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Firm Value and Financial Constraints: Evidence from private firm sellouts and reverse mergers

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FIRM VALUE AND FINANCIAL CONSTRAINTS:
EVIDENCE FROM PRIVATE FIRM SELLOUTS AND REVERSE MERGERS

BY

DANIEL THOMAS GREENE

A Dissertation Submitted in Partial Fulfillment of the Requirements for the Degree

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ACCEPTANCE

This dissertation was prepared under the direction of the Daniel Thomas Greene's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctoral of Philosophy in Business Administration in the J. Mack Robinson College of Business of Georgia State University.

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I thank the Lord Jesus Christ for the strength to persevere and finish this work. “For from Him and through Him and to Him are all things. To Him be glory forever.” Romans 11:36

ABSTRACT

FIRM VALUE AND FINANCIAL CONSTRAINTS:
EVIDENCE FROM PRIVATE FIRM SELLOUTS AND REVERSE MERGERS

BY

DANIEL THOMAS GREENE

APRIL 24, 2014

Committee Chair: *Omesh Kini*

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Essay 1: Financial Constraints and Firm Value: Evidence From Sales of Private Firms

Abstract: I examine sales of private firms to better understand the effect of relaxing financial constraints on firm value. My empirical tests exploit an exogenous shock to financial constraints caused by interstate bank branching deregulation. On a sample of 557 sales of private firms to public acquirers, I find that relaxed financial constraints lead to a statistically significant increase of 7.3% in valuation multiples of private targets. I also find a significant increase in private target valuation multiples benchmarked to public target valuation multiples. These effects are more pronounced for firms in the sample with below median annual sales. Acquirer returns are negatively impacted when financial constraints on private targets are relaxed. The evidence supports the prediction that relaxing financial constraints allows private targets to substitute bank credit for some of the financing benefits provided by acquirers and bargain for a higher valuation.

Essay 2: Reverse Mergers as an Exit Mechanism for Private Firm Owners

Abstract: I examine reverse mergers (RMs) as an exit mechanism for private firm owners and compare RMs to both IPOs and sellouts to a public acquirer. I find evidence that information asymmetry and product market competition influence the choice among the three exit mechanisms. RM firms are significantly different from IPO firms along observable characteristics. Also, I find that RM firm owners have less wealth following the exit than firm owners of matched IPO firms. Together, this evidence suggests that an IPO is not a realistic option for the vast majority of RM firms. In contrast, RM firms are similar to sellout firms along observable characteristics and owners of RM firms receive the same, or greater, wealth as owners of comparable sellout firms. Thus, a sellout appears to be a viable alternative to a RM for many firms. I examine whether or not RMs generate positive synergy. I find that synergy is positive, on average, when synergy is calculated using valuations of private firms that are inferred from industry multiples of private-private takeovers. In contrast, I find that synergy is negative, on average, when synergy is calculated using private firm valuations produced by financial advisors to the public firm board of directors. The evidence leads me to conclude that financial advisors produce inflated valuations of private firms that mechanically drives down the estimate of synergy.

Financial Constraints and Firm Value:
Evidence From Sales of Private Firms

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Georgia State University

April 24, 2014

Abstract

I examine sales of private firms to better understand the effect of relaxing financial constraints on firm value. My empirical tests exploit an exogenous shock to financial constraints caused by interstate bank branching deregulation. On a sample of 557 sales of private firms to public acquirers, I find that relaxed financial constraints lead to a statistically significant increase of 7.3% in valuation multiples of private targets. I also find a significant increase in private target valuation multiples benchmarked to public target valuation multiples. These effects are more pronounced for firms in the sample with below median annual sales. Acquirer returns are negatively impacted when financial constraints on private targets are relaxed. The evidence supports the prediction that relaxing financial constraints allows private targets to substitute bank credit for some of the financing benefits provided by acquirers and bargain for a higher valuation.

JEL Classification: G32, G34, G28
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1. Introduction

Theory demonstrates that firms can face financial constraints which force them to pass up valuable projects (e.g., Stiglitz and Weiss, 1981; Tirole, 2006).[1] Relaxing financial constraints on firms is predicted to increase investment and therefore increase firm value. For example, Tirole (2006) shows that the market power of lenders constrains investment and that investment is greater when firms instead borrow from lenders that compete aggressively. Competition among lenders can improve access to finance, which relaxes financial constraints and allows firms to fund value increasing investments. In this paper, I empirically examine how firm valuations are affected by financial constraints in the context of corporate takeovers. Specifically, I examine how the severity of financial constraints on private targets affects firm valuation in sellouts to public acquirers.

A natural link exists between financial constraints and firm valuation in a sellout because constraints on targets influence the degree to which targets depend on acquirers for financing. Empirical evidence shows that public acquirers provide liquidity to private targets (by financing investment and allowing owners to sell their shares) and that the provision of liquidity affects target valuations (Officer, 2007; Erel, Jang, and Weisbach, 2013). Thus, private targets that are less financially constrained before the sellout should rely less on acquirers to provide liquidity, allowing them to negotiate for a higher valuation.

The hypothesis that financial constraints affect firm valuation is intuitive yet difficult to test empirically. One challenge is that financial constraints are unobservable, which makes it difficult to construct accurate proxies for constraints (Kaplan and Zingales, 2000; Hadlock and

[1] Financial constraints can be caused by a variety of frictions such as moral hazard (Jaffee and Russell, 1976), information asymmetry (Stiglitz and Weiss, 1981), agency costs (Jensen and Meckling, 1976), or market power of lenders (Tirole, 2006). Fazzari, Hubbard, and Petersen (1988), Whited (1992, 2006), and Campello, Graham, and Harvey (2010) empirically examine financial constraints on firms.

Pierce, 2010; Farre-Mensa and Ljungqvist, 2014). Perhaps a greater challenge is that measures of financial constraints are endogenously determined with firm value. For example, a proxy for financial constraints that relies on financial statement data is likely to be correlated with unobserved factors that also affect the firm's valuation. Similarly, a proxy that aims to capture competition among lenders, such as the number of lenders located near the firm, can be positively associated with firm value as a result of reverse causality. Specifically, lenders may locate in areas where firms have good future prospects and, therefore, higher values. In this paper, I implement an empirical design which overcomes both of these challenges and allows for a test of the hypothesis that relaxed financial constraints leads to an increase in firm valuations in sellouts.

I exploit an exogenous shock to financial constraints on firms caused by the staggered deregulation of the U.S. banking industry at the state level. The Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 (IBBEA) removed restrictions on interstate branching.[2] IBBEA provided banks new ways to expand across state lines and allowed existing multi-state banks to build a more efficient branch network. However, IBBEA also allowed states to impose restrictions on out-of-state banks, a feature of the law that results in deregulation that varies across states and over time. I implement a difference-in-differences test which takes advantage of staggered deregulation of interstate bank branching laws.

My empirical design focuses on private firms which are sold to public acquirers. IBBEA will affect private firms to a greater extent than public firms because private firms tend to rely on

[2] See Johnson and Rice (2008) and Rice and Strahan (2010) for a detailed description of IBBEA and subsequent state level legislative responses.

local banks for capital (Berger and Udell, 1998; Petersen and Rajan, 2002).[3] Rice and Strahan (2010) show that IBBEA significantly lowered the cost of credit for small businesses located in states which chose to deregulate interstate bank branching laws. Therefore, private firms located in states that deregulate interstate bank branching laws will experience an easing of financial constraints due to improved access to bank credit.

My first hypothesis predicts that relaxed financial constraints lead to higher valuations for private targets. One of the benefits of a sellout is that the acquirer can finance the private target's projects (Erel, Jang, and Weisbach, 2013). However, private firms that have better access to bank credit following interstate bank branching deregulation can, at least partially, substitute bank credit in place of financing that the acquirer can provide. Better access to finance is likely to result in investment in positive net present value (NPV) projects for private firms because managers typically have concentrated equity positions that mitigate agency problems. The ability to substitute bank credit in place of financing offered by the acquirer strengthens the bargaining position of the private target as it increases the value of the outside option.[4] Thus, deregulation of interstate bank branching is predicted to have a positive effect on valuations of private targets in sellouts.

My second hypothesis predicts that relaxed financial constraints lead to higher valuations of private targets relative to valuations of matched public targets. I use private target valuation multiples benchmarked to public target valuation multiples to capture the extent to which private targets substitute bank credit in place of financing provided by the acquirer. The intuition behind

[3] It is not the distinction between private and public firms per se which determines the impact of IBBEA. Rather, it is the fact that private firms tend to have greater information asymmetries, are more geographically concentrated, and tend to borrow from local banks. IBBEA can affect a public firm that depends on local banks.

[4] Due to limited data on private targets, I cannot determine whether or not private targets actually increase investment following deregulation of interstate bank branching. However, the hypothesis goes through as long as private targets can credibly threaten to walk away from the deal and use bank credit to finance investment.

this measure is that matched public targets provide a baseline valuation of an unconstrained firm. The valuation received by private targets, relative to public targets, depends on the degree to which private targets rely on the acquirer for financing (Officer, 2007). Private targets with more severe financial constraints depend more on acquirers for financing and, therefore, sell for a relatively low price. Private targets with less severe financial constraints depend less on acquirers for financing and, therefore, sell for a relatively high price.

My third hypothesis predicts that relaxing constraints on private targets negatively impacts the wealth gains of the public acquirer's shareholders.[5] Empirical evidence suggests that acquirer wealth gains in sellouts can be partially attributed to providing liquidity to private targets (Fuller, Netter, and Stegemoller, 2002). Improved access to finance for private targets results in less dependence on the acquirer for liquidity and an increase in the target's relative bargaining power. All else equal, the acquirer's portion of the economic gain generated in the sellout decreases as the target's ability to bargain for a higher price increases.

I conduct empirical tests on a sample of 557 sellouts of private firms to public firms announced between 1992 and 2000 and find evidence that supports my hypotheses. The sample includes deals announced from three years before to three years after the private target's home state enacted legislation in response to IBBEA. All states responded to IBBEA by June 1997. As described in more detail below, states are counted as non-deregulating if the state maintains all restrictions on interstate bank branching. States are counted as deregulating if the state removed at least one barrier to interstate bank branching.

The empirical methodology follows a difference-in-differences approach (Roberts and Whited, 2012) based on whether the sellout occurred before or after the state's legislative

[5] Unfortunately, I cannot analyze the sharing of gains (Kale, Kini, Ryan, 2003; Ahern, 2012) or premiums paid to targets (Comment and Schwert, 1995) because the target's pre-sellout value is unknown.

response to IBBEA and whether or not the state's legislative response resulted in deregulation of interstate bank branching. I use the total consideration paid to the private target (deal value) as the value of the firm and scale this number by the firm's annual sales. I use the natural logarithm transformation of deal value-to-sales in empirical tests due to skewness in the distribution.

Univariate tests show that, in states that deregulated interstate bank branching, deal value-to-sales multiples increase significantly following the legislative response to IBBEA. In states that did not deregulate, the change in deal value-to-sales multiples around the state's legislative response to IBBEA is statistically insignificant. A difference-in-differences test compares changes in deal value-to-sales multiples from pre- to post-IBBEA periods between sellouts of firms in deregulating and non-deregulating states. As predicted by my first hypothesis, this difference is positive and statistically significant, indicating that deregulation of interstate bank branching has a positive impact on the valuation of private targets.

My multivariate analysis implements the difference-in-differences methodology in a regression framework that controls for acquirer, target, deal, and state characteristics as well as state, industry, and year fixed effects. This analysis yields a consistently positive and statistically significant effect of interstate bank branching deregulation on deal value-to-sales multiples of private targets. Estimates of the economic significance indicate that deregulation of interstate bank branching results, ceteris paribus, in an increase in deal value-to-sales multiples of 7.3%.

As a proxy for the degree to which the private target depends on the acquirer for financing, I benchmark each private target's deal value-to-sales multiple to deal value-to-sales multiples of matched public targets. Again, I use the natural logarithm transformation of this ratio in empirical tests to address skewness in the distribution. As in Officer (2007), public

targets are matched on industry, deal value, and date of announcement. In support of my second hypothesis, the ratio of private target multiples to public target multiples is positively and significantly impacted by deregulation of interstate bank branching. This evidence indicates that targets depend less on acquirers for financing when access to bank credit improves.

Next, I analyze acquirer cumulative abnormal returns (CARs) calculated over the (-5, +5) day window around the announcement of the sellout. Consistent with the magnitude documented in previous studies, announcement period CARs average 3.51% for public acquirers of private targets (Chang, 1998; Fuller, Netter, and Stegemoller, 2002). For sellouts announced in states that deregulate interstate bank branching, acquirer CARs average 5.00% before deregulation and 2.93% after deregulation. Multivariate tests also show that deregulation of interstate bank branching has a negative effect on acquirer CARs. I note that acquirer CARs remain positive on average, albeit smaller, following deregulation.

I also analyze the dollar abnormal wealth gain of the acquirer relative to the deal value. This variable measures the NPV to the acquirer's shareholders per dollar paid to the target (Morck, Shleifer, and Vishny, 1990). This measure will be lower if relaxed financial constraints enable private targets to obtain more of the economic gains generated in the transaction. In both univariate and multivariate tests, I find that deregulation of interstate bank branching has a negative effect on acquirer wealth gain-to-deal value ratios. Overall, the results indicate that acquirer returns are lower when private targets have relaxed financial constraints, in support of my third hypothesis.

I expect that the effect of deregulation of interstate bank branching is greater for smaller private targets compared to larger private targets. Private firms that are smaller in size are likely to face greater constraints in borrowing and therefore benefit more from IBBEA than larger

private firms. I find that the effect of deregulation on deal value-to-sales multiples and the ratio of private-to-public target multiples is greater for smaller private targets compared to larger private targets. However, there is not an incremental effect for smaller private targets on acquirer wealth gains.

I conduct a falsification test on a sample of takeovers of public targets. Public firms are characterized by less severe information asymmetries than private firms (due more stringent disclosure laws, analyst coverage, media coverage, etc.). As a result, public firms have a variety of financing alternatives and are likely less dependent on banks located in their home state compared to private firms. Therefore, I predict that public targets are not impacted by deregulation of interstate bank branching, in contrast to my finding on a sample of private targets. I find no effect of deregulation on valuation multiples, acquirer CARs, or acquirer wealth gain-to-deal value ratios for a sample of 722 public target takeovers.

An alternative explanation for my empirical results is that a selection effect in sellouts drives the increase in target valuation. Improved access to finance for private firms, caused by deregulation of interstate bank branching, can change the composition of firms that choose a sellout. For example, before deregulation there can be more liquidity motivated sales of firms (leading to lower firm valuations) and after deregulation there can be fewer liquidity motivated sales and more sales of firms motivated by operational synergies (leading to higher firm valuations). To evaluate the selection effect explanation, I test whether or not deal characteristics of sellouts are significantly impacted by deregulation of interstate bank branching laws. I find that the univariate difference-in-differences is not statistically significant for target sales, acquirer market value, relative size of the acquirer and target, the proportion of all cash deals, the proportion of targets and acquirers in the same industry or same state, the proportion of

high tech targets, or state income growth. Also, the proportion of sellouts to firms in the state is not significantly affected by IBBEA.[6] These results do not completely rule out the possibility of a selection effect, but they provide evidence against this alternative explanation.

I perform a series of robustness tests on my empirical results. One concern is that outliers in the data (especially for deal value-to-sales ratios) are driving the results. I test whether or not my results are driven by outliers by winsorizing the data at the 10^{th} and 90^{th} percentiles and transforming the dependent variables into ranked values (e.g., 1 for the lowest value and 557 for the highest value). I find similar results to those described above using both approaches. A second concern is that one state may have an outsized influence on my tests. The sample includes 95 private targets headquartered in California (17.06% of the sample), far more than any other state. In addition, California is home to many "internet firms" in the latter part of the sample period which are likely to have high valuations. I remove all 95 private targets headquartered in California and find similar results as described above.

Because deregulation is a choice made at the state level, state characteristics may drive my results rather than relaxed financial constraints. I address this concern in three ways. First, I include state fixed effects in my empirical tests, which control for unobservable time-invariant state characteristics. Second, I control for time varying state economic growth in multivariate tests. Third, I conduct a test to alleviate the concern that factors which lead a state to deregulate or not are correlated with private firm value and, thus, drive my results. In this test, I predict which states are likely to deregulate or not using state-level variables motivated by Kroszner and Strahan (1999). Then, I replicate my empirical results on a sub-sample of deals where the private target is located in a state where the choice to deregulate or not is less predictable (using

[6] I obtain the annual number of firms in the state from the U.S. Census. I note that this test offers only a rough approximation of the proportion of sellouts in a state due to the fact that not all sellouts are observed.

various cutoffs). I obtain similar results to those reported above in this sub-sample which suggests that my results are not driven by factors that lead a state to deregulate or not. In summary, neither observable nor unobservable state characteristics appear to drive my results.

Further, I find that the results are similar if I calculate acquirer CARs around the (-2, +2) day window around the deal announcement. Finally, the main results in the paper hold when using a more granular definition of deregulation that classifies deregulating states into groups based on how many restrictions on interstate bank branching were relaxed.

This paper makes several contributions to the literature. First, I show that valuations of private targets in sellouts are impacted by the target's ability to access bank credit. This finding contributes to the literature that studies how financial constraints impact economic growth (see, e.g., Jayaratne and Strahan, 1996; Rajan and Zingales, 1998; and Demirguc-Kunt and Maksimovic, 1998). Because I examine a shock to state level banking laws, this finding contributes to the literature that studies how local financial development influences economic outcomes (Guiso, Sapienza, and Zingales, 2004; Becker, 2007; Butler and Cornaggia, 2011; Gilje, 2012). My paper complements Chava and Purnanandam (2011) who find evidence that adverse capital shocks to banks at the national level negatively impacts the value of public firms, especially those without access to public debt markets. My paper focuses on a shock at the state level and finds valuation effects for private firms in the context of corporate takeovers. Despite the importance of private firms to the U.S. economy (Askar, Farre-Mensa, and Ljungqvist, 2014), private firms are relatively underexplored in the literature. My evidence suggests that state-level financial development matters for certain firms, even in an advanced economy such as the U.S. in the 1990s.

Second, a novel finding in this paper is that bank credit offers private target firms a substitute for financing provided by public acquirers. Officer (2007) and Erel, Jang, and Weisbach (2013) show that public acquirers provide liquidity to private targets. I extend this line of research by demonstrating that the ratio of private target valuation multiples to matched public target valuation multiples, a proxy for the degree to which targets depend on acquirers for liquidity, decreases when the target firm has better access to bank credit.

Third, this paper provides new insight into the factors that drive announcement returns to public acquirers of private targets. Specifically, I find that acquirer returns are lower when private targets are less financially constrained. This finding provides direct evidence that acquirer returns in sellouts are driven, at least in part, by the provision of liquidity to targets. Previous research identifies the method of payment, relative size of the acquirer and target, the formation of a blockholder in the acquirer, and valuation uncertainty as determinants of acquirer returns (Chang, 1998; Fuller, Netter, Stegemoller, 2002; Faccio, McConnell, and Stolin, 2006; Officer, Poulsen, and Stegemoller, 2009; Cooney, Moeller, and Stegemoller, 2009). Private targets that are less financially constrained are likely to have greater bargaining power relative to the acquirer. Thus, consistent with Ahern (2012) and Cornaggia, Mao, Tian, and Wolfe (2013), my evidence also suggests that bargaining power is important in acquisition outcomes.

The rest of the paper is organized as follows. Section 2 develops the empirical design and hypotheses. Section 3 describes the sample, the construction of key variables, and univariate tests. Sections 4 and 5 present multivariate empirical results. Section 6 describes robustness tests and Section 7 concludes.

2. Empirical design and hypotheses

The main hypothesis that I test is that private target valuations are positively impacted by relaxed financial constraints. The empirical design addresses the concern that firm value and financial constraints are endogenously determined.

2.1. Exogenous shock to financial constraints and empirical design

To test for the effect of relaxed financial constraints on private target valuations in sellouts, I exploit exogenous variation in financial constraints caused by deregulation of the U.S. banking industry. Private firms are likely to be affected by state level banking law changes because they rely on local banks to resolve information asymmetries and provide capital (Berger and Udell, 1995; Petersen and Rajan, 1994). In a panel of private U.S.-based firms, Zarutskie (2006) finds debt in more than 70% of the observations. In a sample of small businesses with ten or more employees, Black and Strahan (2002) find that 96% use a commercial bank service and 76% have a credit facility. Despite technological advances in the banking industry, Petersen and Rajan (2002) find that the median (75^{th} percentile) distance from a small business to the firm's bank was 5 (20) miles in the early 1990s. Given a reliance on local banks, deregulation of a state's banking industry provides an exogenous shock to financial constraints for private firms located in that state.

IBBEA removed barriers to interstate branching, which allowed banks to cross state lines in ways that were not previously possible (Johnson and Rice, 2008; Rice and Strahan, 2010). However, IBBEA also gave each state a three year window to opt-out of interstate bank branching deregulation or establish restrictions that limit the ability of out-of-state banks to do

business in the state. The result was staggered deregulation that varied across states and over time.

IBBEA provides a nice setting to test for the effect of loosened financial constraints on firm value for the following reasons. First, empirical research shows that deregulation of interstate bank branching laws have a positive effect on the percentage of out-of-state bank branches in a state and a negative effect on the cost of credit for small firms (Johnson and Rice, 2008; Rice and Strahan, 2010). Second, the factors which cause states to deregulate are not likely to be correlated with private firm sales. Kroszner and Strahan (1999) show that Congressional votes which favor deregulation are more likely when large banks have a greater share of bank assets in the legislator's state or bank rivals (such as insurance companies) are less influential in the legislator's state. These factors are unlikely to be systematically correlated with private firm sale prices in a state.

Third, a shock to financial constraints caused by IBBEA should be experienced to a greater degree by private targets (who are likely to depend on banks in the state) than public acquirers (who are likely to have many financing alternatives). Since the sale price of the target is determined through a negotiation, it is ideal to utilize a shock which affects the target's financial constraints but not the acquirer's financial constraints. Fourth, the staggering of deregulation over time allows for a broad control sample compared to a deregulation event that occurs simultaneously for all states.

For these reasons, I use IBBEA as an exogenous shock to financial constraints on private firms located in a state. I implement a difference-in-differences methodology (Roberts and Whited, 2012) where observations are classified based on whether the private target's home state deregulates or not and whether the sellout occurs before or after each state's response to IBBEA.

States are counted as deregulating or not based on the initial legislative response to IBBEA as identified by Johnson and Rice (2008) and Rice and Strahan (2010).

2.2. Hypotheses

Private target value is measured by the total consideration paid to the private firm (deal value), scaled by the firm's annual sales in order to adjust for size (Bayar and Chemmanur, 2012; Officer, 2007; Poulsen and Stegemoller, 2008). Following deregulation of interstate bank branching, private firms will have better access to bank credit. Improved access to finance will strengthen the private target's bargaining position in a sellout as it gives private targets an alternative to the financing an acquirer can provide. Firm owners can increase the standalone value of their firm by using bank credit to finance positive NPV projects. Because owners of private firms typically hold concentrated equity positions, they have strong incentives to choose positive NPV projects when financial constraints are relaxed. The higher standalone value of the firm will be reflected in a higher valuation when the firm is sold.

Better access to bank credit can increase the firm's valuation in the sellout even if the firm has not yet made the investment in a new project. The reason is that the ability to substitute bank credit in place of financing from the acquirer increases the value of the private target's outside option. Thus, the firm owner can bargain for a higher valuation. In summary, relaxing financial constraints improves the bargaining position of the private target and is predicted to increase the valuation of private targets, as formalized in the following hypothesis.

Hypothesis 1: Relaxing financial constraints on private targets leads to higher valuations in sellouts.

The ability of private firms to substitute bank credit for financing provided by acquirers should be reflected not only in the valuation of the private target, but in the valuation of the private target relative to matched public targets. The intuition behind this measure is that public targets do not depend heavily on acquirers for financing and public target valuations should not reflect the provision of financing by acquirers. Thus, public target valuations provide a benchmark that can be used to measure the degree of financing provided by acquirers to private targets (Officer, 2007). If improved access to bank credit offers private target owners a substitute for some part of the financing provided by acquirers, then the relative valuation of the private target should increase. Therefore, I predict that the valuations of private targets, benchmarked to valuations of matched public targets, will increase as the financial constraints faced by them are alleviated.

Hypothesis 2: Relaxing financial constraints on private targets leads to higher valuations of private targets relative to valuations of matched public targets.

The easing of financial constraints on private targets can affect the wealth gain of the acquirer's stockholders. Relaxed financial constraints on private targets allow owners to receive higher prices for their firms and therefore capture a larger portion of the economic gain generated in the transaction. Consequently, the acquirer will capture a smaller portion of the economic gain. Under the assumption that the total economic gain generated in the transaction

does not increase, the wealth gain of the acquirer's stockholders will decrease when the acquirer pays more for the target.[7]

CARs are widely used to measure the effect of acquisitions on the wealth of the acquirer's shareholders. CARs are an estimate of the NPV of the deal from the perspective of the acquirer's shareholders scaled by the acquirer's market value before the transaction. Another way to measure the effect of acquisitions on the wealth of the acquirer's shareholders is to scale the NPV by the price paid for the target. Morck, Shleifer, and Vishny (1990) argue that a benefit of this measure is that the quality of the acquisition is independent of the beginning market value of the acquirer.[8] I predict that loosened financial constraints on private targets result in a decrease in both acquirer CARs and acquirer wealth gains per dollar paid to the target.

Hypothesis 3: Relaxing financial constraints on private targets leads to lower acquirer CARs and lower acquirer wealth gains per dollar paid to the target.

Testing the hypotheses above relies on the implicit assumption that the firms sold before a state responds to IBBEA are similar to the firms sold following a state's response to IBBEA. However, improved access to finance may alter the composition of firms that choose a sellout. For example, before deregulation of interstate bank branching, a large portion of sellouts may be motivated by liquidity reasons. Following deregulation, when access to finance improves, there may be fewer liquidity motivated sellouts and more sellouts motivated by operational synergies.

[7] The assumption that the total economic gain generated in the transaction does not increase is reasonable if the acquirer can finance all the projects that a bank can finance. Then, improved access to finance for private targets will alter the division of gains, but will not increase the total gain generated in the transaction.

[8] For example, two sellouts that generate $50 in wealth for the acquirer shareholders and pay target owners $200 will have the same Morck, Shleifer, and Vishny (1990) ratio of 0.25. If the wealth generated is scaled by the acquirer's market value (say $2,500 and $5,000 respectively), then the quality of the transaction will look quite different across deals when using CARs (0.02 and 0.01 respectively).

Because sellouts motivated by liquidity reasons are likely to result in lower multiples, lower multiples relative to matched public targets, and greater acquirer returns than sellouts motivated by operational synergies, the predictions are the same given a selection effect or a bargaining power effect. I test for the possibility that a selection effect exists below.

3. Data and univariate tests

3.1. State level legislative response to IBBEA

Johnson and Rice (2008) and Rice and Strahan (2010) gather data on the initial legislative response to IBBEA for each of the 50 U.S. states plus the District of Columbia.[9] These data are presented in Table 1. Between 1994 and 1997, each state responded to IBBEA and had the opportunity to implement four restrictive provisions: (1) a minimum age of in-state banks targeted for an acquisition, (2) restrictions on the ability of out-of-state banks to open a new branch (restrictions on de novo interstate branching), (3) restrictions on the ability of out-of-state banks to acquire a single in-state bank branch, and (4) a statewide cap on deposits below 30% of the total deposits in the state.

Rice and Strahan (2010) describe how these four restrictions raise the cost for out-of-state banks to build an interstate branch network or distort a bank's means of entry into the state. States that adopt all four restrictive provisions (or opted-out of IBBEA) are counted as non-deregulating states. States that choose not to adopt all four provisions and therefore remove at least one barrier to interstate branching are counted as deregulating states.

[Insert Table 1: State-level interstate bank branching laws]

[9] For ease of exposition, the District of Columbia is referred to as a state.

3.2. Sample of private target sellouts and takeovers of public targets

The sample of sellouts is from Thomson Reuter's SDC Platinum Mergers and Acquisitions database (SDC). Deals include a private target and a public acquirer with stock price data on CRSP at the time of the acquisition announcement. The acquirer must purchase 100% of the private target's stock in the sellout. Recapitalizations, leveraged buyouts, repurchases, spin offs, acquisitions of partial interest, acquisitions of remaining interest, buybacks, and exchange offers are excluded. Deals with target firms in the financial industry and utility industry are also excluded (SIC codes 6000 to 6999 and 4900 to 4999).

To ensure the inclusion of deals that are likely to disclose financial data on targets, the deal value must be $50 million (Officer, 2007; Rodrigues and Stegemoller, 2007; Poulsen and Stegemoller, 2008) and also the deal value must be 1% of the acquirer's market value of equity. These restrictions likely result in the exclusion of some small private targets that are affected by IBBEA. This will bias against my empirical tests. The announcement of the acquisition must be between three years before and three years after the target's state responded to IBBEA (see Figure 1).[10] This six-year window allows time for the banking industry to respond to new state legislation and for a relatively large sample of deals in both the pre- and post-IBBEA period. The final sample is 557 sellouts announced from 1992 to 2000.

[Insert Figure 1: State laws and sample selection]

I construct a sample of takeovers of public targets from SDC data using similar restrictions. I include only takeovers that involved a public acquirer who purchases 100% of the public target's equity and has no toehold in the target in order to make comparisons to sellouts where the acquirer purchases the entire private firm. Public targets are matched to private targets

[10] A few states changed interstate bank branching laws a second time during the three year post-IBBEA period. In this case, the window for sellouts in the post-IBBEA period extends only until the law is changed the second time.

to create my proxy for the degree to which private targets depend on the acquirer for financing. Also, I use a sample of 722 public target takeovers announced around state's responses to IBBEA as a falsification test in Section 5.

Panel A of Table 2 shows the distribution of private target sellouts by year. The number of sellouts peaks at 125 in 1997, which corresponds to the final wave of state legislative responses to IBBEA. Panel B of Table 2 shows the distribution of sellouts by state. California is home to the most private targets (95, 17.06% of the sample) followed by Texas, Illinois, New York, and Georgia. There are 28 states that are represented by at least five deals in the sample. Eight of the 51 states are not represented in the sample.[11]

[Insert Table 2: Year and state distribution]

3.3. Variables and summary statistics

The first measure of firm value is *Deal Value to Sales* which is equal to deal value divided by annual sales. The natural logarithm transformation (*LN Deal Value to Sales*) is used in tests due to skewness in the distribution. Annual sales is last twelve months sales, measured as close as possible from the quarter of the deal announcement. Sales data are often found in private target financials disclosed in the acquirer's filings on the SEC's EDGAR website. Otherwise, sales numbers are collected from news articles found in LexisNexis.

Each private target is matched to public targets in the same two-digit SIC code, with a deal value within 20%, and announced within the four-year calendar window centered on the

[11] In the sample, there are no sellouts of private targets based in Alaska, Delaware, Hawaii, New Mexico, North Dakota, South Dakota, West Virginia, or Wyoming.

announcement of the sellout.[12] A total of 431 private targets can be matched to at least one public target. In Section 6, I describe robustness results that use alternative matching criteria. On average, each private target is matched to six public targets, while the median number of matched public targets is four. The difference in deal values between private targets and matched public targets averages -1% while the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles are -16%, -11%, -1%, 9%, and 15%, respectively. The private target deal value-to-sales ratio is divided by the average deal value-to-sales ratio of the matched public targets to create *Private to Public Value*. The natural logarithm transformation is used in tests due to skewness in the distribution (*LN Private to Public Value*).[13]

Announcement period abnormal returns to acquirers are calculated from CRSP data using a value-weighted index market model. Daily returns for 240 days beginning 300 days before the announcement of the acquisition are used to estimate the market model parameters (a minimum of 30 observations are required). The acquirer's cumulative abnormal return (*Acquirer CAR*) is measured over the (-5, +5) day window centered on the announcement. The market value of the acquirer's equity, *Acquirer Market Value,* is measured 15 days before the announcement of the sellout and the natural logarithm transformation is *LN Acq Mkt Value.*

The second measure of the acquirer's stock performance is the acquirer's abnormal wealth gain divided by the deal value of the target (*Acquirer Wealth to Deal Value*). The abnormal wealth gain is equal to *Acquirer CAR* multiplied by *Acquirer Market Value*.

Summary statistics are show in Table 3. All continuous variables are winsorized by setting values that are beyond the 1^{st} (99^{th}) percentile to the value of the 1^{st} (99^{th}) percentile. All

[12] To allow for a greater number of potential matches, I don't require that the public target acquisition is announced within the three year window around the response to IBBEA. Also, since I match based on deal value, I don't explicitly require that public target acquisitions are $50 million in deal value or 1% of the acquirer's equity.

[13] Bayar and Chemmanur (2012) use a similar measure to compare the valuation of sellout firms to firms that conduct an IPO.

dollar values are in millions and are adjusted by the Consumer Price Index to year 2000 dollars. Variables are described in Appendix A. The mean (median) deal value is $202.17 ($114.04) million. The mean (median) level of sales for the target is $151.19 ($73.16) million. Mean (median) *Deal Value to Sales* is 15.49 (1.68). The mean (median) of *LN Deal Value to Sales* is 0.76 (0.51). The mean (median) of *LN Private to Public Value* is 0.07 (-0.10). The mean (median) of *Acquirer CAR* is 3.51% (2.17%). Mean (median) acquirer wealth gain is $2.62 ($10.64) million. The mean (median) of *Acquirer Wealth to Deal Value* is 0.07 (0.08).

[Insert Table 3: Summary statistics]

Relative Size is deal value divided by the acquirer's market value 15 days before the announcement of the sellout. The median of this measure is 18%. *State Income Growth* is the annual percentage change in real per capita personal income for the state in the year before the deal announcement. I follow Jayaratne and Strahan (1996) and calculate this variable with data from the Bureau of Economic Analysis. The mean of *State Income Growth* is 2.02%. *High Tech Target* is a dummy variable which indicates that the target operates in one of the four-digit SIC code industries identified by Loughran and Ritter (2004) as a high tech industry (29% of the sample). *All Cash* is a dummy variable that equals one if the consideration paid for the target is all cash and no stock (29% of the sample), and is zero otherwise. *Same State* is a dummy variable that equals one if the acquirer and target are headquartered in the same state (18% of the sample), and is zero otherwise. *Same Industry* is a dummy variable that equals one if the acquirer and target share the same four-digit SIC code (34% of the sample), and is zero otherwise.

The dummy variable *Dereg State* is equal to one for all sellouts of private targets headquartered in states that deregulate (whether or not the deal was announced before the response to IBBEA) and zero for all sellouts of private targets headquartered in states that do not

deregulate. Deregulation is defined as relaxing at least one restriction to interstate bank branching. The dummy variable *After* is equal to one for all sellouts announced in a state after the state responds to IBBEA (whether the state deregulates or not) and is equal to zero for all sellouts announced before the state responds to IBBEA. The interaction *Dereg State x After* indicates sellouts that are announced in a deregulated banking environment. Of 557 sellouts, 84% include private targets in a state which deregulates (*Dereg State = 1)* and 67% are announced after the target's home state has responded to IBBEA (*After = 1).* Therefore, 56% (84% multiplied by 67%) of all sellouts are announced in a deregulating state following deregulation (*Dereg State x After = 1).*

3.4. Univariate tests

Table 4 shows the means and medians of key variables in subsamples divided based on whether a sellout was announced in a deregulating state or non-deregulating state and whether the announcement occurred before or after a state's response to IBBEA. For sellouts of firms based in deregulating states, the changes to *LN Deal Value to Sales, Acquirer CAR,* and *Acquirer Wealth Gain to Deal Value* are all in the predicted direction and statistically significant. For sellouts of firms based in non-deregulating states, changes are measured around the state's legislative response to IBBEA which allowed the state to maintain restrictions on interstate bank branching. The change in each of the four key measures is statistically insignificant. The insignificant result is to be expected since the state essentially maintained the status quo. The bottom four rows of Table 4 show that the difference-in-differences is in the predicted direction for all four variables, and statistically significant for *LN Deal Value to Sales* and *Acquirer Wealth Gain to Deal Value.*

[Insert Table 4: Univariate tests]

I extend the analysis of Table 4 to include deal characteristics in order to examine whether or not deregulation of interstate bank branching affects the types of deals that occur. If deals are systematically different after deregulation, then a selection effect in observed deals can be responsible for an increase in private firm sale prices rather than a shift in bargaining power. In Appendix B: Supplemental Results, I show that the univariate difference-in-differences is not statistically significant for *Sales, Relative Size, All Cash, Acquirer Market Value, High Tech Target, Same State, Same Industry,* and *State Income Growth*. I do find, however, that *State Income Growth* and *High Tech Target* are significantly higher for sellouts announced after deregulation compared to before deregulation in deregulating states. In multivariate tests that follow, I control for these variables.

In another test, I examine whether the likelihood of a sellout changes following deregulation. For each state and year, I calculate the number of sellouts in my sample per 100,000 firms. The time series of number of firms is from Census data and includes all firms with one to 499 employees. Appendix B: Supplemental Results shows that in both deregulating and non-deregulating states, the proportion of sellouts increases on average following the state's response to IBBEA. However, the difference-in-differences is not statistically significant. Overall, the generally similar deal characteristics and lack of change in likelihood of a sellout indicate that a selection effect in observed deals is probably not driving my results.

4. Empirical specification and main empirical results

4.1. Regression specification

Two empirical specifications are used to implement the difference-in-differences approach and test Hypotheses 1, 2, and 3. The first specification of the regression is:

$$y_i = \beta_0 + \beta_1 Dereg\ State + \beta_2 After + \beta_3 Dereg\ State \times After + \lambda_{industry} + \lambda_{year} + \beta Controls_i + \varepsilon_i \ , \quad (1)$$

where the unit of observation is sellout *i*. The dependent variable is either *LN Deal Value to Sales*, *LN Private to Public Value*, *Acquirer CAR*, or *Acquirer Wealth to Deal Value*. The variable *Dereg State* controls for common factors among states that deregulate. The coefficient on the interaction term *Dereg State x After* (β_3) captures the effect of bank branching deregulation. Specifically, the coefficient captures the difference in changes to *LN Deal Value to Sales* (or another dependent variable) before and after responses to IBBEA between sellouts in deregulating states and those in non-deregulating states. The term $\lambda_{industry}$ captures industry fixed effects where industry is defined by two digit-SIC codes and the term λ_{year} captures year fixed effects. Control variables include *Relative Size*, *LN Acq Mkt Value*, *All Cash*, *High Tech Target*, *Same State*, *Same Industry*, and *State Income Growth*. The deal level control variables have been shown to be important in acquisition outcomes (Chang, 1998; Fuller, Netter, Stegemoller, 2002; Faccio, McConnell, Stolin, 2006; Officer, Poulsen, Stegemoller, 2009). To address correlation between observations within each state, robust standard errors are clustered at the state level for all regressions (Petersen, 2009).

A second methodology controls for fixed differences between states by adding state fixed effects to the specification. The specification of the regression for the second approach is:

$$y_i = \gamma_0 + \gamma_1 After + \gamma_2 Dereg\ State \text{ x } After + \lambda_{industry} + \lambda_{state} + \lambda_{year} + \gamma Controls_i + \varepsilon_i , \quad (2)$$

where the unit of observation is sellout *i*; $\lambda_{industry}$ captures industry fixed effects, λ_{state} captures state fixed effects, and λ_{year} captures year fixed effects. The variable *Dereg State* is omitted from the regression due to the inclusion of state fixed effects. The coefficient on the interaction term *Dereg State* x *After* (γ_2) captures the effect of bank branching deregulation.

When implementing equation (1), there is an implicit assumption that deals in deregulating states are comparable to deals in non-deregulating states (the control group). However, the specification of equation (2) takes advantage of staggered responses to IBBEA across time and allows for a control group that includes deals in non-deregulating states plus deals in states that have not yet deregulated, but will do so in the future. For example, North Carolina responded to IBBEA in 1995 while Georgia responded in 1997. Even though both states eventually deregulate interstate bank branching, sellouts of Georgia-based firms announced in 1996 (before deregulation in Georgia) are used as controls for sellouts of North Carolina-based firms announced in 1996 (after deregulation in North Carolina).

Another benefit of implementing the difference-in-differences test with equation (2) instead of equation (1) is the ability to control for state level differences in the value of firms with state fixed effects. For example, firms in North Carolina can be systematically valued higher than firms in Georgia. When all deregulating states are grouped into a single category, as in the first approach, such differences in valuation are not accounted for. However, a potential drawback of including state fixed effects is the difficulty in precisely estimating the average value of a firm in a state with a small number of deals. Given that many states have a small

number of observed deals, it may be difficult to obtain a precise estimate of the true average value of a firm in those states. Thus, the tradeoff for estimating a state fixed effect is that the fixed effect could be estimated imprecisely for many states. As it turns out, the results are largely similar with either specification.

4.2. Multivariate tests of private firm valuations

In Tables 5 and 6, I implement multivariate tests of Hypotheses 1 and 2, respectively. The dependent variable is *LN Deal Value to Sales* in Table 5 and *LN Private to Public Value* in Table 6. Each table shows the estimation of equations (1) and (2) on a sample of sellouts announced in the +/- three year window around a state's response to IBBEA as well as the +/- two year window, and the +/- one year window. The key variable of interest is *Dereg State x After*.

Table 5 shows the results of ordinary least squares (OLS) estimation where the dependent variable is *LN Deal Value to Sales*. In column (1), the coefficient on *Dereg State x After* is 0.597 and is statistically significant at the 1% level (t-stat = 2.936). This finding indicates that deregulation had a significantly positive impact on private target valuations. The coefficient on *Dereg State* is negative and statistically significant at the 10% level, indicating that private firm valuation multiples are lower in deregulating states than non-deregulating states before the response to IBBEA. The positive and significant coefficient on *Relative Size* could be attributed to the greater bargaining power of private firms which are larger relative to the acquirer. The negative coefficient on *All Cash* suggests that private firm owners accept a lower price for the firm in order to receive the immediate liquidity of a cash payment.

In Table 5, column (2) the regression includes state fixed effects and the coefficient on *Dereg State x After* is 0.544 and statistically significant at the 5% level (t-stat = 2.032), again indicating a significantly positive impact of deregulation on private target valuations. In columns (3) and (4) the sample is restricted to sellouts in the +/- two year window around the response to IBBEA and I find similar results. Columns (5) and (6) show positive coefficients on *Dereg State x After* (significant at the 10% level) when the window for sellouts is one year around the response to IBBEA.

To estimate the economic significance of relaxed financial constraints, I take the exponential of both sides of the equation in column (2) and evaluate when the independent variables are at their mean values and *Dereg State x After* changes from 0 to 1. The result is a change in *Deal Value to Sales* of 1.135, an increase of 7.3% from the sample mean.[14] The evidence that deregulation of interstate bank branching restrictions raises private firm valuations supports Hypothesis 1.

[Insert Table 5: LN Deal Value to Sales]

Table 6 tests Hypothesis 2 by examining the impact of deregulation on *LN Private to Public Value*. In column (1), the coefficient on the interaction term *Dereg State x After* is 0.712 and statistically significant at the 1% level (t-stat = 2.774). The negative and significant coefficient on *Dereg State* indicates that *LN Private to Public Value* is lower for firms in deregulating states than non-deregulating states before the response to IBBEA. The negative and significant coefficient on *All Cash* is consistent with results in Officer (2007). In column (2), the coefficient on the interaction term *Dereg State x After* is 0.834 and is also statistically significant at the 1% level (t-stat = 2.808). The results are similar in columns (3) through (6). The evidence

[14] When *Dereg State x After* equals 0, the predicted value of the regression is 0.45143. Then, the change in *Deal Value to Sales* as *Dereg State x After* changes from 0 to 1 is (exp(0.45143+0.544) – exp (0.45143)) = 1.135.

suggests that loosened financial constraints increases the relative value of private targets compared to public targets.

The economic significance of the coefficient in column (2) suggests that the ratio of private target valuations to public target valuations increases by 22% due to deregulation of interstate bank branching restrictions. The evidence supports Hypothesis 2 and indicates that relaxed financial constraints allow private targets to substitute bank credit in place of financing provided by the acquirer.

[Insert Table 6: LN Private to Public Value]

4.3. Multivariate tests of acquirer wealth gains

Tables 7 and 8 test Hypothesis 3. The tables estimate equations (1) and (2) with *Acquirer CAR* and *Acquirer Wealth to Deal Value* as the dependent variables and are structured the same as Tables 5 and 6. The equations are estimated by weighted least squares (WLS) where the weight for each observation is the inverse of the standard deviation of the residual from the value-weighted index market model used to estimate acquirer abnormal returns. This method gives more weight to observations where the market model can better explain the acquirer's time series of returns.

Table 7 shows estimates of equations (1) and (2) with *Acquirer CAR* as the dependent variable. In column (1), the coefficient on *Dereg State x After* is -2.860 but not statistically significant at conventional levels (t-stat = -1.460). In column (2), the coefficient on *Dereg State x After* is -3.292 and statistically significant at the 10% level (t-stat = -1.831). This finding indicates that acquirer returns are significantly negatively impacted by banking deregulation in the private target's home state. Columns (1) and (2) also show that acquirer CARs are lower for

larger acquirers, consistent with Faccio, McConnell, and Stolin (2006). The coefficients on *Dereg State x After* are negative and statistically significant at the 1% level in columns (3) through (6) where the sample is restricted to the +/- two year window and the +/- one year window around the state's response to IBBEA.

[Insert Table 7: Acquirer CAR]

In Table 8, the dependent variable is *Acquirer Wealth to Deal Value*. In column (1), the coefficient on *Dereg State x After* is -0.498 and statistically significant at the 10% level (t-stat = -1.836). A difference-in-differences of -0.498 is in line with the -0.370 drop documented in univariate tests in Table 4 and represents an economically significant decrease in the wealth gain per dollar paid to the target. In column (2), the coefficient on *Dereg State x After* is negative, but not statistically significant. The coefficients on *Dereg State x After* are negative and statistically significant in columns (3), (5), and (6) and the coefficient is negative, but not statistically significant in column (4). Overall, the multivariate evidence presented in Tables 7 and 8 supports Hypothesis 3. Acquirers have lower wealth gains when private targets have relaxed financial constraints.

[Insert Table 8: Acquirer Wealth to Deal Value]

5. Variation in the impact of loosened financial constraints

The effect of loosened financial constraints, achieved by banking industry deregulation, can vary across different types of firms. Empirical evidence shows that firm size is a significant predictor of financial constraints (Hadlock and Pierce, 2010). Large private firms generally have more financing options as information asymmetries are less severe than for small private firms (Berger and Udell, 1998). For example, large private firms may have operations over a wide

geographic area, which makes them less dependent on local banks and therefore less affected by IBBEA. Also, evidence shows that improvements to financing conditions tend to benefit large firms less than small firms (Beck, Demirguc-Kunt, and Maksimovic, 2005). Therefore, I predict that the impact of deregulation is greater for small private targets than large private targets. I test this prediction in Section 5.1.

The effect of IBBEA should also vary across private firms and public firms. In general, public firms tend to have fewer information asymmetries than private firms due to reporting requirements, analyst coverage, and media coverage. Therefore, public firms have a variety of financing alternatives to bank debt. Public firms that do tend to borrow from banks are likely to have access to banks outside of their home state, mitigating the effect of state-level banking law changes. Therefore, I conduct a falsification test using a sample of public targets. I predict that IBBEA will not have a significant effect on firm valuations, acquirer CARs, and acquirer wealth gains per dollar paid to targets for public target takeovers. I test this prediction in Section 5.2.

5.1. The impact of deregulation on small private targets versus large private targets

To test the prediction that the effect of relaxed financial constraints is stronger for small private firms compared to large private firms, I include additional terms in equations (1) and (2). Specifically, I interact *Dereg State x After* with a dummy variable, *Small,* that is set to one for firms which have below median annual sales ($73.16 million) and is set to zero otherwise. A statistically significant coefficient on *Dereg State x After x Small* indicates a differential effect of relaxed financial constraints for small private firms compared to large private firms. I also include *Small* and other interactions with *Small* in the regression as appropriate.

In Table 9, Panel A, the dependent variable is *LN Deal Value to Sales* in columns (1) through (4) and *LN Private to Public Value* in columns (5) through (8). The sample is based on the +/- three year window in columns (1), (2), (5), and (6) and the +/- two year window in columns (3), (4), (7), and (8). The control variables are the same as in the regressions in Tables 5 and 6.

Columns (1) through (4) of Panel A show a positive and statistically significant coefficient on *Dereg State x After x Small* which indicates that the effect of deregulation on *LN Deal Value to Sales* is greater for smaller private firms than for larger private firms. In column (2), the coefficient on *Dereg State x After x Small* is 0.564 (t-stat = 1.923) while in column (4) the coefficient is 0.595 (t-stat = 3.403).

The coefficient on *Dereg State x After x Small* is positive and statistically significant in columns (5) through (8) as well. This evidence indicates that the effect of deregulation on *LN Private to Public Value* is greater for smaller private targets than for larger private targets. For example, the coefficient is 0.852 (t-stat = 2.176) in column (6) and 0.742 (t-stat = 2.863) in column (8). The coefficient on *Small* is positive and statistically significant in every column in Table 9, Panel A, indicating that small private targets have significantly higher valuations than larger private targets.

Table 9, Panel B is structured the same as Panel A. The dependent variable is *Acquirer CAR* in columns (1) through (4) and *Acquirer Wealth to Deal Value* in columns (5) through (8). The coefficient on *Dereg State x After x Small* is positive in every regression, opposite to my prediction, but it is not statistically significant at conventional levels. There does not appear to be an incremental effect of deregulation for small private targets on *Acquirer CAR* or *Acquirer Wealth Gain to Deal Value*.

[Insert Table 9: Interaction with Small]

5.2. Takeovers of public targets and IBBEA

I next conduct a falsification test which investigates whether or not IBBEA had a significant effect on a sample of public targets. I estimate equations (1) and (2) on a sample of 722 takeovers of public targets by public acquirers. I include only takeovers that involved an acquirer who purchases 100% of the target's equity and has no toehold in the target. The average deal value in this sample is $1.29 billion and the average level of sales for the target is $731 million. Acquirer CARs average -2.01% and the target's deal value averages 49.8% of the acquirer's pre-deal market value.

Table 10 presents the results of the estimation, where the dependent variable is *LN Deal Value to Sales* in columns (1) and (2), *Acquirer CAR* in columns (3) and (4), and *Acquirer Wealth to Deal Value* in columns (5) and (6).[15] I predict that the coefficient on *Dereg State* x *After* is not significant in columns (1) through (6). Indeed, I find that the coefficient on *Dereg State x After* is not statistically significant in any regression. The coefficient is 0.038 (t-stat = 0.170) in column (1), 0.659 (t-stat = 0.212) in column (3), and -0.010 (t-stat = -0.026) in column (5).

I also examine whether smaller public targets are impacted more by IBBEA than larger public targets. I interact *Dereg State* x *After* with *Small*, where *Small* is set to one if the public target has sales below the median ($192 million) and zero otherwise. In Appendix B: Supplemental Results, I show that the coefficient on *Dereg State* x *After x Small* is not statistically significant in regressions where the dependent variable is *LN Deal Value to Sales, Acquirer CAR,* or *Acquirer Wealth to Deal Value.* These non-results using public targets suggest

[15] It is not appropriate to construct the variable *LN Private to Public Value* for this sample of takeovers.

that the effect of deregulation of interstate bank branching is insignificant for firms which are not dependent on local banks.

[Insert Table 10: Public Firm Sales, Falsification Test]

6. Robustness tests

This section discusses robustness tests of the empirical results. First, I consider the influence of outliers in the data. I note that I use the natural logarithm transformations *LN Deal Value to Sales* and *LN Private to Public Value* in empirical tests to mitigate the influence of outliers. To further examine the influence of outliers on my empirical results, I winsorize the data at the 10^{th} and 90^{th} percentiles. I find that the average of *Deal Value to Sales* falls from 15.49 when winsorization is at the 1^{st} and 99^{th} percentiles to 3.20 when winsorization is at the 10^{th} and 90^{th} percentiles. The average of *LN Deal Value to Sales* falls from 0.76 to 0.65 while the average of *LN Private to Public Value* falls from 0.07 to 0.001.

I replicate Tables 5 through 9 on the sample of sellouts where winsorization is done at the 10^{th} and 90^{th} percentiles and present the coefficients on the main variable of interest in Table 11. Panels A, B, C, and D show the coefficient on *Dereg State x After* from replications of Tables 5, 6, 7, and 8, respectively. Panel E shows the coefficient on *Dereg State x After x Small* from replications of Table 9, Panel A, columns (1) through (4). Panel F shows the coefficient on *Dereg State x After x Small* from replications of Table 9, Panel A, columns (5) through (8). I do not present replication results of Table 9, Panel B because the results in the original panel (and in replications of that panel) are not statistically significant. The results in Table 11, Panels A through F are very similar to the original results in Tables 5 through 9. Thus, my empirical results do not appear to be driven by large outliers in the data.

[Insert Table 11: Robustness Test]

Second, I examine whether or not my results are robust to excluding private targets headquartered in California. This robustness test addresses two concerns. First, private targets headquartered in California represent 17.06% of the sample (95 deals), much more than any other state. Therefore, private targets in this state could have a large influence on my tests. Second, California is home to many "internet firms" during the latter part of the sample period. Such firms can receive systematically high valuations and drive my results. I exclude 95 private targets headquartered in California and replicate the empirical results in Tables 5 through 9. Table 12 summarizes the findings and is structured the same as Table 11. As show in Table 12, the empirical results in Panels A, B, C, D, and F are similar to the original results. However, Panel E shows that the statistical significance of the coefficient on *Dereg State x After x Small* is noticeably reduced in the first two regressions. In columns (1) and (2) the coefficient is not statistically significant while in columns (3) and (4), it is significant at the 10% and 1% levels, respectively. Overall, the main results in the paper are robust to excluding private targets headquartered in California.

[Insert Table 12: Robustness Test]

Third, factors that influence a state to deregulate or not may also be correlated with private firm valuations. For example, firms with large growth opportunities may lobby the state to deregulate interstate bank branching. In that case, IBBEA may not be a truly exogenous event. I conduct a robustness test which eliminates deals in states that are very likely to deregulate or not (i.e., the test retains deals in states where the choice to deregulate is less predictable). Motivated by Kroszner and Strahan's (1999) analysis of the factors that influence deregulation, I estimate the likelihood that each state deregulates based on the following

independent variables: small bank asset share of bank assets in the state, small firm share of firms in the state, whether or not banks can sell insurance in the state, relative size of insurance and banking sectors in states where banks can (cannot) sell insurance, state income growth, and a dummy if the governor of the state is a Democrat.[16] Then, I eliminate 112 deals (20.1% of the sample) where private targets are headquartered in states that had a high likelihood of making the choice that they eventually made. These deals are headquartered in the two deregulating states with the highest likelihood of deregulating (Arizona and California) and the two non-deregulating states with the lowest likelihood of deregulating (Arkansas and Iowa). Table 13 summarizes the findings and is structured the same as Table 11. Table 13, Panels A through F, show that the results are qualitatively similar to the original results in Tables 5 through 9. In Appendix B: Supplemental Results, I exclude 125 deals in eight states that are likely to deregulate or not and 151 deals in twelve states that are likely to deregulate or not and continue to find similar results. Thus, factors that influence the state's choice to deregulate do not appear to drive my results.

[Insert Table 13: Robustness Test]

Fourth, I calculate *LN Private to Public Value* using different matching criteria. Similar to the results presented above, I match private targets to public targets where the takeover involved a public acquirer who purchases 100% of the public target's equity and has no toehold in the target. Now, I alter the matching criteria in four ways: 1) allow failed takeovers as long as the public acquirer sought 100% of the equity, 2) allow failed takeovers and include deal values within +/-50% of the private target's deal value, 3) choose public targets where the deal is announced within 3 years of the target's home state's response to IBBEA, and 4) match on Fama French 48 industry codes rather than two-digit SIC industry codes. In Appendix B: Supplemental

[16] I thank Phil Strahan for providing data on the year that each state allowed banks to sell insurance products.

Results, I show that my empirical results are similar using each of these four different matching criteria.

Fifth, I construct *Acquirer CAR* and *Acquirer Wealth to Deal Value* based on announcement returns measured over the (-2, +2) day window around the deal announcement. The results are shown in Appendix B: Supplemental Results and are largely similar to those reported in Tables 7 and 8. For example, for sellouts announced two years before to two years after the state's response to IBBEA, with *Acquirer CAR* measured over the (-2, +2) day window, the coefficient on *Dereg State x After* is -3.792 (significant at the 5% level) in the specification without state fixed effects and is -5.134 (significant at the 1% level) in the specification with state fixed effects.

Finally, I examine an alternative definition of deregulation that groups deregulating states into finer categories based on the number of restrictions that were relaxed. The variable *Dereg State* is decomposed into four dummy variables according to whether the state relaxed one, two, three, or four restrictions on interstate bank branching. The results are presented in Appendix B: Supplemental Results. Under this definition of deregulation, I obtain similar conclusions as under the original definition of deregulation. Deregulation results in higher valuations for private targets, higher valuations for private targets benchmarked to public targets, and acquirer returns are lower.

7. Conclusion

This paper provides evidence that valuations of private targets increase when financial constraints are relaxed. I use exogenous variation to financial constraints caused by interstate bank branching deregulation to execute empirical tests. The difference-in-differences approach

demonstrates that relaxing financial constraints leads to a statistically significant increase of 7.3% in private firm valuation multiples.

When financial constraints are eased, private firms also sell for a higher valuation multiple benchmarked to public target valuation multiples, which suggests that they can substitute bank credit in place of financing from an acquirer. The impact on private target valuations and valuations benchmarked to public target valuations is greater for small private targets than large private targets, suggesting that firms with greater information asymmetries benefit more from relaxed financial constraints.

Acquirer abnormal stock returns and acquirer abnormal wealth gains scaled by the price of the target are negatively impacted by relaxed financial constraints on private targets. Acquirer returns are not incrementally lower when the private target is small. It is important to note that acquirer returns are still positive and economically significant, on average, for deals announced in a deregulated environment. Thus, sellouts are still beneficial to acquirers, even after private targets have relaxed financial constraints.

I find no effect of state-level banking deregulation on a sample of takeovers of public targets. This evidence is consistent with state-level banking deregulation having the greatest impact on firms that depend on local banks for financing. The evidence overall suggests that deregulation of the banking industry improves access to finance and allows private firms to substitute bank credit for some of the financing benefits provided by acquirers.

Appendix A: Variable Definitions

Variable Name	Variable Description
LN Deal Value to Sales	Natural logarithm of *Deal Value to Sales,* which is SDC deal value divided by the target's annual sales.
LN Private to Public Value	Natural logarithm of *Private to Public Value*, which is the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. Public targets are matched on two-digit SIC code, deal value within +/- 20%, and date of announcement within +/- 24 months.
Acquirer CAR	Cumulative average abnormal return of the acquirer over the (-5, +5) window.
Acquirer Wealth to Deal Value	Acquirer Wealth Gain divided by SDC deal value. Wealth gain is the cumulative average abnormal return multiplied by the market value of equity 15 days before the announcement of the deal.
Dereg State	Dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws. Deregulation is defined by relaxing at least one restriction to interstate bank branching.
After	Dummy variable that equals 1 if the deal was announced after the target's state initially responded to IBBEA. All states represented in the sample responded to IBBEA between 1995 and 1997, even states that opted out or otherwise maintained restrictions to interstate bank branching.
Dereg State x After	Dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout.
LN Acq Mkt Value	Natural log of the acquirer's market value of equity 15 days before the announcement of the deal.
Relative Size	SDC deal value divided by the acquirer's market value of equity 15 days before the announcement of the deal.
State Income Growth	Annual percentage change in real per capita personal income for the state in the year before the deal was announced. Data are from the Bureau of Economic Analysis.
All Cash Deal	Dummy variable that equals 1 if the consideration paid was cash only. Due to inconsistencies in SDC data, cash only means that the value of cash consideration is at least 99% of the deal value and the value of common stock consideration is 0.
High Tech Target	Dummy variable that equals 1 if the target's four-digit SIC code is one of the high tech industry codes identified by Loughran and Ritter (2004).
Same State	Dummy variable that equals 1 if the target and acquirer are headquartered in the same state and zero otherwise.
Same Industry	Dummy variable that equals 1 if the target and acquirer share the same four-digit SIC code and zero otherwise.

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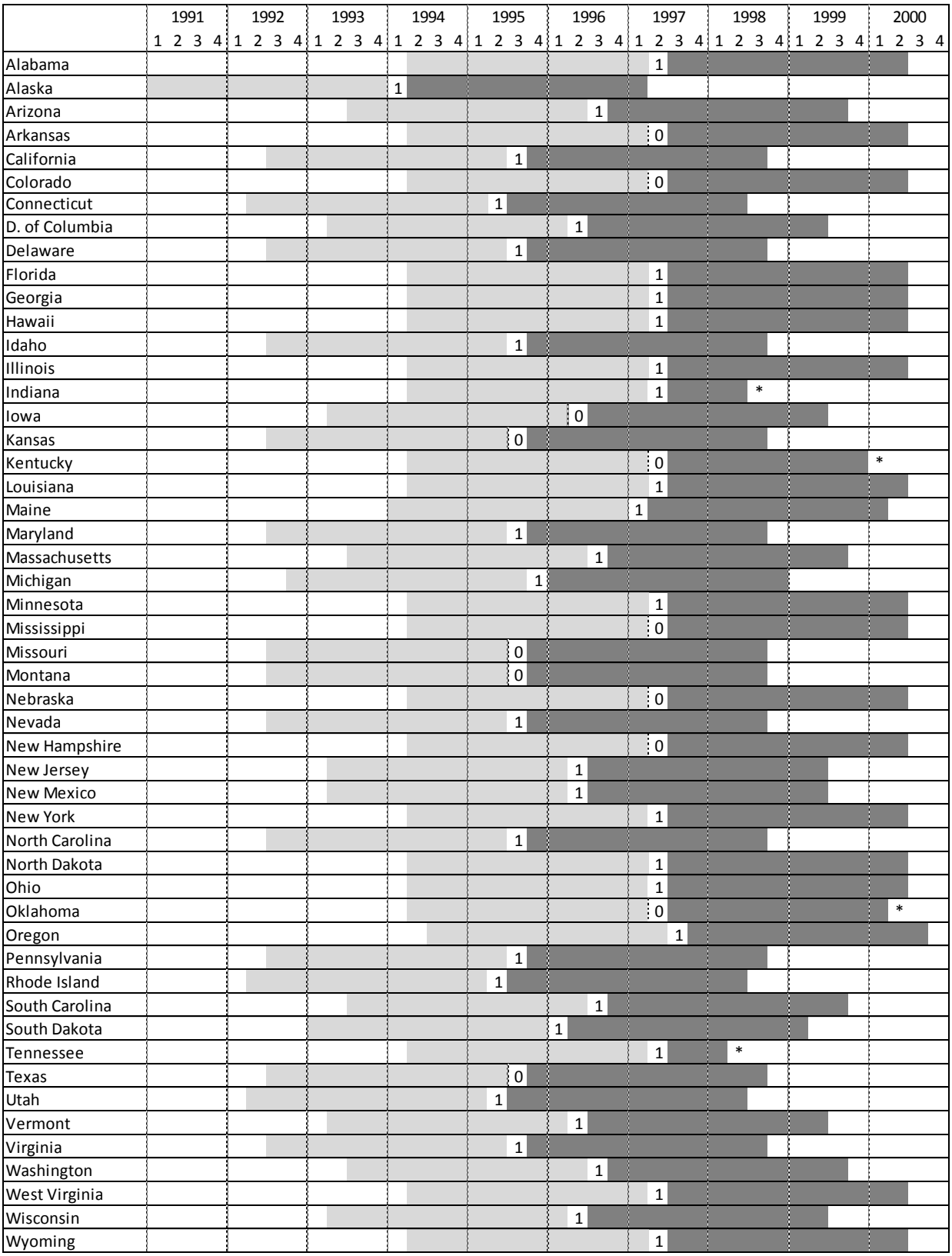

Figure 1: Sample by State and Quarter. The figure shows the sample selection. Light (dark) shaded areas are before (after) the state's response to IBBEA. Zero means that the state did not deregulate and one means that the state did deregulate. An asterisk (*) indicates that the state changed interstate bank branching laws again, and deals after this time are excluded.

Table 1: State level response to IBBEA

This table shows the date of each state's initial response to IBBEA as well as the type of restrictions imposed. There are four types of restrictions: (1) a minimum age of in-state banks targeted for an acquisition, (2) restrictions on the ability of out-of-state banks to open a new branch (restrictions on de novo interstate branching), (3) restrictions on the ability of out-of-state banks to acquire a single in-state bank branch, and (4) a statewide cap on deposits below 30% of the total deposits in the state. If at least one restriction was relaxed, then the state is considered to be a deregulating state. The data are from Rice and Strahan (2010) and Johnson and Rice (2008).

State	Date of Initial Response to IBBEA	Restrict Target Age	Restrict De Novo	Restrict Single Branch Acquisitions	Restrict With Deposit Cap	Deregulating State
Alabama	5/31/1997	Yes	Yes	Yes	No	Yes
Alaska	1/1/1994	Yes	Yes	No	No	Yes
Arizona	9/1/1996	Yes	Yes	Yes	No	Yes
Arkansas	6/1/1997	Yes	Yes	Yes	Yes	No
California	9/28/1995	Yes	Yes	Yes	No	Yes
Colorado	6/1/1997	Yes	Yes	Yes	Yes	No
Connecticut	6/27/1995	Yes	No	No	No	Yes
Delaware	9/29/1995	Yes	Yes	Yes	No	Yes
D. of Columbia	6/13/1996	No	No	No	No	Yes
Florida	6/1/1997	Yes	Yes	Yes	No	Yes
Georgia	6/1/1997	Yes	Yes	Yes	No	Yes
Hawaii	6/1/1997	Yes	Yes	Yes	No	Yes
Idaho	9/29/1995	Yes	Yes	Yes	No	Yes
Illinois	6/1/1997	Yes	Yes	Yes	No	Yes
Indiana	6/1/1997	No	No	No	No	Yes
Iowa	4/4/1996	Yes	Yes	Yes	Yes	No
Kansas	9/29/1995	Yes	Yes	Yes	Yes	No
Kentucky	6/1/1997	Yes	Yes	Yes	Yes	No
Louisiana	6/1/1997	Yes	Yes	Yes	No	Yes
Maine	1/1/1997	No	No	No	No	Yes
Maryland	9/29/1995	No	No	No	No	Yes
Massachusetts	8/2/1996	Yes	No	No	No	Yes
Michigan	11/29/1995	No	No	No	No	Yes
Minnesota	6/1/1997	Yes	Yes	Yes	No	Yes
Mississippi	6/1/1997	Yes	Yes	Yes	Yes	No
Missouri	9/29/1995	Yes	Yes	Yes	Yes	No
Montana	9/29/1995	Yes	Yes	Yes	Yes	No
Nebraska	5/31/1997	Yes	Yes	Yes	Yes	No
Nevada	9/29/1995	Yes	Yes	Yes	No	Yes
New Hampshire	6/1/1997	Yes	Yes	Yes	Yes	No
New Jersey	4/17/1996	No	Yes	No	No	Yes
New Mexico	6/1/1996	Yes	Yes	Yes	No	Yes
New York	6/1/1997	Yes	Yes	No	No	Yes
North Carolina	7/1/1995	No	No	No	No	Yes
North Dakota	5/31/1997	No	Yes	Yes	Yes	Yes
Ohio	5/21/1997	No	No	No	No	Yes
Oklahoma	5/31/1997	Yes	Yes	Yes	Yes	No
Oregon	7/1/1997	Yes	Yes	Yes	No	Yes
Pennsylvania	7/6/1995	No	No	No	No	Yes
Rhode Island	6/20/1995	No	No	No	No	Yes
South Carolina	7/1/1996	Yes	Yes	Yes	No	Yes
South Dakota	3/9/1996	Yes	Yes	Yes	No	Yes
Tennessee	6/1/1997	Yes	Yes	Yes	No	Yes
Texas	8/28/1995	Yes	Yes	Yes	Yes	No
Utah	6/1/1995	Yes	Yes	No	No	Yes
Vermont	5/30/1996	Yes	Yes	No	No	Yes
Virginia	9/29/1995	No	No	No	No	Yes
Washington	6/6/1996	Yes	Yes	Yes	No	Yes
West Virginia	5/31/1997	No	No	No	Yes	Yes
Wisconsin	5/1/1996	Yes	Yes	Yes	No	Yes
Wyoming	5/31/1997	Yes	Yes	Yes	No	Yes

Table 2: Year and State Distribution

This table shows the distribution of sellouts by year and by the private target's home state. The sample is 557 acquisitions of private targets by public acquirers. The deal must be announced three years before to three years after the private firm's home state responded to IBBEA. Targets that are financial or utility firms are excluded.

Panel A: Distribution by Year

Year Announced	Frequency	Percent
1992	9	1.62
1993	19	3.41
1994	54	9.69
1995	55	9.87
1996	103	18.49
1997	125	22.44
1998	120	21.54
1999	53	9.52
2000	19	3.41
	557	100.0

Panel B: Distribution by State

Private Target State	Frequency	Percent	Private Target State	Frequency	Percent
California	95	17.06	Kentucky	5	0.90
Texas	43	7.72	Oklahoma	5	0.90
Illinois	40	7.18	Tennessee	4	0.72
New York	36	6.46	South Carolina	3	0.54
Georgia	30	5.39	Arkansas	2	0.36
Massachusetts	28	5.03	Iowa	2	0.36
Michigan	27	4.85	Kansas	2	0.36
Florida	26	4.67	Maine	2	0.36
Pennsylvania	19	3.41	New Hampshire	2	0.36
North Carolina	16	2.87	D. of Columbia	1	0.18
New Jersey	15	2.69	Idaho	1	0.18
Ohio	15	2.69	Mississippi	1	0.18
Arizona	13	2.33	Montana	1	0.18
Colorado	13	2.33	Nebraska	1	0.18
Minnesota	12	2.15	Nevada	1	0.18
Virginia	12	2.15	Rhode Island	1	0.18
Wisconsin	12	2.15	Vermont	1	0.18
Missouri	10	1.80	Alaska	0	0.00
Indiana	9	1.62	Delaware	0	0.00
Louisiana	9	1.62	Hawaii	0	0.00
Washington	9	1.62	New Mexico	0	0.00
Connecticut	8	1.44	North Dakota	0	0.00
Maryland	8	1.44	South Dakota	0	0.00
Oregon	6	1.08	West Virginia	0	0.00
Utah	6	1.08	Wyoming	0	0.00
Alabama	5	0.90	Total	557	100.00

Table 3: Summary Statistics

This table shows summary statistics for a sample of 557 acquisitions of private targets by public acquirers. All continuous variables are winsorized at the 1% and 99% levels. All dollar values are in millions of 2000 dollars. *Deal Value to Sales* is SDC deal value divided by the target's annual sales. *Private to Public Value* is the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. *Acquirer Market Value* is the acquirer's market value of equity 15 days before the announcement of the deal. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Relative Size* is SDC deal value divided by the acquirer's market value of equity. *State Income Growth* is the annual percentage change in real per capita personal income for the state in the year before the deal was announced. *High Tech Target* is a dummy variable that equals 1 if the target's four-digit SIC code is one of the high tech industry codes identified by Loughran and Ritter (2004). *All Cash* is a dummy variable that equals 1 if the consideration paid was cash only. *Same State* is a dummy variable that equals 1 if the target and acquirer are headquartered in the same state and zero otherwise. *Same Industry* is a dummy variable that equals 1 if the target and acquirer share the same four-digit SIC code and zero otherwise. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated it's bank branching laws or will eventually deregulate its bank branching laws. Deregulation is defined by relaxing at least one restriction to interstate bank branching. *After* is a dummy variable that equals 1 if the deal was announced after the target's state initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 for sellouts announced in deregulating states after deregulation.

Variable Description	N	Mean	Std Dev	Min	10th	25th	Median	75th	90th	Max
Deal Value ($mil)	557	202.17	268.90	50.81	59.23	73.21	114.04	196.50	403.13	1,685.30
Target Sales ($mil)	557	151.19	229.80	0.40	9.11	32.33	73.16	156.70	376.44	1,401.11
Deal Value to Sales	557	15.49	67.59	0.16	0.47	0.83	1.68	4.08	12.68	492.25
LN Deal Value to Sales	557	0.76	1.44	-1.89	-0.74	-0.20	0.51	1.38	2.49	6.20
Deal Value to Sales, Matched Public Targets	431	4.37	6.67	0.29	0.65	1.26	2.46	4.69	8.18	49.48
Private to Public Value	431	3.80	10.37	0.07	0.20	0.42	0.90	2.34	5.99	72.95
LN Private to Public Value	431	0.07	1.42	-2.69	-1.63	-0.88	-0.10	0.85	1.79	4.29
Acquirer Market Value ($mil)	557	1,982.55	3,455.66	27.56	143.97	340.66	704.37	2,035.40	5,316.53	21,606.43
Acquirer CAR (-5,+5)	557	3.51	12.45	-26.11	-10.51	-3.97	2.17	9.68	18.35	52.49
Acquirer Wealth Gain ($mil)	557	2.62	262.73	-1,524.52	-135.89	-25.85	10.64	58.63	199.95	777.20
Acquirer Wealth to Deal Value	557	0.07	1.42	-7.53	-0.92	-0.21	0.08	0.40	1.36	4.77
Relative Size	557	0.34	0.47	0.01	0.03	0.08	0.18	0.39	0.89	2.52
State Income Growth	557	0.02	0.02	-0.02	0.00	0.01	0.02	0.03	0.04	0.06
High Tech Target	557	0.29								
AllCash	557	0.29								
Same State	557	0.18								
Same Industry	557	0.34								
Dereg State	557	0.84								
After	557	0.67								
Dereg State x After	557	0.56								

Table 4: Univariate comparisons

This table shows univariate tests for a sample of 557 acquisitions of private targets by public acquirers. The sample is divided into four groups based on whether the sellout was in a state that deregulates or not and whether the sellout occurred before or after each state's initial response to IBBEA. Differences in means are tested with a t-test and differences in median are tested by a Wilcoxon two sample test. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *LN Private to Public Value* is the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value.

Univariate Tests: Deregulating States vs. Non-Deregulating States, Before and After Response to IBBEA

	Before Response to IBBEA			*After Response to IBBEA*							
Sellouts in Deregulating States	N	Mean	Median	N	Mean	Median	Diff in Mean	p-value		Diff in Median	p-value	
LN Deal Value to Sales	159	0.40	0.40	311	0.94	0.54	0.54	0.00	***	0.14	0.00	***
LN of Relative Value	114	-0.06	-0.09	251	0.16	-0.04	0.22	0.15		0.05	0.44	
Acquirer CAR (-5,+5)	159	5.00	3.59	311	2.93	1.79	-2.07	0.07	*	-1.80	0.04	*
Acquirer Wealth to Deal Value	159	0.31	0.15	311	-0.02	0.07	-0.33	0.01	**	-0.08	0.02	**

	Before Response to IBBEA			*After Response to IBBEA*							
Sellouts in Non-Deregulating States	N	Mean	Median	N	Mean	Median	Diff in Mean	p-value		Diff in Median	p-value	
LN Deal Value to Sales	23	0.86	0.84	64	0.72	0.45	-0.14	0.62		-0.39	0.37	
LN of Relative Value	17	0.01	-0.03	49	-0.06	-0.25	-0.07	0.86		-0.22	0.61	
Acquirer CAR (-5,+5)	23	3.07	0.30	64	2.72	1.15	-0.35	0.90		0.85	0.93	
Acquirer Wealth to Deal Value	23	-0.06	0.01	64	-0.02	0.03	0.04	0.81		0.02	0.75	

	Diff in Diff	p-value	
LN Deal Value to Sales	0.68	0.03	**
LN of Relative Value	0.29	0.49	
Acquirer CAR (-5,+5)	-1.72	0.56	
Acquirer Wealth to Deal Value	-0.37	0.07	*

Table 5: Private Target Deal Value-to-Sales

OLS regression of *LN Deal Value to Sales* for 557 acquisitions of private targets by public acquirers. *LN Deal Value to Sales* is the natural log of SDC deal value divided by the target's annual sales. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. Variables are defined in Appendix A. T-statistics (in parentheses) are based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Dependent variable	*LN Deal Value to Sales*					
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 1 Year	+/- 1 Year
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.597***	0.544**	0.783**	0.757*	0.540*	0.913*
	(2.936)	(2.032)	(2.690)	(2.013)	(1.746)	(1.934)
Dereg State	-0.488*		-0.533**		-0.363	
	(-1.884)		(-2.133)		(-1.091)	
After	-0.232	-0.425	-0.340	-0.559	-0.276	-0.416
	(-0.963)	(-1.341)	(-1.075)	(-1.277)	(-0.838)	(-0.820)
Relative Size	0.376**	0.336**	0.439**	0.400*	0.478	0.583*
	(2.489)	(2.382)	(2.229)	(1.916)	(1.633)	(1.717)
All Cash	-0.289**	-0.302**	-0.186	-0.279*	-0.064	-0.153
	(-2.573)	(-2.448)	(-1.391)	(-1.793)	(-0.260)	(-0.491)
LN Acq Mkt Value	0.288***	0.292***	0.244***	0.247***	0.186***	0.192***
	(5.378)	(4.952)	(5.738)	(5.706)	(4.126)	(3.291)
High Tech Target	0.545**	0.431*	0.500*	0.332	0.640	0.582
	(2.424)	(1.811)	(1.799)	(1.003)	(1.535)	(0.909)
Same State	0.275	0.257	0.138	0.073	0.191	-0.006
	(1.564)	(1.566)	(0.704)	(0.380)	(1.059)	(-0.020)
Same Industry	0.092	0.103	-0.043	-0.051	-0.136	-0.250
	(0.644)	(0.713)	(-0.358)	(-0.372)	(-1.048)	(-1.083)
State Income Growth	7.605	16.060***	5.580	10.390*	7.844	1.963
	(1.671)	(3.168)	(1.459)	(1.851)	(0.987)	(0.137)
Constant	-1.368***	-0.779	-3.656***	-2.584***	-0.717	-0.779
	(-2.958)	(-1.679)	(-5.013)	(-3.354)	(-1.096)	(-1.032)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203
Number of States	43	43	41	41	35	35
R-squared	0.47	0.53	0.46	0.53	0.47	0.59

Table 6: Private Target Value Relative to Public Targets

OLS regression of *LN Private to Public Value* for 557 acquisitions of private targets by public acquirers. *LN Private to Public Value* is the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. This variable is missing for some deals where matches could not be found. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. Variables are defined in Appendix A. T-statistics (in parentheses) are based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Dependent variable	*LN Private to Public Value*					
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 1 Year	+/- 1 Year
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.712***	0.834***	0.857***	1.139**	0.811**	1.799***
	(2.774)	(2.808)	(3.096)	(2.621)	(2.726)	(4.289)
Dereg State	-0.542*		-0.485*		-0.611	
	(-1.717)		(-1.766)		(-1.514)	
After	-0.421	-0.743**	-0.529*	-1.034*	-0.431	-1.411***
	(-1.572)	(-2.251)	(-1.785)	(-2.007)	(-1.267)	(-3.083)
Relative Size	0.267	0.211	0.393	0.282	0.424	0.582
	(1.058)	(0.858)	(1.287)	(0.977)	(1.082)	(1.358)
All Cash	-0.443***	-0.470***	-0.346*	-0.468**	-0.145	-0.547
	(-2.879)	(-2.911)	(-1.780)	(-2.243)	(-0.377)	(-1.130)
LN Acq Mkt Value	0.136*	0.135	0.125	0.127	0.047	0.050
	(1.785)	(1.668)	(1.578)	(1.626)	(0.511)	(0.424)
High Tech Target	0.480**	0.367	0.420	0.232	0.716*	0.298
	(2.227)	(1.479)	(1.566)	(0.716)	(1.830)	(0.456)
Same State	0.328	0.345*	0.173	0.138	0.196	0.051
	(1.507)	(1.986)	(0.742)	(0.740)	(1.176)	(0.154)
Same Industry	0.095	0.146	0.052	0.095	-0.111	-0.414
	(0.565)	(0.882)	(0.353)	(0.607)	(-0.557)	(-1.383)
State Income Growth	3.120	14.355	0.846	7.563	11.854	-20.213
	(0.462)	(1.573)	(0.138)	(0.770)	(1.005)	(-0.961)
Constant	0.083	-2.558***	-0.447	-1.356	0.836	-0.885
	(0.102)	(-3.458)	(-0.512)	(-1.464)	(0.840)	(-1.001)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	431	431	295	295	156	156
Number of States	40	40	38	38	32	32
R-squared	0.24	0.34	0.23	0.38	0.28	0.50

Table 7: Acquirer CARs

Weighted least squares regression of *Acquirer CAR* for 557 acquisitions of private targets by public acquirers. Weights are the inverse of the standard deviation of the market model residuals. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. Variables are defined in Appendix A. T-statistics (in parentheses) are based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Dependent variable	*Acquirer CAR*					
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 1 Year	+/- 1 Year
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.860	-3.292*	-6.380***	-7.868***	-9.511***	-12.319***
	(-1.460)	(-1.831)	(-3.786)	(-5.322)	(-4.347)	(-6.244)
Dereg State	2.620		4.659***		5.217**	
	(1.641)		(2.874)		(2.556)	
After	-3.926	-3.838	-0.904	0.660	3.008	5.869*
	(-1.643)	(-1.445)	(-0.455)	(0.254)	(1.356)	(1.790)
Relative Size	1.986	2.845*	2.032	3.016	0.473	1.634
	(1.202)	(1.759)	(1.100)	(1.537)	(0.271)	(0.773)
All Cash	-0.780	-0.870	-1.062	-0.943	-0.458	-0.783
	(-0.953)	(-1.005)	(-0.977)	(-0.785)	(-0.261)	(-0.374)
LN Acq Mkt Value	-1.258***	-1.078***	-1.202***	-0.975***	-0.810	-0.452
	(-3.491)	(-2.948)	(-3.055)	(-2.781)	(-1.240)	(-0.533)
High Tech Target	0.912	1.236	-0.080	0.418	-0.524	0.158
	(0.634)	(0.733)	(-0.045)	(0.202)	(-0.183)	(0.043)
Same State	1.408	1.152	1.674	0.893	-0.608	-2.235
	(1.494)	(1.232)	(1.216)	(0.639)	(-0.364)	(-0.842)
Same Industry	0.565	0.579	0.892	1.506	1.320	0.999
	(0.608)	(0.580)	(0.767)	(1.082)	(1.003)	(0.491)
State Income Growth	-25.974	23.105	-37.825	-72.351	-32.601	-51.265
	(-0.775)	(0.494)	(-0.956)	(-1.388)	(-0.538)	(-0.404)
Constant	11.161**	23.847***	10.886*	11.333**	1.495	8.606
	(2.208)	(4.358)	(1.991)	(2.325)	(0.210)	(0.698)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203
Number of States	43	43	41	41	35	35
R-squared	0.22	0.30	0.26	0.38	0.31	0.50

Table 8: Acquirer Wealth Gains per Dollar Paid to Target

Weighted least squares regression of *Acquirer Wealth to Deal Value* for 557 acquisitions of private targets by public acquirers. Weights are the inverse of the standard deviation of the market model residuals. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. Variables are defined in Appendix A. T-statistics (in parentheses) are based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Dependent variable	*Acquirer Wealth to Deal Value*					
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 1 Year	+/- 1 Year
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.498*	-0.276	-0.559*	-0.189	-0.757**	-1.024***
	(-1.836)	(-1.036)	(-1.772)	(-0.780)	(-2.066)	(-3.021)
Dereg State	0.555***		0.632***		0.748**	
	(3.826)		(3.645)		(2.257)	
After	-0.376	-0.462	-0.353	-0.116	0.069	0.789
	(-1.272)	(-1.620)	(-1.089)	(-0.333)	(0.211)	(1.406)
Relative Size	-0.050	-0.064	-0.063	-0.013	0.091	0.106
	(-0.304)	(-0.365)	(-0.381)	(-0.085)	(0.450)	(0.353)
All Cash	0.110	0.149	0.002	0.138	0.166	0.293
	(0.726)	(0.867)	(0.009)	(0.614)	(0.626)	(1.208)
LN Acq Mkt Value	-0.095	-0.080	-0.090	-0.045	-0.077	-0.028
	(-1.037)	(-0.829)	(-0.709)	(-0.382)	(-0.421)	(-0.139)
High Tech Target	0.147	0.241	0.046	0.155	-0.027	0.120
	(0.754)	(0.961)	(0.208)	(0.500)	(-0.083)	(0.290)
Same State	0.132	0.123	0.171	0.069	-0.041	-0.143
	(0.579)	(0.568)	(0.594)	(0.233)	(-0.181)	(-0.550)
Same Industry	0.128	0.142	0.101	0.080	0.332	0.181
	(0.813)	(0.714)	(0.512)	(0.353)	(1.263)	(0.533)
State Income Growth	-8.225*	-10.840	-8.525*	-23.384***	-8.531	-23.936
	(-1.759)	(-1.674)	(-1.960)	(-2.952)	(-1.133)	(-1.391)
Constant	0.042	0.096	1.311*	-0.361	-0.122	0.176
	(0.065)	(0.105)	(1.716)	(-0.513)	(-0.124)	(0.126)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203
Number of States	43	43	41	41	35	35
R-squared	0.21	0.27	0.31	0.42	0.37	0.51

Table 9: Impact of IBBEA by Firm Size

This table presents regressions that examine the incremental impact of firm size on the effect of deregulation. The sample is 557 acquisitions of private firms by public firms. Panel A analyzes *LN Deal Value to Sales* (the natural logarithm of SDC deal value divided by the target's annual sales) and *LN Private to Public Value* (the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets). Panel B analyzes *Acquirer CAR* (the cumulative average abnormal return of the acquirer over the (-5, +5) window) and *Acquirer Wealth to Deal Value* (the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value). *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. *Small* is a dummy variable that equals 1 if the firm has below median annual sales ($73 million). Deal level controls are: *Relative Size, All Cash, LN Acq Mkt Value, High Tech Target, Same State, Same Industry,* and *State Income Growth*. Variables are defined in Appendix A. T-statistics (in parentheses) are based on robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Panel A: LN Deal Value to Sales *and* LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years
Dependent variable	*LN Deal Value to Sales*	*LN Deal Value to Sales*	*LN Deal Value to Sales*	*LN Deal Value to Sales*	*LN Private to Public Value*	*LN Private to Public Value*	*LN Private to Public Value*	*LN Private to Public Value*
Dereg State x After x Small	0.625*	0.564*	0.584**	0.595***	1.090*	0.852**	1.655***	0.742***
	(1.892)	(1.923)	(2.060)	(3.403)	(1.783)	(2.176)	(2.936)	(2.863)
Dereg State x After	-0.136	-0.123	0.130	0.149	-0.425	-0.044	-0.694*	0.399
	(-0.986)	(-0.438)	(0.834)	(0.578)	(-1.136)	(-0.104)	(-1.722)	(0.809)
Dereg State x Small	-0.164		-0.044		-0.463		-0.879*	
	(-0.480)		(-0.178)		(-0.741)		(-1.804)	
Dereg State	-0.124		-0.231**		0.105		0.551*	
	(-1.003)		(-2.088)		(0.240)		(1.772)	
After x Small	-0.249	-0.190	-0.336	-0.354**	-0.347	-0.085	-1.017*	-0.027
	(-1.033)	(-0.662)	(-1.603)	(-2.098)	(-0.572)	(-0.206)	(-1.970)	(-0.078)
After	0.149	0.031	0.016	-0.070	0.140	-0.331	0.524	-0.674
	(1.037)	(0.088)	(0.094)	(-0.207)	(0.368)	(-0.736)	(1.530)	(-1.142)
Small	1.411***	1.209***	1.289***	1.196***	1.594**	1.134***	1.904***	0.994***
	(4.400)	(6.097)	(5.497)	(10.111)	(2.504)	(4.539)	(3.698)	(5.116)
Deal Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	431	431	295	295
Number of States	43	43	41	41	40	40	38	38
R-squared	0.63	0.65	0.64	0.67	0.45	0.52	0.43	0.55

continued…

Table 9, continued

Panel B: Acquirer CAR *and* Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years	+/- 3 Years	+/- 3 Years	+/- 2 Years	+/- 2 Years
Dependent variable	*Acquirer CAR*	*Acquirer CAR*	*Acquirer CAR*	*Acquirer CAR*	*Acquirer Wealth to Deal Value*	*Acquirer Wealth to Deal Value*	*Acquirer Wealth to Deal Value*	*Acquirer Wealth to Deal Value*
Dereg State x After x Small	0.354	1.788	4.787	2.195	0.778	0.277	0.724	0.183
	(0.086)	(0.641)	(1.134)	(0.583)	(1.630)	(0.765)	(1.452)	(0.480)
Dereg State x After	-1.558	-3.262	-7.175***	-8.079***	-0.796**	-0.364	-0.844*	-0.235
	(-0.479)	(-1.376)	(-2.974)	(-3.361)	(-2.291)	(-0.917)	(-1.959)	(-0.705)
Dereg State x Small	2.837		-0.430		-0.396		-0.304	
	(0.886)		(-0.144)		(-1.051)		(-0.886)	
Dereg State	0.245		3.836**		0.736***		0.764***	
	(0.089)		(2.524)		(3.679)		(3.640)	
After x Small	1.729	1.068	-0.009	2.931	-0.644	-0.181	-0.549	0.084
	(0.454)	(0.339)	(-0.002)	(0.755)	(-1.566)	(-0.406)	(-1.202)	(0.182)
After	-6.082	-4.959	-1.958	-0.954	-0.130	-0.417	-0.140	-0.170
	(-1.664)	(-1.500)	(-0.734)	(-0.277)	(-0.352)	(-0.942)	(-0.328)	(-0.390)
Small	-4.477	-2.760	-3.140	-4.217**	0.209	-0.081	0.136	-0.194
	(-1.443)	(-1.527)	(-1.056)	(-2.133)	(0.639)	(-0.253)	(0.393)	(-0.557)
Deal Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	557	557	383	383
Number of States	43	43	41	41	43	43	41	41
R-squared	0.22	0.30	0.27	0.39	0.21	0.27	0.31	0.42

Table 10: Public Targets, Falsification Test

The sample is 722 acquisitions of public firms by public firms. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State* is a dummy variable that equals 1 if the target's state has deregulated its bank branching laws or will eventually deregulate its bank branching laws in its initial response to IBBEA. *After* is a dummy variable that equals 1 if the deal was announced after the target's state had initially responded to IBBEA. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. Variables are defined in Appendix A. T-statistics (in parentheses) are based on robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Window for sellouts	+/- 3 Years	+/- 3 Years	+/- 3 Years	+/- 3 Years	+/- 3 Years	+/- 3 Years
Dependent variable	*LN Deal Value to Sales*	*LN Deal Value to Sales*	*Acquirer CAR*	*Acquirer CAR*	*Acquirer Wealth to Deal Value*	*Acquirer Wealth to Deal Value*
Dereg State x After	0.038	-0.059	0.659	0.278	-0.010	-0.019
	(0.170)	(-0.293)	(0.212)	(0.082)	(-0.026)	(-0.043)
Dereg State	-0.219		0.730		-0.030	
	(-1.078)		(0.314)		(-0.166)	
After	0.048	0.143	-2.544	-2.643	-0.235	-0.220
	(0.219)	(0.698)	(-0.845)	(-0.777)	(-0.654)	(-0.592)
Relative Size	0.145*	0.199**	-0.159	-0.339	0.103	0.135
	(1.769)	(2.173)	(-0.178)	(-0.338)	(1.027)	(1.113)
All Cash	-0.321***	-0.331***	3.145***	3.062***	0.320***	0.213**
	(-3.820)	(-3.499)	(4.858)	(4.139)	(3.385)	(2.504)
LN Acq Mkt Value	0.153***	0.157***	0.028	0.050	0.004	0.007
	(5.560)	(5.275)	(0.108)	(0.186)	(0.087)	(0.134)
High Tech Target	0.129	0.068	-1.996	-1.560	-0.008	0.059
	(0.754)	(0.380)	(-1.626)	(-1.213)	(-0.034)	(0.254)
Same State	-0.014	0.030	0.580	0.127	-0.211*	-0.312**
	(-0.164)	(0.375)	(0.738)	(0.143)	(-1.793)	(-2.420)
Same Industry	0.104	0.108	1.517	1.578	0.116	0.046
	(1.338)	(1.197)	(1.623)	(1.503)	(1.110)	(0.462)
State Income Growth	7.228*	10.982**	-39.722	-47.888	-4.438	1.467
	(1.765)	(2.626)	(-1.651)	(-1.394)	(-1.441)	(0.260)
Constant	-1.089	-2.597***	-16.447***	-17.246***	-0.799	-0.374
	(-1.561)	(-3.533)	(-3.455)	(-2.847)	(-1.536)	(-0.444)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	722	722	722	722	722	722
Number of States	46	46	46	46	46	46
R-squared	0.40	0.45	0.17	0.24	0.11	0.17

Table 11: Robustness: Winsorize the Sample at the 10th and 90th percentiles

Replications of Tables 5 through 9 (in Panels A through F) where the sample is winsorized at the 10th and 90th percentiles to mitigate the influence of outliers. Only the coefficient on the variable of interest in each regression is reported. All specifications and variable definitions are the same as the respective tables above. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. T-statistics (in parentheses) are based on robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

LN Deal Value to Sales	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.428**	0.370*	0.547***	0.524**	0.380*	0.637*
	(2.639)	(1.853)	(2.833)	(2.146)	(1.738)	(1.910)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

LN Private to Public Value	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.586**	0.667***	0.621***	0.813***	0.670**	1.290***
	(2.551)	(2.933)	(2.723)	(3.570)	(2.090)	(4.891)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	431	431	295	295	156	156

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.857	-2.643	-5.678***	-6.976***	-8.502***	-11.416***
	(-1.521)	(-1.401)	(-3.304)	(-5.244)	(-3.600)	(-5.964)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.322**	-0.269	-0.463***	-0.435***	-0.652**	-0.920***
	(-2.260)	(-1.636)	(-3.011)	(-3.207)	(-2.473)	(-3.594)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.677**	0.400*	0.534**	0.516**
	(2.285)	(1.892)	(2.122)	(2.175)
State Fixed Effects	No	Yes	No	Yes
Observations	557	557	383	383

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.116**	0.595*	1.417***	0.463**
	(2.289)	(1.904)	(3.246)	(2.050)
State Fixed Effects	No	Yes	No	Yes
Observations	431	431	295	295

Table 12 : Robustness: Exclude Private Targets Headquartered in California

Replications of Tables 5 through 9 (in Panels A through F) where deals with private targets headquartered in the state of California are excluded from the sample. Only the coefficient on the variable of interest in each regression is reported. All specifications and variable definitions are the same as the respective tables above. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. T-statistics (in parentheses) are based on robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.529**	0.488*	0.739**	0.746**	0.531	0.775*
	(2.525)	(1.764)	(2.097)	(2.477)	(1.572)	(1.927)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	462	462	325	325	172	172

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.536**	0.682**	0.672**	0.975**	0.863*	1.574***
	(2.099)	(2.150)	(2.274)	(2.232)	(2.012)	(3.776)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	349	349	244	244	129	129

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.684	-2.836	-6.156***	-7.876***	-9.280***	-11.786***
	(-1.224)	(-1.512)	(-3.056)	(-4.049)	(-3.669)	(-3.972)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	462	462	325	325	172	172

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.356	-0.104	-0.390	-0.055	-0.577**	-0.852***
	(-1.138)	(-0.327)	(-1.073)	(-0.166)	(-2.148)	(-3.671)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	462	462	325	325	172	172

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.485	0.503	0.540*	0.608***
	(1.259)	(1.482)	(1.821)	(2.875)
State Fixed Effects	No	Yes	No	Yes
Observations	462	462	325	325

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.151*	0.909**	1.915***	0.911***
	(1.864)	(2.081)	(3.346)	(2.941)
State Fixed Effects	No	Yes	No	Yes
Observations	349	349	244	244

Table 13: Robustness: Exclude Private Targets Located in States Very Likely to Deregulate or Not

Replications of Tables 5 through 9 (in Panels A through F) where 112 deals in states that were very likely to deregulate or not (AZ, CA, AR, IA) are excluded from the sample. Only the coefficient on the variable of interest in each regression is reported. All specifications and variable definitions are the same as the respective tables above. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated bank branching laws at the time of the sellout. T-statistics (in parentheses) are based on robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

LN Deal Value to Sales	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.618**	0.604*	0.849**	0.752**	0.769**	0.699*
	(2.533)	(1.899)	(2.463)	(2.065)	(2.724)	(1.767)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	445	445	311	311	162	162

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

LN Private to Public Value	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.586**	0.805**	0.639*	0.892*	1.086**	1.433***
	(2.304)	(2.596)	(1.996)	(2.007)	(2.588)	(4.008)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	334	334	232	232	121	121

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-4.260**	-4.541**	-6.967***	-7.892***	-11.021***	-12.693***
	(-2.118)	(-2.571)	(-3.194)	(-4.063)	(-4.190)	(-4.475)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	445	445	311	311	162	162

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.418	-0.112	-0.521	-0.066	-0.766***	-0.939***
	(-1.086)	(-0.316)	(-1.254)	(-0.209)	(-2.850)	(-4.821)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	445	445	311	311	162	162

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.496	0.449	0.667**	0.620**
	(1.229)	(1.222)	(2.245)	(2.674)
State Fixed Effects	No	Yes	No	Yes
Observations	445	445	311	311

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.204*	0.983**	1.970***	1.002***
	(1.901)	(2.066)	(3.411)	(3.220)
State Fixed Effects	No	Yes	No	Yes
Observations	334	334	232	232

Appendix B: Supplemental Results

This appendix presents univariate and multivariate tests. The tables in Appendix B are numbered B-1, B-2, B-3, etc. Tables B-1 and B-2 present univariate tests. This includes an examination of how deal characteristics change from before to after deregulation and an examination of the likelihood of a sellout before and after deregulation.

Tables B-3 through B-8 examine the influence of outliers in the data. This includes transforming dependent variables into a ranked value (e.g., 1 for the lowest value, 2 for the second lowest value, …, 557 for the highest value), trimming the sample by deleting observations below the 1st percentile and above the 99th percentile, trimming the sample by deleting observations below the 5th percentile and above the 95th percentile, trimming the sample by deleting observations below the 10th percentile and above the 90th percentile, estimating regressions to the median, and estimating regressions to the 75th percentile.

Tables B-9 through B-12 examine whether or not the results are driven by deals in states which are likely to deregulate. In the first three tables, I exclude from the sample deals located in states which are likely to deregulate or not. The cutoff for likely to deregulate or not in various tables is: the eight states with the most extreme predicted value of deregulation (4 deregulating states with the highest likelihood of deregulation and 4 non-deregulating states with the lowest likelihood of deregulation), the twelve states with the most extreme predicted value of deregulation (6 deregulating states with the highest likelihood of deregulation and 6 non-deregulating states with the lowest likelihood of deregulation), the top six states with the most extreme predicted value of deregulation (regardless of whether the state deregulates or not). In the fourth table, I create a dummy variable for deals in deregulating states with a high likelihood of deregulation (based on a state-level cutoff which roughly divides deals in deregulating states into two equal groups) and examine whether the effects of IBBEA are greater for deals in these states versus states with a low likelihood of deregulation.

Tables B-13 through B-22 examine other robustness issues. This includes measuring *Relative Size* as target sales divided by acquirer sales, measuring acquirer returns over the (-2,+2), alternative calculations of *LN Private to Public Value,* imposing a *Relative Size* cutoff of 5%, imposing a *Relative Size* cutoff of 10%, sub-samples based on private firm sales, interacting *Dereg State x After* with a dummy variable that indicates changes in out-of-state bank deposits, measuring deregulation at a finer level (relaxing 1, 2, 3 or 4 restrictions), and examining which components of deregulation are the most important (Deposit Cap, De Novo, Single Branch Acquisitions, Age).

Tables B-23 and B-24 examine samples of public targets. First, I show that IBBEA did not have a differential effect on small versus large public targets. Second, I pool public and private targets and show that the effects of IBBEA are significantly greater for private targets than public targets.

Outline of Tables in Appendix B:

Table	Description
Univariate tests	
B-1	Univariate Tests: Differences in firm characteristics
B-2	Likelihood of sellout
Tests to examine the influence of outliers	
B-3	Dependent variable is a ranked version of the underlying variable
B-4	Trimmed sample: delete observations below 1st percentile & above 99th percentile
B-5	Trimmed sample: delete observations below 5th percentile & above 95th percentile
B-6	Trimmed sample: delete observations below 10th percentile & above 90th percentile
B-7	Regression to Median
B-8	Regression to 75th percentile
Analysis of deals in states which are likely to deregulate or not	
B-9	Exclude targets in 8 states; 4 deregulating states and 4 non-deregulating states
B-10	Exclude targets in 12 states; 6 deregulating states and 6 non-deregulating states
B-11	Exclude targets in the 6 states most likely to deregulate
B-12	Divide deregulating states into high and low likelihood of deregulating
Other tests	
B-13	Examine impact of relaxing 1, 2, 3, or 4 restrictions
B-14	Examine impact of relaxing specific components (Deposit Cap, etc.)
B-15	Relative Size measured as target sales divided by acquirer sales
B-16	Acquirer CAR measured of (-2,+2) day window
B-17	Divide sample based on Small vs. Large
B-18	Impose Relative Size cutoff at 5% and 10% level
B-19	Robustness on matching procedure for *LN Private to Public Value*
B-20	Include three-digit SIC code industry dummies
B-21	Interaction with dummy for change in out-of-state bank deposits
B-22	Dependent variable is inverse of the ratio measure
Public targets	
B-23	Public Targets, interact with Small
B-24	Public Targets and Private Targets pooled together, interact with Private

Table B-1: Univariate Analysis

This table shows the mean and median of various variables and the univariate difference-in-differences of the mean. Variables are defined in Appendix A. A t-test is conducted to examine whether the means are significantly different and the p-value is reported.

Sellouts in Deregulating States	*Before Response to IBBEA*			*After Response to IBBEA*					
	N	Mean	Median	N	Mean	Median	Diff in Mean	p-value	
Sales	159	173.23	104.20	311	137.51	62.26	-35.72	0.11	
Relative Size	159	0.33	0.17	311	0.33	0.16	0.00	0.86	
All Cash	159	0.31	0.00	311	0.27	0.00	-0.04	0.28	
LN Acq Mkt Value	159	6.70	6.61	311	6.67	6.59	-0.03	0.82	
High Tech Target	159	0.25	0.00	311	0.35	0.00	0.10	0.02	**
Same State	159	0.16	0.00	311	0.21	0.00	0.05	0.18	
Same Industry	159	0.31	0.00	311	0.31	0.00	0.00	0.95	
State Income Growth	159	0.01	0.01	311	0.02	0.02	0.01	0.00	***

Sellouts in Non-Deregulating States	*Before Response to IBBEA*			*After Response to IBBEA*					
	N	Mean	Median	N	Mean	Median	Diff in Mean	p-value	
Sales	23	211.52	54.29	64	141.25	82.96	-70.27	0.40	
Relative Size	23	0.45	0.26	64	0.42	0.21	-0.03	0.83	
All Cash	23	0.26	0.00	64	0.34	0.00	0.08	0.47	
LN Acq Mkt Value	23	6.40	6.46	64	6.56	6.39	0.16	0.66	
High Tech Target	23	0.13	0.00	64	0.19	0.00	0.06	0.54	
Same State	23	0.13	0.00	64	0.16	0.00	0.03	0.77	
Same Industry	23	0.52	1.00	64	0.44	0.00	-0.08	0.49	
State Income Growth	23	0.01	0.00	64	0.02	0.03	0.01	0.00	***

	Diff in Diff	p-value
Sales	-34.55	0.68
Relative Size	-0.03	0.87
All Cash	0.12	0.27
LN Acq Mkt Value	0.19	0.62
High Tech Target	-0.04	0.59
Same State	-0.02	0.78
Same Industry	-0.08	0.53
State Income Growth	0.00	0.59

Table B-2: Likelihood of a sellout

This tables shows the likelihood of a sellout. For each state and each year, I calculate the number of sellouts per 100,000 firms in the state. The annual number of firms includes those with one to 499 employees, as provided by the Census, and is lagged one year. Then, I average the proportion of sellouts for the three years before deregulation. I also average the proportion of sellouts over the three years following deregulation. I report the average and median number of sellouts to firms in the state before and after deregulation, as well as the change. I also report t-stats for whether the average change is statistically significant and a t-stat for the difference in differences.

		Before		*After*		Change		
	N	Mean	Median	Mean	Median	Mean	Median	t-stat
Dereg State = 0	12	0.59	0.30	1.75	1.38	1.16	1.11	3.36
Dereg State = 1	31	1.43	1.41	2.22	1.94	0.79	0.51	2.56
Difference in Differences						0.33		0.69

Legend for Tables B-3 through B-8

The set-up in Tables B-3 through B-8 is as follows. The sample is 557 sellouts from 1992 to 2000. Only the coefficient on *Dereg State x After* or *Dereg State x After x Small* is reported. The dependent variable is *LN Deal Value to Sales* in Panel A, *LN Private to Public* in Panel B, *Acquirer CAR* in Panel C, and *Acquirer Wealth to Deal Value* in Panel D. If Panels E and F are presented, the dependent variable is *LN Deal Value to Sales* in Panel E and *LN Private to Public* in Panel F. The first two columns of each panel present results from the sample of sellouts announced three years before to three years after the state's response to IBBEA. In the third and fourth (fifth and sixth) columns, sellouts are announced two years (one year) before to two years (one year) after the state's response to IBBEA. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *LN Private to Public Value* is the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated interstate bank branching laws at the time of the sellout. *Small* is a dummy variable that equals 1 if the target has below median sales ($73.16 million) and zero otherwise. Each regression includes the control variables *Relative Size, All Cash, Acquirer Market Value, High Tech Target, Same State, Same Industry*, and *State Income Growth*. Variables are defined in Appendix A. Every regression includes year fixed effects and industry (two-digit SIC code) fixed effects. State fixed effects are included where indicated. T-statistics (in parentheses) are calculated based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table B-3 Ranked Dependent Variables

In this table, the dependent variable is a ranked transformation of *LN Deal Value to Sales, LN Private to Public Value, Acquirer CAR,* or *Acquirer Wealth to Deal Value.* To create *RANK(LN Deal Value to Sales),* the data are sorted by (non-winsorized) *LN Deal Value to Sales. RANK(LN Deal Value to Sales)*=1 for the observation with the smallest value and *RANK(LN Deal Value to Sales)*=557 for the observation with the largest value. The other dependent variables are transformed analogously.

Panel A: Replicate Table 5, dependent variable is RANK(LN Deal Value to Sales)

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	65.164**	50.591	87.283**	75.989*	43.379	85.217
	(2.512)	(1.657)	(2.609)	(1.948)	(1.160)	(1.546)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel B: Replicate Table 6, dependent variable is RANK(LN Private to Public Value)

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	67.194**	69.761**	79.795***	98.652***	67.583*	151.245***
	(2.513)	(2.384)	(3.046)	(3.615)	(1.995)	(4.592)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	431	431	295	295	156	156

Panel C: Replicate Table 7, dependent variable is RANK(Acquirer CAR)

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-55.102	-51.241	-106.323***	-124.354***	-160.251***	-218.083***
	(-1.608)	(-1.622)	(-3.414)	(-4.752)	(-3.963)	(-6.223)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel D: Replicate Table 8, dependent variable is RANK(Acquirer Wealth to Deal Value)

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-74.965**	-66.455	-125.875***	-130.879***	-168.745***	-233.431***
	(-2.093)	(-1.620)	(-3.420)	(-3.426)	(-2.862)	(-5.357)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is RANK(LN Deal Value to Sales)

	(1)	(2)	(3)	(4)
Dereg State x After x Small	81.927*	62.534**	43.979	68.919**
	(1.920)	(2.384)	(1.308)	(2.412)
State Fixed Effects	No	Yes	No	Yes
Observations	557	557	383	383

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is RANK(LN Private to Public Value)

	(5)	(6)	(7)	(8)
Dereg State x After x Small	110.389*	76.128**	140.474**	56.371**
	(1.901)	(2.174)	(2.597)	(2.438)
State Fixed Effects	No	Yes	No	Yes
Observations	431	431	295	295

Table B-4: Trimmed sample: delete observations below 1st percentile and above 99th percentile

In this table, the data are trimmed to exclude observations with extreme values of the dependent variable. The sample is slightly different in each panel, as the data are trimmed based on extreme values of the dependent variable. For example, in Panel A, the sample excludes all observations where *LN Deal Value to Sales* is below the value of the 1st percentile or above the value of the 99th percentile. In Panel B, the sample excludes all observations where *LN Private to Public Value* is below the value of the 1st percentile or above the value of the 99th percentile. The data are trimmed in a similar fashion for the other panels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.567***	0.475*	0.689**	0.674*	0.507	0.886*
	(2.724)	(1.814)	(2.505)	(1.936)	(1.567)	(1.766)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	545	545	376	376	201	201

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.682**	0.817***	0.830***	1.119**	0.803**	1.839***
	(2.655)	(2.757)	(2.948)	(2.611)	(2.550)	(4.793)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	426	426	293	293	154	154

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.784	-3.090*	-6.384***	-7.743***	-9.743***	-12.167***
	(-1.384)	(-1.770)	(-3.615)	(-5.073)	(-4.336)	(-6.136)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	551	551	379	379	200	200

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.509*	-0.263	-0.553*	-0.191	-0.733*	-1.031***
	(-1.832)	(-0.977)	(-1.811)	(-0.807)	(-1.941)	(-3.117)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	551	551	380	380	202	202

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.672**	0.589**	0.553*	0.576***
	(2.138)	(2.374)	(1.959)	(3.040)
State Fixed Effects	No	Yes	No	Yes
Observations	545	545	376	376

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.051*	0.816**	1.618***	0.721***
	(1.758)	(2.042)	(2.871)	(2.778)
State Fixed Effects	No	Yes	No	Yes
Observations	426	426	293	293

Table B-5: Trimmed sample: delete observations below 5th percentile and above 95th percentile

In this table, the data are trimmed to exclude observations with extreme values of the dependent variable. The sample is slightly different in each panel, as the data are trimmed based on extreme values of the dependent variable. For example, in Panel A, the sample excludes all observations where *LN Deal Value to Sales* is below the value of the 5th percentile or above the value of the 95th percentile. In Panel B, the sample excludes all observations where *LN Private to Public Value* is below the value of the 5th percentile or above the value of the 95th percentile. The data are trimmed in a similar fashion for the other panels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.389**	0.334	0.493**	0.505	0.358	0.668
	(2.023)	(1.432)	(2.190)	(1.651)	(1.449)	(1.520)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	501	501	350	350	187	187

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.470*	0.456	0.485*	0.527*	0.341	0.798
	(1.860)	(1.548)	(1.774)	(1.731)	(0.709)	(1.560)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	389	389	272	272	143	143

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-3.049	-2.794	-5.056***	-6.571***	-7.363***	-10.178***
	(-1.544)	(-1.295)	(-2.794)	(-4.487)	(-2.935)	(-5.761)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	503	503	347	347	185	185

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.254	-0.371	-0.425*	-0.652***	-0.571*	-0.985***
	(-1.408)	(-1.599)	(-1.998)	(-3.060)	(-1.734)	(-3.755)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	502	502	348	348	185	185

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.738*	0.495	0.600*	0.650*
	(2.001)	(1.585)	(1.859)	(1.909)
State Fixed Effects	No	Yes	No	Yes
Observations	501	501	350	350

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	0.769**	0.266	0.699	0.264
	(2.158)	(1.183)	(1.590)	(0.959)
State Fixed Effects	No	Yes	No	Yes
Observations	389	389	272	272

Table B-6: Trimmed sample: delete observations below 10th percentile and above 90th percentile

In this table, the data are trimmed to exclude observations with extreme values of the dependent variable. The sample is slightly different in each panel, as the data are trimmed based on extreme values of the dependent variable. For example, in Panel A, the sample excludes all observations where *LN Deal Value to Sales* is below the value of the 10th percentile or above the value of the 90th percentile. In Panel B, the sample excludes all observations where *LN Private to Public Value* is below the value of the 10th percentile or above the value of the 90th percentile. The data are trimmed in a similar fashion for the other panels.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.310**	0.230	0.355**	0.327	0.197	0.224
	(2.666)	(1.584)	(2.394)	(1.587)	(1.051)	(0.845)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	446	446	310	310	166	166

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.525**	0.570**	0.738***	0.871***	0.536	0.858*
	(2.674)	(2.267)	(3.461)	(3.144)	(1.271)	(1.895)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	344	344	243	243	127	127

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-3.068	-3.788***	-4.370**	-6.461***	-6.631*	-11.257***
	(-1.646)	(-2.775)	(-2.067)	(-5.576)	(-1.853)	(-4.312)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	446	446	307	307	162	162

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.078	-0.140	-0.279**	-0.500***	-0.285*	-0.515***
	(-0.678)	(-1.134)	(-2.044)	(-3.949)	(-1.910)	(-2.911)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	447	447	313	313	168	168

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.524*	0.390	0.282	0.492
	(1.961)	(1.374)	(0.964)	(1.323)
State Fixed Effects	No	Yes	No	Yes
Observations	446	446	310	310

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.275***	0.390*	1.407***	0.532**
	(3.890)	(1.975)	(3.362)	(2.083)
State Fixed Effects	No	Yes	No	Yes
Observations	344	344	243	243

Table B-7: Regression to the median

This table presents results from quantile regressions to the median. The estimation is performed with the command "bsqreg" in Stata. Standard errors are calculated by boot-strapping and the number of replications is set to 100.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.403	0.163	0.632	0.415	0.275	0.362
	(1.110)	(0.471)	(1.569)	(0.955)	(0.489)	(0.482)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	1.057*	0.856	1.209**	1.328*	0.808	0.775
	(1.950)	(1.306)	(2.128)	(1.713)	(0.986)	(0.620)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	431	431	295	295	156	156

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-3.639	-0.879	-9.592**	-9.394	-10.631	-9.731
	(-0.787)	(-0.194)	(-1.985)	(-1.601)	(-1.315)	(-1.008)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.042	-0.168	-0.469*	-0.417	-0.491	-0.355
	(0.238)	(-0.786)	(-1.911)	(-1.280)	(-0.931)	(-0.460)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Table B-8: Regression to the 75th percentile

This table presents results from quantile regressions to the 75th percentile. The estimation is performed with the command "bsqreg" in Stata. Standard errors are calculated by boot-strapping and the number of replications is set to 100.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.687	0.592	0.876**	0.557	1.112	0.945
	(1.611)	(1.435)	(1.985)	(0.938)	(1.534)	(1.074)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.703	1.381**	1.279	1.135	0.556	3.918**
	(1.138)	(2.044)	(1.505)	(1.097)	(0.467)	(2.498)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	431	431	295	295	156	156

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.914	-2.384	-10.590**	-9.823	-5.058	-11.347
	(-0.544)	(-0.412)	(-2.093)	(-1.493)	(-0.578)	(-1.011)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.019	-0.224	-0.431	-0.433	-0.426	-0.782
	(-0.076)	(-0.825)	(-1.032)	(-0.957)	(-0.996)	(-1.062)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Legend for Tables B-9 through B-12

The set-up in Tables B-9 through B-12 is as follows. The sample is 557 sellouts from 1992 to 2000. From this sample, I identify deals in states which are likely to deregulate or not. In Tables B-9 through B-11, I exclude deals in states which are likely to deregulate or not using different criteria in each table. In Table B-12, I create indicators for deals in deregulating states which are likely to deregulate or not. Only the coefficient(s) of interest are reported. The dependent variable is *LN Deal Value to Sales* in Panel A, *LN Private to Public* in Panel B, *Acquirer CAR* in Panel C, and *Acquirer Wealth to Deal Value* in Panel D. If Panels E and F are presented, the dependent variable is *LN Deal Value to Sales* in Panel E and *LN Private to Public* in Panel F. In Tables B-9 through B-11, the first two columns of each panel present results from the sample of sellouts announced three years before to three years after the state's response to IBBEA. In the third and fourth (fifth and sixth) columns, sellouts are announced two years (one year) before to two years (one year) after the state's response to IBBEA. In Table B-12, the window of sellouts is indicated. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *LN Private to Public Value* is the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated interstate bank branching laws at the time of the sellout. *Small* is a dummy variable that equals 1 if the target has below median sales ($73.16 million) and zero otherwise. Each regression includes the control variables *Relative Size, All Cash, Acquirer Market Value, High Tech Target, Same State, Same Industry,* and *State Income Growth*. Variables are defined in Appendix A. Every regression includes year fixed effects and industry (two-digit SIC code) fixed effects. State fixed effects are included where indicated. T-statistics (in parentheses) are calculated based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table B-9: Exclude deals in 8 states which are likely to deregulate or not

In this table, the sample excludes deals in eight states which are likely to deregulate or not. The eight states are the four deregulating states with the highest likelihood of deregulation and four non-deregulating states with the lowest likelihood of deregulation.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.549**	0.604*	0.730**	0.739*	0.639**	0.728*
	(2.485)	(1.887)	(2.246)	(2.023)	(2.212)	(1.882)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	432	432	304	304	158	158

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.548**	0.789**	0.551	0.855*	0.908**	1.422***
	(2.053)	(2.529)	(1.612)	(1.899)	(2.322)	(3.853)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	344	344	243	243	127	127

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-2.887*	-4.367**	-5.650***	-7.608***	-10.395***	-12.426***
	(-1.694)	(-2.469)	(-3.217)	(-3.837)	(-4.049)	(-4.510)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	432	432	304	304	158	158

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.190	-0.057	-0.276	0.023	-0.792***	-0.929***
	(-0.528)	(-0.157)	(-0.733)	(0.070)	(-2.804)	(-4.759)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	432	432	304	304	158	158

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.395	0.318	0.741**	0.596**
	(1.049)	(0.825)	(2.434)	(2.463)
State Fixed Effects	No	Yes	No	Yes
Observations	432	432	304	304

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.269*	1.006*	2.214***	1.031***
	(1.951)	(1.802)	(3.796)	(3.069)
State Fixed Effects	No	Yes	No	Yes
Observations	323	323	225	225

Table B-10: Exclude deals in 12 states which are likely to deregulate or not

In this table, the sample excludes deals in twelve states which are likely to deregulate or not. The twelve states are the six deregulating states with the highest likelihood of deregulation and the six non-deregulating states with the lowest likelihood of deregulation.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.517**	0.565*	0.732*	0.726	0.836***	1.092***
	(2.112)	(1.752)	(1.822)	(1.675)	(2.804)	(2.952)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	406	406	283	283	144	144

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.510	0.734**	0.519	0.868	1.119***	1.552***
	(1.684)	(2.195)	(1.299)	(1.677)	(3.341)	(3.966)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	304	304	210	210	108	108

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-1.942	-4.398**	-4.278**	-7.530***	-8.746***	-12.093***
	(-1.192)	(-2.270)	(-2.380)	(-3.486)	(-3.333)	(-3.865)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	406	406	283	283	144	144

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.014	0.031	0.046	0.210	-0.433*	-0.901***
	(0.041)	(0.083)	(0.140)	(0.745)	(-1.776)	(-5.069)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	406	406	283	283	144	144

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.443	0.397	0.790**	0.689**
	(1.230)	(0.967)	(2.449)	(2.745)
State Fixed Effects	No	Yes	No	Yes
Observations	406	406	283	283

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.264*	1.086*	2.229***	1.120***
	(1.889)	(1.971)	(3.655)	(3.105)
State Fixed Effects	No	Yes	No	Yes
Observations	304	304	210	210

Table B-11: Exclude deals in 6 states which are likely to deregulate or not

In this table, the sample excludes deals in six states which are likely to deregulate or not. The six states are identified without regard to whether they are deregulating states or not. It turns out that all six states are deregulating states.

Panel A: Replicate Table 5, dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.582**	0.544*	0.800**	0.776**	0.552	0.770*
	(2.403)	(1.784)	(2.318)	(2.067)	(1.601)	(1.762)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	427	427	297	297	154	154

Panel B: Replicate Table 6, dependent variable is LN Private to Public Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	0.547**	0.694**	0.625*	0.908*	0.883**	1.580***
	(2.092)	(2.095)	(1.940)	(1.941)	(2.133)	(4.241)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	320	320	221	221	115	115

Panel C: Replicate Table 7, dependent variable is Acquirer CAR

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-3.294	-3.423*	-6.373***	-8.082***	-9.544***	-12.896***
	(-1.451)	(-1.789)	(-2.803)	(-4.075)	(-3.653)	(-4.220)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	427	427	297	297	154	154

Panel D: Replicate Table 8, dependent variable is Acquirer Wealth to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.428	-0.145	-0.444	-0.086	-0.576**	-0.934***
	(-1.287)	(-0.424)	(-1.163)	(-0.267)	(-2.078)	(-4.827)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	427	427	297	297	154	154

Panel E: Replicate Table 9, Panel A, Columns (1) through (4), dependent variable is LN Deal Value to Sales

	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.694	0.601	0.777**	0.725***
	(1.675)	(1.653)	(2.616)	(3.082)
State Fixed Effects	No	Yes	No	Yes
Observations	427	427	297	297

Panel F: Replicate Table 9, Panel A, Columns (5) through (8), dependent variable is LN Private to Public Value

	(5)	(6)	(7)	(8)
Dereg State x After x Small	1.334**	1.081**	2.135***	1.123***
	(2.181)	(2.407)	(3.740)	(3.625)
State Fixed Effects	No	Yes	No	Yes
Observations	320	320	221	221

Table B-12: Indicators for deals in states which are highly likely to deregulate or not

In this table, the sample includes all 557 sellouts. Deals located in deregulating states are classified into two groups: *High Likelihood of Deregulating State* and *Low Likelihood of Deregulating State*. The cutoff is at the state level and is such that the number of deals in each group is roughly equivalent. For each regression, I also present the p-value from an F-test where the null hypothesis is that the coefficient on *Low Likelihood of Deregulating State x After* is equal to the coefficient on *High Likelihood of Deregulating State x After*.

Panel A: Dependent variable is LN Deal Value to Sales			
	(1)	(2)	(3)
Window	+/-3	+/-2	+/-1
Low Likelihood of Deregulating State x After	0.566*	0.689*	0.803
	(1.999)	(1.722)	(1.636)
High Likelihood of Deregulating State x After	0.522*	0.823**	1.023*
	(1.748)	(2.112)	(1.958)
State Fixed Effects	Yes	Yes	Yes
Observations	557	383	203
p-value from F-test; null is that coefficients are equal	0.846	0.509	0.526

Panel B: Dependent variable is LN Private to Public Value			
	(1)	(2)	(3)
Window	+/-3	+/-2	+/-1
Low Likelihood of Deregulating State x After	0.932***	1.189**	1.676***
	(3.028)	(2.702)	(3.920)
High Likelihood of Deregulating State x After	0.742*	1.092**	1.921***
	(1.991)	(2.236)	(3.333)
State Fixed Effects	Yes	Yes	Yes
Observations	431	295	156
p-value from F-test; null is that coefficients are equal	0.572	0.773	0.671

Panel C: Dependent variable is Acquirer CAR			
	(1)	(2)	(3)
Window	+/-3	+/-2	+/-1
Low Likelihood of Deregulating State x After	-3.172	-8.357***	-11.740***
	(-1.465)	(-4.862)	(-3.208)
High Likelihood of Deregulating State x After	-3.419*	-7.400***	-12.724***
	(-1.725)	(-4.025)	(-4.489)
State Fixed Effects	Yes	Yes	Yes
Observations	557	383	203
p-value from F-test; null is that coefficients are equal	0.906	0.624	0.850

Panel D:Dependent variable is Acquirer Wealth to Deal Value			
	(1)	(2)	(3)
Window	+/-3	+/-2	+/-1
Low Likelihood of Deregulating State x After	-0.210	-0.124	-0.473
	(-0.672)	(-0.447)	(-1.224)
High Likelihood of Deregulating State x After	-0.347	-0.250	-1.409***
	(-1.313)	(-1.032)	(-3.608)
State Fixed Effects	Yes	Yes	Yes
Observations	557	383	203
p-value from F-test; null is that coefficients are equal	0.580	0.572	0.103

Legend for Tables B-13 through B-22

The set-up in Tables B-13 through B-22 is as follows. The sample is 557 sellouts from 1992 to 2000. Only the coefficients of interest are reported. The dependent variable and window for sellouts are given in the table. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *LN Private to Public Value* is the natural logarithm of the deal value-to-sales ratio of the private target divided by the average deal value-to-sales ratio of matched public targets. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated interstate bank branching laws at the time of the sellout. *Small* is a dummy variable that equals 1 if the target has below median sales ($73.16 million) and zero otherwise. Each regression includes the control variables *Relative Size, All Cash, Acquirer Market Value, High Tech Target, Same State, Same Industry*, and *State Income Growth*. Variables are defined in Appendix A. Every regression includes year fixed effects and industry (two-digit SIC code) fixed effects. State fixed effects are included where indicated. T-statistics (in parentheses) are calculated based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table B-13: Different measurement of deregulation; relax 1, 2, 3, or 4 restrictions

In this table, I define deregulation at a more granular level. I decompose the variable *Dereg* into dummy variables which indicate whether deregulating states relaxed 1, 2, 3, or 4 restrictions when initially responding to IBBEA. Note that the variable *Dereg* is a linear combination of these four indicator variables. Non-deregulating states relaxed 0 restrictions. These indicator variables are then interacted with *After*. In Panel A (B), sellouts are announced three (two) years before to three (two) years after the state responded to IBBEA.

Panel A: +/-3 year window

	(1)	(2)	(3)	(4)
Window	+/-3	+/-3	+/-3	+/-3
Dependent Variable	*LN Deal Value to Sales*	*LN Private to Public Value*	*Acquirer CAR*	*Acquirer Wealth to Deal Value*
Relax 4 Restrictions x After	0.168	-0.039	-2.727	-0.092
	(0.612)	(-0.105)	(-1.169)	(-0.316)
Relax 3 Restrictions x After	0.882**	0.870***	-5.565**	-0.156
	(2.375)	(3.111)	(-2.565)	(-0.317)
Relax 2 Restrictions x After	0.761**	0.796***	-3.249	-0.326
	(2.605)	(2.793)	(-1.415)	(-0.681)
Relax 1 Restriction x After	0.604**	1.236***	-3.252	-0.415
	(2.241)	(4.865)	(-1.528)	(-1.485)
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	557	431	557	557

Panel B: +/-2 year window

	(1)	(2)	(3)	(4)
Window	+/-2	+/-2	+/-2	+/-2
Dependent Variable	*LN Deal Value to Sales*	*LN Private to Public Value*	*Acquirer CAR*	*Acquirer Wealth to Deal Value*
Relax 4 Restrictions x After	0.477	0.401	-8.359***	-0.109
	(1.489)	(0.919)	(-4.862)	(-0.402)
Relax 3 Restrictions x After	0.969**	1.201**	-9.491***	-0.133
	(2.316)	(2.613)	(-3.266)	(-0.361)
Relax 2 Restrictions x After	1.050**	0.563	-4.052	0.318
	(2.608)	(1.175)	(-1.079)	(0.800)
Relax 1 Restriction x After	0.759*	1.432***	-7.613***	-0.326
	(1.986)	(3.580)	(-3.976)	(-1.163)
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	383	295	383	383

Table B-14: Examination of components of deregulation

In this table, I decompose the variable *Dereg* into dummy variables which indicate the specific laws which the state deregulated. All states deregulated by relaxing restrictions on deposit caps (*Deposit Cap Only* = 1). Some states deregulated by relaxing all four restrictions allowed by IBBEA (*Totally Open* = 1). Some states deregulated by relaxing restrictions on deposit caps plus one or two aditional restrictions (*Deposit Cap Plus* = 1). Note that *Dereg* is a linear combination of *Deposit Cap Only*, *Deposit Cap Plus*, and *Totally Open*. I then interact these three indicator variables with *After*.

	(1)	(2)	(3)	(4)
Window	+/-2	+/-2	+/-2	+/-2
Dependent Variable	*LN Deal Value to Sales*	*LN Private to Public Value*	*Acquirer CAR*	*Acquirer Wealth to Deal Value*
Deposit Only x After	0.763*	1.443***	-7.692***	-0.329
	(1.934)	(3.471)	(-4.014)	(-1.174)
Deposit Plus x After	1.010**	0.715*	-4.689	0.388
	(2.637)	(1.789)	(-1.642)	(1.160)
Totally Open x After	0.611*	0.683	-9.002***	-0.189
	(1.709)	(1.369)	(-5.279)	(-0.792)
After	-0.558	-1.061**	0.664	-0.085
	(-1.303)	(-2.047)	(0.250)	(-0.238)
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	383	295	383	383

Table B-15: Measure *Relative Size* as target sales divided by acquirer sales

In this table, I present estimation results from replications of regressions in Tables 5 through 8 where *Relative Size* is defined as the ratio of target sales to acquirer sales. In columns (1) and (2), the window for sellouts is +/-3 years around the response to IBBEA and in columns (3) and (4), the window for sellouts is +/- 2 years around the response to IBBEA.

Panel A: Dependent variable is LN Deal Value to Sales				
	(1)	(2)	(3)	(4)
Dereg State x After	0.490**	0.455*	0.602**	0.634*
	(2.594)	(1.717)	(2.383)	(1.825)
State Fixed Effects	No	Yes	No	Yes
Observations	556	556	382	382

Panel B: Dependent variable is LN Private to Public Value				
	(1)	(2)	(3)	(4)
Dereg State x After	0.602**	0.753**	0.603*	0.954**
	(2.278)	(2.592)	(1.977)	(2.202)
State Fixed Effects	No	Yes	No	Yes
Observations	431	431	295	295

Panel C: Dependent variable is Acquirer CAR				
	(1)	(2)	(3)	(4)
Dereg State x After	-2.541	-2.961	-6.154***	-7.598***
	(-1.271)	(-1.639)	(-3.575)	(-5.208)
State Fixed Effects	No	Yes	No	Yes
Observations	556	556	382	382

Panel D: Dependent variable is Acquirer Wealth to Deal Value				
	(1)	(2)	(3)	(4)
Dereg State x After	-0.479*	-0.264	-0.549*	-0.189
	(-1.817)	(-0.998)	(-1.780)	(-0.774)
State Fixed Effects	No	Yes	No	Yes
Observations	556	556	382	382

Table B-16: Measure Acquirer CAR over (-2,+2) window

In this table, I measure acquirer abnormal returns over the (-2,+2) day window surrounding the announcement of the sellout. The variables *Acquirer CAR* and *Acquirer Wealth to Deal Value* are recalculated using this definition of acquirer returns. Panel A replicates Table 7 and Panel B replicates Table 8 using this alternative measure of acquirer returns. The first two columns of each panel present results from the sample of sellouts announced three years before to three years after the state's response to IBBEA. In the third and fourth (fifth and sixth) columns, sellouts are announced two years (one year) before to two years (one year) after the state's response to IBBEA.

Panel A: Dependent Variable is Acquirer CAR (-2,+2)

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-1.841	-2.021	-3.792**	-5.134***	-3.788*	-6.207***
	(-0.916)	(-1.083)	(-2.204)	(-3.301)	(-1.976)	(-3.760)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Panel B: Dependent Variable is Acquirer Wealth (-2,+2) to Deal Value

	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After	-0.390*	-0.319	-0.522**	-0.381**	-0.451*	-0.746***
	(-2.001)	(-1.505)	(-2.134)	(-2.053)	(-1.879)	(-2.840)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	557	557	383	383	203	203

Table B-17: Sub-Samples of Small vs. Large

In this table, I divide the sample of deals based on *Small*. The sample includes 383 sellouts announced from two years before to two years after the target's state responded to IBBEA.

Panel A: Dependent variable is LN Deal Value to Sales				
	(1)	(2)	(3)	(4)
Sub-sample	Small	Large	Small	Large
Dereg State x After	0.784**	0.147	0.889*	0.178
	(2.735)	(0.754)	(2.016)	(0.681)
State Fixed Effects	No	No	Yes	Yes
Observations	187	196	187	196

Panel B: Dependent variable is LN Private to Public Value				
	(1)	(2)	(3)	(4)
Sub-sample	Small	Large	Small	Large
Dereg State x After	1.214***	-0.616*	1.871***	-0.299
	(3.345)	(-1.703)	(6.027)	(-0.470)
State Fixed Effects	No	No	Yes	Yes
Observations	160	135	160	135

Panel C: Dependent variable is Acquirer CAR				
	(1)	(2)	(3)	(4)
Sub-sample	Small	Large	Small	Large
Dereg State x After	-5.626**	-5.467**	-9.890**	-8.856***
	(-2.128)	(-2.038)	(-2.603)	(-3.898)
State Fixed Effects	No	No	Yes	Yes
Observations	187	196	187	196

Panel D: Dependent variable is Acquirer Wealth to Deal Value				
	(1)	(2)	(3)	(4)
Sub-sample	Small	Large	Small	Large
Dereg State x After	-0.566	-1.141**	-0.474	-0.745
	(-1.075)	(-2.231)	(-0.757)	(-1.471)
State Fixed Effects	No	No	Yes	Yes
Observations	187	196	187	196

Table B-18: *Relative Size* cutoff of 5% or 10%

In this table, I include sellouts which have a *Relative Size* of 5% (columns (1) and (2)) or 10% (columns (3) and (4)). The initial sample includes 383 sellouts announced from two years before to two years after the target's state responded to IBBEA.

Panel A:Dependent variable is LN Deal Value to Sales				
	Relative Size >=5%		Relative Size >=10%	
	(1)	(2)	(3)	(4)
Dereg State x After	0.750***	0.823**	0.538**	0.739**
	(3.542)	(2.296)	(2.712)	(2.464)
State Fixed Effects	No	Yes	No	Yes
Observations	324	324	264	264

Panel B: Dependent variable is LN Private to Public Value				
	Relative Size >=5%		Relative Size >=10%	
	(1)	(2)	(3)	(4)
Dereg State x After	0.754**	1.247**	0.557	1.150**
	(2.600)	(2.373)	(1.590)	(2.666)
State Fixed Effects	No	Yes	No	Yes
Observations	247	247	197	197

Panel C: Dependent variable is Acquirer CAR				
	Relative Size >=5%		Relative Size >=10%	
	(1)	(2)	(3)	(4)
Dereg State x After	-6.934***	-12.391***	-7.295**	-12.711***
	(-2.932)	(-6.566)	(-2.479)	(-5.290)
State Fixed Effects	No	Yes	No	Yes
Observations	324	324	264	264

Panel D: Dependent variable is Acquirer Wealth to Deal Value				
	Relative Size >=5%		Relative Size >=10%	
	(1)	(2)	(3)	(4)
Dereg State x After	-0.660***	-0.861***	-0.453**	-0.741***
	(-3.258)	(-4.469)	(-2.206)	(-5.818)
State Fixed Effects	No	Yes	No	Yes
Observations	324	324	264	264

continued…

Table B-18, continued

Panel E: Dependent variable is LN Deal Value to Sales				
	Relative Size >=5%		Relative Size >=10%	
	(1)	(2)	(3)	(4)
Dereg State x After x Small	0.799***	0.646**	1.844**	0.869**
	(2.771)	(2.594)	(2.584)	(2.082)
State Fixed Effects	No	Yes	No	Yes
Observations	324	324	247	247

Panel F: Dependent variable is LN Private to Public Value				
	Relative Size >=5%		Relative Size >=10%	
	(5)	(6)	(3)	(4)
Dereg State x After x Small	0.963***	0.672**	1.927**	1.064**
	(3.154)	(2.644)	(2.398)	(2.606)
State Fixed Effects	Yes	No	Yes	
Observations	264	264	197	197

Table B-19: Robustness on matching procedure for *LN Private to Public Value*

In this table, I present results for alternative calculations of *LN Private to Public Value*. The window for sellouts is +/-2 years around the response to IBBEA of the private target's home state. In every alternative, the potential public target matches include deals with public acquirers who buy (or seek) 100% of the equity of the public target and have no toehold in the public target. In Panel A, potential public target matches include those in both successful and unsuccessful deals. In Panel B, the difference in deal value between the private target and matched public target is within +/- 50% and both successful and unsuccessful deals are included. In Panel C, only public targets that are announced within 3 years of their home state's response to IBBEA are included as potential matches. In addition, the public target deal announcement must be within 1.5 years of the private target deal announcement. In Panel D, the private and public targets have the same Fama French 48 Industry code.

Panel A: Successful and unsuccessful public target takeovers

	(1)	(2)
Dereg State x After	0.829***	1.055**
	(3.171)	(2.568)
State Fixed Effects	No	Yes
Observations	305	305

Panel B: Successful and unsuccessful public target takeovers, deal value within +/-50%

	(1)	(2)
Dereg State x After	0.905***	0.945**
	(3.023)	(2.208)
State Fixed Effects	No	Yes
Observations	350	350

Panel C: Public targets announced within 3 years of home state response to IBBEA

	(1)	(2)
Dereg State x After	1.084***	1.146***
	(3.336)	(3.026)
State Fixed Effects	No	Yes
Observations	254	254

Panel D: Private target and public target in same Fama French 48 industry

	(1)	(2)
Dereg State x After	0.679**	0.906**
	(2.277)	(2.163)
State Fixed Effects	No	Yes
Observations	321	321

Table B-20: Include three-digit SIC code industry dummies

Regressions include three-digit SIC code industry dummies as well as year fixed effects. There are roughly 140 industry dummies in each regression. The window for sellouts is +/-2 years around the response to IBBEA of the private target's home state.

Panel A:Dependent variable is LN Deal Value to Sales		
	(1)	(2)
Dereg State x After	0.629***	0.773**
	(2.990)	(2.528)
State Fixed Effects	No	Yes
Observations	383	383

Panel B: Dependent variable is LN Private to Public Value		
	(1)	(2)
Dereg State x After	0.746*	1.482***
	(1.700)	(4.015)
State Fixed Effects	No	Yes
Observations	295	295

Panel C: Dependent variable is Acquirer CAR		
	(1)	(2)
Dereg State x After	-6.499**	-7.107**
	(-2.590)	(-2.291)
State Fixed Effects	No	Yes
Observations	383	383

Panel D: Dependent variable is Acquirer Wealth to Deal Value		
	(1)	(2)
Dereg State x After	-0.480	-0.062
	(-1.421)	(-0.202)
State Fixed Effects	No	Yes
Observations	383	383

Table B-21: Interaction with dummy for change in out-of-state bank deposits

Top Quartile Change in Out-of-state Bank Deposits is a dummy which equals 1 for deals where the change in proportion of deposits held by out-of-state-banks in the private target's home state is in the top quartile and zero otherwise. The quartile cutoff is based on deal level data, not state level data. The change is measured from one year before to three years after the state's response to IBBEA. Data is from the FDIC summary of deposits. Each regression includes all seven variables necessary for the triple interaction, although only two are shown. The window for sellouts is +/-3 (2) years around the private target's home state's response to IBBEA in column (1)((2)).

Panel A:Dependent variable is LN Deal Value to Sales		
	(1)	(2)
Dereg State x After x Top Quartile Change in Out-of-state Bank Deposits	1.635***	2.335***
	(3.386)	(5.170)
Dereg State x After	0.495**	0.380*
	(2.506)	(1.911)
State Fixed Effects	No	No
Observations	557	383

Panel B: Dependent variable is LN Private to Public Value		
	(1)	(2)
Dereg State x After x Top Quartile Change in Out-of-state Bank Deposits	1.598***	2.123***
	(2.785)	(3.721)
Dereg State x After	0.769**	0.666**
	(2.482)	(2.072)
State Fixed Effects	No	No
Observations	431	295

Panel C: Dependent variable is Acquirer CAR		
	(1)	(2)
Dereg State x After x Top Quartile Change in Out-of-state Bank Deposits	-15.854***	-10.442**
	(-3.601)	(-2.513)
Dereg State x After	-0.974	-5.621***
	(-0.476)	(-3.646)
State Fixed Effects	No	No
Observations	557	383

Panel D: Dependent variable is Acquirer Wealth to Deal Value		
	(1)	(2)
Dereg State x After x Top Quartile Change in Out-of-state Bank Deposits	-1.001**	-0.976*
	(-2.098)	(-1.880)
Dereg State x After	-0.314	-0.322
	(-1.104)	(-1.177)
State Fixed Effects	No	No
Observations	557	383

Table B-22: Dependent variable is inverse of the ratio measure

Regressions where the dependent variable is calculated as the inverse of the ratio. In Panels A and B, the dependent variable is the natural logarithm of Sales to Deal Value. In Panels C and D, the dependent variable is the matched public target deal value-to-sales ratio divided by the private target's deal-value-to-sales ratio. In columns (1) and (2), the window for sellouts is +/-3 years around the response to IBBEA and in columns (3) and (4), the window for sellouts is +/- 2 years around the response to IBBEA.

Panel A: Dependent variable is LN Sales to Deal Value

	(1)	(2)	(3)	(4)
Dereg State x After	-0.596***	-0.535*	-0.775**	-0.744*
	(-2.819)	(-1.933)	(-2.603)	(-1.944)
State Fixed Effects	No	Yes	No	Yes
Observations	557	557	383	383

Panel B: Dependent variable is LN Sales to Deal Value

	(1)	(2)	(3)	(4)
Dereg State x After x Small	-0.634*	-0.546*	-0.588**	-0.569***
	(-1.915)	(-1.894)	(-2.115)	(-3.307)
State Fixed Effects	No	Yes	No	Yes
Observations	557	557	383	383

Panel C: Dependent variable is LN Public to Private Value

	(1)	(2)	(3)	(4)
Dereg State x After	-0.740***	-0.852***	-0.856***	-1.138**
	(-2.902)	(-2.801)	(-3.092)	(-2.621)
State Fixed Effects	No	Yes	No	Yes
Observations	431	431	295	295

Panel D: Dependent variable is LN Public to Private Value

	(1)	(2)	(3)	(4)
Dereg State x After x Small	-1.135*	-0.875**	-1.654***	-0.736***
	(-1.921)	(-2.336)	(-2.939)	(-2.856)
State Fixed Effects	No	Yes	No	Yes
Observations	431	431	295	295

Legend for Tables B-23 and B-24

The sample is 722 takeovers of public targets by public acquirers and 577 sellouts from 1992 to 2000. Only the coefficients of interest are reported. The dependent variable is *LN Deal Value to Sales* in Panel A, *Acquirer CAR* in Panel B, and *Acquirer Wealth to Deal Value* in Panel C. The first two columns of each panel present results from the sample of sellouts announced three years before to three years after the state's response to IBBEA. In the third and fourth (fifth and sixth) columns, sellouts are announced two years (one year) before to two years (one year) after the state's response to IBBEA. *LN Deal Value to Sales* is the natural logarithm of SDC deal value divided by the target's annual sales. *Acquirer CAR* is the cumulative average abnormal return of the acquirer over the (-5, +5) window. *Acquirer Wealth to Deal Value* is the acquirer's cumulative average abnormal return multiplied by the market value of equity divided by SDC deal value. *Dereg State x After* is a dummy variable that equals 1 if the target's state has deregulated interstate bank branching laws at the time of the sellout. *Small* is a dummy variable that equals 1 if the target has below median sales (calculated separately for public targets and private targets) and zero otherwise. Each regression includes the control variables *Relative Size, All Cash, Acquirer Market Value, High Tech Target, Same State, Same Industry*, and *State Income Growth*. Variables are defined in Appendix A. Every regression includes year fixed effects and industry (two-digit SIC code) fixed effects. State fixed effects are included where indicated. T-statistics (in parentheses) are calculated based on heteroskedasticity-robust standard errors clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels.

Table B-23: Public targets, interact with small

The sample is 722 public-public takeovers announced from 1992 to 2000. The variable of interest is *Dereg State x After x Small*.

Panel A: Dependent variable is LN Deal Value to Sales						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Small	0.392	0.049	0.199	0.089	0.077	-0.170
	(1.549)	(0.271)	(0.630)	(0.474)	(0.131)	(-0.485)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	722	722	508	508	247	247

Panel B:Dependent variable is Acquirer CAR						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Small	4.278	1.978	8.013	1.419	7.346	3.563
	(0.785)	(1.192)	(1.236)	(0.444)	(1.224)	(1.029)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	722	722	508	508	247	247

Panel C: Dependent variable is Acquirer Wealth to Deal Value						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Small	0.511	0.280	0.135	-0.195	0.283	-0.350
	(1.014)	(1.180)	(0.266)	(-0.710)	(0.399)	(-0.662)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	722	722	508	508	247	247

Table B-24: Private and public targets, Interact with dummy for private target
The sample includes 722 public-public takeovers plus 557 sellouts from 1992 to 2000. The variable *Private* is a dummy variable that is one for private targets and zero for public targets.

Panel A: Dependent variable is LN Deal Value to Sales						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Private	0.453**	0.362**	0.894***	0.565***	0.808	0.529*
	(2.041)	(2.388)	(3.363)	(4.332)	(1.477)	(1.999)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	1,279	1,279	891	891	450	450

Panel B:Dependent variable is Acquirer CAR						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Private	-3.033	-1.438	-5.904	-2.828	-11.784**	-6.637**
	(-0.740)	(-0.725)	(-1.438)	(-1.077)	(-2.176)	(-2.191)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	1,279	1,279	891	891	450	450

Panel C: Dependent variable is Acquirer Wealth to Deal Value						
	(1)	(2)	(3)	(4)	(5)	(6)
Dereg State x After x Private	-0.552*	-0.112	-0.766**	-0.105	-1.199**	-0.444
	(-1.781)	(-0.595)	(-2.543)	(-0.511)	(-2.548)	(-0.867)
State Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	1,279	1,279	891	891	450	450

Reverse Mergers as an Exit Mechanism for Private Firm Owners

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Abstract

I examine reverse mergers (RMs) as an exit mechanism for private firm owners and compare RMs to both IPOs and sellouts to a public acquirer. I find evidence that information asymmetry and product market competition influence the choice among the three exit mechanisms. RM firms are significantly different from IPO firms along observable characteristics. Also, I find that RM firm owners have less wealth following the exit than firm owners of matched IPO firms. Together, this evidence suggests that an IPO is not a realistic option for the vast majority of RM firms. In contrast, RM firms are similar to sellout firms along observable characteristics and owners of RM firms receive the same, or greater, wealth as owners of comparable sellout firms. Thus, a sellout appears to be a viable alternative to a RM for many firms. I examine whether or not RMs generate positive synergy. I find that synergy is positive, on average, when synergy is calculated using valuations of private firms that are inferred from industry multiples of private-private takeovers. In contrast, I find that synergy is negative, on average, when synergy is calculated using private firm valuations produced by financial advisors to the public firm board of directors. The evidence leads me to conclude that financial advisors produce inflated valuations of private firms that mechanically drives down the estimate of synergy.

JEL Classification: G34, G32, G30
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1. Introduction

Private firms often gain access to public equity markets when owners exit the firm by selling a portion of their stake to other investors. Two commonly studied exit mechanisms are an initial public offering (IPO) and the sale of the firm to a public acquirer (sellout) (see, e.g., Ritter and Welch, 2002; Brau, Francis, Kohers, 2003). A third exit mechanism that has received less attention in the literature is a reverse merger (RM). In a RM, the private firm merges with a public firm and the private firm owners control the combined publicly-traded firm. The public firm in a RM is often a defunct operating company or a shell company.[1] From 1996 to 2010, over 2,700 private firms executed a RM (Comment, 2010). Although the typical RM firm is small, some well-known firms have used the RM mechanism such as the New York Stock Exchange, Siebert Financial, Jamba Juice, American Apparel, and Summer Infant.

Several studies compare RMs to IPOs (Gleason, Jain, and Rosenthal, 2008; Adjei, Cyree, and Walker, 2008; Floros and Sapp, 2010) while other studies compare sellouts to IPOs (Brau, Francis, and Kohers, 2003; Poulsen and Stegemoller, 2008; and Bayar and Chemmanur, 2011, 2012). In this paper, I examine all three exit mechanisms in a unified framework. I address three research questions: 1) What factors influence the choice of RM, IPO, or sellout?, 2) How much wealth do private firm owners have following a RM compared to what could have been obtained in an IPO or a sellout?, and 3) Do RMs generate positive synergy?

To answer the first research question, I hypothesize that differences across firms regarding information asymmetries between insiders and potential investors, the ability to compete in product markets, and private benefits of control influence the choice of a RM, IPO, or sellout. I first consider the influence of information asymmetries on the choice of exit

[1] Shell companies are firms that have no or nominal operating assets or assets that consist mostly of cash. Appendix A provides institutional details on reverse merger transactions.

mechanism. Both RMs and sellouts are negotiated transactions between two parties in which only one party (the public firm) needs to become informed about the private firm. In contrast, in an IPO, many investors must become informed about the private firm. Chemmanur and Fulghieri (1999) show theoretically that negotiating with a single party (e.g., a RM or sellout) is preferable to selling to dispersed investors (e.g., an IPO) when information asymmetries are more severe. Therefore, I predict that firms characterized by greater information asymmetries are more likely to choose a RM (or sellout) compared to an IPO.

Next, I consider the influence of product market competition and private benefits of control on the choice of exit mechanism. An RM and an IPO result in a stand-alone firm, which allows owners to retain private benefits of control but forces the firm to compete independently in product markets. In contrast, private firm owners in sellouts forfeit private benefits of control but gain support in product market competition as the firm is integrated into the public firm (Bayar and Chemmanur, 2011). Therefore, I predict that firms which are better prepared to compete in product markets are more likely to choose a RM (or an IPO) compared to a sellout. I also predict that firms with greater private benefits of control are more likely to choose a RM (or an IPO) compared to a sellout.

To address the second research question, I hypothesize that the wealth of private firm owners following a RM is greater than, or equal to, the wealth that can be obtained in either an IPO or a sellout. This prediction is motivated by theoretical models that assume that private firm owners choose the exit mechanism that maximizes the value of their wealth (Zingales, 1995; Chemmanur and Fulghieri, 1999; Bayar and Chemmanur, 2011). For example, in Chemmanur and Fulghieri (1999), the private firm owner maximizes the cash flow accruing to him/her after the exit. I calculate the wealth of private firm owners (scaled by annual sales) following a RM,

IPO, or sellout. This includes the value of their equity position as well as cash and other consideration received in the transaction. I predict that the wealth of the private firm owner of an RM firm is not significantly different than that of the owner of an IPO firm (or sellout firm), controlling for factors that influence the choice of exit mechanism.

To answer the third research question, I make predictions regarding the level of synergy generated in a RM. As a business combination, the RM will generate synergy if the combined firm is more valuable than the sum of the values of the stand-alone firms. Therefore, synergy is estimated as the value of the combined firm less the value of public firm and private firm. One potential source of synergy is the ability of private firm owners to access finance to fund positive net present value (NPV) projects. A second potential source of synergy is from managers and directors of the public firm who have valuable experience and remain as part of the combined firm. I predict that the synergy is positive and is larger when RM firms have access to better quality projects or when the public firm contributes valuable human capital to the combined firm.

I test my hypotheses on a sample of 110 RMs, 455 IPOs, and 805 sellouts completed from 2005 to 2010. I require that the RM firm, IPO firm, as well as the public acquirer in the sellout, list their stock on a major U.S. exchange (NYSE, NASDAQ, or AMEX) following the transaction in order to draw a sample of firms that are likely to be comparable. I find that RM firms, IPO firms, and sellout firms tend to come from the same industries. About half the sample is from two-digit SIC codes 73 (Business Services), 28 (Chemicals), and 36 (Electronics). However, within each industry, RM firms have significantly lower sales than IPO firms, but a similar level of sales as sellout firms. For example, the average IPO firm in SIC code 73 reports

sales of $184 million while the average RM firm reports sales of $35 million and the average sellout firm reports sales of $31 million.

In my first series of tests, I analyze how information asymmetries, product market competition, and private benefits of control influence the choice of exit mechanism. In univariate and multivariate analyses, I find that firms with greater information asymmetries are more likely to execute a RM than an IPO and are more likely to execute a sellout than an IPO. Specifically, I find that both a RM and a sellout are more likely than an IPO for firms with fewer sales, foreign firms, and firms that execute the transaction when stock market returns are more volatile. I also find that a RM is more likely than an IPO for firms with a non-Big 4 auditor, firms that lack venture capital backing, and younger firms.

Next I examine how product market competition and private benefits of control influence the choice of exit mechanism. I find that foreign firms and firms that execute a transaction when future conditions are more favorable for small business owners (as measured by the Small Business Optimism Index) are more likely to choose a RM over a sellout.[2] I also find evidence that the level of sales, industry Tobin's Q, and the size of the largest firm in the industry influence the choice of an IPO over a sellout. Overall, this evidence supports the prediction that product market competition influences the choice of exit mechanism. I proxy for private benefits of control based on perquisite compensation at the industry level (Bayar and Chemmanur, 2012; Rajan and Wulf, 2006) and find that RMs and IPOs are more likely than a sellout when private benefits of control are larger. I also proxy for private benefits of control with an indicator for whether or not a family name is in the name of the firm and find opposite results, although the

[2] The Small Business Optimism Index is calculated monthly by the National Federation of Independent Business Research Foundation. The index measures, among other things, plans to increase employment and capital outlays, expectation of future sales, credit conditions, and whether or not now is a good time to expand.

statistical significance is often weak. Thus, the evidence that private benefits of control influence the choice among a RM, IPO, and sellout is mixed.

A striking finding in my study is that RM firms are significantly different than IPO firms along nearly every measurable dimension (size, age, market conditions before the deal, presence of venture capitalist, etc.). I note that my sample is biased *towards* finding similarities between RM and IPO firms because I select only RM firms that list on a major U.S. exchange. Therefore, one of the main conclusions to draw from my study is that comparisons of RM firms to IPO firms should be done with caution. In sharp contrast, RM firms have similar characteristics to many sellout firms. I conclude that RM firms, at least those that list on a major exchange, are more appropriately compared to sellout firms than IPO firms.

In my second series of tests, I empirically examine the post-deal wealth of RM firm owners and compare this value to the post-deal wealth of IPO firm owners and sellout firm owners. I use three methods: 1) matching by size and industry, 2) matching by propensity score, and 3) forecasts of wealth that control for self-selection bias. I find that the wealth-to-sales ratio of a RM firm is, on average, significantly lower than the wealth-to-sales ratio of the matched IPO firm. For example, only 21% of RM firms have a larger wealth-to-sales ratio than the size- and industry-matched IPO firm. Similarly, only 28% of RM firms have a larger wealth-to-sales ratio than the propensity score matched IPO firm. I find some evidence that the proportion of RM firms with larger wealth-to-sales ratios than the propensity score matched IPO firm increases as the quality of the match increases. However, even for a sample of 58 RMs that are closely matched to IPO firms, I still find that only 37% have a larger wealth-to-sales ratio than the propensity score matched IPO firm. My findings also hold on a sample of only domestic firms.

The third methodology generates forecasts of the wealth-to-sales ratio that each RM firm owner can obtain in an IPO while controlling for self-selection bias (Dunbar, 1995). I find that RM firm owners have, on average, significantly lower wealth-to-sales ratios than the forecasted wealth-to-sales ratio that can be obtained in an IPO. In summary, all three methodologies demonstrate that RM firm owners have lower wealth-to-sales ratios than IPO firm owners.

The finding that the wealth of RM firm owners is significantly less than the wealth of owners of similar IPO firms does not support the prediction that firm owners choose the exit mechanism that maximizes their wealth. However, when combined with the earlier evidence on the significant differences between RM firms and IPO firms, an alternative explanation is that an IPO is not a realistic option for most RM firms. Sjostrom (2008) contends that a comparison between RMs and IPOs is irrelevant for most RM firms because they fail to meet the revenue, net income, and potential for growth criteria of investment banks that underwrite IPOs. Thus, an IPO may not be a realistic option for most firms that eventually choose a RM.

Next, I analyze the post-deal wealth of RM firm owners and compare this to the post-deal wealth of sellout firm owners. Overall, I find that the wealth-to-sales ratio of RM firms is comparable to that of matched sellout firms. Specifically, I find that 61% of RM firms have a larger wealth-to-sales ratio than the size- and industry- matched sellout firm. Across a variety of propensity score matching criteria, slightly more than 50% of RM firms have a larger wealth-to-sales ratio than the matched sellout firm.

Using Dunbar's (1995) methodology, I find that RM firm owners have wealth-to-sales ratios that are similar to the forecasted wealth-to-sales ratios of sellout firm owners. However, this finding only holds when forecasting wealth-to-sales ratios based on sellout firms matched to RM firms on size and industry. Taken as a whole, the evidence supports my prediction that RM

firm owners have similar, or greater, wealth-to-sales ratios as owners of comparable sellout firms.

In my third series of tests, I analyze the synergy generated in a RM. To calculate synergy, I begin with the value of the combined firm and then subtract the value of the public firm and the value of the private firm and adjust for cash exchanged in the transaction. The value of the combined firm's equity and the public firm's equity are available from stock price data in CRSP. To obtain the equity value of the private firm, I use two different measures. First, I use valuations prepared by financial advisors to the public firm. Financial advisors often prepare discounted cash flow valuations of the stand-alone value of the private firm. However, these valuations may be inflated, as demonstrated by prior research on takeovers of public targets by public acquirers (Cain and Denis, 2013). Public firm directors and managers in RMs may use inflated valuations to convince shareholders to vote for the RM or to protect themselves against litigation following the transaction. An inflated value of the private target will drive the estimate of synergy down mechanically. A notable difference in my sample compared to other studies of financial advisor valuations is that the financial advisors in RM transactions are hired solely for the purpose of providing a fairness opinion and their compensation is not tied to deal completion.

Second, I use valuations of the private firm inferred from industry valuation multiples of takeovers of private targets by private acquirers. The benefit of the second approach is that I do not rely on (possibly inflated) valuations of financial advisors. However, the inferred valuations are not true stand-alone values of the firm as industry valuation multiples of takeovers include a portion of the synergy generated in the transaction. Thus, inferred valuations could also be biased upwards, which will reduce synergy. Weighing the pros and cons of each method, I

believe using industry multiples of private-private takeovers provides a better estimate of the value of the private firm than using valuations produced by financial advisors to the public firm.

I calculate the synergy generated in 33 RM transactions using data on private firm valuations from financial advisors. Perhaps not surprisingly, I find that synergy averages -$107.74 million while the median is -$74.02 million. Thus, RMs appear to destroy a substantial amount of wealth. In cross-sectional regressions of synergy on deal characteristics, I find that synergy is larger when the private firm is in an industry with greater Tobin's Q or operating margins. Synergy is also larger when the CEO of the private firm has previous experience at a public company. I do not find evidence that synergy is larger when the public firm contributes human capital to the combined firm.

Next, I calculate the synergy generated in 36 RM transactions using inferred valuations from private-private takeovers. I find that synergy averages $45.08 million and the median is $20.60 million. Thus, RMs appear to generate wealth, on average. In cross-sectional regressions of synergy on deal characteristics, I find that synergy is larger when the RM firm has venture capitalist backing and when small business optimism is higher before the RM. Thus, RMs appear to generate positive synergy when using valuations inferred from private-private takeovers and negative synergy when using valuations prepared by financial advisors to the public firm.

This paper makes several contributions to the literature. First, this paper extends the literature on the choice of exit mechanism by examining reverse mergers. Previous work examines reverse mergers compared to IPOs (Gleason, Jain, and Rosenthal, 2008; Adjei, Cyree, and Walker, 2008; Floros and Sapp, 2010) and sellouts compared to IPOs (Brau, Francis, and Kohers, 2003; Poulsen and Stegemoller, 2008; and Bayar and Chemmanur 2011, 2012). I

examine reverse mergers, IPOs, and sellouts in a unified framework. I find that information asymmetry and product market competition are important factors that influence the choice among these three exit mechanisms. These empirical findings support theoretical models that show the importance of evaluation costs (Chemmanur and Fulghieri, 1999) and product market competition (Bayar and Chemmanur, 2011; Chemmanur and He, 2011) in exit decisions.

Second, this paper contributes to the literature that examines reverse mergers (Gleason, Rosenthal, and Wiggins, 2005; Gleason, Jain and Rosenthal, 2008; Adjei, Cyree, and Walker, 2008; Floros and Sapp, 2010). My empirical evidence indicates that reverse merger firms are drastically different from IPO firms based on observable characteristics. A novel finding in my study is that the wealth of RM firm owners is significantly less than the wealth of matched IPO firm owners. At face value, this finding suggests that RM firms should have instead chosen an IPO. However, I conclude that an IPO is not a realistic option for the vast majority of RM firms. To the best of my knowledge, this is the first study to compare reverse merger firms to sellout firms. I find that RM firms are comparable to sellout firms along observable characteristics and RM firm owners have a similar level of wealth as comparable sellout firm owners. Thus, the evidence suggests that a sellout is a viable alternative to a reverse merger. I note that my results are obtained on a sample of RMs which list on a major exchange and may not be generalizable to the universe of RMs.

Third, my analysis lends insight into the recent Securities and Exchange Commission (SEC) rulings on reverse mergers. Following allegations of fraud in reverse merger transactions, the SEC adopted a regulation that limited the ability of reverse merger firms to trade on major U.S. exchanges.[3] These "seasoning" rules, implemented in November 2011, stipulate that the firm must trade on another exchange for one year before being up-listed to a major exchange.

[3] The press release can be found at: http://www.sec.gov/news/press/2011/2011-235.htm

These rules are designed to protect investors by forcing the firm to distribute a full year's worth of audited financial statements and demonstrate that the firm's stock can maintain its share price. My evidence suggests that the sellout exit mechanism is a feasible alternative for private firm owners who are considering a reverse merger, thus mitigating potential negative effects of restrictions on reverse merger transactions.

Fourth, this paper contributes to the literature on the role of financial advisors in M&A transactions, specifically the rendering of fairness opinions by financial advisors (see, e.g., Davidoff, Makhija, Narayanan, 2011; Cain and Denis, 2013). I analyze valuations of private firms produced by financial advisors to the public firm in a reverse merger. Based on these valuations, the synergy generated in the transaction is often negative and private firm owners experience a reduction in wealth by choosing a reverse merger rather than staying private. Conversely, I find that synergy is positive, on average, when valuations of private firms are inferred from private-private takeover multiples. A possible explanation for this evidence is that financial advisors in reverse merger transactions produce inflated valuation estimates of private firms. A novel finding of my study is that valuations produced by financial advisors appear to be inflated despite the fact that the financial advisor's compensation is structured as a flat fee not contingent of deal completion.

The rest of the paper is organized as follows. Section 2 develops hypotheses and presents empirical proxies. Section 3 describes the data. Section 4 examines the choice of RM, IPO, or sellout. Section 5 examines the wealth of RM firm owners versus IPO and sellout firm owners. Section 6 examines the synergy generated in RMs. Section 7 concludes.

2. Hypotheses and empirical proxies

2.1. Choice among the RM, IPO, and sellout exit mechanisms

I develop hypotheses regarding the choice of exit mechanism based on theoretical models in Chemmanur and Fulghieri (1999) and Bayar and Chemmanur (2011). As described in more detail below, the choice among a RM, IPO, and sellout can be influenced by information asymmetries between private firm owners and potential investors, product market competition considerations, and private benefits of control. My hypotheses focus on the choice of an RM compared to either an IPO or sellout but I also make predictions for the choice among all exit mechanisms. I also discuss empirical proxies that are used to test hypotheses.

First, I consider the impact of information asymmetries on the choice of exit mechanism. Chemmanur and Fulghieri (1999) model the decision to raise funds from numerous small investors (e.g., an IPO) or by negotiating with a single party (e.g., a RM or sellout). The benefit of negotiating an exit, as in a RM, is the lower aggregate cost to resolve information asymmetries since only the public firm must learn about the private firm's projects. In contrast, selling to dispersed investors, as in an IPO, results in a large aggregate cost to resolve information asymmetries as each investor learns about the firm.[4] However, the benefit of the RM mechanism (a lower aggregate cost to resolve information asymmetries) can be offset by the public firm's ability to extract a portion of the economic gain due to its relative bargaining power. This tradeoff leads to the conclusion that negotiation with a single party is preferable to selling to dispersed outsiders when information asymmetries are more severe (Chemmanur and Fulghieri, 1999). Therefore, I predict that firms characterized by greater information asymmetry are more likely to choose a RM rather an IPO.

[4] The number of investors that become informed about an IPO is potentially large. Cornelli and Goldreich (2003) find that the average number of bids per IPO is 411 for a sample of 37 order books.

Hypothesis 1: Firms that are characterized by greater information asymmetry are more likely to choose a RM compared to an IPO.

Following the logic above, I also expect that firms characterized by greater information asymmetry are more likely to choose a sellout compared to an IPO. This prediction is consistent with predictions in empirical papers such as Brau, Francis, and Kohers (2003), Poulsen and Stegemoller (2008), and Bayar and Chemmanur (2012). Because both a RM and a sellout are negotiated transactions, I do not expect that information asymmetries influence the choice between these two exit mechanisms.

In empirical tests, I proxy for information asymmetry using firm level, industry level, and market variables. In support of the first hypothesis, previous studies have shown that RM firms tend to be smaller and younger than IPO firms (Floros and Sapp, 2010; Gleason, Jain and Rosenthal, 2008; Adjei, Cyree, and Walker, 2008). I introduce several new proxies for greater information asymmetry including the ratio of intangible assets to total assets, an indicator if the firm lists R&D expenses on the income statement, greater dispersion of valuation multiples in the firm's industry, greater volatility in the equity markets, an indicator if the firm is headquartered outside the U.S., and an indicator for high tech targets. Previous research shows that reputable auditors and venture capitalists can resolve information asymmetries (see Beatty, 1989 and Megginson and Weiss, 1991, respectively). Therefore, I proxy for greater information asymmetry with an indicator if the firm does not use a Big 4 auditor and an indicator if the firm does not have venture capital or private equity (VC) backing.

Next, I consider the influence of product market competition and private benefits of control on the choice among exit mechanisms. Both a RM and an IPO allow private firm owners to retain private benefits of control. However, the tradeoff is that the firm must compete independently in product markets following a RM or an IPO. In contrast, a firm that chooses a sellout can better compete in product markets following integration with the public acquirer. For example, the public acquirer can provide assistance in the product market by utilizing existing distribution channels, marketing campaigns, research and development, and other firm resources to support the private firm's products. However, the benefit of product market support must be weighed against the cost of forfeiting private benefits of control in the sellout exit mechanism.

Bayar and Chemmanur (2011) show theoretically that the choice between an IPO and a sellout is influenced by private benefits of control and product market competition. They show that an IPO is more likely than a sellout when the firm is better able to compete in product markets and when the private benefits of control are greater. I extend their logic to the choice between a RM and a sellout to develop my second and third hypotheses. Specifically, I predict that firms that are better able to compete as stand-alone firms in product markets (e.g. larger firms) are more likely to choose a RM over a sellout. I also predict that firms that will benefit more from integration with a public acquirer are more likely to choose a sellout while firms that will benefit less are more likely to choose a RM. I summarize these predictions in my second hypothesis.

Hypothesis 2: Firms that are more competitive in product markets and/or would benefit less from integration with a public operating firm are more likely to choose a RM compared to a sellout.

Next, I consider how private benefits of control affect the choice between a RM and a sellout. Following the logic above, the RM mechanism allows private firm owners to retain private benefits of control while the owners of sellout firms forfeit their private benefits of control. Therefore, my third hypothesis predicts that firms with greater private benefits of control are more likely to choose a RM compared to a sellout firm.

Hypothesis 3: Firms with greater private benefits of control are more likely to choose a RM compared to a sellout.

I also expect that firms that are more competitive in product markets or have greater private benefits of control are more likely to choose an IPO over a sellout. This prediction is consistent with Bayar and Chemmanur (2011, 2012). Both the RM and IPO exit mechanism allow firm owners to retain private benefits of control and require that firms compete independently in product markets. Therefore, I do not expect private benefits of control and product market considerations to significantly influence the choice between a RM and an IPO.

I proxy for product market competition considerations with five variables. Firms that are larger or operate in industries with more growth opportunities (i.e., industry Tobin's Q is higher) are likely to be more successful as stand-alone firms. I also predict that firms that execute a transaction when the optimism of small business owners is higher are better prepared to compete in product markets as a stand-alone firm. I measure optimism by the Small Business Optimism Index, calculated monthly by the National Federation of Independent Business Research Foundation. This index measures, among other things, plans to increase employment and capital outlays, expectation of future sales, credit conditions, and whether or not now is a good time to

expand. I also predict that firms that compete in an industry dominated by a large firm are less competitive in product markets (Bayar and Chemmanur, 2012). Finally, I predict that foreign based firms will benefit less from integration with a public U.S. based firm as there are likely to be fewer operational synergies between such firms. Thus, foreign firms are more likely to choose a RM over a sellout.

I proxy for private benefits of control with two variables. First, I proxy for private benefits of control based on the firm's industry as in Bayar and Chemmanur (2012). Firms in industries where CEOs have high perquisite consumption (both in absolute terms and relative to other firm executives) are likely to be characterized by greater private benefits of control. Following Bayar and Chemmanur (2012), firms in two-digit SIC industry codes 13 (Oil and Gas Extraction), 28 (Chemicals), 29 (Oil Refining), and 37 (Transportation Equipment) are classified as having high private benefits of control.[5] I also proxy for private benefits of control by identifying firms that appear to have a family name in the name of the firm such as The Clark Group (Gompers, Ishii, and Metrick, 2010).

2.2. Hypotheses related to the wealth obtained in RMs

Next, I develop hypotheses regarding the wealth obtained by private firm owners following RMs, IPOs, and sellouts. Theoretical models assume that private firm owners sell their firm by the mechanism that maximizes the value of their wealth (Zingales, 1995; Chemmanur and Fulghieri, 1999; Bayar and Chemmanur, 2011). For example, in Chemmanur and Fulghieri (1999), the private firm owner's objective is to maximize the cash flow accruing to him/her following the exit. Therefore, the wealth obtained through the RM mechanism is

[5] Bayar and Chemmanur (2012) use perquisite compensation statistics in Rajan and Wulf (2006) to create their industry level measure. Perks include country club membership, chauffer service, and access to a company plane.

predicted to be greater than, or equal to, the wealth that could have been obtained in either an IPO or a sellout. In other words, firm owners should not choose a RM if they could obtain more wealth in an IPO or sellout. The following hypotheses predict that RM firm owners could not have achieved a higher level of wealth had they instead executed an IPO or a sellout.

Hypothesis 4: The wealth obtained in a reverse merger is greater than, or equal to, the wealth that could have been obtained in an IPO.

Hypothesis 5: The wealth obtained in a reverse merger is greater than, or equal to, the wealth that could have been obtained in a sellout.

In empirical tests of Hypotheses 4 and 5, I control for firm characteristics that affect the tradeoffs inherent among the RM, IPO, and sellout exit mechanisms. Also, I control for both observable firm characteristics as well as unobservable factors that can influence the tradeoffs among the three mechanisms.

The above hypotheses assume that a firm can choose between all three exit mechanisms (RM, IPO, or sellout). It may be the case, however, that certain frictions prevent private firms from gaining equal access to each exit mechanism. Sjostrom (2008) and Comment (2010) suggest that many RM firms are not good candidates for an IPO because of their size. Specifically, Sjostrom (2008) notes that most RM firms fail to meet the revenue, net income, and potential for growth criteria of investment banks that underwrite IPOs. This reasoning is supported by anecdotal evidence gleaned from conversations with practitioners. If many RM firms do not have access to the IPO mechanism, then we may observe lower levels of wealth for

RM firms than seemingly comparable IPO firms. The same logic applies to comparisons between RM firms and sellout firms. However, I am not aware of a friction that could systematically prevent a RM firm from executing a sellout.

2.3. Hypothesis related to the synergy generated in RMs

If the combined firm is worth more than the sum of the value of the private firm and public firm then synergy is created (Ross, Westerfield, Jaffe, 2002). RM transactions have the potential to generate synergy because private firms gain access to finance to fund positive NPV projects. Indeed, the most common reason for a RM cited by the private firm is "growth" (Gleason, Rosenthal, and Wiggins; 2005). Empirical research shows that economic gains are generated by supplying finance to a firm (Hill, Kelly, Lockhart, 2012). If RMs allow private firms to accept positive NPV projects then the increase in firm value will be reflected in the synergy generated. Thus, I predict that synergy will, on average, be positive.

An alternative prediction is that RMs do not generate positive synergy because this mechanism is used by firm owners who exploit public firm shareholders by committing fraud. Indeed, RMs are often associated with fraud in academic research (e.g., Ang, Jiang, and Wu, 2012) and in the business press (Stengold, 2011). In June 2011, the SEC issued a bulletin that warned investors to be wary of RMs due to accusations of rampant fraud in such transactions (see Appendix A). If RMs are often used to commit fraud then stock market investors may bid down the price of the combined firm's stock. Thus, synergy generated in the transaction may, on average, be negative.

I also predict that the level of synergy generated in the transaction will vary across RMs. I predict that private firms with access to higher quality projects will generate greater synergy in

RMs. I proxy for the quality of the firm's projects by operating margins and Tobin's Q in the firm's industry. Because a RM is a business combination, the public firm has the opportunity to contribute personnel and operations to the combined firm. Managers and directors of the public firm are likely to have valuable human capital that can benefit the combined firm. For example, the public firm managers and directors have experience in running a publicly listed company, which can be valuable to the combined firm (this experience can include meeting disclosure requirements, interacting with analysts and media, etc.). Therefore, I also predict that the synergy generated in the transaction is larger when the public firm contributes managers or directors to the combined firm.

Hypothesis 6: The synergy generated in RM transactions is positive, on average, and is larger when the private firm has access to better projects or the public firm contributes managers or directors to the combined firm.

3. Data

3.1. Sample of reverse mergers

Gleason, Rosenthal, and Wiggins (2005, page 56) define a reverse merger (also called a reverse takeover) as "a specific corporate governance event where a private company is acquired by a public company in order to obtain the public listing, and where the private partner is the surviving public entity". My definition of reverse merger is consistent with Gleason, Rosenthal, and Wiggins (2005). Specifically, I define a reverse merger as a business combination that meets the following criteria:

1. A private operating company is combined with a public company.

2. The combined firm is a public company and carries on the business of the private operating company.
3. Private company shareholders or management control the combined firm. More specifically, either of the following occurs:
    a. Private company shareholders receive greater than 50% of the fully diluted equity, or
    b. Private company shareholders receive less than, or equal to, 50% of the equity of the combined firm and the CEO of the private company becomes the CEO of the combined company.[6]

RMs are identified by: 1) 8-K filings, 2) data used in Comment (2010), and 3) newspaper articles.[7] First, RMs are identified by searching 8-K filings obtained from the SEC's EDGAR system. The goal is to identify RMs that were completed from 2005 to 2010. The sample begins in 2005 because an SEC ruling in 2005 greatly expanded the disclosure requirements of RMs (see Appendix A). Before this rule change, there was only very sparse information available on RMs. The search procedure was executed in mid-2011, leaving time for the disclosure of deals completed in late 2010. In the first step of the search procedure, I use the Perl programming language to search about 600,000 filings for keywords that indicate that a RM has occurred.[8] The primary keywords are reverse acquisition, reverse merger, reverse takeover, change in shell company status, blank check, and variations of these words. In addition, 8-K reports with an unusually large number of disclosures are identified as potentially reporting a RM. This first step results in a sample of about 6,000 filings. In the second step, the contents of the filing are

[6] In the final sample, there are 29 RMs where the private company shareholders do not own a majority of the combined firm's equity. Among these firms, private company shareholders own 33.36% of the combined firm's equity, on average. Private firm shareholders are typically more concentrated than public firm shareholders. Thus private firms shareholders with less than majority ownership can still represent a significant voting bloc in the combined firm.

[7] I thank Bob Comment for generously providing his data on reverse mergers.
[8] I thank Yuehua Tang and Andy Leone for Perl code and programming tips.

examined by hand to determine if a RM occurred or not. This step identifies 1,300 deals that are likely to be RMs and were completed from 2005 to 2010.

The second source of data is a sample of RMs used in Comment (2010). Comment analyzes SEC filings and identifies 1,460 RMs completed from 2005 to 2010. There is substantial overlap between my hand collected sample and Comment's sample.[9] The third source of data is newspaper articles that identify a small number of RMs that were not included in the previous two data sets.

The sample of RM deals is filtered in order to draw comparisons to IPO and sellout firms. The first, and most significant, filter is that the firm's stock must appear in the CRSP database within one year of the closing of the transaction. About 10% of RMs meet this criterion.[10] This filter is important to ensure that RM firms are reasonably comparable to IPO firms and sellout firms. Therefore, I exclude RM firms whose stock only trades in over-the-counter markets as they are likely to be quite different from the typical IPO and sellout firm. An implication of this restriction is that my sample of RM firms is biased towards finding similarities to IPO firms.

The second filter is that financials and utilities (SIC code between 6000 and 6999 or 4910 and 4949) are excluded from the sample. Also, I remove deals that appear in both the RM and IPO sample. After applying these filters, the sample is 110 RMs that were completed from January 1, 2005 to December 31, 2010. All data described below for RM firms is hand collected, with the exception of stock price data which is from CRSP, and the industry of the firm, which is from Compustat.

[9] In the final sample, about 75% of the deals are identified by my hand collection procedure and Comment (2010).
[10] In the final sample, about 75% of RMs appear on CRSP within six months, which is a common criterion in the IPO literature. About 90% appear on CRSP within 8.5 months. Many RMs include public firms that are not listed on a major exchange. In these deals, the public firm typically does not seek approval to list on a major exchange until after the transaction. The approval process is often started soon after the deal closes and can take several months. The twelve month cutoff allows time for the approval process.

3.2. Sample of IPOs and sellouts

IPOs are identified by the SDC Platinum New Issues database. The offering must occur between January 1, 2005 and December 31, 2010 and must be an original IPO. Unit offers, ADRs, closed-end funds, REITs, rollups, limited partnerships, firms with a dual-class stock structure, and firms that appear in the RM sample are excluded. The firm's stock must appear in CRSP within one year of the offering. The firm must have financial data on Compustat for the period ending one year before the offer date. Industry is defined based on the historical SIC code in Compustat and financials and utilities are excluded. The final sample includes 455 IPOs.

Sellouts are identified by the SDC Platinum M&A domestic and international databases. The target must be private and the acquirer must be a U.S. based firm with stock price data on CRSP. The deal must be completed between January 1, 2005 and December 31, 2010 and the acquirer must acquire 100% of the target's shares in the transaction. Deals with target firms in the financial industry and utility industry are excluded as are deals with a deal value of less than one million dollars. The target firm must have sales data reported in SDC. The final sample includes 805 sellouts.[11]

3.3. Descriptive statistics of the sample

Table 1 shows the frequency distribution of the sample. To give a feel for the total volume of each deal type, Panel A shows the frequency of RMs, IPOs, and sellouts where few restrictions are put on the sample.[12] The number of RMs is from Comment (2010) and holds

[11] Sellouts where the consideration is all cash might not be comparable to RMs as an exit mechanism. I exclude such sellouts from the sample and find similar results as those presented below.

[12] For example, the RM and IPO firms are not required to be listed on CRSP and the sellout firms are not required to have sales data available.

steady at over 200 per year from 2005 to 2010 while the number of IPOs varies from 31 to 181 during this time. The number of sellouts varies significantly over time, but is always greater than the number of RMs or IPOs.

Table 1, Panel B shows the frequency distribution of the final sample by year, as well as sub-samples of small firms (using the cutoff in Gao, Ritter, and Zhu (2013) of sales less than $50 million) and foreign firms. The distribution shows a dramatic decrease in the number of IPOs from 118 in 2007 to 12 in 2008. However, the number of sellouts does not decline until 2009 while the number of RMs remains fairly stable at about 20 per year until 2010. A greater proportion of RMs are small firms (69 of 110, 63%) than IPO firms (169 of 455, 37%). Small firms account for 600 of 805 sellouts (75%). There are nearly as many RMs of foreign firms (42) as IPOs of foreign firms (47).

Table 2 provides descriptive statistics of the sample of 110 RMs. Panel A shows frequency counts for dummy variables. In 75% of the transactions, the firm owners receive only equity of the combined firm as consideration for their firm, while in 25% of the transactions they receive cash or other consideration (e.g. promissory notes or assumption of the firm's debt). In the sample, 38% of RMs involve a private foreign firm.[13] In 58% of the transactions, the public firm has declared that it is a shell company before the transaction. In 42% of the transactions, the public firm is an operating company. However, most of the operating companies have suspended operations and are seeking strategic alternatives or have operations that will be spun off or sold after the transaction.

It is common that the private company CEO has previous work experience or has been a director of a public company (42%). In 92% of the transactions, the private company CEO

[13] The majority of those are firms headquartered in China or Hong Kong while other nations that are represented in the sample are Canada, France, Germany, Israel, Netherlands, Sweden, and the United Kingdom.

immediately becomes the CEO of the combined company. In 33% of the sample the private company nominates all the directors of the combined firm, while in 61% of the sample the private company nominates a majority of the directors. In 56% of the transactions, the public company managers or directors have expertise in the private company's industry (defined as previous work or director experience in the private firm's industry or the public company states it's intent on conducting a RM with a firm in the private firm's industry). In 12% of the transactions the public company managers or directors have previous experience with a RM.

Table 2, Panel B shows summary statistics for continuous variables, winsorized at the 1st and 99th percentiles. Private company shareholders retain 63.37% of the combined company's equity on average (on a fully-diluted basis). Public company shareholders retain 33.19% while new investors receive 3.34% on average. The variable *Equity Wealth* is the value of the private company owners' equity after the deal (market value of the firm multiplied by percentage of shares retained) and its mean value is $125.06 million. Net cash paid to private company owners is $12.35 million while other consideration is $1.44 million. Private firms in RMs, on average, have a greater level of total assets than public firms ($78.55 vs. $52.96 million) and 63% of deals involve a private firm with larger total assets than the public firm. Public firms, on average, have $17.40 million in cash (42% of total assets).

Table 3 presents an analysis of the sample by two-digit SIC code industry. Panel A shows that RMs, IPOs, and sellouts tend to come from the same industries. The top two-digit SIC code industries by deal volume for RMs and IPOs are 73 (Business Services), 36 (Electronics), and 28 (Chemicals) while the top three industries for sellouts are 73 (Business Services), 36 (Electronics), and 38 (Measuring Instruments).

While RMs and IPOs tend to come from the same industries, RM firms are significantly smaller, in terms of sales, than IPO firms in the same industry. Panel B shows the mean and median sales for RM, IPO, and sellout firms in the eight industries with the greatest number of transactions. In industry 73 (Business Services), RM firms average $35.1 million in sales while IPO firms average $184.2 million. The difference is statistically significant at the 1% level. However, sellout firms average $30.9 million in sales, not significantly different from RM firms. The same pattern holds in industries 36 (Electronics), 48 (Communications) and 13 (Oil and Gas Extraction). Within an industry, RM firms in my sample appear to be much smaller than IPO firms, but of similar size as sellout firms.

4. Characteristics of RM firms, IPO firms, and sellout firms and choice of exit mechanism

In this section I empirically test Hypotheses 1, 2, and 3, which make predictions regarding the choice of exit mechanism. First, I make comparisons between RMs and IPOs. Then, I make comparisons between RMs and sellouts. Finally, I use multinomial regressions to test predictions on the pooled sample of RMs, IPOs, and sellouts.

4.1. Sales and wealth

In Table 4, Panel A, I show the mean and median values of sales, wealth measures, and other variables for sub-samples of RM firms, IPO firms, and sellout firms. Variables are described in Appendix B. In Appendix C: Supplemental Results, I show detailed summary statistics including the minimum and maximum of each variable. All continuous variables are winsorized at the 1% and 99% level and the winsorization is done separately for RM, IPO, and sellout sub-samples due to the potential for differences in the distribution of variables in the sub-

samples. All dollar values are in millions of 2009 dollars unless otherwise specified. Table 4, Panel A, shows that RM firms have a significantly lower level of sales ($73.79) than IPO firms ($375.03), on average. The median level of sales is lower for RM firms ($26.68) than for IPO firms ($85.02) as well, and the difference in distribution is significant based on a Wilcoxon rank-sum test. However, the mean and median of sales for sellout firms is $65.56 and $17.46, respectively, and not significantly different from that of RM firms.

I measure the wealth of the private firm owners after the close of the transaction *(Wealth)* as the market value of equity holdings *(Equity Wealth)* plus cash and other consideration received in the transaction. For RM firms, *Equity Wealth* is calculated as the percentage of shares retained multiplied by the market value of the combined firm at the close of the first day of trading after completion of the deal. For sellout firms, *Equity Wealth* is calculated as the value of stock consideration received in the transaction, using the acquirer's stock price at the close of the first day of trading after completion of the deal. For IPO firms, *Equity Wealth* is calculated as the percentage of shares retained multiplied by the market value of the firm at the close of the first day trading.[14] To calculate *Wealth* for RMs and sellouts I add *Equity Wealth* to cash or other consideration (e.g., the assumption of liabilities) received in the transaction. For IPOs, I add *Equity Wealth* to cash obtained from selling secondary shares of stock. *Wealth* for RM firms is, on average, $151.76 million, which is significantly less than *Wealth* for IPO firms ($537.24) but not significantly different than *Wealth* for sellout firms ($116.05). When scaling *Wealth* by sales (*Wealth to Sales),* there is no statistically significant difference in the average

[14] In Appendix C: Supplemental Results, I conduct a robustness test where I measure *Equity Wealth* for RMs and IPOs in two different ways: 1) percentage of shares retained multiplied by the market value of the firm at the close of trading 30 days after the first trade on CRSP (for both RMs and IPOs) and 2) percentage of shares retained multiplied by the market value of the firm 270 days after deal completion (for RMs) and 270 days after the offering (for IPOs). This test addresses the concern that my results are influenced by large first day returns for IPOs (thus generating high wealth for IPO firm owners) and the fact that RM firms do not start trading until several months after deal completion.

between RMs (18.60) and IPOs (28.07) or between RMs and sellouts (30.84). Due to skewness in this measure, I use the natural log transformation in empirical tests.

4.2. Univariate evidence on differences between RM firms, IPO firms, and sellout firms

I compare proxies for information asymmetry across RM firms and IPO firms to test Hypothesis 1. I measure dispersion of industry valuation multiples by the standard deviation of market value of equity-to-sales ratios for Compustat firms in the same four-digit SIC code (*Dispersion of Multiples).* I find no significant difference between RM firms and IPO firms. I find that RMs are executed when the return to the value-weighted stock market index over the past six months (*Market Returns)* is lower compared to IPOs. However, RMs tend to occur when market volatility has been higher over the past six months *(Market Volatility)*. RM firms are more likely to be headquartered outside the U.S. (38%) than IPO firms (10%).

RM firms are significantly younger than IPO firms on average (11.88 years versus 19.59 years).[15] RM firms are significantly more likely to employ a non-Big 4 auditor (61% versus 20%) and significantly less likely to have venture capital or private equity backing (45% versus 80%). Contrary to my prediction that RM firms are more R&D intensive, 36% of RM firms report R&D expenses on the income statement (*R&D Dummy)* compared to 58% of IPO firms. Also, only 16% of RM firms are considered high tech, according to Loughran and Ritter's (2004) four-digit SIC code classification, compared to 31% of IPO firms.

Next, I compare proxies for product market competition between RM firms and sellout firms to test Hypothesis 2. Table 4, Panel A, shows no significant difference between RM firms and sellout firms in the average Small Business Optimism Index value (*Small Business*

[15] Firm age for IPO firms is calculated based on data from the Field-Ritter dataset of company founding dates (Field and Karpoff, 2002; Loughran and Ritter, 2004), available on Jay Ritter's website: http://bear.warrington.ufl.edu/ritter/FoundingDates.htm

Optimism), percentage of sales of the largest Compustat firm in the 4-digit SIC code industry (*Leader Market Share)*, or median Tobin's Q of firms in the same 4-digit SIC code industry *(Industry Tobin's Q).*[16]

I also examine proxies for private benefits of control to test Hypothesis 3. RM firms are significantly more likely to operate in an industry with high private benefits of control *(Private Benefits Industry)* than sellout firms (35% versus 9%). Contrary to my prediction, however, RM firms are less likely than sellout firms to have a person's name in the firm's name (7% versus 13%).

4.3. Information asymmetry and the choice of exit mechanism

I test Hypothesis 1 in a multivariate setting by estimating a probit model where the dependent variable is 1 for RM firms and 0 for IPO firms. Sellout firms are excluded from this analysis. Specifically, I estimate the equation

$$\begin{aligned}\Pr(RM_i = 1) = F(\beta_0 + \beta * Proxies\ for\ Information\ Asymmetry \\ + \beta * Proxies\ for\ Product\ Market\ Competition \\ + \beta * Proxies\ for\ Private\ Benefits\ of\ Control \\ + \beta * Controls + \lambda_{year} + \lambda_{industry} + \varepsilon_i),\end{aligned} \quad (1)$$

where each observation, *i*, is a firm that has executed either a RM or an IPO. F(*) is the cumulative distribution function of a standard normal variable. The proxies for information asymmetry are described above: *Log of Sales, Log of Age, Foreign, Tangible Assets, R&D*

[16] SIC codes for sellouts are from SDC. As a result, some four-digit SIC codes cannot be matched to Compustat data. In this case, I fill in with Compustat data at the three-digit SIC code level.

Dummy, *Non-Big 4 Auditor*, *VC Backing*, *Dispersion of Multiples*, *Market Volatility*, and *High Tech*. I also control for stock market returns because a firm's choice of exit mechanism and timing of exit may be influenced by noise traders that are present in the market (Ljungqvist, Nanda, and Singh, 2006; Derrien, 2005). In certain specifications, I also include proxies for product market competition (*Small Business Optimism, Industry Tobin's Q,* and *Leader Market Share*) and proxies for private benefits of control (*Private Benefits Dummy* and *Family Name*). The regression includes year and industry dummies. Because some two-digit SIC codes have a small number of deals, I measure industry at the SIC division level, which groups two-digit SIC codes (Kahle and Walkling, 1996).

The independent variables in the regression can be highly correlated since many variables proxy for the same economic force. For example, Table 4, Panel C shows that *Log of Sales* is highly correlated with *Log of Age* (0.5527) and *R&D Dummy* (-0.4104). In un-tabulted results, I estimate Equation (1) and remove variables that are highly correlated with other variables in the regression. I estimate Equation (1) multiple times, removing *Log of Sales*, *VC Backing*, *R&D Dummy*, *Market Returns,* and combinations of these variables each time. The results are similar to those presented below and are not discussed for brevity.[17]

The results of the estimation of Equation (1) are presented in Table 5. In column (1), I exclude proxies for product market competition and private benefits of control in order to focus on proxies for information asymmetry. The coefficient on *Log of Sales* is -0.307 and statistically significant at the 1% level, which indicates that larger firms are less likely to execute a RM compared to an IPO. The results in column (1) also show that a RM is more likely if the firm is

[17] One noteworthy specification removes *Log of Sales* from the regression in Table 5, column (1) and shows a negative and statistically significant coefficient on *Log of Age* (t-stat = -3.582) and a positive and statistically significant coefficient on *Dispersion of Multiples* (t-stat = 2.335). The coefficients on the other independent variables remain similar to those presented in the table.

headquartered outside the U.S. or uses a non-Big 4 auditor and less likely if the firm has VC backing or a greater proportion of tangible assets. Firms that execute a RM appear to have more severe information asymmetries than firms that execute an IPO, which supports Hypothesis 1. However, the coefficient on *R&D Dummy* is negative and statistically significant, opposite to my prediction.[18]

In column (2), I add proxies for product market competition and proxies for private benefits of control. The results show that a RM is more likely than an IPO when *Small Business Optimism* is higher, however the coefficients on *Industry Tobin's Q* and *Leader Market Share* are statistically insignificant. The significant coefficient on *Small Business Optimism* likely reflects the fact that RM firms tend to be small firms that exit when conditions are favorable for small business owners. The coefficient on *Private Benefits Dummy* is positive and statistically significant at the 10% level, while the coefficient on *Family Name* is not statistically significant. Therefore, I do not find strong evidence that private benefits of control influence the choice between a RM and an IPO.

Because domestic firms could be systematically different from foreign firms, I re-estimate the probit regression on the sub-sample of only domestic firms. Column (3) shows that most of the results hold on the sub-sample of domestic firms. In column (4), I add proxies for product market competition and private benefits of control. The coefficient on *Small Business Optimism* is statistically significant at the 10% level and the coefficients on the other proxies are statistically insignificant.

Next, I investigate whether differences exist between RM firms and IPO firms that are similar in terms of size and industry. I construct a matched sample based on industry and annual

[18] One possible explanation for this finding is that RM firms use less reputable auditors and therefore may not always disclose R&D expenses on the Income Statement.

sales. I match each RM firm to the IPO firm in the same two-digit SIC code industry that has the closest level of sales. I exclude matches where the level of sales differs by more than +/- 40%. A total of 67 RM firms can be matched to an IPO firm, for a sample of 134 firms in the *Match 1 to 1* sample. The difference in sales between the RM firm and matched IPO firm is 9.0% on average, while the median is 5.5% and the 75th percentile is 14.0%. I estimate a conditional logit model on the *Match 1 to 1* sample and present the results in Table 5, column (5).[19] Several of the results disappear after matching as the coefficients on *Non Big 4 Auditor, VC Backing, Market Returns,* and *Small Business Optimism* are statistically insignificant. Thus, many of the differences between RM firms and IPO firms can be attributed to differences in size and industry. However, is support of Hypothesis 1, the coefficient on *Foreign* is positive and statistically significant. The coefficient on *Tangible Assets* is positive and statistically significant and the coefficient on *R&D Dummy* is negative and statistically significant, opposite to my prediction.

4.4. Product market competition, private benefits of control, and choice of exit mechanism

I test Hypotheses 2 and 3 in a multivariate setting by estimating a probit model where the dependent variable is 1 for RM firms and 0 for sellout firms. IPO firms are excluded from this analysis. The estimation is analogous to estimating Equation (1). The independent variables include the proxies for product market competition and private benefits of control described above: *Log of Sales, Foreign, Small Business Optimism, Industry Tobin's Q, Leader Market Share, Private Benefits Dummy,* and *Family Name.* I also include proxies for information asymmetry where data for sellout firms are always available (*Market Volatility* and *High Tech)*

[19] In order for the estimation to execute, several variables are excluded from the regression. In unreported tests, I find the coefficients on *Log of Sales, Log of Age, Dispersion of Multiples, Market Volatility, Industry Tobin's Q,* and *High Tech* are statistically insignificant.

and *Market Returns* to capture market conditions are the time of exit. The regression includes year and industry (SIC division) dummies.

Again, the independent variables in the regression can be highly correlated since many variables proxy for the same economic force. For example, Table 4, Panel B shows that the correlation of *Log of Sales* and *Industry Tobin's Q* is -0.2851. In un-tabulated results, I estimate the regression and remove variables that are highly correlated with other variables in the regression. I estimate the regression multiple times, removing *Log of Sales, Industry Tobin's Q, Small Business Optimism, Leader Market Share, Market Returns, High Tech, Private Benefits Dummy*, and combinations of these variables each time. The results are similar to those presented below and a discussion of the results is omitted for brevity.

Table 6, column (1) shows that the coefficient on *Foreign* and *Small Business Optimism* is positive and statistically significant, indicating a greater likelihood to execute a RM rather than a sellout for foreign firms or when small business optimism is higher. The coefficients on *Industry Tobin's Q* and *Leader Market Share* are statistically significant and in the predicted direction. This evidence supports Hypothesis 2. However, the coefficient on *Log of Sales* is statistically insignificant. In column (2), I add proxies for private benefits of control to the regression. The coefficients on *Foreign* and *Small Business Optimism* remain statistically significant, but the coefficients on *Industry Tobin's Q* and *Leader Market Share* become statistically insignificant. In support of Hypothesis 3, the coefficient on *Private Benefits Industry* is positive and statistically significant. However, the coefficient on *Family Name* has the opposite sign to what is predicted. Thus, the evidence for Hypothesis 3 is inconclusive.

I present evidence on the sub-sample of domestic firms in Table 6 columns (3) and (4). The results are generally similar to those in column (1). However, the coefficient on *Log of*

Sales is negative and statistically significant, suggesting that larger firms are less likely to choose a RM over a sellout, opposite to my prediction. In column (5), the sample is the *Match 1 to 1* sample, constructed in a manner analogous to that described above for RM firms and IPO firms. The *Match 1 to 1* sample is 69 RM firms matched to 69 sellouts in the same industry that are closest in annual sales.[20] The difference in sales between the RM firm and matched IPO firm is 8.8% on average, while the median is 5.6% and the 75th percentile is 12.7%. The results in column (5) show that foreign firms and firms that execute a transaction when small business optimism is high are more likely to execute a RM compared to a sellout. This evidence supports Hypothesis 2.

4.5. Multivariate tests of the exit decision- pooled sample of RMs, IPOs, and sellouts

I test the predictions generated in Section 2 on the pooled sample of RMs, IPOs, and sellouts. First, I test for the influence of information asymmetry on the exit mechanism choice. I predict that a RM and a sellout are both more likely than an IPO for firms that are characterized by greater levels of information asymmetry. I test this prediction in two ways: 1) a multinomial logistic regression and 2) a logistic regression where I pool RM and sellout firms and compare them to IPO firms. Table 7, Panel A shows the coefficient estimates from a multinomial logistic regression where the base case is an IPO and the other two choices are a RM (column (1)) and a sellout (column (2)).[21] The independent variables are proxies for information asymmetry: *Log of Sales, Foreign, Dispersion of Multiples, Market Volatility,* and *High Tech*. In support of my

[20] The regression in column (5) excludes one pair of observations due to missing data on *Industry Tobin's Q* and *Leader Market Share* for one sellout firm.

[21] I test whether or not the specification satisfies the independence of irrelevant alternatives (IIA) assumption. The null hypothesis is that the multinomial logit gives the same results as a specification where sellouts are deleted from the sample. The p-value from a nonlinear Hausman-like test is 0.9713, suggesting that the IIA assumption is met. Similar results obtain for the other two multinomial logisitic regressions in Table 7.

prediction, the coefficients on *Log of Sales*, *Foreign*, and *Market Volatility* indicate that both a RM and a sellout are more likely than an IPO when information asymmetry is more severe. The coefficients on *High Tech* and *Dispersion of Multiples* are opposite to my prediction and often statistically significant, however.[22] I obtain similar results in column (3) of Panel A when IPOs are compared to the pooled sample of RMs and sellouts in a logistic regression.

Next, I test the prediction that both RMs and IPOs are more likely than a sellout for firms that are better prepared to compete in product markets or for firms that have higher private benefits of control. Table 7, Panel B shows the coefficient estimates from a multinomial logistic regression where the base case is a sellout and the other two choices are a RM (column (1)) and an IPO (column (2)). The independent variables are proxies for product market competition and private benefits of control: *Log of Sales, Foreign, Small Business Optimism, Industry Tobin's Q, Leader Market Share*, *Private Benefits Dummy*, and *Family Name.* Column (1) shows that foreign firms are more likely to execute a RM than a sellout, in support of the prediction that foreign firms benefit less than domestic firms from integration with a public acquirer. In column (2), the statistically significant coefficients on *Log of Sales*, *Small Business Optimism*, *Industry Tobin's Q,* and *Leader Market Share* indicates that firms that execute an IPO are better prepared to compete in product markets than firms that execute a sellout. Again, I find mixed evidence on the prediction regarding private benefits of control. In columns (1) and (2), the coefficient on *Private Benefits Dummy* is in the predicted direction and statistically significant while the coefficient on *Family Name* is in the opposite direction and statistically significant. I obtain similar results in column (3) of Panel B when sellouts are compared to the pooled sample of RMs and IPOs in a logistic regression.

[22] I note that both of these results are insignificant in Table 5 when additional variables are added to the regression.

In Table 7, Panel C, I include proxies for information asymmetry, product market competition, and private benefits of control in one specification. The specification is a multinomial logistic regression where the base outcome is RMs. Overall, the results are consistent with the multinomial logistic regressions in Table 7, Panels A and B and the probit models presented in Tables 5 and 6. The choice between RM and IPO appears to be significantly affected by proxies for information asymmetry (*Log of Sales, Foreign, Market Volatility)* and not significantly affected by proxies for product market competition (*Industry Tobin's Q* and *Leader Market Share)* with the exception of *Small Business Optimism*. Proxies for private benefits of control are either weakly statistically significant (*Private Benefits Dummy)* or not statistically significant (*Family Name).* The choice between RM and sellout appears to be significantly impacted by three proxies for product market competition (*Foreign, Small Business Optimism,* and *Industry Tobin's Q)* and the evidence is mixed for proxies for private benefits of control. Proxies for information asymmetry also provide mixed evidence (e.g., the coefficient on *High Tech* is negative and significant, but the coefficient on *Market Volatility* is insignificant).

4.6. Summary of evidence

The univariate and multivariate evidence demonstrates that, compared to IPO firms, RM firms are significantly smaller, younger, have a lower proportion of tangible assets, are less likely to have VC backing, are more likely to be headquartered outside the U.S., and are more likely to employ a non-Big 4 auditor. The evidence provides support for Hypothesis 1, that RM firms are characterized by greater information asymmetries than IPO firms. I also find that RM firms, compared to sellout firms, are more likely to be headquartered outside the U.S. and execute a transaction when small business optimism is higher. This evidence provides some

support for Hypothesis 2. The evidence in support of Hypothesis 3, that RM firms have greater private benefits of control than sellouts firms, is mixed.

5. Wealth obtained in RMs versus IPOs and sellouts

This section addresses the second research question which asks whether or not private firm owners who execute a RM obtain a similar level of wealth as those who execute an IPO or sellout. Three methods are used to address this question: 1) matching RM firms to IPO and sellout firms by size and industry, 2) matching by propensity score, and 3) forecasts of wealth that control for self-selection bias.

5.1. Matching methods: RM and IPO firms

I match RM firms to IPO firms by size and industry (the *Match 1 to 1* sample), and by propensity score. Propensity score matching is used to match each RM firm to an IPO firm based on observable firm, industry, and market characteristics. The propensity score is the conditional probability that a firm will choose a RM given the vector of observed covariates (Rosenbaum and Rubin, 1983). I calculate the propensity scores from the probit regressions in Table 5 column (1).

Following Bayar and Chemmanur (2012), the wealth of the matched IPO firm owners is multiplied by the annual sales of the RM firm to obtain the imputed wealth of the RM firm (Equation (2)). The imputed wealth is an estimate of the wealth that would have been obtained in an IPO.

$$Imputed\ Wealth = \frac{Wealth\ of\ Matched\ IPO}{Sales\ of\ Matched\ IPO} * Sales\ of\ RM \tag{2}$$

I divide the actual wealth of the RM firm owners *(Wealth)* by the imputed wealth to obtain *Relative Wealth*, as in Equation (3). Note that *Relative Wealth* is equivalent to the RM firm's wealth-to-sales ratio divided by the matched IPO firm's wealth-to-sales ratio.

$$Relative\ Wealth = \frac{Wealth\ of\ RM\ firm}{Imputed\ Wealth\ of\ RM\ firm} \tag{3}$$

$$Relative\ Wealth = \frac{Wealth\ of\ RM/Sales\ of\ RM}{Wealth\ of\ Matched\ IPO/Sales\ of\ Matched\ IPO}$$

Tests for significance are conducted using the natural log transformation, *LN Relative Wealth*, due to skewness of the ratio measures. If *LN Relative Wealth* equals zero, then the wealth obtained in the RM is exactly the same as the estimate of what could have been obtained in an IPO (*Wealth* equals *Imputed Wealth* and therefore *Relative Wealth* equals one). In other words, *Wealth to Sales* for RM firm owners is equal to *Wealth to Sales* for matched IPO firm owners. If *LN Relative Wealth* is positive, then the wealth of firm owners of the RM firm is greater than the wealth of firm owners of the matched IPO firm. If *LN Relative Wealth* is negative, then the wealth of owners of the RM firm is less than the wealth of owners of the matched IPO firm.[23]

In Table 8, I present an analysis of *LN Relative Wealth* for various matching procedures. I show the mean, median, t-stat (for a test that mean equals zero), and the percentage of

[23] I require non-zero sales to create this measure. In the sample, 14% of RM firms, 6% of IPO firms and 0% of sellout firms have zero sales. One reason that there are no sellouts with zero sales in my sample is that very small private target acquisitions are often not disclosed.

observations where *LN Relative Wealth* is greater than zero. Each row of Table 8, Panel A shows *LN Relative Wealth* calculated with RM firms and IPO firms under a different matching procedure. In the first row of Panel A, I calculate *LN Relative Wealth*, where IPO firms are matched on two-digit SIC code industry and sales (the *Match 1 to 1* sample). The mean of *LN Relative Wealth* is -0.80 and the t-stat is -5.76, indicating that RM firm owners obtain significantly less wealth than matched IPO firm owners. In fact, only 21% of the observations have positive *LN Relative Wealth*.

In the next eight rows of Table 8, Panel A, the matched IPO firms are from propensity score matching.[24] I provide an analysis of the quality of the match in Appendix C: Supplemental Results. I first match every RM firm to the IPO firm with the closest propensity score with replacement. The average value of *LN Relative Wealth* is -2.14 and the t-stat is -6.46, again indicating that RM firm owners obtain significantly less wealth than matched IPO firm owners. The median of *LN Relative Wealth* is -1.80 and only 28% of the observations have *LN Relative Wealth* greater than zero. Next, I restrict the matched IPO firm to have a propensity score within 0.01 of the RM firm. This restriction increases the quality of the match, at the expense of a loss in sample size. I can find an appropriate match for 58 RM firms and I find that average *LN Relative Wealth* is -0.81 (t-stat = -2.42). Although *LN Relative Wealth* is closer to zero when the quality of the match is increased, the results still show that RM firm owners obtain significantly less wealth than matched IPO firm owners.[25] Similar results obtain when I match without replacement or run the tests on only domestic RM and IPO firms, as shown in the remaining

[24] I use the Stata program "psmatch2" to execute the matching procedure (Leuven and Sianesi, 2003).

[25] I also match using the probit regression which includes proxies for product market competition and private benefits of control (as in Table 5, column (2)) to generate propensity scores. Matching with caliper = 1 results in average *LN Relative Wealth* of -0.37 (t-stat = -1.44) with 45% of the observations less than zero. Matching with caliper = 0.01 results in average *LN Relative Wealth* of -0.77 (t-stat = -2.25) with 39% of the observations less than zero. However, I note that most of the proxies for product market competition and private benefits of control are statistically insignificant in the probit regression, and therefore I do not report these results.

rows of Panel A. The evidence indicates that RM firm owners obtain lower *Wealth to Sales* than matched IPO firms, inconsistent with Hypothesis 4.

5.2. Dunbar (1995) Method: RM and IPO firms

One drawback of the propensity score matching method is the inability to address unobservable factors that cause firm owners to prefer RMs or IPOs. Dunbar (1995) proposes a method to address self-selection bias. As described in more detail in Appendix C: Supplemental Results, I apply Dunbar's (1995) method to obtain a forecast of the wealth-to-sales ratio *(LN Wealth to Sales)* that each RM firm owner would have obtained in an IPO. Then, I compare the actual wealth-to-sales ratio obtained in the RM to the forecasted wealth-to-sales ratio that would have been obtained in an IPO. For each RM, I subtract the forecast of *LN Wealth to Sales* from the actual *LN Wealth to Sales* obtained in the RM. This measure is equivalent to *LN Relative Wealth*, where the forecast of the wealth-to-sales ratio replaces the matched firm wealth-to-sales ratio in Equation (3).

Table 9, Panel A, presents the mean and median of actual *LN Wealth to Sales*, forecasted *LN Wealth to Sales*, and *LN Relative Wealth* where the Dunbar (1995) procedure is applied to various samples of RM and IPO firms. The far right column shows the t-statistic from a t-test where the null hypothesis is that *LN Relative Wealth* is equal to zero. The results from the full sample show that RM firm owners have actual *LN Wealth to Sales* of 1.17 on average, but the forecast of *LN Wealth to Sales* is 2.29, on average. The average difference of -1.12 is statistically significant at the 1% level (t-stat = -9.42).[26] Similar results are obtained for the sub-sample of domestic firms.

[26] I also execute Dunbar's (1995) method using a probit regression which includes proxies for product market competition and private benefits of control (as in Table 5, column (2)). I find very similar results as those reported.

An issue with the Dunbar (1995) method is that forecasts of wealth are computed based on wealth-to-sales ratios of the entire sample of IPO firms. To the extent that many IPOs are not comparable to RM firms, these forecasts could be inaccurate. To address this concern, I obtain forecasts of *LN Wealth to Sales* from IPO firms that are in similar industries and are of similar size as RM firms. Specifically, I use the *Match 1 to 1* sample and another sample, which I refer to as the *Match 1 to Many* sample. The *Match 1 to Many* sample includes all the RM firms in the *Match 1 to 1* sample plus all IPO firms that have sales within +/- 40% and are in the same two-digit SIC code as any of those RM firms. There are 192 IPO firms that meet this criterion. Thus, the *Match 1 to 1* sample is a sub-sample of the *Match 1 to Many* sample. I find that the average of *LN Relative Wealth* is -2.66 (t-stat = -19.43) for the *Match 1 to Many* sample and -2.65 (t-stat = -18.27) for the *Match 1 to 1* sample.[27] In summary, the results provide evidence against Hypothesis 4.

5.3. Matching methods: RM and sellout firms

I use the same techniques as above to test Hypothesis 5, which says that the wealth obtained in a RM is greater than, or equal to, the wealth that could have been obtained in a sellout. In Table 8, Panel B, I present an analysis of *LN Relative Wealth* for various matching procedures where RM firms are matched to sellout firms. Panel B is structured the same as Panel A and shows the mean, median, t-stat (for a test that mean equals zero), and the percentage of observations where *LN Relative Wealth* is greater than zero. In the first row of Panel B, I analyze *LN Relative Wealth* where the denominator is calculated from sellouts matched on

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For the full sample (N=93), the mean of *LN Relative Wealth* is -1.13 (t-stat = -9.43). Again, I note that most of the proxies for product market competition and private benefits of control are statistically insignificant in the probit regression, and therefore I do not report these results.

[27] I cannot calculate *LN Relative Wealth* for one RM firm due to missing data on *Dispersion of Multiples.*

industry and size (the *Match 1 to 1* sample). I find that the average of *LN Relative Wealth* is 0.34 with a t-stat of 2.07. Also, the median is 0.50 and 61% of RM firms have *LN Relative Wealth* greater than zero. This evidence suggests that RM firms obtain wealth levels that are significantly greater than that of matched sellout firms, on average.

Next, I match based on propensity scores calculated from the probit regression in Table 6, column (2).[28] In the second row of Table 8, Panel B, I analyze *LN Relative Wealth* where each RM firm is matched to the sellout that has the closest propensity score. The average of *LN Relative Wealth* is 0.04 (t-stat = 0.17) and 46% of the observations have *LN Relative Wealth* greater than zero. Thus, RM firms appear to obtain a similar level of wealth as matched sellouts when matching is done based on propensity scores. I restrict the match to those where the propensity score is within 0.01. A total of 75 RM firms can be matched and the results again show that *LN Relative Wealth* is positive (0.07), but not significantly different from zero (t-stat = 0.28). Matching without replacement shows similar results.

In the last four rows of Table 8, Panel B, I restrict the sample to only RM firms and sellout firms that are headquartered in the U.S. I match 56 RM firms to sellout firms and find that the average *LN Relative Wealth* is 0.69 and statistically significant (t-stat = 2.25). Also, 59% of the observations have *LN Relative Wealth* greater than zero. Thus, domestic RM firms appear to obtain wealth that is greater than matched domestic sellout firms. The results are slightly weaker when the caliper is reduced to 0.01 and when matching is done without replacement. Overall, the results provide support to Hypothesis 5.

[28] I provide an analysis of the quality of the match in Appendix C: Supplemental Results.

5.4. Dunbar (1995) Method: RM and sellout firms

I use Dunbar's (1995) method to forecast the wealth that an RM firm owner could obtain in a sellout. Table 9, Panel B, shows the mean and median of actual *LN Wealth to Sales*, forecasted *LN Wealth to Sales*, and *Relative Wealth* where the forecasts correspond to the wealth that RM firm owners could have obtained in a sellout. For the full sample of RM firms, actual *LN Wealth to Sales* is 1.13 on average while forecasted *LN Wealth to Sales* is 2.13. The mean of *LN Relative Wealth* is -1.00 (t-stat = -7.83), which indicates that RM firm owners obtain significantly less wealth than they could have obtained in a sellout. The results are similar for a sample of only domestic firms. On this sub-sample, the mean of *LN Relative Wealth* is -1.62 (t-stat = -8.91). This evidence, using the full sample of sellouts to forecast *LN Wealth to Sales*, does not support Hypothesis 5.

Next, I calculate forecasted *LN Wealth to Sales* and *LN Relative Wealth* on the *Match 1 to 1* sample and the *Match 1 to Many* sample of RM and sellout firms. The *Match 1 to Many* sample includes all the RM firms in the *Match 1 to 1* sample plus 380 sellouts that have sales within +/- 40% and are in the same two-digit SIC code as any of those RM firms. When using the *Match 1 to Many* sample, actual *LN Wealth to Sales* is 1.24, on average, while forecasted *LN Wealth to Sales* is 1.20. The mean of *LN Relative Wealth* is 0.04 (t-stat = 0.24), which indicates that RM firm owners obtain roughly the same wealth that they could have obtained in a sellout. In the *Match 1 to 1* sample, the mean of actual *LN Wealth to Sales* is 1.24, of forecasted *LN Wealth to Sales* is 0.37, and of *LN Relative Wealth* is 0.86 (t-stat = 5.08). Thus, compared to sellouts of similar size and in the same industry, RM firm owners obtain similar or greater wealth compared to what could have been obtained in a sellout. This evidence supports Hypothesis 5.

5.5. Cross-sectional variation in LN Relative Wealth

In Table 10, I examine whether or not *LN Relative Wealth* varies cross-sectionally with characteristics of RM firms and characteristics of the public firm. Specifically, I estimate the equation

$$LN\ Relative\ Wealth_i = \beta_0 + \beta * Private\ Firm\ Characteristics + \beta * Public\ Firm\ Characteristics + \varepsilon_i\ , \quad (4)$$

where the unit of observation is RM firm *i*. The private firm characteristics include an indicator for sales less than $50 million, *Log of Age, Foreign, High Tech, Non-Big 4 Auditor, VC Backing,* an indicator for whether or not the private firm hired a financial advisor, and an indicator for whether or not the private firm CEO has public company experience. The public firm characteristics include an indicator for shell companies and an indicator for whether or not the public company has industry expertise in the private firm industry.

In Table 10, Panel A, *LN Relative Wealth* is calculated by comparing RM firms to IPO firms. In column (1), *LN Relative Wealth* is calculated using the propensity score method (caliper = 1, with replacement) on the full sample of firms. In column (2), *LN Relative Wealth* is calculated using the *Match 1 to 1* sample of firms. In column (3), *LN Relative Wealth* is calculated using the Dunbar (1995) method on the full sample of firms. In column (4), *LN Relative Wealth* is calculated using the Dunbar (1995) method on the *Match 1 to 1* sample of firms.

Column (1) shows that *LN Relative Wealth* is greater in magnitude when the private company CEO has experience in a publicly-traded company, although the statistical significance is weak (t-stat = 1.694). The size, age, foreign status, high tech status, auditor reputation, and presence of VC backing of the company does not significantly affect *LN Relative Wealth*. Also, there appears to be no effect if the public company is a shell, or if public company managers/directors have industry experience in the private firm's industry. Column (2) shows similar results. In columns (3), *LN Relative Wealth* is greater for RM firms that did not employ a Big 4 auditor. Column (4) shows a similar result and also shows that foreign based firms have lower *LN Relative Wealth*. I also find that *LN Relative Wealth* is positively associated with private company CEOs who have experience in a publicly-traded company (t-stat = 2.741).

Panel B of Table 10 is structured the same as Panel A. However, in Panel B, *LN Relative Wealth* is calculated by comparing RM firms to sellout firms. Columns (1) and (2) show a positive association between *LN Relative Wealth* and small firms and between *LN Relative Wealth* and firms with VC backing. Column (4) shows a positive association between *LN Relative Wealth* and foreign firms, firms with VC backing, and firms where the CEO has experience in a publicly-traded company.

5.6. Discussion of Results

The above empirical tests are designed to test Hypotheses 4 and 5, which state that the wealth obtained in a RM is greater than, or equal to, the wealth that could have been obtained in either an IPO or a sellout. The evidence from industry and size matched samples, propensity score matched samples, and forecasts of wealth that control for self-selection bias clearly

indicates that RM firm owners achieve significantly lower levels of wealth than IPO firm owners.[29] Therefore, Hypothesis 4 can be rejected.

The evidence from industry and size matched samples and propensity score matched samples supports Hypothesis 5. Specifically, RM firm owners obtain wealth that is greater than, or equal to, the wealth that sellout owners obtain. The evidence from Dunbar's (1995) method is more nuanced. When estimating forecasts based on the full sample of sellouts, RM firm owners appear worse off. When using matched samples to estimate forecasts, RM firms owners appear no worse off or better off than sellout firm owners. When taken together, the evidence supports Hypothesis 5.

6. Synergy and Public Firm Valuation

In this section, I calculate the synergy generated in RMs and examine how the synergy is divided between the private firm owners and public firm owners.

6.1. Synergy calculation

Synergy is created in a RM if the combined firm is worth more than the sum of the stand-alone values of the private firm and public firm (Ross, Westerfield, Jaffe, 2002). I calculate synergy as the combined firm's equity value ($V_{combined}$) less the stand-alone equity values of the public company (V_{public}) and the private company ($V_{private}$), as in Equation (5).

$$V_{combined} = V_{public} + V_{private} + Synergy \tag{5}$$

$$Synergy = V_{combined} - V_{public} - V_{private}$$

[29] In Appendix C: Supplemental Results, I find similar results if I replicate the analysis using wealth-to-assets measures for RM and IPO firms rather than wealth-to-sales measures.

The equity value of the combined firm and public firm are calculated based on data from CRSP. The value of the combined firm is measured by the share price multiplied by the number of shares outstanding after the deal is complete. The stand-alone value of the public firm is measured as the share price multiplied by the number of shares outstanding one day before the announcement of the transaction.

The value of the private firm is challenging to obtain since, by definition, the firm does not have publicly traded stock. I use two methods to calculate the stand-alone value of the private firm: 1) private firm valuations produced by financial advisors to the public firm and 2) private firm valuations inferred from industry multiples of takeovers of private targets by private acquirers.

The benefit of financial advisor valuations is that the valuation is specific to each firm and includes a detailed discounted cash flow analysis. The downside to financial advisor valuations is that the valuation may be inflated. Managers (and boards of directors) of public companies in RMs can benefit from inflated private firm valuations that help convince shareholders to vote for the completion of the RM and/or protect management from lawsuits that can arise following a bad deal. Thus, managers may encourage advisors to present inflated valuations of the target or choose advisors who have a reputation for issuing inflated valuations. In contrast, valuations inferred from private-private takeovers are not subject to manipulation by the public firm managers. While valuations inferred from private-private takeovers are not truly stand-alone firm values, they are constructed with prices of firms that choose to remain private. However, the downside to valuations inferred from private-private takeovers is that the valuation multiple will contain a portion of any synergy generated in the transaction. Thus, the valuation

may be biased upwards. Also, the valuation is inferred from industry multiples rather than calculated based on the individual private firm's future cash flows. In the end, I believe that the issues associated with financial advisor valuations render them unreliable and more emphasis should be put on valuations inferred from private-private takeovers.

I also make two adjustments to the synergy measure to deal with cash transactions. First, I subtract the value of private placement funds as these will otherwise inflate the value of the combined firm. Second, I add back cash paid to the private firm owners in the transaction as these funds are part of the synergy accruing to the private firm owners. Thus, the variable *Synergy* is equal to the market value of the combined firm's equity minus the market value of the public firm's equity minus the equity value of the private firm minus private placement funds plus cash paid to private firm owners.

6.2. Data on private firm valuations by financial advisors

The data for stand-alone private firm valuations produced by financial advisors are obtained from the public firm's filings around the RM transaction. These data are available for a sub-sample of 60 RMs in which the board of directors of the public firm obtained and disclosed a fairness opinion.[30] A fairness opinion, written by a financial advisor, renders an opinion about whether or not the transaction is fair, from a financial point of view, to the firm's shareholders.[31] The fairness opinion is obtained after the terms of the merger have been agreed upon and it only says whether or not the transaction is fair, given the terms of the merger. It does not make a recommendation about whether shareholders should vote for the merger, offer guidance on how

[30] Many public shell companies are required to combine with firms which are valued at 80% or more of the value of the public firm's assets. Fairness opinions are one tool used to demonstrate that the deal meets this criteria. However, many public companies which do not have to meet the 80% rule obtain fairness opinions as well.
[31] The phrase "fair, from a financial point of view" is not well defined (Cain and Denis 2013).

the public firm should structure the transaction, or offer strategic alternatives to the public company.

With a few exceptions, the financial advisor does not provide investment banking or other advisory services to the public company. The fees paid to financial advisors are almost always flat fees that are not contingent on the merger outcome. The fee ranges from $50,000 to $500,000, but are typically between $75,000 and $100,000. The most common financial advisors in my sample are Capitalink (8 firms) and Jefferies & Company (5 firms). The board of directors rendered the fairness opinion for 8 firms. Financial advisors typically have access to non-public internal accounting records and management forecasts from both the public and private firms to assist with their valuation of the private firm. The private firm managers often meet with the financial advisor as well.

I read the filings and record the valuations of the private firm. The valuations are typically done based on a discounted cash flow analysis (DCF), a comparable public firm analysis (Comparables), or a similar transaction analysis (Transactions). The similar transaction is often a regular merger rather than a reverse merger, although in a few cases reverse mergers are used. Typically, advisors disclose a range of valuations from which I record the lowest and highest valuation for each methodology. In a small number of cases, the advisor discloses several different valuations and I record a median valuation in addition to the low and high valuation. If the advisor only discloses one valuation, or if the text of the fairness opinion focuses on one valuation as the most reliable, then I record this valuation as the "best" valuation.

Financial advisors typically disclose either an equity valuation or an enterprise valuation. In either case, I use the information in the fairness opinion and/or the firm's financial statements to calculate the value of the firm's net debt and subsequently obtain both enterprise and equity

valuations for each firm. The variables *Enterprise Value* and *Equity Value* are the best valuation, if available. If not available, I use the median valuation. If neither the best nor median are available, I use the midpoint of the low and high valuation. For DCF valuations, 9 of the 51 are best valuations and the remaining are the midpoint of the low and high valuations.

Table 11, Panel A presents frequency counts for 60 private firm valuations. The breakdown across valuation types is: 51 DCF, 49 Comparables, and 39 Transactions. Many firms receive valuations from more than one method as 40 firms have both DCF and Comparables, 35 have both DCF and Transactions, 38 have both Comparables and Transactions, and 34 have all three types of valuations. Table 11, Panel B shows the differences between the sub-sample of RM firms with and without valuations. RM firms with valuations have a significantly larger level of sales, on average ($100.57 versus $41.65 million), than other RM firms. Also, they are significantly older, on average (16.95 versus 6.24 years), and less likely to be a foreign firm (28% versus 50%). There is no significant difference in *Wealth to Sales*, the presence of VC backing, or the percentage of public firms that are shell companies.

In Table 11, Panel C, I report summary statistics on valuations. In discussing the results, I focus on the median rather than the mean due to outliers. The median of *Enterprise Value*, based on the DCF valuation, is $179.35 million. The median of *Enterprise Value to Sales* is 2.82. The median of *Equity Value,* based on the DCF valuation, is $145.50 million and the median of *Equity Value to Sales* is 2.37. Next, I consider the return to the private firm owners by comparing the post-deal equity wealth of firm owners (*Equity Wealth)* to the pre-deal equity value. The median of *Equity Wealth to Equity Value* is 0.48. This finding is puzzling because it suggests that private firm owners lose more than half of their (equity) wealth by completing a RM. However, this is consistent with financial advisors producing inflated valuations of private

firms. The median discount rate used in the DCF analysis is 17.50. The rest of Panel C presents similar statistics for the Comparables and Transactions methodologies. In general, the DCF methodology gives the highest valuations followed by the Comparables methodology and then the Transactions methodology.

6.3. Synergy from financial advisor valuations and public firm valuation

I calculate the synergy generated in each RM. Table 12, Panel A, shows that the median of *Synergy* is -$74.02 million when using the DCF method to value the private firm. I find that the median is -$52.96 million when using the Comparables method and -$4.46 million when using the Transactions method. In 70% of the deals, *Synergy* is negative when using the DCF method. For the Comparables method this number is 66% and for the Transactions method this number is 52%. Thus, it appears that the majority of RMs fail to generate positive *Synergy*.

The next three rows in the table show *Synergy* calculated using the low equity values reported in the range of financial advisor valuations. If the equity values of the private firm are accurate, this should give an upper estimate of *Synergy*. Again, I find negative median *Synergy* for the DCF method and Comparables method (-$38.54 and -$15.62). Using the low DCF valuation, 68% of the observations have *Synergy* less than zero. The Transactions method shows $15.60 as the median *Synergy* with 39% as negative values.

The sample is restricted to 33 observations when I require data on private firm valuations and market price data for public firms. I loosen this restriction, and replace the market value of the public firm with the book value of assets or the book value of equity (assets minus liabilities). Panel A shows that median *Synergy* is again negative for the majority of RMs using both of these methods.

Next, I examine the returns to public companies in RMs. In Table 12 Panel B, I calculate the returns to 56 public companies that conducted a RM. The average (median) market value for these firms is $87 ($48) million one day before the transaction. The market value of the combined firm is $173 ($105) million at the close of trading on the first day after completion, of which an average of 38.76% (34.41%) is owned by the original public firm shareholders. I calculate the percentage change in the value of their holdings (*PubCo Return)* as 19% on average and -17% at the median. Despite the large magnitude, the average percentage change is not statistically different from zero (t-stat = 1.33). Thus, the returns to public firm shareholders are not significantly different from zero, on average.

6.4. Valuations from takeovers of private firms by private firms

Next, I infer valuations of private firms from private-private takeovers. I construct a sample of private-private takeovers from *Pratt's Stats* data. The sample is 286 takeovers from 2005 to 2010. The sample includes private firms with sales of $5 million or more. Table 13, Panel A, provides a basic description of the data. There are 51 two-digit SIC code industries represented by the 286 takeovers. The average level of sales for private targets is $29.9 million while the median is $9.10 million. Thus, these firms are smaller than the RM firms in my sample.

I calculate the median price-to-sales ratio for private targets for each two-digit SIC code. Then, I multiply the industry median price-to-sales ratio by the actual sales of the private firm in each RM transaction to obtain the inferred value of the private firm. Finally, I calculate *Synergy* using the inferred value of the private firm. To make comparisons to previous calculations of *Synergy*, I only use the 60 RM firms that have valuations from financial advisors in these

calculations. Table 13, Panel B shows data on 56 RM firms for which calculations can be made. On average, there are 6.73 private-private takeovers in the RM firm's industry from which to calculate price-to-sales ratios. The average price-to-sales ratio is 0.83. The inferred value of the 56 private firms averages $42.19 million, while the median is $25.95 million. This value is less than the average (median) of *Enterprise Value* calculated from DCF valuations of $249.61 ($179.35) million shown in Table 11, Panel C. The average of *Synergy* for the 36 firms with available data is $45.08 million while the median is $20.60. Thus, the value of *Synergy* changes dramatically when private firms are valued according to industry multiples of private-private takeovers rather than valuations produced by financial advisors.

6.5. Cross-sectional variation

In Table 14, I examine whether *Synergy (calculated from DCF valuation)* and *Synergy (calculated from private-private valuations)* vary systematically with characteristics of RMs. Specifically, I estimate the equation

$$Y_i = \beta_0 + \beta * Private\ Firm\ Characteristics + \beta * Public\ Firm\ Characteristics + \beta * Deal\ Characteristics + \varepsilon_i\ , \quad (6)$$

where the unit of observation is RM *i*. The private firm characteristics include *Log of Age*, *Foreign*, *High Tech*, *Non-Big 4 Auditor*, *VC Backing*, *Industry Operating Margins*, *Industry Tobin's Q,* and an indicator for whether or not the private firm CEO has public company experience. The public firm characteristics include an indicator for shell companies, an indicator for whether or not the public company has industry expertise in the private firm industry, an

indicator for whether or not managers and/or directors remain with the combined firm, and an indicator for whether or not some of the operations of the public company remain with the combined firm. The deal characteristics include *Small Business Optimism, Market Returns,* and *Market Volatility.*

In Table 14 the dependent variable is *Synergy (calculated from DCF valuation)* in column (1) and *Synergy (calculated from private-private valuations)* in column (2). Table 14, column (1) shows that *Synergy (calculated from DCF valuation)* is significantly higher when the private firm operates in an industry with higher margins or Tobin's Q. This evidence supports Hypothesis 6, which says that synergy is greater when the private firm has access to better quality projects. I also find that synergy is significantly higher when the public firm has expertise in the private firm's industry, the private company CEO has experience at a publicly traded company, overall market returns are higher before the deal announcement, or the firm is headquartered outside the U.S. In column (2), *Synergy (calculated from private-private valuations)* is positively associated with *Small Business Optimism* and *VC Backing*.

6.6. Discussion of results

Taken at face value, the results in this section indicate that RMs fail to generate positive synergy, on average, when using valuations of private firms from financial advisors. However, an alternative explanation is that financial advisors provide inflated valuations to the public company which drive the estimate of synergy down mechanically. This explanation is consistent with Cain and Denis (2013), who find that financial advisors to acquirers systematically inflate the valuations of the target. Synergy is positive on average (and at the median) when valuations

of private firms are calculated using multiples of price-to-sales ratios from private-private takeovers.

7. Conclusion

This paper examines RMs, IPOs, and sellouts as exit mechanisms for private firm owners. I find that information asymmetries influence the choice between the three mechanisms. Firms that are smaller, headquartered outside the U.S., or execute a transaction in more volatile markets are more likely to choose either a RM or a sellout than an IPO. I also find that product market considerations influence the choice of exit mechanism. Firms that execute a transaction when small business owners are more optimistic and foreign firms are more likely to choose a RM compared to a sellout. I find that proxies for the ability to compete in product markets such as firm size, industry growth opportunities, and lack of a large competitor influence the choice of an IPO compared to a sellout. I find mixed evidence for the influence of private benefits of control on the choice of exit mechanism.

I examine the wealth obtained in RMs compared to IPOs and sellouts using matching methods as well as methods that control for self-selection bias. The evidence demonstrates that RM firm owners obtain less wealth per dollar of sales than firm owners of seemingly comparable IPO firms. In conjunction with the evidence that RM firms are significantly different from IPO firms along observable characteristics, I interpret this finding as evidence that many RM firms do not have the option of executing an IPO. However, RM firm owners obtain the same or greater wealth per dollar of sales than comparable sellout firms. Thus, a sellout appears to be a viable alternative to a RM for many firms.

Finally, I examine whether or not RMs generate positive synergy. I find that RMs generate negative synergy, on average, when synergy is calculated using private firm valuations from financial advisors to the public firm board of directors. In contrast, I find that synergy is positive, on average, when synergy is calculated using valuations of private firms that are inferred from industry multiples of private-private takeovers. Overall, the evidence leads me to conclude that financial advisors produce inflated valuations of private firms that mechanically drive down the estimate of synergy.

This paper extends the existing literature on exit mechanisms by examining RMs, IPOs, and sellouts in a unified framework. Also, this is the first paper to examine the wealth generated in RMs compared to IPOs and sellouts. My analysis informs regulators by demonstrating that a sellout is a viable alternative exit mechanism for many RM firms, thus mitigating negative effects from placing restrictions on the listing of RM firms on major U.S. exchanges. Finally, I uncover evidence that financial advisors to public firm boards of directors produce inflated valuations of private firms in RMs.

Appendix A: Institutional Details on Reverse Mergers

Description of a reverse merger and examples

A reverse merger (RM) is a business combination between a private firm and a public firm. The public firm often has no operations and assets that mainly consist of cash. The private firm shareholders and/or management control the combined company after the transaction which carries on the business of the private firm. See Sjostrom (2008) for a nice overview of the reverse merger process and the players involved.

Reverse mergers are typically structured so that the public firm (or a newly created subsidiary of the public firm) survives the deal. The public firm is legally the acquirer and therefore the surviving entity. However, the private firm shareholders are issued a large enough portion of the public firm's equity to give them a majority stake in the surviving firm. In other words, the public firm shareholders are diluted out of their majority ownership position to a minority ownership position. While the public firm is legally the acquirer and the surviving entity, the historical accounting statements of the public company are replaced by those of the private company. To the best of my understanding, the term "reverse" in reverse merger refers to the fact that the legal surviving entity is different than (reversed) from the surviving firm for accounting purposes.

As an example, consider the February 2009 RM of privately held Cardiovascular Systems and NASDAQ listed Replidyne. Cardiovascular Systems is a medical device company that filed a registration statement to conduct an IPO in January 2008, but withdrew it due to equity market conditions. Replidyne was a biopharmaceutical company that suspended its operating activities in August 2008 after a series of setbacks in the development of its main product. As of December 2008, Replidyne had about $34 million in cash on its balance sheet and relatively few liabilities. After the RM, Cardiovascular Systems shareholders held about 80% of the equity of the combined firm and appointed a majority of the directors. The Cardiovascular Systems CEO and management team operate the combined firm, which continued to trade on NASDAQ.

Another example is the RM of privately held Summer Infant (Summer) with the public shell company KBL Healthcare Acquisition II (KBL). KBL's IPO was on April 27, 2005 with a business plan to effect a merger, capital stock exchange, asset acquisition or other similar business combination with an unidentified operating business in the healthcare industry. The net proceeds of the IPO, $49,168,000, were deposited into a trust account. In February 2006, KBL was introduced to Summer Infant, a company in the juvenile health, safety and wellness products industry. An acquisition agreement was signed on September 1, 2006. Summer shareholders received $20 million in cash and 3,916,667 in KBL common stock (25.9% of outstanding common stock). Also, KBL will assume $11.7 million of Summer's net debt. The deal was approved March 6, 2007 by KBL stockholders. Summer stock trades on NASDAQ (SUMR). One of KBL's directors remains as Chairman. Summer's management team runs the combined company.

Reverse mergers and the Securities and Exchange Commission

Although a RM is legally a business combination, the Securities and Exchange Commission (SEC) considers them to be capital transactions in substance. According to SEC interpretations and guidance on accounting and financial reporting**,** "the merger of a private operating company into a non-operating public shell corporation with nominal net assets typically results in the owners and management of the private company having actual or effective operating control of the combined company after the transaction, with shareholders of the former public shell continuing only as passive investors. These transactions are considered by the staff to be capital transactions in substance, rather than business combinations. That is, the transaction is equivalent to the issuance of stock by the private company for the net monetary assets of the shell corporation, accompanied by a recapitalization".[32]

Many public company acquirers are considered shell companies. In July 2005, the SEC defined a shell company as a registrant with: no or nominal operations and either no or nominal assets, assets consisting solely of cash and cash equivalents, or assets consisting of any amount of cash and cash equivalents and nominal other asset. Registrants must declare in their filings if they are a shell company and must disclose changes in shell company status in an 8-K. A company can achieve shell status if the firm: 1) went public and raised capital in order to seek a private firm with which to conduct a reverse merger (these are called blank check or special purpose acquisition vehicles), 2) is in development stage with few assets, or 3) is a defunct operating companies that has sold most of its assets. In addition, the SEC required firms that conduct a RM to disclose, in an 8-K filing, all information that would be on a Form 10 or 10-SB. This disclosure change greatly expanded the amount of information available on firms that conduct a RM. Prior to this rule change, RM firms disclosed very limited information about themselves when conducting a RM.

Recent SEC pronouncements and regulation have impacted the RM mechanism of going public. In June 2011, the SEC published an investor bulletin which warned investors to be wary of reverse merger companies.[33] This bulletin was motivated by a large number of reverse merger firms that had recently made a financial restatement, been delisted from a stock exchange, or the managers were accused of fraud. See Sterngold (2011) for an example of the types of fraud that have occurred in RM firms. In November 2011, the SEC approved regulations which limit the ability of RM firms to trade on the NASDAQ, AMEX or NYSE. A firm that executes a RM to become public must trade for one year on a different exchange and maintain a requisite minimum share price for 30 of the 60 days prior to its listing application and the exchange's decision to list.[34] These "seasoning" rules are intended to protect investors from fraud in RM firms.

[32] See: http://www.sec.gov/divisions/corpfin/guidance/cfactfaq.htm
[33] The bulletin can be found at: http://www.sec.gov/investor/alerts/reversemergers.pdf
[34] The SEC's press release can be found at: http://www.sec.gov/news/press/2011/2011-235.htm

Sample of 110 Reverse Mergers

	Date	PERMNO	Private Firm Name
1	8/16/2005	77097	IDM, S.A.
2	11/4/2005	91286	VirtualScopics, LLC
3	9/21/2005	76020	Synergetics, Inc.
4	11/30/2005	89831	Syntax Groups Corporation
5	6/10/2005	91140	Kuhlman
6	5/25/2005	91180	Pegasus Wireless Corp
7	5/20/2005	91164	House of Taylor Jewelry
8	4/29/2005	91170	Blue Concept,LLC
9	3/23/2005	86165	Pacific Ethanol, Inc
10	5/20/2005	90159	Lander Co., Inc.
11	10/31/2005	91211	Aurora Energy Ltd.
12	1/14/2005	90738	Particle Drilling Technologies, Inc.
13	11/8/2005	90976	State Harvest Holdings Limited
14	12/31/2006	86080	Protalix Ltd
15	12/28/2006	91661	Partner Success Holdings Limited
16	12/15/2006	83790	BPO Management Services
17	12/26/2006	91665	Great Lakes Dredge & Dock Holdings Corp
18	11/29/2006	92298	InfoLogix, Inc.
19	10/31/2006	92130	Pipex Therapeutics, Inc
20	10/31/2006	92216	Nanchang Best Animal Husbandry Co.
21	10/27/2006	92197	Sureland Industrial Fire Safety Limited
22	10/11/2006	92047	Kinfair Holdings Limited
23	6/28/2006	91355	Hill International, Inc.
24	10/3/2006	88148	TorreyPines Therapeutics
25	8/30/2006	90727	FiberTower
26	5/8/2006	91283	RAM Energy, Inc.
27	9/13/2006	88434	Infinity Pharmaceuticals, Inc.
28	11/29/2006	90795	Jamba Juice Company
29	5/5/2006	89863	Micromet AG
30	3/27/2006	90098	Cyclacel
31	2/9/2006	91092	etrials Worldwide, Inc.
32	11/24/2006	91913	Yucheng Technologies Limited
33	8/16/2006	84420	Predix Pharmaceuticals Holdings, Inc.
34	12/18/2007	77865	Rise and Grow Limited
35	12/12/2007	91136	American Apparel Inc.
36	11/13/2007	91091	The Orchard
37	6/5/2007	87838	VIA Pharmaceuticals, Inc
38	10/2/2007	89756	HowStuffWorks
39	9/17/2007	92686	Nile Therapeutics, Inc.
40	8/10/2007	68145	Neonode Inc

	Date	PERMNO	Private Firm Name
41	8/3/2007	90926	PharmAthene, Inc.
42	7/23/2007	92553	Sino Palace Holdings Limited
43	7/5/2007	87084	VirnetX
44	6/29/2007	92589	Continental Development Limited
45	6/8/2007	92456	Athersys, Inc.
46	5/15/2007	86812	M & I Electric Industries, Inc.
47	10/1/2007	88830	Snowflake Entities
48	4/17/2007	38755	Southern Bay Oil & Gas L.P.
49	3/30/2007	92037	Equity Broadcasting Corporation
50	3/27/2007	82567	Acuity Pharmaceuticals and Froptix Corporation
51	10/12/2007	90826	eToys Direct
52	3/6/2007	91909	Summer Infant, Inc.
53	6/21/2007	92193	Alsius Corporation
54	2/12/2007	84321	New Motion
55	2/12/2007	91817	ClearPoint Resources, Inc.
56	7/2/2007	90830	American Community Newspapers Inc.
57	1/19/2007	92218	Total Site Solutions
58	1/12/2007	92054	Towerstream Corporation
59	9/20/2007	92769	HollySys Holdings
60	12/24/2008	93130	Xinda
61	11/10/2008	92936	Shisheng
62	10/31/2008	90183	Cornerstone BioPharma Holdings, Inc.
63	10/15/2008	91161	VBV LLC
64	9/3/2008	92982	Deer International Group Ltd
65	8/19/2008	82526	OncoGenex Pharmaceuticals, Inc
66	8/11/2008	77186	Model Reorg, Inc.
67	7/31/2008	92773	Primoris Corporation
68	6/26/2008	90777	Crusader entities
69	9/19/2008	90873	PepperBall
70	4/14/2008	92875	Shenyang Taiyu Machinery & Electronic Equipment
71	3/7/2008	10258	Celldex Therapeutics, Inc.
72	2/22/2008	92105	Boise Paper Group
73	2/12/2008	80452	Dara BioSciences, Inc.
74	2/12/2008	91199	The Clark Group, Inc.
75	2/8/2008	92768	Color Man Holdings Limited
76	1/28/2008	48020	Frederick's of Hollywood, Inc
77	1/16/2008	92805	Raygere Limited
78	1/4/2008	92689	XLNT Veterinary Care, Inc.
79	1/18/2008	92519	GaoKe Head Dragon Holdings Limited
80	4/9/2008	92749	China Cablecom Ltd

	Date	PERMNO	Private Firm Name
81	8/12/2008	91453	Global Ship Lease, Inc.
82	6/24/2009	92159	China Networks Media Limited
83	12/23/2009	88244	Wuhan Vogue-Show Jewelry Co
84	12/8/2009	93188	Cambium Learning, Inc
85	12/14/2009	88177	Bio-Quant
86	11/5/2009	93353	Hongkong Chenxin International Development
87	10/30/2009	92486	SearchMedia International Limited
88	10/29/2009	92547	Ultimate Escapes Holdings, LLC
89	10/21/2009	92507	Windrace International Company Limited
90	10/15/2009	92422	Hong Kong Mandefu Holding Limited
91	12/21/2009	90968	Neuromed Pharmaceuticals Inc.
92	9/29/2009	92596	Iridium Holdings LLC
93	9/25/2009	93031	Resolute Natural Resources
94	9/22/2009	77011	Gold Lion Holding Limited
95	6/26/2009	93274	China Net Online Media Group Limited
96	5/28/2009	93195	Westway Holdings Corporation
97	3/31/2009	92541	HUGHES Telematics, Inc
98	2/25/2009	91365	Cardiovascular Systems, Inc.
99	1/30/2009	91279	Transcept Pharmaceuticals, Inc.
100	1/27/2009	85293	ARCA biopharma, Inc
101	6/1/2009	86444	VGX PHARMACEUTICALS
102	4/9/2009	93075	AutoChina Group Inc.
103	11/20/2009	92475	Jinjiang Hengda Ceramics Co., Ltd
104	7/2/2010	12485	Liaoning Creative Bellows Co., Ltd
105	4/22/2010	12311	Keyuan International Group Limited
106	4/7/2010	93106	CAMAC Energy Holdings Limited
107	3/9/2010	84581	Pernix Therapeutics, Inc
108	2/5/2010	88278	Top Favour, SinoCoking
109	10/1/2010	91969	Dialogic Corporation
110	8/18/2010	92265	Shanghai ConnGame Network

Appendix B: Variable Construction

Variable	Details
Annual Sales	RM: Annual sales is net revenues disclosed in annual financial statements close to the first trade of the firm's stock. In about 10% of the sample, sales are estimated from quarterly or semi-annual data. IPO: Annual sales before the offering, from Compustat Sellout: Net sales from SDC
Percent Retained	RM: Percent of firm retained, including any equity purchased in private placement by original owners. IPO: One minus ((primary shares + secondary shares)/SHROUT)
Equity Wealth	RM & IPO: Market value of firm at end of first trade on CRSP multiplied by Percent Retained
Wealth	RM: Equity Wealth plus net cash received in transaction plus other consideration. Net cash is cash consideration less cash paid for shares in private placement. IPO: Equity Wealth plus proceeds from sales of secondary shares (offer price multiplied by shares sold) Sellout: Deal value, adjusted so that stock consideration reflects the acquirer's post-deal closing stock price.
Small Business Optimism	Level of Small Business Optimism Index measured in the month before the announcement of the RM or sellout, or before the initial filing date of the IPO.
Tangible Assets	Net Property, Plant, and Equipment divided by Total Assets (PPENT/AT)
R&D Dummy	Dummy variable that equals 1 if the firm reports R&D expenses (XRD) and 0 otherwise.
HighTech	Following Loughran and Ritter (2004), if the firm's four-digit SIC code is: 3571, 3572, 3575, 3577, 3578, 3661, 3663, 3669, 3671, 3672, 3674, 3675, 3677, 3678, 3679, 3812, 3823, 3825, 3826, 3827, 3829, 3841, 3845, 4812, 4813, 4899, 7371, 7372, 7373, 7374, 7375, 7378, or 7379 then the firm is a high tech firm.
Market Return	Buy and hold return to the value weighted CRSP index (vwretd) over the 126 trading days (about 6 months) before the announcement of the RM or sellout or the initial filing date of the IPO.
Market Volatility	Standard Deviation of the daily returns of the value weighted CRSP index (vwretd) over the 126 trading days (about 6 months) before the announcement of the RM or sellout or the initial filing date of the IPO.
Dispersion of Multiples	The market value to sales ratio of each firm in Compustat is calculated at the end of every calendar quarter. This ratio is winsorized each industry-quarter due to outliers. Then, for each industry-quarter, the standard deviation of this ratio is calculated. Industry is four-digit SIC code.
Leader Market Share	Sales of the largest firm in the four-digit SIC code industry in Compustat divided by total industry sales.
Industry Tobin's Q	Median Tobin's Q of firms in the same four-digit SIC code industry in Compustat
Private Benefits Industry	Dummy that equals 1 for firms in two-digit SIC codes 13 (Oil & Gas Extraction), 28 (Chemicals), 29 (Oil Refining), and 37 (Transportation Equipment) and 0 otherwise. Bayar and Chemmanur (2012) identify these industries as having high private benefits of control, based on data on managerial perks in Rajan and Wulf (2006).
Family Name	Dummy that equals 1 if the firm's name contains the name of a person or family and 0 otherwise.
Non-Big 4 Auditor	Dummy that equals 1 if the firm's auditor is not Deloitte, Ernst & Young, KPMG, or PricewaterhouseCoopers.
Industry Operating Margins	Average operating margins (operating income / sales) of firms in the four-digit SIC code industry in Compustat.

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Table 1: Frequency of RMs, IPOs, and Sellouts

Panel A shows the volume of RMs, IPOs, and sellouts, where few restrictions are placed on the data. RM data are from Comment (2010) while IPO and sellout data are from SDC. Panel B reports the volume of RMs, IPOs, and sellouts in my final sample. RM data in Panel B are from hand collection and Comment (2010) while IPO and sellout data are from SDC. In Panel B, IPOs must be listed on CRSP and have financial statement data on Compustat while sellouts must have data on the target's annual sales.

Panel A: Broad Sample

Year	Comment RMs	SDC IPOs	SDC Sellouts
2005	238	178	936
2006	227	175	971
2007	248	181	927
2008	208	31	658
2009	216	47	370
2010	323	136	506
Total	**1,460**	**748**	**4,368**

Panel B: Final Sample

				Sales < $50M			Foreign Firms		
Year	RMs	IPOs	Sellouts	RMs	IPOs	Sellouts	RMs	IPOs	Sellouts
2005	13	108	176	10	36	128	2	10	32
2006	20	118	152	15	52	113	9	13	29
2007	26	118	171	19	53	119	5	9	29
2008	22	12	186	10	3	145	10	1	17
2009	22	34	67	11	3	55	11	3	7
2010	7	65	53	4	22	40	5	11	11
Total	110	455	805	69	169	600	42	47	125
Percent of Total				63%	37%	75%	38%	10%	16%

Table 2: Reverse Merger Summary Statistics

Summary statistics of the sample of 110 RM transactions from 2005-2010. Panel A shows frequency counts of deal characteristics and private firm characteristics. Panel B shows statistics on ownership of various parties after the deal, consideration paid, and assets of private firms and public firms in RMs. Private firms in RMs are referred to as "PrvCo" while the public company is referred to as "PubCo". All data are hand collected from SEC filings except for stock price data which are from CRSP. Dollar values are in millions of 2009 dollars. Continuous variables are winsorized at the 1% and 99% levels.

Panel A: Frequency Counts

	Num	Percent		Num	Percent
Deal Characteristics			*Public company experience*		
All Equity Transaction	83	75%	PrvCo CEO has public company experience	42	42%
Mix of Equity, Cash, and Other Consideration	27	25%	PrvCo CFO has public company experience	31	28%
			PrvCo director or other executive officer has public company experience	49	45%
PrvCo is based in U.S.	68	62%			
PrvCo is not based in U.S.	42	38%	*Who becomes CEO of Combined Company?*		
			PrvCo CEO becomes Combined Co CEO	101	92%
PubCo is a Shell	64	58%	PrvCo CEO and PubCo CEO are co-CEOs	3	3%
PubCo is an operating company	46	42%	Public Co CEO remains as Combined Co CEO	6	5%
Type of Shell			*Who controls board of Combined Co?*		
Blank Check / SPAC	32	29%	PrvCo directors replace all PubCo directors	37	33%
Defunct Operating Company	22	20%	PrvCo directors control a majority of board seats	67	61%
No or nominal operations	10	9%	PrvCo directors control a minority of board seats	6	5%
Type of Operating Company					
Seeking Strategic Alternatives	27	25%	*Other*		
Operations will be spun off / sold	7	6%	Concurrent private placement	21	19%
Operations are likely to remain	12	11%	PubCo has industry expertise	62	56%
			PubCo CFO remains as CFO of Combined Co	18	16%
			PubCo has previous reverse merger experience	13	12%
			PubCo has stock price data on CRSP before RM	56	51%

continued…

Table 2, continued

Panel B: Summary Statistics

Summary Statistics	**N**	**Mean**	**Std. Dev.**	**Min**	**10th**	**25th**	**Median**	**75th**	**90th**	**Max**
PrvCo shareholders percent owned	110	63.37	22.61	2.32	31.38	49.50	66.00	79.87	92.34	98.90
PubCo shareholders percent owned	110	33.19	22.69	0.84	5.50	14.00	32.25	46.53	67.27	97.68
New Investors percent owned	110	3.34	10.14	0.00	0.00	0.00	0.00	0.00	10.04	46.70
Equity Wealth ($millions)	110	125.06	159.76	1.46	15.60	38.55	82.12	161.3	219.74	1,064.30
Net Cash Paid to PrvCo ($millions)	110	12.35	42.93	-25.04	0.00	0.00	0.00	0.00	41.11	250.58
Other Consideration Paid ($millions)	110	1.44	6.29	0.00	0.00	0.00	0.00	0.00	0.00	41.95
Total Assets of PrvCo ($millions)	108	78.55	126.79	0.37	2.43	10.91	26.31	70.18	220.83	639.85
Total Assets of PubCo ($millions)	110	52.96	83.35	0.00	0.00	0.18	24.95	66.5	135.05	406.05
Dummy if TA PrvCo > TA PubCo	110	0.63								
Cash of PubCo ($millions)	110	17.4	56.94	0.00	0.00	0.05	0.79	8.16	41.06	404.61
Cash / Total Assets of PubCo	106	0.42	0.41	0.00	0.00	0.02	0.24	0.97	1.00	1.00

Table 3: Industry analysis

Sample is 110 RMs, 455 IPOs, 805 Sellouts from 2005-2010. Industry is defined as two-digit SIC code. Panel A shows the industry distribution by deal type for the eight industries with the greatest number of deals. Panel B shows mean and median sales by deal type for the eight industries with the greatest number of total deals. T-tests evaluate difference in the mean from the mean of RMs while Wilcoxon rank-sum tests evaluate whether the distribution is different from that of RMs. Significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Panel A: Industry Distribution

	RM Firms				*IPO Firms*				*Sellout Firms*		
SIC	Industry	Num	Pct	SIC	Industry	Num	Pct	SIC	Industry	Num	Pct
28	Chemicals	30	27%	73	Business Services	82	18%	73	Business Services	265	33%
73	Business Services	14	13%	28	Chemicals	77	17%	36	Electronics	81	10%
36	Electronics	7	6%	36	Electronics	44	10%	38	Measuring Instruments	71	9%
48	Communications	7	6%	38	Measuring Instruments	38	8%	87	Engineering Services	56	7%
13	Oil And Gas Extraction	7	6%	35	Indust. & Comm. Mach.	25	5%	28	Chemicals	45	6%
38	Measuring Instruments	4	4%	48	Communications	21	5%	35	Indust. & Comm. Mach.	29	4%
34	Fabricated Metal	3	3%	13	Oil And Gas Extraction	20	4%	50	Wholesale Trade, durables	27	3%
16	Heavy Construction	3	3%	44	Water Transportation	13	3%	80	Health Services	26	3%

Panel B: Sales by Industry and Deal Type

			RM Firm Sales			*IPO Firm Sales*				*Sellout Firm Sales*				
SIC	Name	Deals	N	Mean	Median	N	Mean		Median		N	Mean		Median	
73	Business Services	361	14	35.1	25.0	82	184.2	***	75.2	***	265	30.9		11.0	
28	Chemicals	152	30	17.5	2.2	77	218.1	*	2.4		45	72.4	**	17.6	***
36	Electronics	132	7	42.3	40.2	44	236.8	**	70.2	*	81	38.5		11.2	*
38	Measuring Instruments	113	4	13.9	14.3	38	72.7	n/a	11.7	n/a	71	31.5	n/a	12.3	n/a
87	Engineering Services	66	2	77.6	77.6	8	593.5	n/a	134.2	n/a	56	65.4	n/a	14.4	n/a
35	Indust. & Comm. Mach.	56	2	136.7	136.7	25	240.4	n/a	85.0	n/a	29	32.0	n/a	17.9	n/a
48	Communications	49	7	57.0	6.8	21	353.1	**	239.0	***	21	96.1		29.5	
13	Oil And Gas Extraction	44	7	54.8	21.2	20	228.7	***	118.9	**	17	92.9		25.5	

Table 4: Univariate comparison of RM firms, IPO firms, and sellout firms

Panel A reports means, medians, and significance tests for a sample of 111 RMs and 456 IPOs from 2005-2010. Panels B and C report correlation matrices. All dollar values are in millions of 2009 dollars. Continuous variables are winsorized at the 1% and 99% levels respectively. The winsorization is performed separately for RM firms, IPO firms, and sellout firms. Variables are described in Appendix B. T-tests evaluate difference in the mean from the mean of RMs while Wilcoxon rank-sum tests evaluate whether the distribution is different from that of RMs. Significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Panel A: Means and Medians

	RMs			IPOs				Sellouts			
Variable	N	Mean	Median	N	Mean		Median		N	Mean		Median	
Sales	110	73.79	26.68	455	375.03	***	85.02	***	805	65.56		17.46	
Wealth	110	151.76	86.10	455	537.24	***	281.12	***	801	116.05		38.58	***
Wealth to Sales	95	18.60	2.58	426	28.07		3.03		801	30.84		2.00	
Small Business Optimism	110	95.38	96.60	455	97.21	***	98.20	***	805	95.64		96.70	
Dispersion of Multiples	108	50.12	19.40	451	49.21		23.52		759	45.86		30.43	
Market Returns	110	4.08	5.61	455	8.36	***	7.91	***	805	2.06		4.41	*
Market Volatility	110	0.011	0.01	455	0.008	***	0.01	***	805	0.01		0.01	
Leader Market Share	110	0.39	0.33	455	0.36		0.33		773	0.42		0.35	**
Industry Tobin's Q	110	1.82	1.70	455	1.84		1.73		773	1.79		1.72	
Foreign	110	0.38		455	0.10	***			805	0.16	***		
Private Benefits Industry	110	0.35		455	0.24	**			805	0.09	***		
Family Name	110	0.07		455	0.11				805	0.13	*		
High Tech	110	0.16		455	0.31	***			805	0.44	***		
NYSE Listed	110	0.02		455	0.28	***			805	0.27	***		
Age	110	11.88	6.00	455	19.59	***	10.00	***					
Tangible Assets	108	0.22	0.12	455	0.24		0.14						
Non-Big 4 Auditor	110	0.61		455	0.20	***							
R&D Dummy	110	0.36		455	0.58	***							
VC Backing	110	0.45		455	0.80	***							

continued…

Table 4, continued

Panel B: Correlation Matrix, RM, IPO, and sellouts (N=1,318)

	Log of Sales	Foreign	Dispersion of Multiples	Market Volatility	Market Returns	High Tech	Small Business Optimism	Industry Tobin's Q	Leader Market Share	Private Benefits Dummy	Family Name
Log of Sales	1										
Foreign	-0.024	1									
Dispersion of Multiples	-0.3389	-0.0149	1								
Market Volatility	-0.0667	0.0009	-0.0628	1							
Market Returns	0.1278	0.0241	0.0795	-0.6094	1						
High Tech	-0.2264	-0.0034	0.1253	0.0633	-0.0853	1					
Small Business Optimism	0.0663	-0.0054	0.0483	-0.7857	0.3605	-0.0471	1				
Industry Tobin's Q	-0.2851	-0.0448	0.5375	-0.0558	0.0203	0.1572	0.2072	1			
Leader Market Share	0.0709	0.0126	-0.2807	0.0176	-0.005	-0.1755	-0.0223	-0.1095	1		
Private Benefits Dummy	-0.1215	-0.0381	0.4449	-0.0651	0.0731	-0.3531	0.0687	0.2848	-0.2919	1	
Family Name	0.1082	-0.0298	-0.0564	-0.0163	-0.0266	-0.1295	0.0285	-0.027	0.0815	0.0197	1

Panel C: Correlation Matrix, RM and IPO sample (N=563)

	Log of Sales	Log of Age	Tangible Assets	R&D Dummy	Non-Big 4 Auditor	VC Backing
Log of Sales	1					
Log of Age	0.5527	1				
Tangible Assets	0.2211	0.0272	1			
R&D Dummy	-0.4104	-0.1632	-0.4139	1		
Non-Big 4 Auditor	-0.1624	-0.1848	-0.0066	-0.1794	1	
VC Backing	0.0084	0.0742	-0.1742	0.2563	-0.3906	1

Table 5: Multivariate analysis on choice between RM and IPO

This table presents coefficients from probit (columns (1) through (4)) and conditional logit (column (5)) regressions. The sample is RMs and IPOs from 2005 to 2010. In column (5), each RM firm is matched to the IPO firm in the same two-digit SIC code industry with closest sales not exceeding +/- 40% of the RM firm's sales. The dependent variable equals 1 for RMs and 0 for IPOs. Variables are described in Appendix B. Industry dummies are based on SIC divisions, which group two-digit SIC codes. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Sample		(1) All RM & IPO	(2) All RM & IPO	(3) Domestic RM & IPO	(4) Domestic RM & IPO	(5) Match 1 to 1 RM & IPO
Dependent Variable		RM=1	RM=1	RM=1	RM=1	RM=1
	Prediction					
Log of Sales	-	-0.307***	-0.314***	-0.293***	-0.297***	
		(-5.580)	(-5.560)	(-4.777)	(-4.614)	
Log of Age	-	-0.079	-0.090	-0.220*	-0.241*	
		(-0.751)	(-0.886)	(-1.675)	(-1.904)	
Foreign	+	0.809***	0.757***			100.387***
		(3.635)	(3.241)			(13.286)
Tangible Assets	-	-0.871**	-0.878**	-0.310	-0.437	8.934**
		(-2.159)	(-2.035)	(-0.651)	(-0.849)	(2.402)
R&D Dummy	+	-1.173***	-1.215***	-1.052***	-1.060***	-79.963***
		(-4.324)	(-4.407)	(-3.430)	(-3.291)	(-13.544)
Non-Big 4 Auditor	+	0.560***	0.525***	0.551***	0.545***	3.214
		(3.126)	(2.855)	(2.693)	(2.646)	(0.985)
VC Backing	-	-0.471**	-0.531***	-0.453**	-0.531***	0.638
		(-2.560)	(-2.882)	(-2.275)	(-2.655)	(0.220)
Dispersion of Multiples	+	0.001	-0.001	-0.001	-0.003	
		(0.413)	(-0.629)	(-0.615)	(-1.353)	
Market Volatility	+	32.701	74.677**	16.985	52.880	
		(1.032)	(2.085)	(0.400)	(1.131)	
Market Returns	-	-0.017*	-0.018**	-0.014	-0.015	-0.223
		(-1.943)	(-2.028)	(-1.370)	(-1.436)	(-0.829)
High Tech	+	-0.042	0.188	-0.080	0.151	
		(-0.205)	(0.745)	(-0.362)	(0.516)	
Small Business Optimism			0.097**		0.087*	-0.123
			(2.428)		(1.876)	(-0.764)
Industry Tobin's Q			-0.084		-0.158	
			(-0.342)		(-0.572)	
Leader Market Share			0.439		0.146	
			(0.892)		(0.253)	
Private Benefits Dummy			0.616*		0.606	
			(1.740)		(1.409)	
Family Name			-0.229		-0.054	
			(-0.856)		(-0.198)	
Year & Industry Dummies		Yes	Yes	Yes	Yes	Yes
Observations		557	557	471	471	134
Pseudo- R squared		0.421	0.437	0.359	0.373	0.914

Table 6: Multivariate analysis on choice between RM and sellout

This table presents coefficients from probit and conditional logit regressions. The sample is RMs and sellouts from 2005 to 2010. The dependent variable equals 1 for RMs and 0 for sellouts. Columns (1) through (4) are probit specifications. Column (5) is a conditional logit where the sample is RM firms plus matched sellout firms in the same two-digit SIC code industry with the closest sales. Variables are described in Appendix B. Industry dummies are based on SIC divisions, which group two-digit SIC codes. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Sample		(1) All RM & Sellout	(2) All RM & Sellout	(3) Domestic RM & Sellout	(4) Domestic RM & Sellout	(5) Match 1 to 1 RM & Sellout
Dependent Variable		RM=1	RM=1	RM=1	RM=1	RM=1
	Prediction					
Log of Sales	+	-0.069	-0.057	-0.101**	-0.092*	
		(-1.643)	(-1.333)	(-2.004)	(-1.812)	
Foreign	+	0.738***	0.713***			1.039*
		(5.401)	(5.208)			(1.882)
Small Business Optimism	+	0.145***	0.148***	0.131***	0.127***	0.270**
		(4.110)	(4.139)	(2.956)	(2.837)	(2.058)
Industry Tobin's Q	+	0.319**	0.203	0.237	0.168	-0.148
		(2.420)	(1.340)	(1.482)	(0.912)	(-0.217)
Leader Market Share	-	-0.559*	-0.298	-0.877**	-0.688*	-1.263
		(-1.811)	(-0.925)	(-2.170)	(-1.687)	(-0.903)
Private Benefits Dummy	+		0.452**		0.299	
			(1.969)		(1.127)	
Family Name	+		-0.401*		-0.265	-1.183
			(-1.862)		(-1.193)	(-1.478)
Market Returns		-0.004	-0.004	-0.016	-0.016	-0.022
		(-0.386)	(-0.411)	(-1.245)	(-1.291)	(-0.512)
Market Volatility		3.291	2.376	-92.057**	-94.664***	-25.793
		(0.108)	(0.078)	(-2.560)	(-2.626)	(-0.207)
High Tech		-0.741***	-0.611***	-0.752***	-0.673***	-2.190**
		(-5.016)	(-3.743)	(-4.403)	(-3.668)	(-2.159)
Constant		-11.024***	-11.440***	-8.261*	-7.871	
		(-2.829)	(-2.902)	(-1.689)	(-1.593)	
Year Dummies		Yes	Yes	Yes	Yes	Yes
Industry Dummies		Yes	Yes	Yes	Yes	No
Observations		883	883	719	719	136
Pseudo- R squared		0.233	0.245	0.230	0.235	0.340

Table 7: Multivariate analysis of all three exit mechanisms (RM, IPO, sellout)
This table presents results from multinomial logistic regressions and standard logistic regressions. The sample is RM firms, IPO firms, and sellout firms pooled together. Panel A includes independent variables which proxy for information asymmetry. Panel B includes independent variables which proxy for product market competition and private benefits of control. In each panel, columns (1) and (2) present coefficient estimates from a multinomial logistic regression while column (3) presents coefficient estimates from a standard logistic regression. Panel C includes independent variables which proxy for information asymmetry, product market competition, and private benefits of control. Columns (1) and (2) present coefficient estimates from a multinomial logistic regression. Variables are defined in Appendix C. Industry dummies are based on SIC divisions, which group two-digit SIC codes. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Panel A: Information Asymmetry

	Prediction	Multinomial Logit Base Case is IPO		Logit
		Indicator for RM	Indicator for Sellout	Indicator for RM or Sellout
		(1)	(2)	(3)
Log of Sales	-	-0.573***	-0.499***	-0.510***
		(-7.584)	(-10.305)	(-10.775)
Foreign	+	1.663***	0.470**	0.697***
		(6.081)	(2.311)	(3.574)
Dispersion of Multiples	+	-0.005**	-0.009***	-0.008***
		(-2.376)	(-5.591)	(-5.490)
Market Volatility	+	112.909***	90.348***	93.810***
		(5.680)	(5.837)	(6.165)
High Tech	+	-1.012***	0.240	0.111
		(-3.327)	(1.593)	(0.748)
Constant		0.684	-12.808***	1.088
		(0.343)	(-10.710)	(0.759)
Year Dummies		No	No	No
Industry Dummies		Yes	Yes	Yes
Observations		1,318	1,318	1,318
Pseudo R-squared		0.184	0.184	0.202

continued…

Table 7, continued

Panel B: Product Market Competition and Private Benefits of Control

	Prediction	Multinomial Logit Base Case is Sellout		Logit
		Indicator for RM	Indicator for IPO	Indicator for RM or IPO
		(1)	(2)	(3)
Log of Sales	+	0.050	0.523***	0.424***
		(0.723)	(10.877)	(9.803)
Foreign	+	1.243***	-0.500**	0.043
		(5.161)	(-2.520)	(0.251)
Small Business Optimism	+	-0.015	0.038***	0.025**
		(-0.784)	(3.004)	(2.129)
Industry Tobin's Q	+	0.137	0.424***	0.369***
		(0.590)	(2.883)	(2.639)
Leader Market Share	-	0.118	-1.025***	-0.718**
		(0.218)	(-2.737)	(-2.090)
Private Benefits Dummy	+	1.564***	1.006***	1.178***
		(4.657)	(3.649)	(4.592)
Family Name	+	-0.804*	-0.618***	-0.667***
		(-1.761)	(-2.733)	(-3.172)
Constant		13.572***	7.549***	10.323***
		(6.910)	(3.528)	(7.858)
Year Dummies		No	No	No
Industry Dummies		Yes	Yes	Yes
Observations		1,338	1,338	1,338
Pseudo R-squared		0.177	0.177	0.165

continued…

Table 7, continued

Panel C: Proxies for information asymmetry, product market competition, and private benefits

	Multinomial Logit Base Case is RM			
	Prediction	Indicator for IPO (1)	Prediction	Indicator for Sellout (2)
Log of Sales	+	0.560***	-	0.048
		(7.436)		(0.666)
Foreign	-	-1.908***	-	-1.354***
		(-6.547)		(-5.206)
Dispersion of Multiples	-	0.007**		0.006**
		(2.343)		(2.134)
Market Volatility	-	-142.005***		-9.718
		(-2.620)		(-0.198)
Market Returns	+	0.017		0.007
		(1.050)		(0.489)
High Tech	-	0.700**		1.048***
		(2.054)		(3.244)
Small Business Optimism		-0.229***	-	-0.266***
		(-3.354)		(-4.213)
Industry Tobin's Q		0.180	-	-0.640**
		(0.575)		(-2.073)
Leader Market Share		-0.785	+	0.748
		(-1.169)		(1.139)
Private Benefits Dummy		-0.722*	-	-1.116***
		(-1.675)		(-2.577)
Family Name		0.258	-	0.704*
		(0.582)		(1.690)
Constant		24.125***		16.044**
		(3.239)		(2.392)
Year Dummies		Yes		Yes
Industry Dummies		Yes		Yes
Observations		1,318		1,318
Pseudo R-squared		0.251		0.251

Table 8: Analysis of wealth obtained in RMs compared to IPOs and sellouts, using matching methods

This table presents means and medians for *LN Relative Wealth*. The sample is 95 RMs from 2005 to 2010. Each RM firm is matched to an IPO firm (Panel A) or a sellout firm (Panel B) based on industry and sales or propensity score. *Relative Wealth* is the *Wealth to Sales* of the RM firm divided by the *Wealth to Sales* of the matched firm. The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero.

Panel A: RM firms matched to IPO firms

					LN Relative Wealth			
Matching Technique	Foreign Firms	Caliper	Replacement	N	Mean	Median	T-stat	Pct > 0
Industry & closest sales	Yes	N/A	N/A	67	-0.80	-0.71	-5.76	21%
Propensity score	Yes	1	With	93	-2.14	-1.80	-6.46	28%
Propensity score	Yes	0.01	With	58	-0.81	-0.97	-2.42	37%
Propensity score	Yes	1	Without	89	-0.70	-0.94	-2.56	44%
Propensity score	Yes	0.01	Without	49	-0.99	-0.97	-2.91	39%
Propensity score	No	1	With	54	-2.15	-1.73	-4.97	27%
Propensity score	No	0.01	With	37	-1.15	-1.03	-2.32	38%
Propensity score	No	1	Without	55	-0.85	-0.78	-2.00	38%
Propensity score	No	0.01	Without	35	-1.19	-1.03	-2.27	33%

Panel B: RM firms matched to sellout firms

					LN Relative Wealth			
Matching Technique	Foreign Firms	Caliper	Replacement	N	Mean	Median	T-stat	Pct > 0
Industry & closest sales	Yes	N/A	N/A	69	0.34	0.50	2.07	61%
Propensity score	Yes	1	With	95	0.04	-0.25	0.17	46%
Propensity score	Yes	0.01	With	75	0.07	-0.15	0.28	49%
Propensity score	Yes	1	Without	95	0.14	0.07	0.63	52%
Propensity score	Yes	0.01	Without	74	0.13	0.24	0.49	51%
Propensity score	No	1	With	56	0.69	0.45	2.25	59%
Propensity score	No	0.01	With	50	0.53	0.33	1.62	54%
Propensity score	No	1	Without	56	0.59	0.33	1.89	52%
Propensity score	No	0.01	Without	50	0.50	0.33	1.55	52%

Table 9: Analysis of wealth obtained in RMs compared to IPOs and sellouts, using Dunbar's (1995) method
This table presents statistics for *LN Wealth to Sales* and *LN Relative Wealth*. The sample is 95 RMs from 2005 to 2010. Forecasted *LN Wealth to Sales* is obtained using Dunbar's (1995) method. *LN Relative Wealth* is equivalent to Actual *LN Wealth to Sales* minus Forecasted *LN Wealth to Sales.* Forecasts are obtained from four samples of IPO firms or sellout firms: the full sample, domestic firms only, firms in the same industry within +/-40% of sales (Match 1 to Many), firm with the closest sales to an RM firm in an industry (Match 1 to 1). The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero.

Panel A: RM firms compared to IPO firms

		Actual *LN Wealth to Sales*	Forecasted *LN Wealth to Sales*	*LN Relative Wealth*	t-stat
Full Sample, N=93	Mean	1.17	2.29	-1.12***	-9.42
	Median	0.97	2.18	-1.10	
Domestic Firms Only, N=56	Mean	1.14	4.22	-3.08***	-19.24
	Median	0.96	4.45	-2.96	
Match 1 to Many, N=66	Mean	0.81	3.47	-2.66***	-19.43
	Median	0.65	3.47	-2.64	
Match 1 to 1, N=66	Mean	0.81	3.46	-2.65***	-18.27
	Median	0.65	3.25	-2.72	

Panel B: RM firms compared to sellout firms

		Actual *LN Wealth to Sales*	Forecasted *LN Wealth to Sales*	*LN Relative Wealth*	t-stat
Full Sample, N=95	Mean	1.13	2.13	-1.00***	-7.83
	Median	0.95	2.08	-1.09	
Domestic Firms Only, N=56	Mean	1.14	2.76	-1.62***	-8.91
	Median	0.96	2.69	-1.71	
Match 1 to Many, N=69	Mean	1.24	1.20	0.04	0.24
	Median	1.09	1.19	-0.07	
Match 1 to 1, N=69	Mean	1.24	0.37	0.86***	5.08
	Median	1.09	0.42	0.71	

Table 10: Cross Sectional analysis of *LN Relative Wealth*

This table presents the results from a regression (OLS) of *LN Relative Wealth* on RM firm characteristics. The dependent variable is *LN Relative Wealth*, calculated from either propensity score matching or Dunbar's (1995) method. The Match 1 to 1 sample includes RM firms plus a matched IPO (or sellout) firm, where matching is done based on two-digit SIC industry code and closest sales, as long as sales are within +/-40%. In Panel A (B), *LN Relative Wealth* is calculated with RM firms and IPO (sellout) firms. Private firms in RMs are referred to as "PrvCo" while the public company is referred to as "PubCo". Variables are described in Appendix B. Industry dummies are based on SIC divisions, which group two-digit SIC codes. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Panel A: RM firms and IPO firms

Method	Propensity Score Matching		Dunbar (1995) Method	
Sample	Full Sample	Match 1 to 1	Full Sample	Match 1 to 1
Dependent Variable	*LN Relative Wealth*	*LN Relative Wealth*	*LN Relative Wealth*	*LN Relative Wealth*
	(1)	(2)	(3)	(4)
Sales <= $50 million	0.679	-0.206	-0.060	-0.377
	(0.890)	(-0.623)	(-0.218)	(-1.021)
PubCo is a shell	-0.303	0.409	0.114	0.060
	(-0.387)	(1.108)	(0.380)	(0.151)
PubCo is an industry expert	-0.840	-0.249	-0.111	0.048
	(-1.289)	(-0.615)	(-0.456)	(0.143)
PrvCo hired a financial advisor	0.949	0.059	-0.262	-0.356
	(1.124)	(0.163)	(-0.850)	(-0.968)
PrvCo CEO has public company experience	1.249*	0.482	0.367	0.768***
	(1.694)	(1.543)	(1.480)	(2.741)
Log of Age	0.149	-0.164	0.035	0.170
	(0.388)	(-0.883)	(0.254)	(1.053)
Foreign	-1.109	0.114	-0.014	-1.084***
	(-1.347)	(0.355)	(-0.055)	(-3.062)
High Tech	0.074	0.623	0.261	0.455
	(0.071)	(1.065)	(0.778)	(1.278)
Non-Big 4 Auditor	-0.820	0.632	0.610*	0.773**
	(-0.950)	(1.625)	(1.907)	(2.076)
VC Backing	-0.087	0.478	-0.275	0.297
	(-0.123)	(1.182)	(-0.966)	(0.963)
Year Dummies	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes
Constant	-0.247	-1.154	-0.496	-3.287***
	(-0.094)	(-1.056)	(-0.563)	(-3.750)
Observations	92	67	93	66
R-squared	0.47	0.31	0.41	0.50

continued…

Table 10, continued

Panel B: RM firms and sellout firms

Method	Propensity Score Matching		Dunbar (1995) Method	
Sample	Full Sample	Match 1 to 1	Full Sample	Match 1 to 1
Dependent Variable	*LN Relative Wealth*	*LN Relative Wealth*	*LN Relative Wealth*	*LN Relative Wealth*
	(1)	(2)	(3)	(4)
Sales <= $50 million	1.913***	1.114**	0.123	0.080
	(3.587)	(2.494)	(0.384)	(0.189)
PubCo is a shell	-0.182	-0.367	0.097	-0.151
	(-0.341)	(-0.889)	(0.291)	(-0.328)
PubCo is an industry expert	0.111	-0.326	-0.109	-0.025
	(0.217)	(-0.753)	(-0.417)	(-0.065)
PrvCo hired a financial advisor	-0.570	-0.328	-0.119	-0.255
	(-0.998)	(-0.665)	(-0.338)	(-0.535)
PrvCo CEO has public company experience	0.340	-0.052	0.340	0.803*
	(0.715)	(-0.131)	(1.192)	(1.984)
Log of Age	0.204	-0.116	-0.141	-0.113
	(0.718)	(-0.509)	(-0.883)	(-0.530)
Foreign	0.687	-0.181	0.126	1.009**
	(1.154)	(-0.492)	(0.430)	(2.468)
High Tech	-0.521	0.436	0.164	0.156
	(-0.760)	(1.171)	(0.449)	(0.317)
Non-Big 4 Auditor	-0.467	0.287	0.339	0.364
	(-0.819)	(0.566)	(1.123)	(0.680)
VC Backing	1.032*	0.656*	0.375	0.765*
	(1.963)	(1.707)	(1.363)	(1.747)
Year Dummies	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes
Constant	-1.768	0.319	-0.888	0.698
	(-1.104)	(0.225)	(-0.918)	(0.578)
Observations	95	69	95	69
R-squared	0.39	0.46	0.36	0.42

Table 11: Private firm valuations, summary statistics

This table presents summary statistics of private firm valuations. Valuations are disclosed in the public company filings for 60 RMs. The valuations are typically done as part of a fairness opinion, written by a financial advisor (although in 8 cases, the board of directors provides the valuation). The valuation can include a discounted cash flow (DCF) analysis, a comparison to publicly traded firms (Comparables), and/or a similar transactions analysis (Transactions). Private firms in RMs are referred to as "PrvCo" while the public company is referred to as "PubCo". Panel A provides a frequency count. Panel B compares RM firms with and without valuations. Panel C provides summary statistics of valuations.

Panel A: Frequency counts

PrvCo has at least 1 valuation	60
PrvCo has DCF valuation	51
PrvCo has Comparables valuation	49
PrvCo has Transactions valuation	39
PrvCo has both DCF and Comparables	40
PrvCo has both DCF and Transactions	35
PrvCo has both Comparables and Transactions	38
PrvCo has all three valuation methods	34

Panel B: Comparison of RM firms with and without valuations

	RMs without valuations			*RMs with valuations*				
Variable	N	Mean	Median	N	Mean		Median	
Sales	50	41.65	9.47	60	100.57	***	39.49	**
Wealth to Sales	43	21.28	3.71	52	16.38		1.82	**
Age	50	6.24	5.50	60	16.95	***	6.50	**
PubCo is a shell	50	0.62		60	0.55			
VC Backing	50	0.40		60	0.48			
Foreign	50	0.50		60	0.28	**		

continued…

Table 11, continued

Panel C: Summary statistics of private firm valuations

	N	Mean	25th	Median	75th
Discounted Cash Flow					
Enterprise Value	51	249.61	73.05	179.35	399.55
Enterprise Value to Sales	44	8.41	1.36	2.82	10.41
Equity Value	51	209.35	62.95	145.50	343.00
Equity Value to Sales	44	7.15	1.07	2.37	7.48
Equity Wealth to Equity Value	51	0.59	0.17	0.48	0.75
Discount Rate	48	18.66	12.50	17.50	23.25
Comparables					
Enterprise Value	49	194.30	80.85	181.60	300.09
Enterprise Value to Sales	44	6.00	1.17	2.57	6.59
Equity Value	49	161.93	65.60	132.10	245.30
Equity Value to Sales	44	5.31	0.84	1.49	4.67
Equity Wealth to Equity Value	49	0.74	0.24	0.46	0.76
Transactions					
Enterprise Value	39	186.98	69.95	100.35	321.30
Enterprise Value to Sales	35	3.96	1.10	2.24	4.36
Equity Value	39	141.52	68.58	88.25	193.15
Equity Value to Sales	35	3.15	0.86	1.24	4.04
Equity Wealth to Equity Value	39	0.87	0.30	0.66	1.07

Table 12: Synergy and returns to public company shareholders in RMs

This table presents estimates of synergy generated in RMs as well as returns to public company stockholders. RMs are completed between 2005 and 2010. *Synergy* is calculated as the market value of the combined firm's equity minus the market value of the public firm's equity minus the equity value of the private firm minus private placement funds plus cash paid to private firm owners. Private firm valuations are based on a discounted cash flow (DCF) analysis, a comparison to publicly traded firms (Comparables), and/or a similar transactions analysis (Transactions). Private firms in RMs are referred to as "PrvCo" while the public company is referred to as "PubCo". Panel A presents *Synergy* calculated using different PrvCo and PubCo valuations. Panel B presents equity values of the PubCo and Combined Co's stock, as well as the raw return to PubCo shareholders.

Panel A: Synergy

	N	Mean	25th	Median	75th	% Negative
PubCo valuation is market value of the public company's stock						
Synergy using DCF	33	-107.74	-270.52	-74.02	12.50	0.70
Synergy using Comparables	29	-27.64	-138.10	-52.96	21.53	0.66
Synergy using Transactions	23	14.20	-119.84	-4.46	26.60	0.52
PubCo valuation is market value of the public company's stock and PrvCo valuation is the "Low" valuation						
Synergy using Low DCF	28	-39.08	-197.31	-38.54	22.22	0.68
Synergy using Low Comparables	27	51.11	-84.30	-15.62	44.24	0.56
Synergy using Low Transactions	23	80.03	-39.49	15.60	48.68	0.39
PubCo valuation is book value of assets						
Synergy using DCF	51	-93.57	-242.47	-58.85	13.83	0.73
Synergy using Comparables	49	-47.40	-150.94	-50.66	11.78	0.69
Synergy using Transactions	39	-25.38	-122.54	-22.55	19.82	0.62
PubCo valuation is book value of equity (assets – liabilities)						
Synergy using DCF	51	-65.95	-204.31	-38.49	14.54	0.71
Synergy using Comparables	49	-22.45	-134.06	-29.83	13.04	0.65
Synergy using Transactions	39	3.91	-102.90	-1.43	26.17	0.54

Panel B: Returns to PubCo Shareholders

	N	Mean	25th	50th	75th	t-stat
PubCo Market Value Before, $mil	56	87.80	18.21	48.04	92.68	
Combined Company Market Value, $mil	56	173.01	54.98	105.86	222.08	
Pct Owned by PubCo	56	38.76	24.25	34.41	49.00	
PubCo Return	56	0.19	-0.59	-0.17	0.51	1.33

Table 13: Private firm value inferred from private-private takeovers

This table presents data from an analysis of private firm value and synergy calculated using valuations obtained from takeovers of private firms by private firms. Panel A shows basic information on private-private takeovers from Pratt's Stats data. The takeovers are from 2005 to 2010 and include deals where the target has annual sales of $5 million or more. Panel B shows valuation data for 56 RM firms. For each private firm in a RM, the value of the firm is calculated based on industry price-to-sales multiples of private targets in private-private takeovers.

Panel A: Basic Information on Private-Private Takeover Sample

Number of private firm takeovers of other private firms	286
Number of two-digit SIC codes represented in the data	51
Average sales of the 286 private takeover targets (millions of dollars)	$29.90
Median sales of the 286 private takeover targets (millions of dollars)	$9.10

Panel B: Inferred Valuation and Synergy

	N	Mean	25th	Median	75th
Number of private-private takeovers in RM firm's industry	56	6.73	2.00	4.50	8.00
Median industry price-to-sales ratio for RM firm's industry	56	0.83	0.54	0.73	1.00
Inferred value from private-private takeovers ($millions)	56	42.19	2.04	25.95	62.10
Synergy, calculated with inferred value ($millions)	36	45.08	-22.01	20.60	103.89

Table 14: Cross-sectional evidence on synergy and returns to firms in RMs

OLS regression results are presented. RMs are completed between 2005 and 2010. The dependent variables are *Synergy* calculated using DCF valuations in column (1) and *Synergy* calculated using private-private takeover industry valuations in column (2). *Synergy* is the market value of the combined firm's equity minus the market value of the public firm's equity minus the equity value of the private firm minus private placement funds plus cash paid to private firm owners. The constant is not reported. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

Dependent Variable	(1) *Synergy, DCF*	(2) *Synergy, Private-private*
PubCo is a shell	52.377	-7.254
	(0.627)	(-0.201)
PubCo is an industry expert	220.392*	15.258
	(2.023)	(0.408)
PrvCo CEO has public company experience	197.193**	23.403
	(2.793)	(0.953)
PubCo has RM experience	-154.490	-18.760
	(-1.264)	(-0.533)
PubCo managers/directors remain	163.215	-41.915
	(1.769)	(-0.976)
PubCo operations remain	-208.068	23.270
	(-1.400)	(0.905)
Small Business Optimism	-1.225	9.758*
	(-0.058)	(1.732)
Industry Operating Margins	7.877**	0.257
	(2.229)	(0.606)
Industry Tobin's Q	189.975**	12.600
	(2.820)	(0.665)
Market Returns	9.198**	1.851
	(2.937)	(1.405)
Market Volatility	1,148.809	4,612.228
	(0.074)	(1.294)
Log of Age	41.235	-14.130
	(1.017)	(-0.974)
Foreign	226.600*	0.788
	(1.947)	(0.026)
High Tech	-126.975	6.822
	(-0.986)	(0.202)
Non-Big 4 Auditor	124.745	40.593
	(1.254)	(1.232)
VC Backing	195.450	73.820**
	(1.227)	(2.450)
Year Dummies	Yes	Yes
Observations	33	52
R-squared	0.80	0.57

Appendix C: Supplemental Results

This appendix contains additional results which are discussed in the text, but not presented above. An outline of the tables contained in Appendix C is given below. The tables in Appendix C are numbered C-1, C-2, C-3, etc.

Table	Description
C-1	Summary statistics for RM, IPO, and sellout samples
C-2	Analysis of Propensity Score Match Quality for RMs and IPOs
C-3	Analysis of Propensity Score Match Quality for RMs and sellouts
C-4	Dunbar (1995) Method, intermediate steps
C-5	Analysis of wealth obtained in RMs compared to IPOs using alternatives of *Equity Wealth*
C-6	Analysis of wealth to assets of RM firms and IPO firms
C-7	Analysis of wealth to assets obtained in RMs compared to sellouts where consideration is not all cash

Table C-1: Summary Statistics

This table shows summary statistics for samples of RMs (Panel A), IPOs (Panel B), and sellouts (Panel C). The variables are described in Appendix B. Variables are winsorized at the 1st and 99th percentiles separately for RMs, IPOs, and sellouts.

Panel A: RMs

	N	Mean	Min	25th	50th	75th	Max
Sales	110	73.79	0.00	3.19	26.68	79.99	562.99
Wealth	110	151.76	5.58	41.4	86.1	187.96	1533.36
Wealth to Sales	95	18.6	0.15	0.77	2.58	9.28	370.85
Small Business Optimism	110	95.38	84.1	89.3	96.6	99.8	105.9
Dispersion of Multiples	108	50.12	0.19	1.66	19.4	99.89	178.04
Market Returns	110	4.08	-35.59	0.12	5.61	11.15	32.67
Market Volatility	110	0.01	0.00	0.01	0.01	0.01	0.03
Leader Market Share	110	0.39	0.10	0.24	0.33	0.5	1.00
Industry Tobin's Q	110	1.82	0.91	1.30	1.70	2.26	3.65

Panel B: IPOs

	N	Mean	Min	25th	50th	75th	Max
Sales	455	375.03	0.00	27.05	85.02	305.62	7833.37
Wealth	455	537.24	0.00	149.69	281.12	635.79	4575.15
Wealth to Sales	426	28.07	0.00	1.10	3.03	8.09	809.75
Small Business Optimism	455	97.21	86.5	94.6	98.2	100.8	106.1
Dispersion of Multiples	451	49.21	0.16	2.06	23.52	91.65	198.06
Market Returns	455	8.36	-12.95	2.81	7.91	11.91	44.96
Market Volatility	455	0.01	0.00	0.01	0.01	0.01	0.02
Leader Market Share	455	0.36	0.10	0.20	0.33	0.47	0.97
Industry Tobin's Q	455	1.84	0.78	1.40	1.73	2.23	3.27

Panel C: Sellouts

	N	Mean	Min	25th	50th	75th	Max
Sales	805	65.56	0.06	5.99	17.46	50.81	1025.07
Wealth	801	116.05	1.78	15.03	38.58	115.22	1517.04
Wealth to Sales	801	30.84	0.10	0.97	2.00	4.41	1443.08
Small Business Optimism	805	95.64	81.00	91.1	96.7	100.7	106.1
Dispersion of Multiples	759	45.86	0.17	1.73	30.43	77.69	174.27
Market Returns	805	2.06	-41.09	-2.54	4.41	9.09	32.98
Market Volatility	805	0.01	0.00	0.01	0.01	0.01	0.04
Leader Market Share	773	0.42	0.11	0.3	0.35	0.52	1.00
Industry Tobin's Q	773	1.79	0.91	1.4	1.72	2.19	3.15

Table C-2: Analysis of Propensity Score Match Quality for RMs and IPOs

This table shows coefficients from probit regressions from three samples. The dependent variable is a dummy variable which equals 1 for RMs and 0 for IPOs. Sellouts are excluded in this analysis. In column (1), the sample is all RMs and IPOs. In column (2), the sample is RMs plus propensity score matched IPOs. Each RM is matched to the IPO with the closest propensity score. In column (2), the sample is RMs plus propensity score matched IPOs where each RM is matched to the IPO with the closest propensity score and the difference in propensity scores cannot exceed 0.01 in absolute value. Variables are defined in Appendix B. Industry dummies are at the SIC division level. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

	(1) Full Sample	(2) Propensity Score Matched Caliper=1	(3) Propensity Score Matched Caliper=0.01
	RM=1	RM=1	RM=1
Log of Sales	-0.307***	-0.174*	-0.074
	(-5.580)	(-1.670)	(-0.637)
Log of Age	-0.079	0.030	0.133
	(-0.751)	(0.189)	(0.774)
Foreign	0.809***	0.538*	0.243
	(3.635)	(1.826)	(0.678)
Tangible Assets	-0.871**	-0.271	0.032
	(-2.159)	(-0.452)	(0.049)
R&D Dummy	-1.173***	-0.477	0.041
	(-4.324)	(-1.147)	(0.081)
Non-Big 4 Auditor	0.560***	0.258	0.204
	(3.126)	(0.847)	(0.647)
VC Backing	-0.471**	-0.412	-0.304
	(-2.560)	(-1.504)	(-0.982)
Dispersion of Multiples	0.001	0.000	0.001
	(0.413)	(0.076)	(0.206)
Market Volatility	32.701	20.178	-11.224
	(1.032)	(0.477)	(-0.206)
Market Returns	-0.017*	-0.010	-0.001
	(-1.943)	(-0.748)	(-0.054)
High Tech	-0.042	-0.189	-0.144
	(-0.205)	(-0.557)	(-0.358)
Constant	1.390*	0.956	-0.236
	(1.653)	(0.983)	(-0.188)
Year Dummies	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes
Observations	557	138	105
Pseudo R-squared	0.421	0.133	0.0398

Table C-3: Analysis of Propensity Score Match Quality for RMs and sellouts

This table shows coefficients from probit regressions from three samples. The dependent variable is a dummy variable which equals 1 for RMs and 0 for sellouts. IPOs are excluded in this analysis. In column (1), the sample is all RMs and sellouts. In column (2), the sample is RMs plus propensity score matched sellouts. Each RM is matched to the sellout with the closest propensity score. In column (2), the sample is RMs plus propensity score matched sellouts where each RM is matched to the sellout with the closest propensity score and the difference in propensity scores cannot exceed 0.01 in absolute value. Variables are defined in Appendix B. Industry dummies are at the SIC division level. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

	(1) Full Sample	(2) Propensity Score Matched Caliper=1	(3) Propensity Score Matched Caliper=0.01
	RM=1	RM=1	RM=1
Log of Sales	-0.057	0.017	0.032
	(-1.333)	(0.226)	(0.394)
Foreign	0.713***	0.293	0.029
	(5.208)	(1.272)	(0.110)
Small Business Optimism	0.148***	0.012	-0.028
	(4.139)	(0.203)	(-0.451)
Industry Tobin's Q	0.203	0.047	-0.106
	(1.340)	(0.165)	(-0.344)
Leader Market Share	-0.298	0.043	-0.017
	(-0.925)	(0.074)	(-0.027)
Private Benefits Dummy	0.452**	0.136	0.003
	(1.969)	(0.373)	(0.008)
Family Name	-0.401*	-0.365	-0.187
	(-1.862)	(-0.982)	(-0.499)
Market Returns	-0.004	-0.008	-0.003
	(-0.411)	(-0.655)	(-0.224)
Market Volatility	2.376	-28.732	-16.991
	(0.078)	(-0.780)	(-0.409)
High Tech	-0.611***	-0.257	-0.159
	(-3.743)	(-0.810)	(-0.494)
Constant	-11.440***	3.376	3.156
	(-2.902)	(0.527)	(0.483)
Year Dummies	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes
Observations	883	164	142
Pseudo R-squared	0.2450	0.0570	0.0295

Dunbar (1995) Method

The method begins with a probit regression to estimate the likelihood that a firm chooses a RM or an IPO (Table C-4, column (1)). The specification of the probit is the same as the model in Table 5, column (1), except that the dependent variable is equal to 1 for IPOs and 0 for RMs to simplify later calculations. Also, the specification adds a dummy variable that indicates whether or not the firm's stock is listed on NYSE. This variable is included in order to meet the exclusion restriction. The justification for this variable is that the listing of a firm's stock on NYSE is much more common for IPO firms than RM firms. Also, the choice of listing exchange (NYSE, NASDAQ, or AMEX) should not affect the wealth of private firm owners since all three listing exchanges are competitive markets to value the firm's stock. However, the choice of listing exchange could affect the wealth of the firm's owners because of correlation with unobserved factors that affect the decision to execute an IPO or RM (e.g., firm quality).

In the second step, the coefficients from the probit regression are used to estimate the inverse Mills ratio for each observation.

Third, I regress *LN Wealth to Sales* of each IPO firm on factors that affect the firm's valuation as well as the inverse Mills ratio (Table C-4, column (2)). This OLS regression is estimated on the sample of IPO firms only and the independent variables include those in the probit except the NYSE dummy. The inverse Mills ratio is included in the OLS regression to control for unobservable factors that affect both the decision to execute an IPO and the valuation of IPO firms.

Fourth, the coefficients of the OLS estimation of IPO firm valuation are applied to the data on RM firms to calculate a forecast of *LN Wealth to Sales* for each RM firm, had that firm chosen an IPO. The forecast of *LN Wealth to Sales* is the product of the OLS regression coefficients and the independent variables for each RM observation. As in Dunbar (1995), the inverse Mills ratio is not used in this calculation since its purpose is to control for selection bias.

The procedure is repeated for RMs and sellout firms. The regressions for the full sample of firms are shown in Table C-4, columns (3) and (4).

Table C-4: Dunbar (1995) methodology, intermediate steps

This table shows the results from intermediate steps of the Dunbar (1995) procedure on a sample of RM and IPO firms (columns (1) and (2)) and RM and sellout firms (columns (3) and (4)). Columns (1) and (3) are probit regressions and columns (2) and (4) are OLS. Variables are defined in Appendix B. Industry dummies are at the SIC division level. t-stats are reported in parentheses and significance is indicated by ***, **, * at the 1%, 5%, and 10% levels respectively.

	(1)	(2)		(3)	(4)
Sample	RMs & IPOs	IPOs Only		RMs & Sellouts	Sellouts Only
Dependent Variable	IPO=1	*LN Wealth to Sales*		Sellout=1	*LN Wealth to Sales*
Log of Sales	0.260***	-0.720***	Log of Sales	-0.018	-0.553***
	(4.594)	(-18.051)		(-0.374)	(-16.370)
Log of Age	0.064	-0.127**	Foreign	-0.858***	0.283**
	(0.564)	(-2.010)		(-5.718)	(2.119)
Foreign	-0.874***	0.387**	Small Business Optimism	-0.145***	0.031
	(-3.809)	(2.269)		(-3.767)	(1.047)
Tangible Assets	0.565	0.084	Industry Tobin's Q	-0.157	0.357***
	(1.290)	(0.335)		(-0.978)	(2.835)
R&D Dummy	1.228***	0.097	Leader Market Share	0.384	-0.732***
	(4.460)	(0.624)		(1.104)	(-2.786)
Non-Big 4	-0.397**	-0.493***	Private Benefits Industry	-0.509**	0.473**
	(-2.084)	(-3.651)		(-2.113)	(2.154)
VC Backing	0.493**	0.582***	Family Name	0.321	0.096
	(2.551)	(4.362)		(1.353)	(0.655)
Dispersion of Multiples	-0.001	0.001	Market Returns	0.004	-0.002
	(-0.534)	(0.662)		(0.424)	(-0.176)
Market Volatility	-34.208	-36.960	Market Volatility	0.591	-26.525
	(-1.060)	(-1.362)		(0.021)	(-1.094)
Market Returns	0.011	-0.006	High Tech	0.645***	0.213*
	(1.214)	(-1.003)		(3.809)	(1.763)
High Tech	0.040	-0.116			
	(0.190)	(-1.040)			
NYSE	1.460***		NYSE	1.805***	
	(3.426)			(5.943)	
Inverse Mills Ratio		1.056	Inverse Mills Ratio		3.630***
		(0.931)			(3.116)
Year Dummies	Yes	Yes	Year Dummies	Yes	Yes
Industry Dummies	Yes	Yes	Industry Dummies	Yes	Yes
Constant	-1.285	4.420***	Constant	11.237***	0.540
	(-1.502)	(4.694)		(2.659)	(0.182)
Observations	557	415	Observations	883	770
Pseudo R^2/ R^2	0.45	0.73	Pseudo R^2/ R^2	0.33	0.39

Reference: Dunbar, Craig, 1995, The Use Of Warrants As Underwriter Compensation In Initial Public Offerings, *Journal of Financial Economics* 38, 59-78.

Table C-5: Analysis of wealth obtained in RMs compared to IPOs

This table presents means and medians for *LN Relative Wealth*. *Relative Wealth* is the *Wealth to Sales* of the RM firm divided by the *Wealth to Sales* of the matched IPO firm. The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero.

		LN Relative Wealth				
Matching Technique	Measurement of market value	N	Mean	Median	T-stat	Pct > 0
Industry & closest sales	First trade on CRSP	67	-0.80	-0.71	-5.76	21%
Industry & closest sales	30 days after first trade on CRSP	67	-0.87	-0.69	-5.64	27%
Industry & closest sales	270 days after deal completion (RM) or offering (IPO)	59	-0.94	-0.78	-5.35	22%

Table C-6: Analysis of wealth to assets obtained in RMs compared to IPOs, using matching methods
Panel A presents means and medians for *LN Relative Wealth*. The sample is 106 RMs from 2005 to 2010. Each RM firm is matched to an IPO firm based on industry and assets or propensity score. *Relative Wealth* is the *Wealth to Assets* of the RM firm divided by the *Wealth to Assets* of the matched firm. The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero. Panel B presents statistics for *LN Wealth to Assets* and *LN Relative Wealth*. Forecasted *LN Wealth to Assets* is obtained using Dunbar's (1995) method. *LN Relative Wealth* is equivalent to Actual *LN Wealth to Assets* minus Forecasted *LN Wealth to Assets*. Forecasts are obtained from four samples of IPO firms or sellout firms: the full sample, domestic firms only, firms in the same industry within +/-40% of assets (Match 1 to Many), firm with the closest assets to an RM firm in an industry (Match 1 to 1). The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero.

Panel A: RM firms matched to IPO firms, scale by assets

					LN Relative Wealth			
Matching Technique	Foreign Firms	Caliper	Replacement	N	Mean	Median	T-stat	Pct > 0
Industry & closest assets	Yes	N/A	N/A	70	-0.85	-0.79	-5.27	32%
Propensity score	Yes	1	With	106	0.75	-0.87	-3.78	37%
Propensity score	Yes	0.01	With	64	-0.33	-0.30	-1.48	41%
Propensity score	Yes	1	Without	103	-0.18	-0.49	-0.85	43%
Propensity score	Yes	0.01	Without	49	-0.63	0.60	-2.79	32%
Propensity score	No	1	With	63	-0.72	-0.71	-3.02	36%
Propensity score	No	0.01	With	40	-0.69	-0.70	-2.65	29%
Propensity score	No	1	Without	66	-0.15	-0.62	-0.47	37%
Propensity score	No	0.01	Without	38	-0.76	-0.70	-2.66	26%

Panel B: Dunbar (1995) Method, RM firms compared to IPO firms

		Actual	Forecasted		
		LN Wealth to Assets	*LN Wealth to Assets*	*LN Relative Wealth*	t-stat
Full Sample, N=106	Mean	1.15	1.98	-0.82***	-6.82
	Median	0.87	1.97	-0.77	
Domestic Firms Only, N=67	Mean	1.36	1.77	-0.41***	-2.65
	Median	0.97	1.89	-0.30	
Match 1 to Many, N=71	Mean	0.87	3.04	-2.16***	-13.77
	Median	0.66	3.13	-2.15	
Match 1 to 1, N=71	Mean	0.87	4.15	-3.27***	-21.12
	Median	0.66	4.31	-3.24	

Table C-7: Analysis of wealth to assets obtained in RMs compared to sellouts where consideration is not all cash
This table presents means and medians for *LN Relative Wealth*. The sample is 110 RMs from 2005 to 2010. Each RM firm is matched to a sellout firm based on propensity score. The sample of sellouts includes 421 deals where the consideration was not all cash. The t-stat is from a test of whether or not the mean of *LN Relative Wealth* equals zero.

					LN Relative Wealth			
Matching Technique	Foreign Firms	Caliper	Replacement	N	Mean	Median	T-stat	Pct > 0
Propensity score	Yes	1	With	95	0.64	0.50	2.95	62%
Propensity score	Yes	0.01	With	70	0.43	0.23	1.61	56%
Propensity score	Yes	1	Without	95	0.41	0.27	1.77	55%
Propensity score	Yes	0.01	Without	35	0.64	0.31	1.77	57%
Propensity score	No	1	With	56	0.47	0.25	1.80	54%
Propensity score	No	0.01	With	44	0.78	0.75	2.53	64%
Propensity score	No	1	Without	56	0.61	0.45	2.33	57%
Propensity score	No	0.01	Without	35	0.64	0.31	1.77	57%