we exclude investment companies, mutual funds, real estate investment trusts (REITs), and other collective investment vehicles. In Panel B of Appendix Figure A.1, we show the time-series of the aggregate listing count for non-U.S. advanced economies and developing/emerging economies from 1980-2020.

[21] For example, the WDI Canadian listings includes only the Toronto Stock Exchange (TSX) prior to 2003, and the sum of the TSX and TSX Venture Exchange (TSXV) afterward (resulting in a one-year jump in the number recorded listed firms from 1,252 to 3,578). The TSXV was formed in 1999 by combining regional Canadian stock exchanges (primarily Alberta and Vancouver). The firm population in these smaller regional stock exchanges is different from that of the country's major stock exchange(s): new ventures are typically smaller and more risky than the more established firms. Based on this population difference, and in order to preserve a consistent time series within any given country, we exclude changes in the WDI listing counts resulting from regional exchange consolidations. In the case of Canada, we therefore use the TSX listing count net of the TSXV. Similarly, for Japan, we exclude listings on the Osaka Exchange from the Japan Exchange Group (JPX) after the exchange consolidation in 2013. While the WDI listing count data for Spain include regional exchanges, these exchanges are consistent over time and we thus keep these data as recorded. Were we to instead use data from Spain's primary exchange (the Mercado Continuo) only, we would have observed a listing peak in 2007 instead of 2015.

We identify international merger transactions using SDC. Deals are required to be completed, result in 100% ownership by the acquirer, and take the deal form merger, acquisition, or acquisition of majority/partial/remaining interest (since the latter also results in delisting). To be counted as public, a target or acquirer must be listed on a major exchange. Targets listed on minor or OTC exchanges are counted as private.

We identify listing peaks if a country's actual listing count is lower in 2020 than earlier in the sample period. The listing-peak year is then the year of the country's listing count maximum. When a country has two identical peak years, we use the most recent year. For five non-advanced countries (Brazil, Bulgaria, Kenya, Nigeria, and Poland), there are two identical peak years. Furthermore, if a country has a second peak at least ten years after the first and with a listing count within 95% of the first peak, we use the year of the second peak. This applies to Belgium, Mexico and Norway.

Appendix Table A. 1: **New lists and delists in the U.S. by type, 1981–2020**

This table shows the total annual (year-end) number of new lists (Panel A) and delists (Panel B) on NYSE, NASDAQ and AMEX. The change in the actual listing count, ΔL is the sum of the following six variables, all of which are defined in Table 1:

$$\Delta L = \begin{cases} Newlists: & IPO + Spin + Misc_{New} \\ Delists: & Merge_{Public-to-Public} + Merge_{Public-to-Private} + Misc_{Del} \end{cases}$$

IPO are initial public offerings, $Spin$ are spinoffs, and $Misc_{New}$ are miscellaneous new listings. $Misc_{Del}$ are miscellaneous delists. The subscript in $Merge$ indicates the direction of the change in the target's public/private status.

A: $Newlists = IPO + Spin + Misc_{New}$

Year	Total lists (L)	$Newlists$	IPO	$Spin$	$Misc_{New}$ Uplists	Relist	Reorg.	Form
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1981	5,073	646	309	0	315	14	4	4
1982	4,999	326	105	0	182	34	4	1
1983	5,571	944	638	0	263	34	5	4
1984	5,691	621	318	8	242	47	4	2
1985	5,652	570	293	11	208	49	4	5
1986	5,930	984	603	10	292	65	1	13
1987	6,222	828	453	13	292	64	5	1
1988	5,955	437	191	14	175	47	8	2
1989	5,770	419	181	14	163	55	3	3
1990	5,634	414	156	15	177	52	7	7
1991	5,672	529	344	5	129	42	3	6
1992	5,801	650	463	13	145	23	2	4
1993	6,334	894	587	15	238	47	4	3
1994	6,634	747	495	15	210	24	3	0
1995	6,861	796	514	13	220	37	8	4
1996	7,325	1,028	747	19	212	30	14	6
1997	7,315	709	490	21	164	21	8	5
1998	6,873	523	299	10	174	21	11	8
1999	6,539	633	467	20	104	28	12	2
2000	6,246	585	347	15	153	47	18	5
2001	5,550	196	76	11	57	37	6	9
2002	5,129	170	69	9	50	32	8	2
2003	4,807	192	68	8	69	42	4	1
2004	4,750	320	172	8	71	52	7	10
2005	4,684	320	160	10	99	43	6	2
2006	4,616	304	164	10	86	35	4	5
2007	4,524	349	195	14	93	41	4	2
2008	4,259	144	36	19	46	33	3	7
2009	4,005	126	44	5	54	16	2	5
2010	3,874	194	100	5	59	25	2	3
2011	3,721	150	88	11	27	20	2	2
2012	3,601	161	116	10	26	3	2	4
2013	3,594	232	173	11	33	11	3	1
2014	3,713	317	225	21	44	20	5	2
2015	3,681	219	140	22	31	21	4	1
2016	3,542	155	84	16	40	13	1	1
2017	3,515	230	140	9	60	12	5	4
2018	3,520	232	156	12	42	12	2	8
2019	3,520	231	153	6	33	13	1	25
2020	3,633	312	228	10	40	20	2	12
Total		17,837	10,587	458	5,118	1,282	201	191
Average	5,108	446	265	11	128	32	5	5

Continued on next page

Appendix Table A. 1: Continued (page 2 of 2)

B: $Delists = Merge_{Public-to-Public} + Merge_{Public-to-Private} + Misc_{Del}$

Year	Actual listing count (L)	$Delists$	$Merge_{Pub-to-Pub}$	$Merge_{Public-to-Private}$				$Misc_{Del}$		
				Acq. by U.S. priv.	Acquired by non-U.S. public	Acquired by non-U.S. private	Acq. by unknown	Cause	Voluntary	Unknown
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1981	5,073	290	97	40	10	11	12	96	1	23
1982	4,999	397	114	51	8	8	10	162	1	43
1983	5,571	373	121	53	0	3	7	144	4	41
1984	5,691	501	127	95	9	4	4	201	15	46
1985	5,652	607	161	78	10	4	10	263	12	69
1986	5,930	708	169	94	23	2	16	317	10	77
1987	6,222	535	160	68	25	4	12	204	9	53
1988	5,955	704	164	145	36	10	13	275	15	46
1989	5,770	605	116	103	33	4	5	280	16	48
1990	5,634	550	97	57	26	5	8	307	7	43
1991	5,672	491	86	20	6	1	1	325	13	39
1992	5,801	520	115	16	2	0	1	328	21	37
1993	6,334	361	131	32	5	1	4	151	9	28
1994	6,634	449	200	28	19	0	1	157	9	35
1995	6,861	567	247	47	20	1	1	204	11	36
1996	7,325	565	305	57	25	4	0	152	6	16
1997	7,315	719	353	76	37	3	2	217	4	27
1998	6,873	967	392	98	47	7	0	368	5	50
1999	6,539	965	377	92	80	6	0	333	7	70
2000	6,246	879	373	109	74	5	0	273	8	37
2001	5,550	891	268	86	49	10	0	394	25	59
2002	5,129	590	161	50	15	4	0	286	28	46
2003	4,807	515	145	68	16	2	0	217	24	43
2004	4,750	376	162	67	14	2	0	94	17	20
2005	4,684	389	142	53	23	6	0	110	30	25
2006	4,616	369	146	82	23	7	1	76	7	27
2007	4,524	441	164	119	40	12	0	85	7	14
2008	4,259	410	105	71	40	3	0	143	25	23
2009	4,005	380	66	38	17	0	0	181	49	29
2010	3,874	326	97	71	22	3	0	105	18	10
2011	3,721	303	65	90	26	5	0	90	8	19
2012	3,601	282	81	76	16	4	0	84	5	16
2013	3,594	239	85	65	13	8	0	48	7	13
2014	3,713	197	79	41	18	3	0	36	6	14
2015	3,681	251	99	35	33	4	0	54	9	17
2016	3,542	293	101	56	27	13	0	84	2	10
2017	3,515	273	94	52	31	11	0	54	8	23
2018	3,520	211	85	42	21	6	0	42	3	12
2019	3,520	232	55	62	24	13	0	59	8	11
2020	3,633	198	39	37	21	8	0	64	13	16
Total		18,919	6,144	2,620	984	207	108	7,063	482	1,311
Average	5,108	473	154	66	25	5	3	177	12	33

Appendix Table A. 2: **Merger-adjusted new lists and delists in the U.S. by type, 1990–2020**

This table shows the total annual (year-end) number of new lists and delists on NYSE, NASDAQ and AMEX that impact the merger-adjusted listing count. The change in the all-merger-adjusted listing count, ΔL_A is the sum of the following six variables, all of which are defined in Table 1:

$$\Delta L_A = \begin{cases} Newlists_A: & IPO + Merge_{Private-to-Public} + Misc^N_{New} \\ Delists_A: & Merge^N_{Public-to-Private} + Divest_{Subsidiary-to-Private} + Misc^N_{Del} \end{cases}$$

The superscript N indicates that the count adjusts for the acquisition index (Eq. 4). IPO are initial public offerings and $Misc^N_{New}$ are miscellaneous new listings. $Misc^N_{Del}$ are misc. delists. The subscript in $Merge^{(N)}$ and $Divest$ indicates the direction of the change in the target's public/private status.

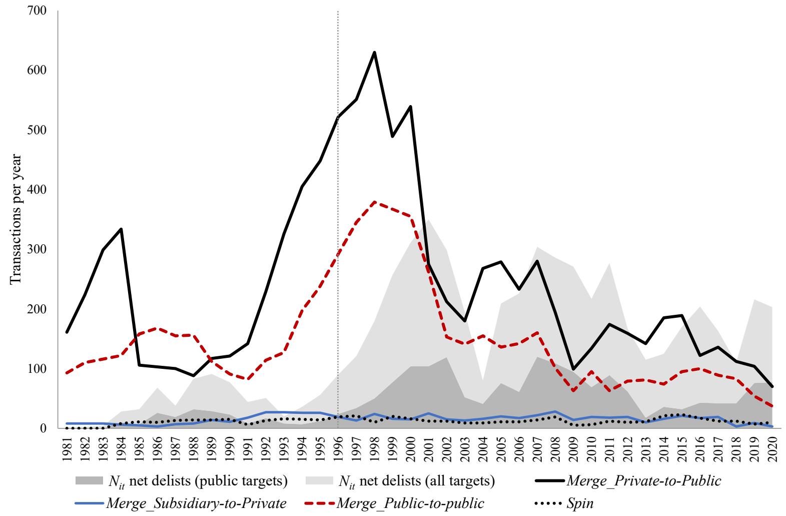
Year	All-merger-adjusted count (L_A)	$Newlists_A$	IPO	$Merge_{Priv-to-Pub}$ U.S. priv. target	$Merge_{Priv-to-Pub}$ Non-U.S. target	$Misc^N_{New}$	$Delists_A$	$Merge^N$ $_{Pub-to-Priv}$	$Divest$ $_{Sub-to-Priv}$	$Misc^N_{Del}$
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1981	5,320	812	309	160	1	342	208	80	8	120
1982	5,574	553	105	224	0	224	299	82	8	209
1983	6,551	1,248	638	298	1	311	271	69	8	194
1984	7,085	951	318	330	4	299	417	140	6	271
1985	7,264	691	293	103	3	292	512	145	5	362
1986	7,730	1,082	603	99	4	376	616	175	3	438
1987	8,220	936	453	96	4	383	446	158	7	281
1988	8,092	523	191	79	9	244	651	278	8	365
1989	8,016	531	181	99	18	233	607	186	14	407
1990	7,989	563	156	108	13	286	590	163	11	416
1991	8,183	692	344	124	18	206	498	40	18	440
1992	8,565	876	463	199	30	184	494	29	27	438
1993	9,488	1,229	587	297	29	316	306	62	27	217
1994	10,311	1,150	495	360	45	250	327	67	26	234
1995	11,130	1,250	514	389	59	288	431	107	26	298
1996	12,279	1,565	747	454	68	296	416	164	19	233
1997	13,010	1,262	490	469	82	221	531	209	13	309
1998	13,361	1,178	299	501	129	249	827	258	24	545
1999	13,592	1,140	467	384	105	184	909	326	16	567
2000	13,850	1,156	347	439	100	270	898	374	15	509
2001	13,305	473	76	216	59	122	1,018	274	25	719
2002	12,924	409	69	158	54	128	790	112	15	663
2003	12,705	416	68	134	46	168	635	155	13	467
2004	12,967	647	172	198	70	207	385	173	16	196
2005	13,073	623	160	208	71	184	517	234	20	263
2006	13,129	578	164	174	59	181	522	319	17	186
2007	13,137	653	195	214	66	178	645	456	22	167
2008	12,833	347	36	134	60	117	651	308	28	315
2009	12,452	239	44	70	29	96	620	151	14	455
2010	12,307	356	100	74	60	122	501	270	19	212
2011	12,084	350	88	117	57	88	573	375	18	180
2012	12,005	327	116	110	49	52	406	197	19	190
2013	12,085	427	173	81	61	112	347	217	10	120
2014	12,302	529	225	137	48	119	312	170	16	126
2015	12,340	437	140	136	53	108	399	195	21	183
2016	12,186	314	84	88	34	108	468	289	17	162
2017	12,174	397	140	93	43	121	409	258	19	132
2018	12,265	356	156	92	20	88	265	172	3	90
2019	12,190	361	153	78	26	104	436	261	9	166
2020	12,195	394	228	58	12	96	389	202	3	184
Total		28,021	10,587	7,782	1,699	7,953	20,542	7,900	613	12,029
Average	10,907	701	265	195	42	199	514	198	15	301

Appendix Figure A. 1: Firm size thresholds and transactions in the merger-adjusted series

The transformation from unadjusted to all-merger-adjusted listing count requires a firm size threshold for $Merge_{Private-to-Public}$ and $Divest_{Subsidiary-to-Private}$. While ignoring industry matching, Panel A shows the time series of three such alternative firm size thresholds (measured in 2020 USD million). These are the 1^{st} percentile market values of IPOs, all listed firms, and all listed firms that also survive and stay listed over the following year. In the empirical analysis, the size threshold is the 1^{st} percentile of listed firms with survivorship requirement, matched with the Fama-French 12 industry classification of the firm. Panel B shows the annual count of the transactions that differentiate the unadjusted, public-to-public merger-adjusted, and merger-adjusted listing counts after applying this size threshold. N_it net delists are delists of accumulated targets minus relists. All transactions are defined in Eqs. (2), (3), and (4) in the text. The vertical dotted line indicates the date of the U.S. listing peak. Sample period 12/31/1980–12/31/2020. Data are from CRSP and SDC.

A: Firm size thresholds for private-to-public mergers and subsidiary divestitures

B: Transactions differentiating the unadjusted and merger-adjusted listing counts



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CHAPTER 3

Debt and Equity Crowdfunding in the Financial Growth Cycle

Debt and Equity Crowdfunding in the Financial Growth Cycle

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Abtract

Since 2016, Regulation CF of the JOBS Act allows small businesses in the US to offer securities to the investing public via online crowdfunding platforms. We investigate firms' choice between issuing crowdfunded debt and equity and how this decision relates to their stage in the financial growth cycle and access to bank financing. We find that firms that are less profitable, are in an earlier developmental stage, and have stronger ties to the banking system are more likely to issue crowdfunded equity than debt. Successful crowdfunding is associated with increases in firm size, revenue, and profitability for early-stage firms, but not for late-stage firms. Our findings suggest that crowdfunding can alleviate capital constraints and stimulate growth for early-stage startups, but has a negligible impact on established firms that are already profitable.

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1 Introduction

It is becoming increasingly challenging for small businesses to take out loans. According to the Federal Reserve's April 2023 Senior Loan Officer Opinion Survey, a large fraction of banks reported tightening lending standards for firm loans, credit card loans, and home equity lines of credit — three of the most common sources of financing for startups — in the first quarter of 2023. This is particularly likely to impact small firms that do not qualify for public listing but are simultaneously unable to attract venture capital (VC) funding.[1] For these firms, alternative sources of capital are likely to become more important as catalysts of economic growth.

In this paper, we address two such alternatives: debt crowdfunding and equity crowdfunding. Since 2016, Regulation CF of the JOBS Act allows small businesses in the US to offer securities to individual investors via online crowdfunding platforms, with $530 million raised as of 2021. We investigate firms' decision to issue crowdfunded debt versus equity and how this choice relates to their stage in the financial growth cycle (Berger and Udell, 1998; Cole, Liang, and Zhang, 2020) as well as access to other sources of external financing. We find that firms that are less profitable, are in an earlier developmental stage, and have stronger ties to the banking system are more likely to issue crowdfunded equity than debt. Successful crowdfunding is associated with increases in firm size, revenue, and profitability for early-stage firms, but not for late-stage firms. Our findings suggest that crowdfunding can alleviate capital constraints and foster growth for early-stage firms, but has a negligible impact on more mature firms that are already profitable.

In order to issue debt or equity via crowdfunding, an entrepreneur needs to file Form C with the Securities and Exchange Commission (SEC), disclosing information about the firms' financials, risk factors, business plan, leadership team and intended use of proceeds, as well as the type of security issued (debt or equity) and the crowdfunding goal (the amount that the entrepreneur intends to raise). The registrant also needs to select a crowdfunding

[1] Nanda and Phillips (2022) report that only 0.5% (0.4%) of the firms in the US Survey of Business Owners use VC funding to start (expand) their business, while 22% (20%) use business loans from banks, 14% (18%) credit cards, and 7% (4%) home equity.

platform (website) on which to issue securities, with platforms generally specializing in either equity or debt securities.[2] An important function of both Form C disclosure and platform due diligence (Cumming, Johan, and Zhang, 2019) is to reduce the information asymmetry that traditionally makes it difficult for entrepreneurs to secure external debt from providers other than banks (Diamond, 1984, 1991). If the entrepreneur manages to meet their crowdfunding goal, the campaign is considered successful and the securities are issued. If not, the funds are returned to the investors.[3]

We collect data from SEC Form C filings to construct a sample of 2,052 crowdfunding campaigns from 2016–2021, 1,697 of which are equity issuances and 355 debt. We supplement these data with firm-level characteristics from FactSet, SEC Form D filings on previous security issuances, and industry classifications from Capital IQ and web searches. We also include ZIP- and county-level data from the US Census Bureau, IPUMS (Manson, Schroeder, Riper, Kugler, and Ruggles, 2022), and the Federal Deposit Insurance Corporation (FDIC), among others.

We start by examining the factors associated with a firm's choice between debt and equity crowdfunding. The pecking order theory (Myers and Majluf, 1984) suggests that firms prefer debt over equity when seeking external capital due to lower information costs. Alternatively, the financial growth cycle framework proposed by Berger and Udell (1998) suggests that the hierarchy of financing options depends on firm size and development stage, as there are different levels of information asymmetry and financial needs for each phase of growth. In the spirit of Cole, Liang, and Zhang (2020), we categorize firms into three stages of the financial growth cycle that are appropriate for smaller entrepreneurial firms: a first stage where firms

[2]Equity issuances most often consist of common stock. Debt contracts vary; Some resemble traditional bonds with a predetermined yield and maturity, while others entitle investors to a percentage of the business's revenue each quarter until they reach a predetermined return on their investment or the note reaches maturity (thus resembling a royalty contract with maturity and capped payouts).

[3]The focus of our paper is securities crowdfunding (also referred to as return-based crowdfunding), which is distinct from project-based crowdfunding via platforms like Kickstarter. In the latter, individuals pledge capital in exchange for a specific product or service, whereas the former gives retail investors shares in the company itself (equity) or the right to pre-specified cash flows (debt). The incentives for entrepreneurs differ between these two types of crowdfunding; Project-based crowdfunding aims to deliver a specific product within a defined timeframe, while return-based crowdfunding is appropriate for investors with a long-run investment horizon due to the illiquidity of crowdfunded securities. Unless otherwise specified, "crowdfunding" in this paper refers to securities crowdfunding.

have assets in place but do not generate revenue, a second stage where firms have positive revenue but are unprofitable, and a third stage where firms achieve profitability to generate positive revenue and net income. We find that the capital structure of crowdfunded firms tends to follow a growth cycle pattern. More specifically, early-stage startups are more likely than late-stage startups to fund themselves with equity crowdfunding. As firms move on from their introductory developmental phase, they tend to rely more on debt-based crowdfunding, consistent with improved financial stability and creditworthiness.

Next, we investigate how the availability of traditional bank financing is related to the firm's choice of crowdfunding offering. Previous studies in the banking literature document that banks are prone to establish lending relationships with borrowers located in close proximity to their branches and that lending to small businesses is usually restricted to local markets (Agarwal and Hauswald, 2010; Brevoort, Wolken, and Holmes, 2010; Nguyen, 2019). Likewise, the distance between entrepreneurs and offline early-stage investors, such as banks, venture capitalists, and angel investors, has been shown to be a barrier to small business financing (Cumming and Dai, 2010; Stuart and Sorenson, 2003). Since online funding platforms can reduce these distance-related costs, we hypothesize that debt crowdfunding can serve as a substitute for bank lending when the entrepreneur has limited access to traditional offline funding sources (Agrawal, Catalini, and Goldfarb, 2015; Vulkan, Åstebro, and Sierra, 2016).

Our results support the substitution hypothesis. We find that firms located in areas with access to a larger number of bank branches (proxying for access to bank loans) are more likely to issue crowdfunded equity. We also observe the same pattern for firms located in areas with higher house prices (proxying for access to home equity).

To conclude our analysis, we investigate whether successful crowdfunding is associated with realized gains in firm size and performance. Theoretically, it is ex-ante ambiguous whether to expect crowdfunding to result in positive firm outcomes, i.e., whether entrepreneurs are willing and able to put the funding to productive use. For example, due to high information asymmetry and moral hazard in crowdfunding markets, entrepreneurs may be less competent, take on riskier projects, and be more likely to commit fraud than entrepreneurs seeking traditional sources of funding (Agrawal, Catalini, and Goldfarb, 2014).

To analyze the relationship between crowdfunding and firm growth, we compare firms that successfully issue crowdfunded debt or equity to a sample of matched private firms from Factset in a diff-in-diff setting (as in Boucly, Sraer, and Thesmar (2011)). We find that crowdfunding firms increase their total assets, revenue, and profitability relative to the control sample. We also show that this difference is largest for first-stage firms, with the relationship weakening as firms mature. While the change in profitability associated with crowdfunding is positive and significant for both first- and second-stage firms, it is insignificant for third-stage firms. Our results suggest that crowdfunding can improve operational performance for firms that are not yet profitable but has a negligible impact on more mature, profitable, firms.

Related literature. Our paper primarily contributes to two strands of literature. First, we add to the literature on securities crowdfunding (see Mochkabadi and Volkmann (2020) and Bollaert, Lopez-de Silanes, and Schwienbacher (2021) for recent surveys) and Regulation Crowdfunding (CF) of the Jumpstart Our Business Startups (JOBS) Act. This paper is, to our knowledge, the first to investigate the choice between issuing crowdfunded debt or equity as well as how firm characteristics relate to this decision. While several papers explore either debt or equity crowdfunding in isolation, what motivates firms to choose between these two security types has not previously been documented. The only other paper addressing equity and debt crowdfunding simultaneously that we are aware of is Cumming, Johan, and Reardon (2022), who show that equity offerings are more likely to be successful and raise more capital than debt offerings.

Previous empirical evidence on whether securities crowdfunding facilitates firm growth is limited and mixed. Using a sample of UK firms, Eldridge, Nisar, and Torchia (2021) find that equity crowdfunding is associated with improved return on assets (ROA) but not increased innovation activity. Havrylchyk and Mahdavi Ardekani (2020) do not observe any relationship between debt crowdfunding and sales growth, investment, employment, or profitability for a sample of French firms. Hornuf, Schmitt, and Stenzhorn (2017), Butticè, Di Pietro, and Tenca (2020), and Dolatabadi, Fracassi, and Yang (2021) show that successful equity crowdfunding campaigns are associated with a higher likelihood of subsequent venture capital funding and higher survival rates. Our results show that post-crowdfunding growth is related to the firm's growth cycle stage, which may partially reconcile why prior papers have observed positive effects associated with equity (early-stage) crowdfunding, but not debt (late-stage) crowdfunding.[4]

Second, we contribute to prior work on the capital structure and growth of small entrepreneurial firms (see Ewens and Farre-Mensa (2022) and Nanda and Phillips (2022) for recent surveys). Due to data limitations, most studies on entrepreneurial financing decisions focus on small, privately held firms using data from surveys like the Federal Reserve Board's Surveys of Small Business Finances or the Kauffman Firm Surveys (Berger and Udell, 1998; Cole and Sokolyk, 2018; Coleman, 2002; Robb and Robinson, 2012). Berger and Udell (1998) find that small firms rely more on debt financing during their early growth stages but decrease their reliance on debt as they mature. Robb and Robinson (2012) show that young firms rely more on external debt financing and less on friends-and-family-based funding sources. More recently, Cole, Liang, and Zhang (2020) look at sources of debt financing for small firms that trade over-the-counter (OTC). We contribute by providing the first evidence on the relationship between growth cycle patterns for startups and crowdfunding decisions, as well as showing that growth outcomes following crowdfunding are related to the firm's growth cycle stage.

The rest of this article proceeds as follows. In Section 2, we describe the institutional framework that motivates the article. In Section 3, we describe the data and provide summary statistics. Sections 4 through 5 present the empirical analysis, and Section 6 concludes.

2 Institutional background

The JOBS Act, signed into law on April 5, 2012, aims to facilitate capital raising for startups and small businesses by allowing them to offer securities to a wider pool of investors at lower costs. On October 30, 2015, the SEC adopted the final rules for Regulation CF, which became effective on May 16, 2016. Under Regulation CF, US private firms can raise up to $1.07 million

[4]While this study focuses on existing firms' growth, other studies analyze whether crowdfunding is conducive to new business formation. Rashidi Ranjbar (2022) finds that the passage of both state-level crowdfunding legislation and Regulation CF increases the number of new business applications, but that only the former results in successful business formation. Lambert, Ralcheva, and Roosenboom (2022) show that project-based crowdfunding (Kickstarter) is positively associated with business formation and average establishment size at the county level.

in a 12-month period by issuing debt or equity securities. As of 2021, the maximum aggregated offering amount in a 12-month period is increased to \$5 million.

Prior to Regulation CF, debt and equity crowdfunding was limited to accredited investors, typically high-income or high-net-worth individuals. Regulation CF expands investment opportunities to non-accredited (retail) investors, allowing them to purchase debt or equity securities issued through crowdfunding. To comply with SEC requirements, issuers must disclose both quantitative and qualitative information by filing Forms C, C-U, and C-AR, making this information publicly available at least 21 days before the securities are sold. Additionally, the offering must be conducted through a broker-dealer or a SEC-registered portal, which is a new type of intermediary introduced by the JOBS Act.

The disclosure requirements in Regulation CF are designed to protect investors from fraud and ensure the reliability of the information provided by businesses. To mitigate the risk of fraudulent activities, the JOBS Act introduces three additional measures. First, it sets limits on the amount that individuals can invest annually (up to 10% of their income or net worth), thereby limiting potential losses. Second, it enables civil actions against issuers, directors, and officers who provide false or misleading statements. Third, it grants the SEC authority over funding portals to enforce regulations and mandates for both issuers and intermediaries.

3 Data

3.1 Data sources

Our primary data source is the Electronic Data Gathering, Analysis, and Retrieval system (EDGAR) operated by the SEC. EDGAR serves as the primary system for companies and other entities submitting documents under various securities acts. We construct a sample of crowdfunding security offerings in the US under Regulation CF from July 2016 to the end of 2021. Regulation CF requires companies issuing securities through crowdfunding to disclose Forms C and C-U with the SEC, investors, and the intermediary facilitating the offering. These filings detail the firms' financials, risk factors, business plan, leadership team, and intended use of proceeds, as well as the type of security issued (debt or equity) and the crowdfunding goal (the amount that the entrepreneur intends to raise). These filings allow us to record information about the issuing firms' financial statements at the time of the offering, one year prior to the offering, and, if the offering is successful, one year after the crowdfunding campaign (Form C-AR).

Our main sample of analysis is a cross-section of 2,052 firms that launched a crowdfunding campaign in 2016–2021. To exclude firms that are crowdfunding but have not yet formed, we require firms to have non-zero assets. We winsorize all continuous variables at the 2% and 98% tails. Since industry codes are not specified in Form C filings, we manually collect SIC codes using Capital IQ and via manual web searches.

To get information about prior security issuances, we collect information from Form D filings in EDGAR. Firms that raise capital through private placement of securities under Regulation D are required to fill out Form D. These data allow us to get information about additional capital raised through institutional investors by firms in our sample. In particular, we are able to assess whether firms raise capital by issuing securities through other venues before and/or after the crowdfunding offering.

In order to investigate the relationship between crowdfunding and bank lending, we gather data on banks from the Board of Governors of the Federal Reserve System. These data provide information about the number of bank branches at the ZIP code or county level. We also collect the house price index (HPI) at the ZIP code level from Federal Housing Finance Agency).

To construct a control group of private firms, we rely on FactSet. FactSet allows us to access information about private firms in the US from 2015 to 2021. We construct a matched control sample by matching crowdfunding firms in the year before they issue crowdfunding securities to FactSet firms using propensity score matching on industry (SIC-2), ROA, and total assets.

Finally, we supplement our analysis with macroeconomic variables and Census data at the ZIP-code level from the IPUMS National Historical Geographic Information System (NHGIS) (Manson, Schroeder, Riper, Kugler, and Ruggles, 2022). IPUMS NHGIS offers easy access to summary tables and time series of population, housing, agriculture, and economic data for various levels of US census geography. In particular, we use data from the 2020 American Community Survey: 5-Year Data (2016-2020) for county-level control variables.

3.2 The US crowdfunding market

In this section, we describe the crowdfunding market governed by Regulation CF from 2016–2021. We start by presenting information about the number of total and successful offerings by year. Figure 1 shows that the number of security offerings increases from 192 in 2016 to 1,586 by 2021. The unconditional probability for a campaign to be successful remains fairly stable at around 40% during 2016–2020, but dips to 24% in 2021.

Figure 2 Panel A shows the quarterly amount successfully raised by crowdfunding firms in USD millions. Firms raised around $10 million in the third quarter of 2016, an amount which grows to $115 million by the fourth quarter of 2021, in part because Regulation CF was amended in 2021 to allow an increase in the maximum amount firms are allowed to raise via crowdfunding. In Panel B, we plot the average number of days that it takes a campaign to reach its goal. On average, it takes 150 days for a firm to meet its funding goal, but this figure starts to decline in 2021. The dramatic drop in the fourth quarter is mechanical: Since the sample ends in 2021, the closing date is recorded for only the fastest and most successful crowdfunding campaigns.

Next, we provide a more granular analysis of crowdfunding intermediaries. As of 2021, more than 100 internet portals are registered with the SEC. Figure 3 plots the number of internet portals acting as intermediaries from 2016–2021. However, more than 70% of the offerings are intermediated by only eight portals and the two most popular portals (Wefunder and StartEngine) manage as much as 45% of the offerings (see Figure 4). Thus, even though the number of registered portals is large, the intermediaries market is heavily concentrated, likely because network effects attract issuers to platforms that already have a large investor base.

Finally, we are interested in the location and legal status of issuing firms. Figure 5 shows the number of offerings per county in our sample period. Most of the issuers are headquartered in California, Florida, New York, and Oregon. Furthermore, 60% of the companies are corporations and 38% limited liabilities companies.

3.3 Summary statistics

Table 1 presents summary statistics for our sample of 2,052 crowdfunding firms. The table is divided into three panels: issuing firm characteristics (Panel A), offering characteristics (Panel B), and macro variables (Panel C). Table A1 provides detailed definitions for all variables.

Panel A displays firm characteristic sample statistics for several variables: profitability (ROA), the size of the firm measured as the natural log of total assets (Size), cash holdings (Cash), leverage measured as total debt over total assets (Leverage), sales measured as the natural log of total sales (Log sales), firm age in a number of years (Age), and the number of employees measured as the natural log of the total number of employees (Log employees). Issuers tend to be small firms both in terms of size (mean assets are $708,000 and median $103,000) and number of employees (mean 9 employees and median 4). Comparing firms issuing debt and equity reveals that the former is on median smaller but has a higher fraction of large issuers (resulting in a higher average), with average (median) assets of $1.07 million ($66,000) versus $632,000 ($114,000) for equity issuers. Equity issuers also tend to be younger, more levered, and less profitable than debt issuers.

Panel B summarizes offering campaign characteristics, with the amount of funding sought (Amount offered), price per security (Price security), type of security offered (Type of security, where 1 is debt and 0 equity), whether the campaign was successful (Success), and whether the firm had previously raised capital from institutional or accredited investors (Previous Institutional Funding). Firms seek to raise $63,000 on average ($25,000 median), with an average security price of $487 for debt and $92 for equity. 17% of the issuances are debt versus 83% equity, and 37% of campaigns are successful. Notably, around 25% of the sample has previous funding from institutional or accredited investors according to Form D filings, with a smaller fraction of debt issuers (14%) than equity (27%).

Panel C presents information regarding macro variables. Bank Density is the natural log of the total number of bank branches within 150 miles of the issuer's location. Top Bank is a dummy variable that takes the value of 1 if the issuer is located in an area that is in the top quartile of the Bank Density distribution, and 0 otherwise. Total population, Median Income, Frac. White, and Num. of Establishment are variables at the county level. A comparison of debt and equity issuers suggests that debt issuers are headquartered in areas with more access to banks and slightly lower median income.

Finally, Table 2 shows the industry distribution (classified by SIC-2 code) of our full sample as well as the subsamples of debt and equity issuers. Business services (in particular computer software) is the largest industry among equity issuers (19%) and second largest among debt issuers (15%). Food products (often breweries and distilleries) and eating and drinking places (mostly restaurants) also account for a large fraction of debt issuers (29%) and a smaller, but still significant, fraction of equity issuers (12%). Other represented industries among equity (debt) issuers include miscellaneous retail and wholesale trade at 8% (8%), engineering, research, and management services at 4% (5%), amusement and recreation services at 4% (3%), and chemicals and allied products at 3% (2%).

4 The Choice of Debt versus Equity Crowdfunding

4.1 Crowdfunding and the Financial Growth Cycle

How do firms choose between debt and equity crowdfunding? The pecking order theory, as developed by Myers and Majluf (1984), predicts that if capital is needed for new investment opportunities, firms have a preference for internal financing over external financing due to adverse selection. When outside funds are needed, firms prefer debt over equity because debt issues are associated with lower information costs. Equity is seldom issued. However, this theory does not account for several broad patterns of corporate finance. In particular, small high-growth firms are typically thought to have significant information asymmetries, making them particularly susceptible to adverse selection problems. Frank and Goyal (2009) find evidence that such firms generally do not act in accordance with the pecking order theory.

In Table 3, we run cross-sectional firm-level OLS regressions with security choice (1 if debt, 0 if equity) as the dependent variable. The control variables include a set of firm characteristics (profitability, size, cash holdings, long-term leverage, and short-term leverage) as well as year and industry fixed effects varying by column. Columns 1–3 contain the full sample of 2,052 firms, 4–5 the subsample of successful issuers, and 6–7 the subsample of failed issuers. The table shows that more profitable firms are more likely to issue debt, which is consistent with them being better able to service debt than less profitable firms. We also find that larger issuers are more likely to issue equity crowdfunding, although this relationship is not statistically significant for the subsample of successful crowdfunders. Finally, we note that firms with higher leverage are more likely to issue equity than debt. This could have several potential explanations, including levered firms (1) not needing to turn to crowdfunding for debt funding since they already have access to bank lending (which we explore further in Section 4.2), (2) being unable to issue further debt due to borrowing constraints, or (3) using crowdfunding to reduce their leverage and bankruptcy risk.

As noted by Berger and Udell (1998), the pecking order hierarchy depends on the size and stage of development of the firm, as there are different levels of information asymmetry and financial needs for each phase of growth. We next investigate whether the likelihood of issuing debt crowdfunding increases as the firm progresses through the financial growth cycle. We define three growth cycle stages appropriate for startups in the spirit of Cole, Liang, and Zhang (2020): a first stage where firms are pre-revenue, a second stage where firms have positive revenue but are not yet profitable (negative or zero net income), and a third stage where firms achieve profitability to generate positive revenue and net income. Since businesses establish more solid track records (reducing information asymmetry) and start to generate steady revenue streams as they progress through these stages, we expect debt crowdfunding to become a more viable financing option for these firms as they mature.

In Table 4, we run the same set of regressions as in Table 3, but add two additional independent variables: a dummy designating that the firm is a second-stage firm (revenue-generating but not profitable) and a dummy for third-stage firms (revenue-generating and profitable). Our results indicate a monotonic and positive relationship between stage and the likelihood of issuing debt: As per Column 3, firms in the second stage are 4.6pp likelier to issue debt over equity, and firms in the third stage are 13.3pp likelier. In other words, more mature firms with positive cash flows are more likely to choose debt crowdfunding when available, allowing them to access funding without relinquishing ownership or control of their business. In contrast, early-stage firms that have not started generating revenues are the most likely to opt for equity issuance. These startups do not have a track record of stable cash flows and may be more informationally opaque for investors, which makes debt financing less attractive.

In Table A2 of the Appendix, we present consistent results when using age as an alternative measure for the firm's financial growth cycle. There are several reasons why we use age to proxy for growth cycle stage only for robustness. Faff, Kwok, Podolski, and Wong (2016) argue that firm age is not a reliable indicator of a firm's growth cycle stage, as the time it takes for a firm to transition across growth cycle stages can vary by industry, and firms of the same age can learn at different rates based on their feedback mechanisms. Furthermore, using age as a proxy for the growth cycle stage assumes that a firm progresses linearly through the cycle, which may not be the case (Dickinson, 2011).

4.2 Crowdfunding and Access to Bank Lending

Next, we ask whether debt crowdfunding can act as a substitute for bank lending for borrowers with limited access to capital through traditional banking channels. A large body of research in banking establishes that banks constrain their lending to areas surrounding their bank branches, and that lending to small businesses is usually restricted to local markets (Agarwal and Hauswald, 2010; Brevoort, Wolken, and Holmes, 2010; Nguyen, 2019). Accordingly, areas with a higher concentration of bank branches are known to have more competitive banking markets, resulting in improved credit access. In the same vein, the distance between entrepreneurs and offline early-stage investors, such as banks, venture capitalists, and angel investors, has been shown to be a barrier to small business financing (Cumming and Dai, 2010; Stuart and Sorenson, 2003). Since online funding platforms can reduce these distance-related costs, crowdfunding is anticipated to improve the odds for entrepreneurs located in areas underserved by traditional offline funding sources to secure outside capital (Agrawal, Catalini, and Goldfarb, 2015; Vulkan, Åstebro, and Sierra, 2016).

To distinguish between the effects of bank access and demographic differences in loan demand, we follow a similar approach as Erel and Liebersohn (2022) and control for county fixed effects. These capture systematic differences in the financial environment across counties (e.g., local business cycle or economic factors). In addition, we control for plausible demand-side factors by adding local demographic and income controls such as median income, the proportion of the white population, the total population, and the number of establishments within each ZIP code. Our baseline regression specification is as follows:

$$Equity_{i,t} = \beta BankAccess_{t-1} + Controls_{z,t} + \varphi_t + \gamma_s + \delta_c + \epsilon_{i,t} \tag{15}$$

where i, s, z, c and t index crowdfunding campaign, industry sectors, ZIP codes, counties, and time, respectively. We are primarily interested in β, the coefficient on bank access measurements. It is difficult to measure a firm's access to bank lending directly, which makes it necessary to apply proxies instead. We proxy for bank access using two different measures. The first is the log local house price index (HPI) measured at the ZIP code level. Home equity is one of the most frequent sources of funding for startups (Nanda and Phillips, 2022), so we expect HPI to be positively correlated with greater access to bank lending. The second measure is the number of bank branches within 150 miles. We also use a dummy equal to one if the firm is located in the top quartile of ZIP codes by the number of bank branches within 150 miles.

To investigate whether debt crowdfunding can substitute for bank lending, Table 5 presents similar cross-sectional regressions as in Tables 3 and 4, but with the addition of the HPI variable, controls for ZIP-level economic and demographic conditions, and county fixed effects. We observe a negative and significant relationship between local house prices and a firm's likelihood of issuing debt instead of equity. In Column (5), which controls for year, industry, and county fixed effects, we estimate that a one standard deviation increase in HPI corresponds to a 2.9% lower likelihood for a firm to choose debt financing. This suggests that as home values increase — and entrepreneurs have more home equity to tap for funding — firms become more likely to seek equity crowdfunding instead of debt.

In Table 6, we again address the same question but with the second proxy for bank access: the number of bank branches within 150 miles of the firm's headquarters. Column 1 shows that firms located in areas with more bank branches (proxying for better access to bank loans) are less likely to issue crowdfunded debt. One log-point increase in bank branches within 150 miles is associated with a decrease in the likelihood of obtaining crowdfunded debt by about 0.62. The standard deviation of Log Bank Density is 0.69, so a one standard deviation increase in the log number of bank branches within 150 miles is associated with an approximately 42.9% decline in the odds of getting debt crowdfunding compared to the median. In Columns 2 and 3, year fixed effects are used to control for intertemporal variation in the crowdfunding choice, and industry fixed effects are used to control for unobservable, time-invariant differences across industries. The estimates obtained when including county fixed effects alone, as shown in Column 1, exhibit a similar magnitude to those obtained when incorporating year and industry effects, as presented in Columns 2 and 3. In Columns 4–6, we rerun our analysis with Top Bank Density (150 miles) as the alternative measure of bank access, showing consistent results across all specifications. As per Column 6, we estimate that a firm is 9.8pp less likely to choose debt financing if it is located in a ZIP code that is in the top quartile in terms of the number of nearby bank branches.

5 Crowdfunding and growth

In this section of the paper, we assess whether successful crowdfunding is associated with real growth outcomes, and how these outcomes relate to the firm's stage in the financial growth cycle. As discussed in Section 1, theory does not give a clear indication of whether to expect crowdfunding to result in improved performance due to issues of information asymmetry and moral hazard. Moreover, prior empirical evidence is ambiguous on whether crowdfunding fosters growth.

To analyze the relationship between crowdfunding and firm growth, we compare firms that successfully issued crowdfunded debt or equity to a sample of matched private firms from Factset in a diff-in-diff setting (as in Boucly, Sraer, and Thesmar (2011)). We create a matched set of control firms from the period 2016–2021 using propensity-score matching on the following variables, measured in the year before the treated firm launches its crowdfunding campaign: SIC-2 industry, ROA, and total assets. We additionally require matched firms to have non-missing data in the year after they are matched (i.e., the counterfactual year after crowdfunding). Due to data limitations, we are only able to analyze a two-period setting: one year before crowdfunding and one year after. Consequently, we can only evaluate short-term effects associated with crowdfunding.[5]

In Table 7, we run diff-in-diff panel regressions with two-way fixed effects (firm and year) for six different outcome variables: size (log total assets), log revenue, profitability (ROA), cash holdings, book leverage, short-term leverage, and long-term leverage. We include a post-period control dummy (equal to one if the observation represents the (matched) year after crowdfunding) and a post-period and treated interaction variable, which is our primary variable of interest and captures the estimated effect associated with crowdfunding after the campaign has concluded.

We find that crowdfunding firms increase their total assets, revenue, and ROA relative to similar firms that do not issue securities via crowdfunding. More specifically, successful crowdfunding is associated with a 42% increase in size, 46% increase in revenue, and a 0.96 higher ROA (for comparison, the pre-crowdfunding sample average ROA is 2.26). Short-term leverage is expected to decrease by 0.17, consistent with a majority of the offerings in the sample being equity. In other words, compared to similar firms that do not issue crowdfunded securities, issuers appear to grow in size while simultaneously improving their performance. This suggests that any information asymmetry and moral hazard problems present during crowdfunding do not fully disincentivize entrepreneurs from putting crowdfunded capital to good use.

[5]Our sample is limited since firms that issue securities according to Regulation CF are only required to disclose financials once prior to crowdfunding and once after the campaign succeeds (no more than 120 days after fiscal year-end). Thus, we can only observe multiple post-crowdfunding years of data for a firm if it for some reason has to extend its filing period or if it makes subsequent Form C filings in conjunction with follow-on crowdfunding campaigns.

In Section 4, we showed that the firm's choice of debt versus equity securities is related to its stage in the financial growth cycle. Next, we investigate whether the growth effects seen above also vary by developmental stage. To do so, we include controls in Table 8 for the growth stage as well as a pair of three-way interaction variables: post-period times treated times growth stages two and three, respectively. This allows us to estimate the relative growth effects associated with successful crowdfunding for startups in their first, second, and third stages of development.

Table 8 shows large increases in size (83%), revenue (90%), and ROA (1.25) for first-stage startups that successfully crowdfund versus similar firms that do not. Relative to first-stage firms, however, second- and third-stage firms see significantly lower gains in size (-71% and -50%) and revenue (-59% and -74%), with third-stage firms additionally seeing less of an increase in ROA (-1.19). Relative to control firms without crowdfunding, only second-stage firms see gains in revenue (90%–59%=31%, significant at the 5% level) and profitability (1.25–0.21=1.04, significant at the 10% level). In contrast, third-stage firms that successfully crowdfund do not see significant gains in size, revenue, or profitability. In other words, the positive real economic effects associated with crowdfunding appear related to the firm's developmental stage, with startups that have yet to become profitable seeing significant operating gains while profitable, more mature, firms do not show signs of improvement.

Our findings may provide new context as to why prior empirical studies yield mixed predictions regarding the relationship between crowdfunding and growth. In particular, Eldridge, Nisar, and Torchia (2021) finds a positive relationship between equity crowdfunding and ROA for UK firms, while Havrylchyk and Mahdavi Ardekani (2020) do not observe any relationship between debt crowdfunding and sales growth or profitability for a sample of French firms. We document that both the firm's choice of security type — debt versus equity — and post-issuance gains in revenue and profitability are closely related to the firm's stage in the financial growth cycle.

6 Conclusion

Regulation CF of the JOBS Act allows small businesses in the US to offer crowdfunded debt and equity securities to individual investors. In this paper, we raise several questions regarding this recent source of startup capital: Which types of firms choose to issue crowdfunded debt, and which choose equity? How does this decision relate to the firm's stage in the financial growth cycle and access to bank lending? Is successful crowdfunding associated with realized improvements in firm size and profitability?

We start by examining the factors associated with a firm's choice between debt and equity crowdfunding. We find that larger, less profitable, and more levered firms are less likely to select debt when issuing securities via crowdfunding. We also find that the capital structure of crowdfunded firms tends to follow a growth cycle pattern. Specifically, early-stage startups are more likely than late-stage startups to finance their growth through equity crowdfunding. As firms develop, they tend to rely more on debt-based crowdfunding, potentially because improved financial stability and creditworthiness make debt financing less costly.

Next, we investigate how the availability of traditional bank financing is related to the firm's choice of crowdfunding security type. We find evidence consistent with debt crowdfunding serving as a substitute for bank lending. We show that firms located in areas with higher house prices (proxying for access to home equity, a frequent source of funding for startups) and a higher number of bank branches (proxying for access to bank loans) are more likely to issue crowdfunded equity.

To conclude our analysis, we investigate whether successful securities crowdfunding is associated with realized increases in firm size and performance. We compare firms that successfully issued crowdfunded debt or equity to a sample of matched private firms from Factset. We find that crowdfunding firms increase their total assets, revenue, and ROA relative to the control sample. This difference is largest for first-stage firms, with the relationship weakening as firms mature. While the positive association between crowdfunding and ROA is positive and significant for both first- and second-stage firms, it is insignificant for third-stage firms. Our results suggest that crowdfunding can improve operational performance for firms that are not yet profitable but has a negligible impact on more mature, profitable, firms.

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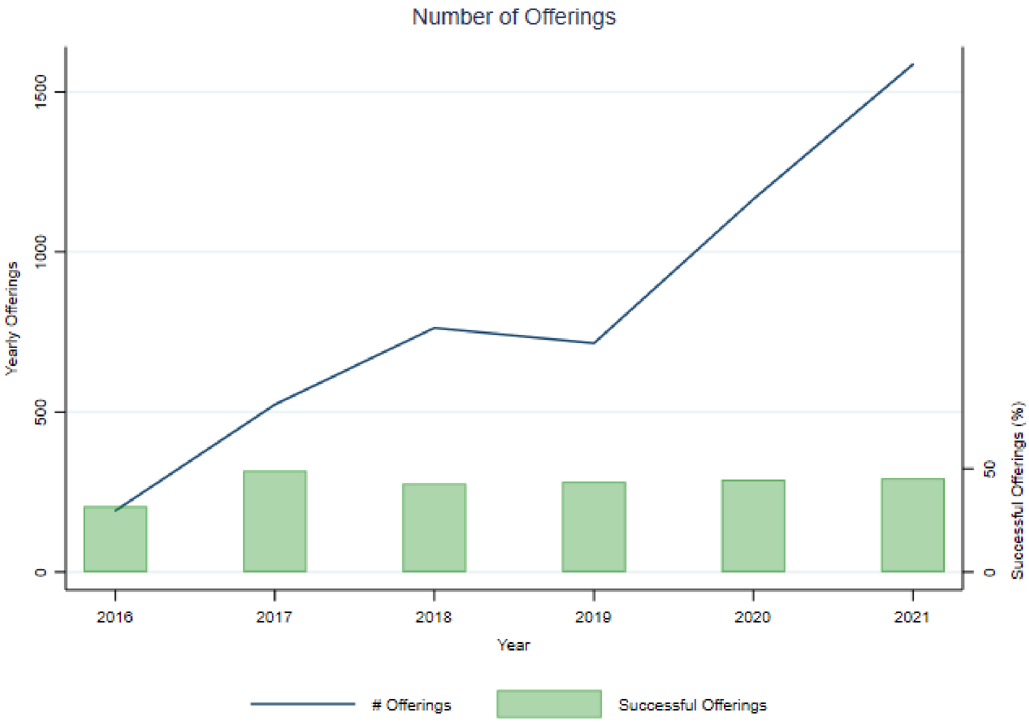

Figure 1: **Yearly offerings**. This figure shows the total number of offerings and the number of successful offerings from 2016 through 2021. Data come from EDGAR.

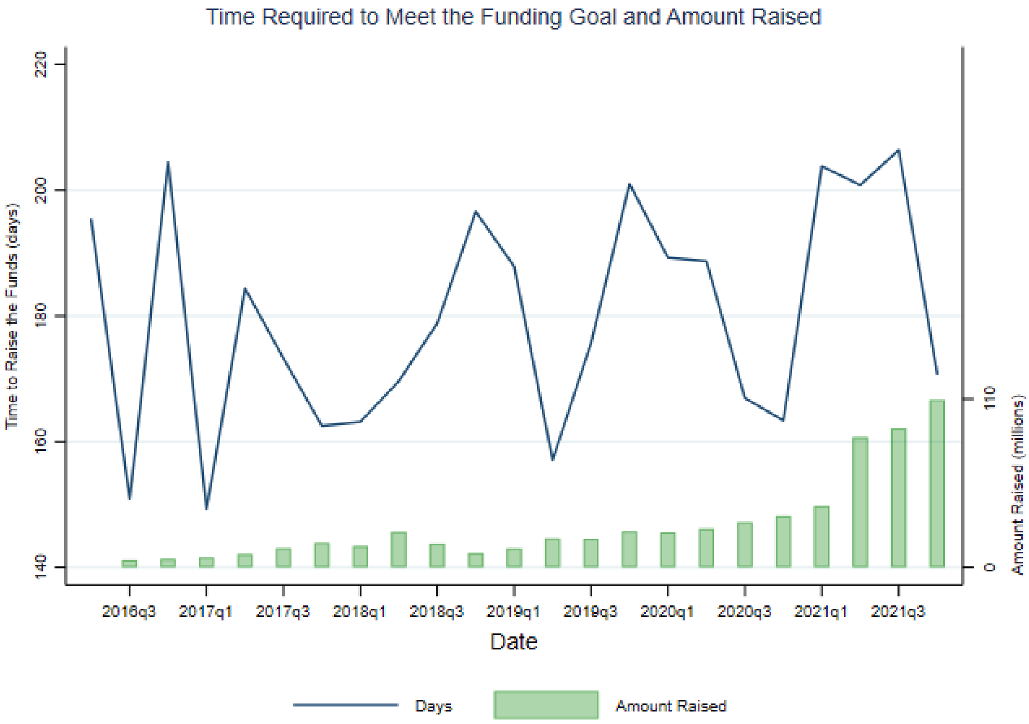

Figure 2: **Time required to meet the funding goal and total amount raised**. These figures show respectively the total amount raised through crowdfunding (in millions USD) and the time required to raise the funds (in days). Data comes from EDGAR.

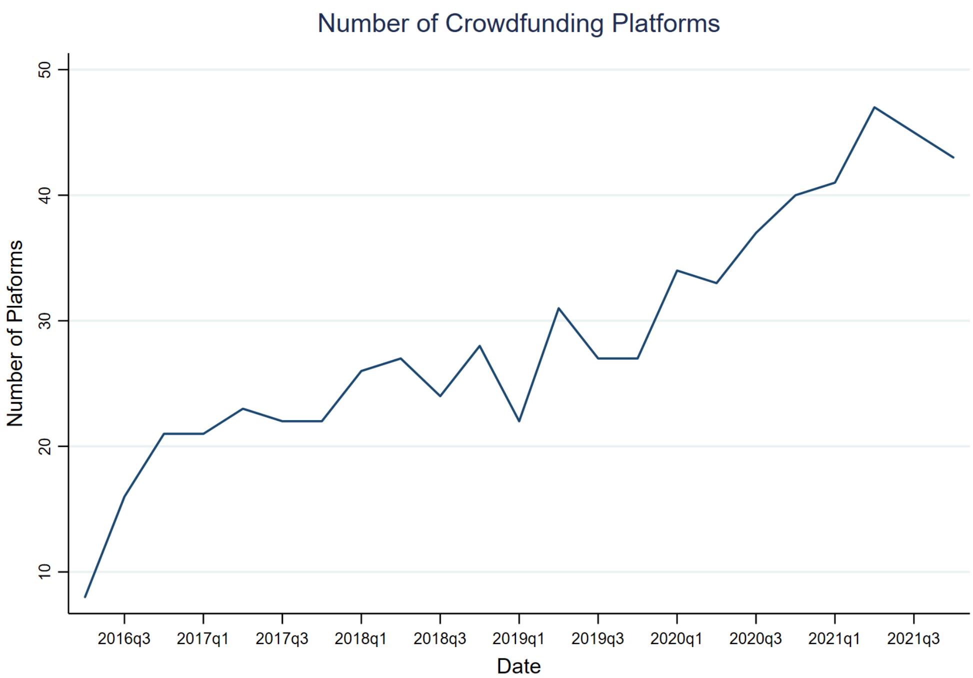

Figure 3: **Number of crowdfunding platforms**. This figure shows the evolution of the total number crowdfunding from 2016 through 2021.

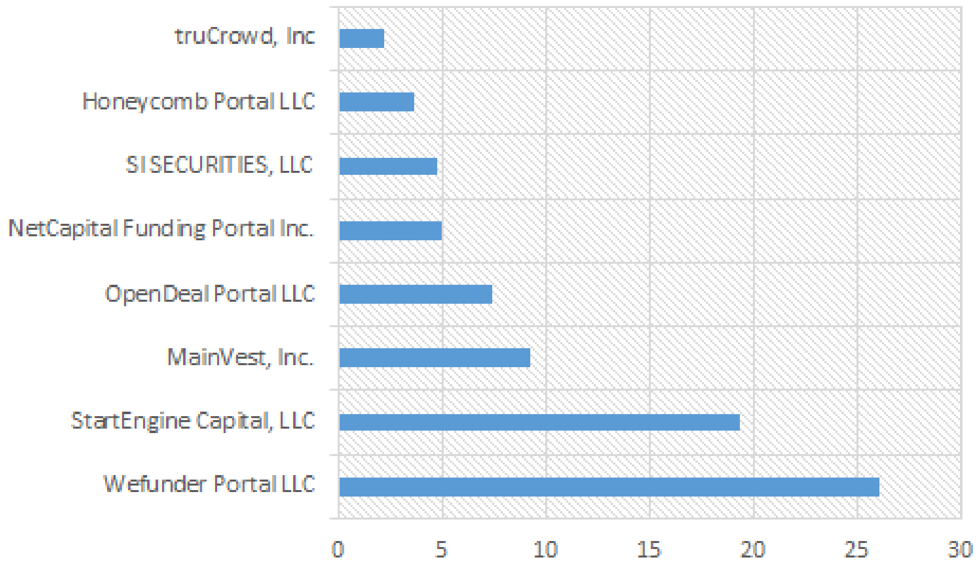

Figure 4: **Most popular crowdfunding platforms**. This figure shows the percentage of the offerings managed by the most eight most popular crowdfunding portals. Data come from EDGAR.

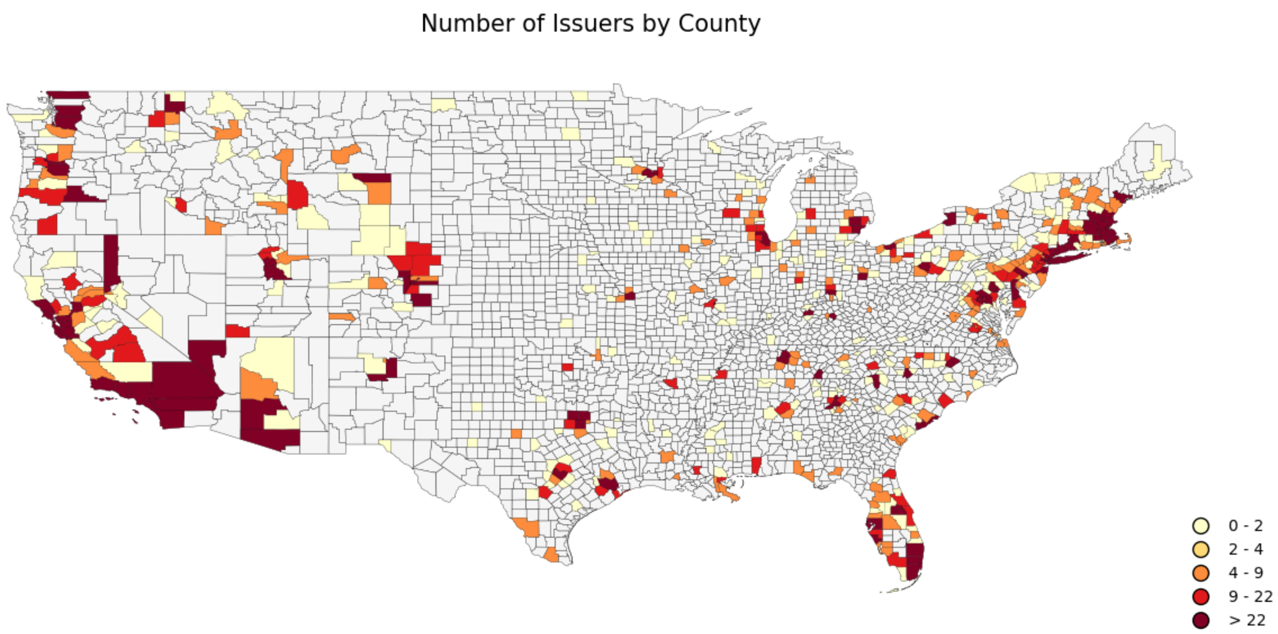

Figure 5: **Crowdfunding geography**. This figure shows the country-level graph of the numbers of crowdfunding offerings across US Counties. Colors correspond to bins of the number of offerings. Data come from EDGAR.

Table 1: Summary Statistics

The table presents descriptive statistics for financial variables (Panel A), crowdfunding variables (Panel B), and macro variables (Panel C). The sample covers 2,052 US crowdfunded firms from June 2016 through December 2021. We require non-zero total assets and winsorize data at (2,98) level. All variables are defined in the Appendix (Table A1). Total Assets are in millions of dollars. Columns 4, 5 and 6, 7 show the subsamples of debt-based crowdfunding (CF) and equity-based CF, respectively. The p-value in column 9 is the significance of a t-test for the difference in mean between debt and equity crowdfunding. Data sources: EDGAR, FactSet, Board of Governors of the Federal Reserve System, IPUMS National Historical Geographic Information System.

	Full Sample (N = 2052)			Debt-based CF (N = 355)		Equity-based CF (N = 1657)		Difference	p-value of
	N	Mean	Median	Mean	Median	Mean	Median	in mean	difference
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Firm Characteristics									
Total Assets	2052	708,164	103,416	1,070,204	65,549	632,428	113,741	437,776	0.236
Profitability	2052	2.36	0.35	3.75	1.33	2.07	0.25	1.68	0.000
Size	2052	11.28	11.55	11.02	11.09	11.33	11.64	-0.31	0.018
Cash holdings	2052	0.46	0.36	0.43	0.28	0.47	0.38	-0.03	0.135
Book Leverage	2052	5.07	0.92	3.35	0.85	5.43	0.94	-2.08	0.017
LT Leverage	2052	2.51	0.09	1.59	0.02	2.71	0.10	-1.11	0.019
ST Leverage	2052	1.51	0.18	1.21	0.15	1.57	0.18	-0.36	0.144
Log (Sales)	2052	11.61	11.87	11.56	11.81	11.63	11.88	-0.07	0.680
Age	2052	2.67	1.00	3.23	2.00	2.55	1.00	-0.68	0.004
Financial Growth Cycle	2052	1.86	2.00	2.07	2.00	1.81	2.00	-0.26	0.000
Log (Employees)	2052	8.98	4.00	6.96	4.00	9.43	4.00	-2.48	0.316
Panel B: Crowdfunding									
Amount Offered	2052	63631	25000	62734	25000	63834	25000	-1100	0.862
Price Security	2052	146	1	487	1	92	1	395	0.012
Type of Security	2052	0.17	0.00						
Success	2052	0.37	0.00	0.37	0.00	0.37	0.00	0.00	0.950
Previous Institutional Funding	2052	0.25	0.00	0.14	0.00	0.27	0.00	-0.12	0.000
Panel C: Macro variables									
Bank Density (150 miles)	2001	7.500	7.550	7.606	7.819	7.478	7.539	0.13	0.003
Top Bank	2001	0.25	0.00	0.33	0.00	0.23	0.00	0.09	0.001
Total population	2001	10.15	10.28	10.09	10.27	10.17	10.28	-0.08	0.130
Median Income	2001	82.74	78.07	78.80	72.16	83.58	79.27	-4.78	0.030
Frac. White	2001	21.82	20.02	21.35	18.79	21.91	20.04	-0.56	0.507
Num. of Establishment	2001	47.80	33.00	40.53	31.00	49.35	34.00	-8.82	0.007

Table 2: Industry Distribution of Sample Crowdfunded Firms and Financing Choice

The table presents the distribution of sample firms based on their Standard Industrial Classification (SIC) 2-digit industry code, sorted by frequency. It also shows the number and percentage of firms that opt for debt-based crowdfunding (CF) and equity-based crowdfunding within each industry category. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. All variables are defined in the Appendix (Table A1).

		All firms		Debt-based CF		Equity-based CF	
SIC2	Industry	Num.	Percent	Num.	Percent	Num.	Percent
73	Business Services	384	18.71	54	15.21	330	19.45
20	Food and Kindred Products	199	9.7	62	17.46	137	8.07
58	Eating and Drinking Places	111	5.41	42	11.83	69	4.07
87	Engineering, Accounting, Research, and Management Services	93	4.53	18	5.07	75	4.42
59	Miscellaneous Retail	87	4.24	14	3.94	73	4.3
51	Wholesale Trade - Nondurable Goods	80	3.9	15	4.23	65	3.83
79	Amusement and Recreation Services	76	3.7	11	3.1	65	3.83
28	Chemicals and Allied Products	56	2.73	7	1.97	49	2.89
54	Food Stores	50	2.44	15	4.23	35	2.06
80	Health Services	50	2.44	5	1.41	45	2.65
50	Wholesale Trade - Durable Goods	49	2.39	4	1.13	45	2.65
38	Measuring, Photographic, Medical, & Optical Goods, & Clocks	48	2.34	4	1.13	44	2.59
36	Electronic & Other Electrical Equipment & Components	47	2.29	2	0.56	45	2.65
72	Personal Services	46	2.24	8	2.25	38	2.24
48	Communications	42	2.05	5	1.41	37	2.18
35	Industrial and Commercial Machinery and Computer Equipment	38	1.85	3	0.85	35	2.06
82	Educational Services	38	1.85	7	1.97	31	1.83
27	Printing, Publishing and Allied Industries	30	1.46	2	0.56	28	1.65
78	Motion Pictures	30	1.46	2	0.56	28	1.65
37	Transportation Equipment	29	1.41	1	0.28	28	1.65
56	Apparel and Accessory Stores	28	1.36	6	1.69	22	1.3
39	Miscellaneous Manufacturing Industries	27	1.32	3	0.85	24	1.41
65	Real Estate	27	1.32	5	1.41	22	1.3
61	Nondepository Credit Institutions	26	1.27	3	0.85	23	1.36
62	Security & Commodity Brokers, Dealers, Exchanges & Services	24	1.17	5	1.41	19	1.12
67	Holding and Other Investment Offices	23	1.12	2	0.56	21	1.24
83	Social Services	23	1.12	5	1.41	18	1.06
47	Transportation Services	22	1.07	4	1.13	18	1.06
49	Electric, Gas and Sanitary Services	18	0.88	2	0.56	16	0.94
75	Automotive Repair, Services and Parking	17	0.83	4	1.13	13	0.77
89	Services, Not Elsewhere Classified	15	0.73	2	0.56	13	0.77
86	Membership Organizations	13	0.63	3	0.85	10	0.59
1	Agricultural Production - Crops	12	0.58	5	1.41	7	0.41
23	Apparel, Finished Products from Fabrics & Similar Materials	12	0.58	3	0.85	9	0.53
31	Leather and Leather Products	12	0.58	1	0.28	11	0.65
55	Automotive Dealers and Gasoline Service Stations	12	0.58	0	0	12	0.71
34	Fabricated Metal Products	11	0.54	0	0	11	0.65
15	Construction - General Contractors & Operative Builders	10	0.49	5	1.41	5	0.29
42	Motor Freight Transportation	10	0.49	1	0.28	9	0.53

Table 3: Financing Choice and Firm Characteristics

The table presents the relationship between firm characteristics and the choice of security type in crowdfunding campaigns. The dependent variable is a dummy that takes the value of 1 when the issued security is in the form of debt, and 0 otherwise. Columns (1), (2), and (3) display the estimated coefficients for the full sample. Columns (4) and (5) present results for successful campaigns, while columns (6) and (7) report coefficients for failed campaigns. All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	Full Sample			Successful CF		Failed CF	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Profitability	0.009***	0.009***	0.008***	0.008**	0.007**	0.012***	0.010***
	(3.60)	(3.71)	(3.38)	(2.48)	(2.25)	(4.46)	(3.69)
Size	-0.013***	-0.012***	-0.011**	-0.006	-0.004	-0.022**	-0.022**
	(3.03)	(2.96)	(2.32)	(1.24)	(0.71)	(2.37)	(2.47)
Cash holdings	-0.056	-0.057	-0.027	-0.016	0.010	-0.113*	-0.082*
	(1.47)	(1.53)	(0.92)	(0.50)	(0.32)	(1.94)	(1.90)
LT Leverage	-0.003***	-0.003***	-0.003**	-0.003**	-0.002*	-0.003***	-0.003*
	(2.85)	(2.85)	(2.37)	(2.28)	(1.80)	(2.75)	(1.73)
ST Leverage	-0.005**	-0.004**	-0.003*	-0.002	-0.000	-0.010***	-0.008**
	(2.41)	(2.24)	(1.83)	(0.72)	(0.20)	(2.79)	(2.38)
Year FE		Y	Y	Y	Y	Y	Y
Industry FE			Y		Y		Y
Observations	2,052	2,052	2,045	1,292	1,286	760	741
Adjusted R-squared	0.024	0.025	0.057	0.025	0.049	0.041	0.084

Table 4: Financing Choice and Growth Stage

The table presents the relationship between the stage of a firm's financial growth and the choice of security type in crowdfunding campaigns. The dependent variable is a dummy that takes the value of 1 when the issued security is in the form of debt, and 0 otherwise. Columns (1), (2), and (3) display the estimated coefficients for the full sample. Columns (4) and (5) present results for successful campaigns, while columns (6) and (7) report coefficients for failed campaigns. We categorize firms into three stages of the financial growth cycle: pre-revenue (Growth Stage 1), positive revenue but not yet profitable (Growth Stage 2), and profitable with positive revenue and net income (Growth Stage 3). All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	Full Sample			Successful CF		Failed CF	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Growth Stage 2	0.063***	0.065***	0.046*	0.092***	0.076**	0.022	0.013
	(3.69)	(3.15)	(1.97)	(3.39)	(2.44)	(0.82)	(0.38)
Growth Stage 3	0.179***	0.153***	0.133***	0.146***	0.130***	0.156***	0.122**
	(4.88)	(4.38)	(3.63)	(3.26)	(2.80)	(4.24)	(2.64)
Profitability		0.005*	0.004	0.003	0.003	0.007***	0.007**
		(1.80)	(1.62)	(0.95)	(0.87)	(2.67)	(2.46)
Size		-0.018***	-0.015***	-0.013**	-0.010	-0.024**	-0.024***
		(3.92)	(3.28)	(2.42)	(1.64)	(2.46)	(2.79)
Cash holdings		-0.041	-0.015	0.003	0.025	-0.097	-0.071
		(1.17)	(0.50)	(0.09)	(0.84)	(1.65)	(1.59)
LT Leverage		-0.003**	-0.002**	-0.003**	-0.002*	-0.003**	-0.002
		(2.61)	(2.14)	(2.14)	(1.69)	(2.55)	(1.65)
ST Leverage		-0.004*	-0.003	-0.001	-0.000	-0.009**	-0.007**
		(1.92)	(1.55)	(0.54)	(0.05)	(2.39)	(2.11)
Year FE		Y	Y	Y	Y	Y	Y
Industry FE			Y		Y		Y
Observations	2,032	2,032	2,025	1,280	1,274	752	733
Adjusted R-squared	0.023	0.037	0.064	0.037	0.056	0.054	0.092

Table 5: Housing Price Changes and Financing Choice of Crowdfunding

The table presents the relationship between house prices and the choice of security type in crowdfunding campaigns. The dependent variable is a dummy that takes the value of 1 when the issued security is in the form of debt, and 0 otherwise. All variables are defined in the Appendix (Table A1). Firm-level variables and HPI are lagged by one year. HPI and the macro controls are at the ZIP code level. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Variables	(1)	(2)	(3)	(4)
log HPI	-0.049***	-0.052***	-0.040**	-0.041*
	(2.65)	(2.68)	(2.13)	(1.92)
log Med. Inc		0.004	0.001	0.015
		(0.13)	(0.02)	(0.44)
log Population		0.009	0.005	0.002
		(0.40)	(0.20)	(0.08)
Establishments Per Cap.		-0.002	-0.002	-0.002
		(0.30)	(0.36)	(0.28)
Firm Controls	Y	Y	Y	Y
County FE	Y	Y	Y	Y
Year FE			Y	Y
Industry FE				Y
Observations	1320	1180	1180	1166

Table 6: Bank-lending Availability and Crowdfunding Choice

The table reports results from the bank-lending availability and the choice of security type in crowdfunding campaigns regression estimations. The dependent variable is a dummy that takes the value of 1 when the issued security is in the form of debt, and 0 otherwise. All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

Variables	(1)	(2)	(3)	(5)	(6)	(8)
Log Bank Density (150 miles)	-0.622**	-0.588**	-0.551*			
	(2.41)	(2.27)	(1.88)			
Top Bank Density (150 miles)				-0.114***	-0.134***	-0.098*
				(2.96)	(3.47)	(1.85)
Log Med. Inc	0.000	0.000	0.000	0.000	0.000	0.000
	(0.32)	(0.41)	(0.39)	(0.31)	(0.40)	(0.39)
Frac. White	-0.036	-0.039	-0.023	-0.031	-0.034	-0.019
	(0.63)	(0.70)	(0.39)	(0.55)	(0.62)	(0.33)
Log Population	0.001	0.001	0.001	0.001	0.001	0.001
	(1.38)	(1.41)	(1.12)	(1.32)	(1.35)	(1.07)
Establishments Per Cap.	-0.000	-0.000*	-0.000*	-0.000*	-0.000*	-0.000*
	(1.67)	(1.71)	(1.70)	(1.78)	(1.82)	(1.78)
Firm Controls	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Year FE		Y	Y		Y	Y
Industry FE			Y			Y
Observations	1,826	1,826	1,826	1,826	1,826	1,826
Adjusted R-squared	0.141	0.142	0.152	0.139	0.141	0.150

Table 7: Institutional Investors and Financing Choice

The table provides regression results for the relationship between institutional funding and the choice of security type in crowdfunding campaigns. Columns (1), (2), and (3) display the estimated coefficients for the full sample. Columns (4) and (5) present results for successful campaigns, while columns (6) and (7) report coefficients for failed campaigns. The dependent variable is a dummy that takes the value of 1 when the issued security is in the form of debt, and 0 otherwise. All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 2,052 US crowdfunding campaigns, 2016-2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	Full Sample			Successful CF		Failed CF	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Previous Institutional Funding	-0.094***	-0.075***	-0.065***	-0.059**	-0.040	-0.102***	-0.088**
	(5.03)	(4.07)	(3.95)	(2.14)	(1.51)	(3.01)	(2.48)
Profitability		0.009***	0.008***	0.008**	0.007**	0.011***	0.010***
		(3.66)	(3.35)	(2.41)	(2.23)	(4.23)	(3.47)
Size		-0.008*	-0.007	-0.003	-0.002	-0.015*	-0.016*
		(1.76)	(1.44)	(0.62)	(0.37)	(1.79)	(1.94)
Cash holdings		-0.046	-0.018	-0.009	0.014	-0.095*	-0.066
		(1.26)	(0.64)	(0.29)	(0.46)	(1.71)	(1.56)
LT Leverage		-0.003***	-0.003**	-0.003**	-0.002*	-0.003***	-0.003*
		(2.84)	(2.41)	(2.24)	(1.80)	(2.96)	(1.92)
ST Leverage		-0.003*	-0.003	-0.001	0.000	-0.009***	-0.008**
		(1.76)	(1.45)	(0.31)	(0.08)	(2.89)	(2.50)
Year FE		Y	Y	Y	Y	Y	Y
Industry FE			Y		Y		Y
Observations	2,052	2,052	2,045	1,292	1,286	760	741
Adjusted R-squared	0.011	0.031	0.061	0.029	0.050	0.053	0.092

Table 8: Crowdfunding and Growth

The table presents results from the regression estimation of crowdfunding and growth. Post is a dummy variable that takes a value of 1 after the crowdfunding campaign, and 0 otherwise. Treated is a dummy variable that takes a value of 1 if the firm successfully concluded a crowdfunding campaign, and 0 otherwise. All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 349 US crowdfunding firms and their matched controls, 2016-2021. Data frequency is yearly. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Size	Log Revenue	Profitability	Cash holdings	Book Leverage	ST Leverage	LT Leverage
Post	0.064	-0.065	-0.476	0.002	-0.861**	0.008	0.007
	(0.38)	(0.51)	(0.74)	(0.06)	(2.50)	(0.42)	(0.17)
Post x Treated	0.424***	0.464***	0.964*	0.044	0.274	-0.168***	-0.141
	(2.67)	(4.33)	(1.73)	(1.56)	(0.71)	(2.99)	(1.21)
Year FE	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y
Observations	1,359	1,113	1,352	1,349	1,241	1,363	1,241
Adjusted R-squared	0.700	0.871	0.673	0.693	0.494	0.511	0.619

Table 9: Crowdfunding, Growth, and Financial Growth Cycle

The table presents the relationship between crowdfunding, growth, and the financial growth cycle. Post is a dummy variable that takes a value of 1 after the crowdfunding campaign, and 0 otherwise. Treated is a dummy variable that takes a value of 1 if the firm successfully concluded a crowdfunding campaign, and 0 otherwise. We categorize firms into three stages of the financial growth cycle: pre-revenue (Growth Stage 1), positive revenue but not yet profitable (Growth Stage 2), and profitable with positive revenue and net income (Growth Stage 3). All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 349 US crowdfunding firms and their matched controls, 2016-2021. Data frequency is yearly. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	(1) Size	(2) Revenue	(3) Profitability	(4) Cash holdings	(5) Book Leverage	(6) ST Leverage	(7) LT Leverage
Post	-0.027	-0.079	-0.526	0.019	-0.361	0.005	0.009
	(0.17)	(0.66)	(0.82)	(0.57)	(1.21)	(0.24)	(0.23)
Post x Treated	0.829***	0.899***	1.248**	0.028	-0.468	-0.162*	-0.338*
	(4.54)	(5.43)	(2.23)	(0.81)	(1.18)	(1.93)	(1.79)
Post x Treated x Growth Stage 2	-0.705***	-0.588***	-0.212	0.023	0.214	-0.092	0.369
	(4.80)	(3.02)	(1.07)	(0.63)	(0.51)	(0.73)	(1.42)
Post x Treated x Growth Stage 3	-0.498***	-0.738***	-1.190***	-0.075	0.697*	0.221*	0.450*
	(2.70)	(4.05)	(3.02)	(1.40)	(1.82)	(1.83)	(1.87)
Growth Stage 2	0.748	-0.351**	3.525**	-0.265	0.112	0.332	-0.054
	(1.59)	(2.31)	(2.28)	(1.10)	(0.21)	(1.14)	(0.37)
Growth Stage 3	1.184**		1.662	-0.252	0.130	0.306	-0.070
	(2.53)		(0.99)	(1.04)	(0.21)	(1.04)	(0.45)
Year FE	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y
Observations	1,339	1,109	1,336	1,329	1,221	1,343	1,221
Adjusted R-squared	0.719	0.876	0.683	0.709	0.558	0.502	0.620

Table A1: Variable Definition

Variables	Source	Description
A. Firm Characteristics (measured at the most recent fiscal year (t-1))		
Profitability	Edgar Form C	Revenue/Total Assets
Total Assets	Edgar Form C	Total Assets (in $ million)
Size	Edgar Form C	Natural log of total assets
Cash holdings	Edgar Form C	Cash and cash equivalents/Total Assets
Book Leverage	Edgar Form C	Total Debt/Total Assets
LT Leverage	Edgar Form C	Long-term Debt/Total Assets
ST Leverage	Edgar Form C	Short-term Debt/Total Assets
Log (Sales)	Edgar Form C	Natural log of total revenues
Age	Edgar Form C	First form C filing date - Date in corporation
Financial Growth Cycle	Edgar Form C	Growth stage = 1 if Revenue = 0 & Net Income <= 0 Growth stage = 2 if Revenue > 0 & Net Income <= 0 Growth stage = 3 if Revenue > 0 & Net Income > 0
Log (Employees)	Edgar Form C	Natural log of current employees
B. Crowdfunding		
Amount Offered	Edgar Form C	Amount offered
Price Security	Edgar Form C	Price security
Num. of Securities	Edgar Form C	Number of securities issued
Time to raise funds	Edgar Form C	First form C Filing date - Filing date form C/U (signaling the success of the crowdfunding campaign)
Interest Rate	Edgar Form C	Interest rate that the issuer pays to the intermediary
Type of Security	Edgar Form C	Dummy that takes the value of 1 when the issued security is in the form of debt, and 0 if equity. Equity definition includes common stock, preferred stock, and other securities
Success	Edgar Form C	Dummy that takes the value of 1 when firms raise the crowdfunding campaign target amount and 0 otherwise
Previous Institutional Funding	Edgar Form D	The variable takes a binary value of 1 if a firm filed Form D prior to the crowdfunding campaign, indicating that the firm received financing from Private Equity, Venture Capital, or Hedge Funds. Otherwise, it takes a value of 0 if the firm did not file Form D, indicating no such financing.
C. Macro variables		
Num. Bank Branches	FDIC Summary of Deposits Database	Number of bank branches per ZIP code
Bank Density (100 miles)	FDIC Summary of Deposits Database	Log of the total number of bank branches within 100 miles from the issuer's location
Bank Density (150 miles)	FDIC Summary of Deposits Database	Log of the total number of bank branches within 150 miles from the issuer's location
Total population	American Community Survey, 2016–2020	Total population per ZIP code
Median Income	American Community Survey, 2016–2020	Median income per ZIP code
Num. of Establishment	American Community Survey, 2016–2020	Number of establishment per ZIP code

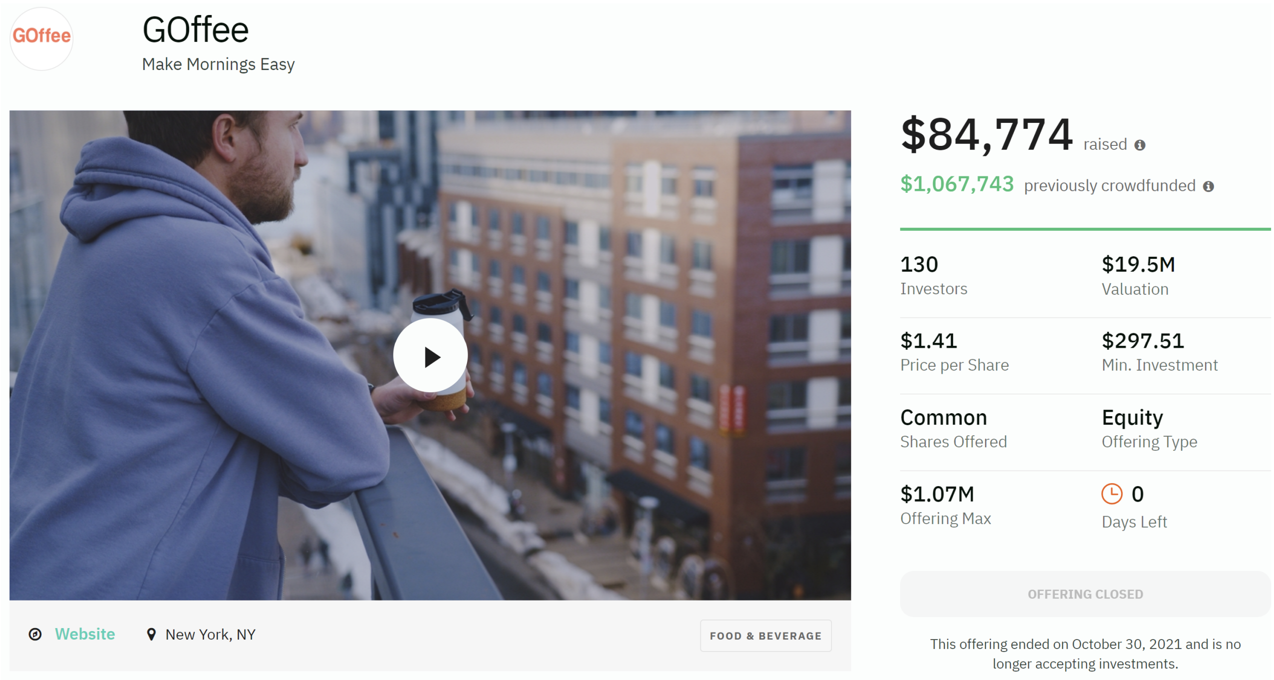

Figure A1: Example of a crowdfunding offering. This figure shows the example of a crowdfunding offering from StartEngine.

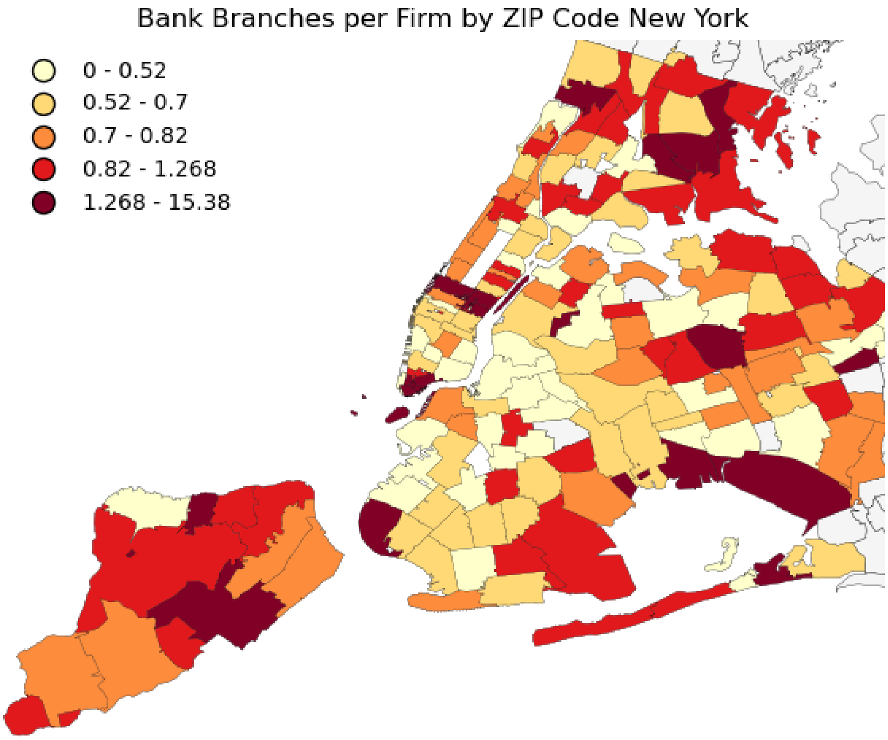

Figure A2: **Number of bank branches New York Metropolitan Area**. This figure shows data from New York County, Bronx County, Queens County, Kings County, and Richmond County (New York Metropolitan Area) ZIP Codes. Colors correspond to bins of the number bank branches per establishment per ZIP code.

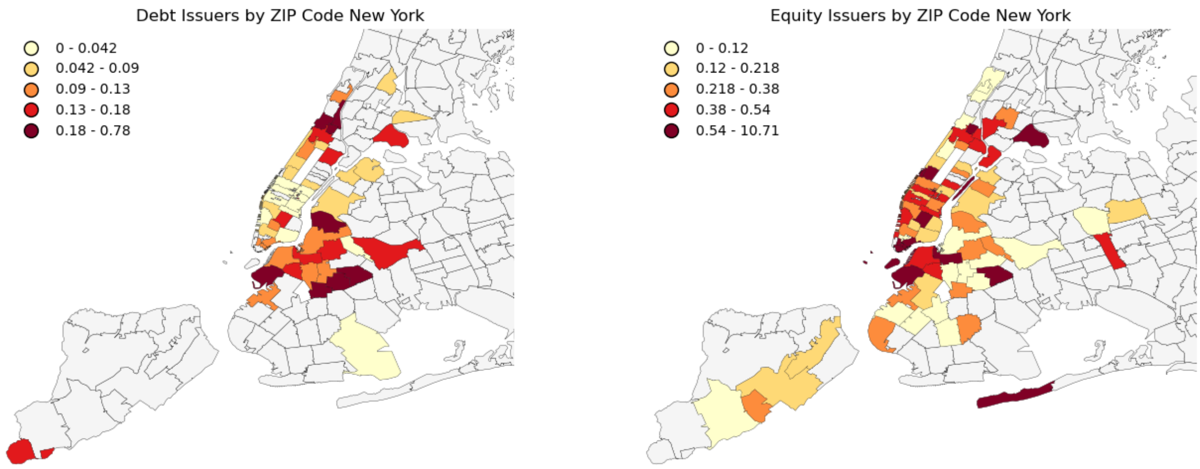

Figure A3: **Number of debt and issuers New York Metropolitan Area:** This figure shows data from New York County, Bronx County, Queens County, Kings County, and Richmond County (New York Metropolitan Area) ZIP Codes. The left panel colors correspond to bins of the percentage of establishments that issued debt securities through crowdfunding. The right panel colors correspond to bins of the percentage of establishments that issued equity securities through crowdfunding.

Table A2: Financing Choice and Growth Stage (proxied by Age)

The table presents the relationship between the choice of security type in crowdfunding campaigns and age. All variables are defined in the Appendix (Table A1). Firm-level variables are lagged by one year. The sample contains 2,052 US crowdfunding campaigns, 2016–2021. T-statistics are in parentheses and standard errors are clustered at the industry level. *, **, and *** indicate statistical significance at 10%, 5%, and 1%, respectively.

	Full Sample			Successful CF		Failed CF	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	0.006*	0.006**	0.007**	0.005	0.005	0.009**	0.011***
	(1.69)	(2.07)	(2.22)	(1.46)	(1.38)	(2.33)	(2.81)
Profitability		0.009***	0.008***	0.008**	0.007**	0.011***	0.010***
		(3.67)	(3.30)	(2.40)	(2.19)	(4.56)	(3.60)
Size		-0.015***	-0.014***	-0.008**	-0.007	-0.026**	-0.027**
		(3.35)	(2.93)	(2.00)	(1.39)	(2.56)	(2.68)
Cash holdings		-0.048	-0.020	-0.006	0.019	-0.105*	-0.075*
		(1.31)	(0.68)	(0.18)	(0.59)	(1.87)	(1.81)
LT Leverage		-0.003***	-0.003**	-0.003**	-0.003**	-0.003***	-0.003*
		(3.12)	(2.51)	(2.49)	(2.03)	(2.73)	(1.74)
ST Leverage		-0.005**	-0.004*	-0.002	-0.001	-0.011***	-0.010**
		(2.29)	(1.81)	(0.80)	(0.31)	(2.88)	(2.53)
Year FE		Y	Y	Y	Y	Y	Y
Industry FE			Y		Y		Y
Observations	2,011	2,011	2,002	1,258	1,253	753	735
Adjusted R-squared	0.004	0.029	0.062	0.028	0.055	0.050	0.094