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Hometown Biased Acquisitions

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Abstract

We show that chief executive officers (CEOs) exhibit a hometown bias in acquisitions. Firms are over twice as likely to acquire targets located in the states of their CEOs' childhood homes than similar targets domiciled elsewhere. Small, private home-state deals underperform other small, private deals, and the bias is stronger when acquirer governance is lax, suggesting that CEOs acquire private home-state targets for their own benefits. In contrast, large, public home-state acquisitions are value enhancing. CEOs create value in public home-state acquisitions by avoiding extremely poor deals and through deals with higher synergies. Thus, both agency issues and hometown advantages drive home-state acquisitions.

I. Introduction

There is a large literature in economics and finance on home bias. Most individual investors hold the majority of their portfolios in domestic securities (French and Poterba (1991), Tesar and Werner (1995), and Kang and Stulz (1997)), and those domestic securities are often held in "local" firms (Grinblatt and Keloharju (2001), Ivković and Weisbenner (2005), and Seasholes and Zhu (2010)). Professional money managers also tilt their portfolios toward local (Coval and Moskowitz (1999)) and home-state (Pool, Stoffman, and Yonker (2012)) stocks. Institutional investors, especially public pension funds, exhibit similar bias when allocating their investments in private equity (Hochberg and Rauh (2013)). While much of the evidence from this literature shows that investors are home biased in their *financial* investments, we ask whether corporate managers also exhibit this bias when making *real* investments. Specifically,

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we investigate whether chief executive officers (CEOs) are more likely to acquire targets in the states where they grew up and, if so, what are the causes and consequences of this behavior?

There are a number of reasons CEOs may be more likely to acquire firms from their home states.[1] The literature on local bias finds evidence that both information (Coval and Moskowitz (2001), Ivković and Weisbenner (2005)) and familiarity (Huberman (2001), Grinblatt and Keloharju (2001), Seasholes and Zhu (2010), and Pool et al. (2012)) drive investors to make biased portfolio decisions. These channels could also play a role in acquisitions; however, acquisition decisions and portfolio allocation decisions are different in important ways. Namely, in acquisitions, executives play active roles in the execution of the deal, requiring managers to reallocate the resources of acquired firms. If CEOs are better at this reallocation with firms near their homes, the acquisitions could create value. However, since corporate acquisitions are made by *delegated* managers, there is also a potentially large role for agency conflicts in these investments. We, therefore, broadly categorize the motives for hometown acquisitions into *home advantages* and *agency conflicts*.

Under the *home advantage hypothesis*, CEOs are likely to acquire targets in their home states to exploit their geography-specific skills and/or advantages. Home advantages could manifest as *ex ante* information; that is, CEOs could have better knowledge of the prospects of target firms near their hometowns.[2] This might provide hometown CEOs with advantages in identifying suitable targets that otherwise would not have been pursued. This idea is similar in spirit to Van Nieuwerburgh and Veldkamp (2009), who provide a general equilibrium model that leads to home bias whereby agents specialize in local investments in which they have an initial information advantage. In the model, an initial information advantage in local stocks sparks further investment in information acquisition in local stocks, which creates more precise signals in these securities, and ultimately leads to home bias. While most of the literature documents local advantages based on where agents currently reside (e.g., Bae, Stulz, and Tan (2008), Coval and Moskowitz (2001), and Malloy (2005)), it is likely that agents also have more information about the places where they grew up than other areas, on average.[3] Home advantages could also come *ex post*. Following acquisitions, hometown CEOs may be more effective at executing deals by reducing the costs of integration.[4] Hometown CEOs are likely to understand local culture better, allowing them

[1] Throughout this paper, we refer to the state where the CEO grew up as the CEO's home state. We identify home states using Social Security data, as explained later.

[2] This is most similar to the "information" story proposed in the traditional home bias literature.

[3] Examples of potential sources of information advantage include familiarity with local culture, close contacts (family, friends, and classmates), and frequent visits.

[4] Industry practitioners and anecdotal evidence suggest that many deals fail because of large post-acquisition integration costs arising from barriers such as cultural differences and communication costs. Graham, Harvey, Popadak, and Rajgopal (2016) report that "cultural fit in merger and acquisition (M&A) deals is so important that 54% of executives would walk away from a target that is culturally misaligned" (p. 2). Fiordelisi, Li, Stentella-Lopes, and Ricci (2016) find that differences in corporate culture have important impact on M&As. High profile deals that supposedly failed due to corporate culture clashes include Daimler-Chrysler and Sprint-Nextel, transactions that ended up destroying billions of dollars in shareholder value (Bouwman (2013)).

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to communicate more effectively with both management and labor of target firms. In turn, this enables them to fully comprehend the efficiencies and inefficiencies of their targets, ultimately leading to more efficient integration of combined firms. Overall, under the *home advantage hypothesis*, we would expect a greater propensity to acquire home-state targets (Van Nieuwerburgh and Veldkamp (2009)) and that home-state acquisitions create shareholder value for the bidders.

Under the *agency conflicts hypothesis*, CEOs acquire hometown targets in pursuit of private benefits. The potential private benefits CEOs could receive from these investments are numerous and could stem from either direct monetary benefits or from the utility CEOs receive from helping people near their homes. For example, purchasing a distressed hometown company may increase the CEO's (or the CEO's family's) status or popularity in his home state. Increased status/popularity could lead to monetary benefits, such as paid speaking engagements or increased access to government officials. This same action could be motivated by less cynical reasons. For instance, CEOs may have more empathy for workers in their hometowns and try to "rescue" workers in trouble near their homes. There is both anecdotal and empirical evidence of this type of behavior. For example, in Nov. 2011, Berkshire Hathaway Inc. purchased the Omaha World-Herald Co., Warren Buffett's ailing hometown newspaper (Holm and Stewart (2011)). Additionally, Yonker (2017b) shows evidence that affective bonds that CEOs establish with their homes drive them to treat workers from their hometowns better than workers located elsewhere. In the sociology and environmental psychology literatures, these bonds are known as "place attachments" or "place identity" (Fisher, Gerson, and Stueve (1977), Altman and Low (1992)), and Hidalgo and Hernandez (2001)). We could also liken this latter reason to corporate philanthropy (whereby managers "donate" shareholder money to their hometowns), which has been shown to increase with agency conflicts (Masulis and Reza (2015), Cheng, Hong, and Shue (2014)). Finally, if CEOs enjoy spending time near their homes, then acquiring a target nearby gives them an excuse to use company resources to do so.[5]

Similar to the *home advantage hypothesis*, the *agency conflicts hypothesis* also predicts that CEOs are more likely to acquire targets in their home states. However, the *agency conflicts hypothesis* predicts that the biased acquisitions in CEO home states underperform because they are likely to be driven by agency issues. We recognize, however, that these two hypotheses are not mutually exclusive. Both channels might be at work simultaneously and which channel dominates may depend on the potential costs and incentives in the deal. We examine acquisition performance to distinguish between these two hypotheses, which is similar to the methodology used by Hochberg and Rauh (2013).

We begin the analysis by testing whether firms have an abnormal propensity to acquire target firms near their CEOs' homes. We follow an empirical strategy similar to that of Pool et al. (2012), who rely on CEO states of origin to identify CEO homes. These data identify CEO home states based on the states in

[5]This is consistent with Yonker (2017a), who shows that CEOs exhibit geographic preferences for their homes.

which CEOs registered with the Social Security Administration.[6] Our strategy effectively tests whether a firm located in New York with a CEO who grew up in Iowa is more likely to acquire targets based in Iowa. We include industry-year fixed effects to control for merger waves (Harford (2005)) and bidder and target headquarters state-pair-year fixed effects to control for all observed and unobserved time-varying state-specific and state-pair-specific factors that affect acquisition propensity. For instance, these fixed effects absorb the effects of physical or cultural distance between target and acquirer headquarters states. So, if auto firms in Michigan are more likely to acquire auto parts firms in Ohio, then our specification controls for this. These fixed effects also control for differences in sizes, populations, economic conditions, and legal environments between acquirer and target headquarters states, among other state and state-pair level factors that could influence acquisitions.

Estimates from our baseline specification imply that firms are 130% more likely to acquire targets from their CEOs' home states than similar targets from other states. In our most stringent specification, which is identified by within-firm variation in CEO states of origin, we estimate that firms are over 80% more likely to acquire targets located in their CEOs' home states than similar targets located elsewhere. Moreover, the bias is driven by cross-state acquisitions. When combined, these findings confirm that it is indeed a manager and not a firm effect driving the results. We further show that these results are unlikely to be driven by firms selecting CEOs to exploit their home-state acquisition skills.

We further find the acquisition bias is more pronounced when CEOs have strong connections in their home states. In particular, we report evidence that the bias is stronger for CEOs who lived longer in their home states and for those who sit on the boards of firms located in their home states.

Having established the existence of a hometown bias in corporate acquisitions, we next ask what drives it. Recognizing that both motivations for home-state acquisitions can coexist, we split the sample based on private and public targets. The mergers and acquisitions (M&A) literature establishes that acquisitions of public and private targets differ in their frequency, deal size, public attention, and acquisition performance (e.g., Fuller, Netter, and Stegemoller (2002), Betton, Eckbo, and Thorburn (2008), and Bhagwat, Dam, and Harford (2016)). If CEOs were to invest in home-state targets for personal benefits, the ideal targets for that purpose should be private firms due to the smaller size of and lower attention given to private deals.[7] Warren Buffett's purchase of his hometown newspaper, discussed previously, is a nice example of such an acquisition. This particular acquisition would not have a major impact on the bottom line of Berkshire Hathaway, so it went largely unchecked.

When splitting the sample between private and public targets, we find evidence of a hometown bias in each sample. In addition, we find that the hometown bias in private deals is intensified in firms with weaker corporate governance,

[6] Yonker (2017a) shows that for over 80% of CEOs, this identifies where they lived during their teenage years.

[7] A recent paper by Biggerstaff, Cicero, and Puckett (2015) finds unethical CEOs are more likely to acquire private (not public) companies to perpetuate their financial frauds.

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as measured by the agency index of Duchin and Sosyura (2013). Meanwhile, we find no such effect for public target deals. This initial evidence is supportive of private (but not public) hometown deals being driven primarily by agency conflicts.

To further understand the motivation for the hometown bias in acquisitions, we investigate the stock market reaction of bidders around acquisition announcements. If agency conflicts indeed drive the hometown bias in private target deals, then we expect to see unfavorable market reactions to hometown private deals. After controlling for a variety of firm and deal characteristics, we find that acquirers' announcement returns in hometown private deals underperform by 1.5%. This translates into $22 million lower shareholder value for the average bidder in our sample. Consistent with the earlier finding that acquisition bias in private deals is stronger in weakly governed firms, we find evidence that the unfavorable market reactions in private hometown deals are primarily driven by bidders with weaker governance. These results are consistent with private hometown deals being driven by agency conflicts.

In contrast, the market reaction to public hometown deals suggests it is unlikely that agency conflicts drive these deals. Unlike other public deals that, on average, experience negative announcement returns, public hometown deals create value for acquiring firms. After controlling for a variety of firm and deal characteristics, we find that acquirers in public hometown deals have 2% higher abnormal announcement returns. This translates into about $100 million more shareholder value for the average bidder in the public deal sample. This more favorable market reaction toward public hometown deals is consistent with our conjecture that home advantages lead to better public hometown acquisition decisions.

One natural follow-up question is what drives the difference in performance between public and private hometown deals. Deal size in public deals is about 10 times that in private deals in our sample, so the costs (of conducting a bad acquisition) and incentives (to make the deal work) are much larger for public acquisitions. We report evidence that deal size contributes to the difference in performance between private and public hometown deals. Large public hometown deals outperform their corresponding non-hometown deals the most.

Given the importance of public deals in M&A studies, we conduct additional tests to gain more insights regarding the sources of value creation in public hometown deals. First, we use CEO resident information to shed some light on the channel through which CEOs create value in public hometown deals. We expect that if CEOs have an information advantage about firms in their home states, this advantage will be stronger if they still have a physical presence in the state. Specifically, we investigate whether CEOs owning second homes in their home states achieve better acquisition returns than those that do not. These CEOs are actually local investors in the Coval and Moskowitz (2001) sense and will be privy to the same sources of information. We find evidence supporting this conjecture. Moreover, we show that the closer the CEOs' second homes are to the target headquarters, the higher the announcement returns. So, for example, having a home in Los Angeles may not improve the acquisition of a target in Silicon Valley.

The implication from Van Nieuwerburgh and Veldkamp (2009) is that noise in the information on hometown deals should be lower than in other deals. This

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could imply that CEOs should be able to avoid extremely bad deals in their home states. We test this conjecture. The univariate statistics are striking: not a single public hometown deal experiences a loss of $1 billion or more based on the 3-day cumulative abnormal returns (CAR), whereas about 10% of other public deals do. Multiple regression analysis yields similar findings.

One way that managers could create value in acquisitions is by identifying targets that they could buy cheaply.[8] We test whether CEOs are able to buy hometown targets at a discount and find no evidence that bidders pay lower takeover premiums or that targets have lower announcement returns in hometown deals than other deals. Instead, we find that the abnormal announcement returns of combined firms (value-weighted average of target and bidder abnormal returns) are significantly greater in public hometown deals. This is consistent with markets anticipating higher synergies or less costly integration for hometown deals.

To test whether public hometown deals realize these anticipated synergies, we investigate post-acquisition operating performance. We find that changes in return on assets (ROA) in hometown deals are higher than other deals in the 3 years following the acquisitions. These findings are suggestive that CEOs are better at post-acquisition resource reallocations between target and acquiring firms or identifying targets with higher synergies in hometown deals.

We make contributions to three areas of the finance literature. First, we contribute to the literature on home bias by documenting the existence of home bias in the domain of *real* investments. We highlight the difference in the motivating factors of home bias in real vs. financial investments, which could provide a basis for future work. Moreover, while most existing studies of home bias focus on overweighting investments near investors (i.e., local bias),[9] we document a related but different type of home bias that is due to top managers' attachment to and relation with the states where they grew up. We also shed light on the causes and consequences of the hometown bias in M&A. Our results are intriguing: Hometown deals are value enhancing for large public acquisitions but not for small private deals. Our paper is the first to show that managerial hometown bias can be either an agency problem or a valuable skill, depending on the amount of attention and scrutiny the transaction receives.[10] Our results suggest that corporate monitors should be leery of small private hometown deals but should also recognize the existence of home advantages.

Second, we contribute to the M&A literature by showing an important additional determinant of acquisition behavior: CEO origin. We recognize Chung, Green, and Schmidt (2016) as independent, contemporaneous work that also documents a hometown bias in corporate acquisitions using the alternative method of Rhodes-Kropf and Robinson (2008). Consistent with this paper, Chung et al. find that the bias is driven by cross-state acquisitions, intensified by weak

[8]Due to data availability, the rest of the tests (on target premium, combined synergies, and post-acquisition ROA changes) can be done only for public acquisitions.

[9]The exceptions are Pool et al. (2012) and Yonker (2017b).

[10]In comparison, existing studies on home/local bias find evidence for either information (Coval and Moskowitz (2001), Ivković and Weisbenner (2005)), familiarity (Huberman (2001), Grinblatt and Keloharju (2001), Seasholes and Zhu (2010), and Pool et al. (2012)), or political pressure (Hochberg and Rauh (2013)) as the motive, but not different driving forces under different conditions.

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governance, and that home-state acquisitions underperform. They conclude that agency conflicts drive the bias. We find similar evidence and draw similar conclusions for acquisitions of private hometown deals, but we also show evidence for the *home advantage* hypothesis for public hometown acquisitions, consistent with the theory of Van Nieuwerburgh and Veldkamp (2009). This latter result is an important contribution to the M&A literature. Public acquisitions generally destroy shareholder value of acquiring firms. There is sparse evidence of systematic outperformance in acquisitions of public targets (see, e.g., Cai and Sevilir (2012), Custodio and Metzger (2013)).

We identify a new factor that appears to lead to value-enhancing deals for acquirers and show suggestive evidence of the mechanism: CEOs avoid extremely poor deals and create deals with higher synergies in their home states.

Finally, the paper adds to the growing literature in behavioral corporate finance on the impact of managers on corporate decisions.[11] Most relevant in this literature is Malmendier and Tate (2005), (2008), who show that overconfident CEOs overinvest in both capital expenditures and in value-destroying acquisitions. We show that CEOs' attachments to and relationships with the areas where they grew up influence which firms they target in acquisitions.

II. Data and Sample Description

We rely on three main databases to construct the sample in our study: i) Securities Data Company's (SDC) U.S. Mergers and Acquisitions database, to identify completed domestic acquisitions; ii) Standard & Poor's ExecuComp database (ExecuComp), to identify CEOs and chief financial officers (CFOs); and iii) LexisNexis Public Records Database, to identify CEO and CFO geographic origin.

Our sample of M&A transactions comes from the SDC U.S. Mergers and Acquisitions database. We begin with all domestic M&A announced between Jan. 1992 and Dec. 2014 and require that the acquirer owns less than 50% of the target prior to the acquisition announcement and owns at least 95% after the completion.[12] We exclude deals that are not mergers or acquisitions.[13] Additionally, we exclude privatizations, acquisitions made by penny stocks, all deals with missing deal value, and small transactions in which the deal value is less than $1 million or less than 1% of the acquirer's market capitalization (Moeller, Schlingemann, and Stulz (2004), Masulis, Wang, and Xie (2007), and Fich, Harford, and Tran (2015)).[14] We also require the existence of return data from the Center for

[11] The literature that uses identifiable manager characteristics to help explain corporate policies has found evidence that managers affect, for example, corporate financing (Malmendier, Tate, and Yan (2011), Cronqvist, Makhija, and Yonker (2012)), corporate investment and acquisitions (Malmendier and Tate (2005, 2008), Jenter and Lewellen (2015)), corporate risk taking (Cain and McKeon (2016), Hutton, Jiang, and Kumar (2014), and Faccio, Marchica, and Mura (2016)), and corporate employment decisions (Yonker (2017b)).

[12] The sample starts in 1992 because this is when the ExecuComp coverage begins.

[13] These include repurchases, recapitalizations, spinoffs, self-tender offers, exchange offers, and minority stake purchases.

[14] We find similar results if we include acquisitions with deal value less than 1% of the acquirer's market capitalization.

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Research in Security Prices (CRSP) and accounting data from Compustat for the acquiring firms.[15]

Our main analysis focuses on CEOs, as it is likely that they have the most influence on firm acquisition decisions among top management. Much of the literature that investigates the impact of managers on acquisitions makes the same assumption (e.g., Malmendier and Tate (2008), Yim (2013), and Benmelech and Frydman (2015)). We rely on ExecuComp to identify CEOs of non-financial and non-utility U.S.-based firms from 1992 to 2014. ExecuComp data are also used to construct control variables for other sources of heterogeneity in managers (specifically, CEO age, gender, and tenure). There are 26,541 firm-year observations with complete CEO data.

We are interested in testing whether firms are more likely to make acquisitions near their CEOs' homes. Thus, we use the LexisNexis Public Records Database to identify the geographic origin of CEOs, which we define as the state in which the CEO acquired his Social Security number (Yonker (2017a), Pool et al. (2012)). The first 5 digits of social security numbers are not random. The first 3 digits indicate the state of issuance, while the fourth and fifth digits are linked to the sequence of issuance. Therefore, the Social Security number identifies the year and state in which a CEO acquired his Social Security number. Yonker (2017a) shows that for nearly 85% of CEOs in his sample (ExecuComp from 1997 to 2007), the Social Security number identifies the states where CEOs lived during their teenage years. We find this is true for a similar percentage of CEOs in our sample.

The LexisNexis Public Records Database includes Social Security data (excluding the last 4 digits) for most individuals living in the United States. We manually search the database to identify CEOs. To do so, we follow the procedure in Yonker (2017a), which makes use of first, middle, and last names, as well as age.[16] Only uniquely identified CEOs are included in the sample. Following this procedure, we obtain CEO geographic origin data for 23,718 (89.4%) of the firm-years covered by ExecuComp. These observations include 2,291 unique firms and 4,269 unique CEOs.[17]

Table 1 presents summary statistics. Panel A reports the deal and acquirer characteristics for the acquisition sample. In this panel, each deal is an observation. Over the sample period, there are 5,505 deals involving both "private" and public targets with an aggregate deal value of $4.15 trillion.[18] Of these deals, about 20% include public targets and the majority of acquisitions are made with cash (59%). The average 3-day cumulative abnormal merger announcement return is 1.1%.[19] However, it is important to note that this average is driven by private target deals, which constitute 80% of deals but only 28% of deal value. While the average bidder abnormal announcement return is positive (1.7%) for private target

[15] Stock return and accounting information are not available for private targets.

[16] For more details on the data-collection procedure, see the Appendix in Yonker (2017a).

[17] Since there is a growing interest in understanding the role of CFOs in a firm's decision-making (e.g., Malmendier, Pezone, and Zheng (2016)), we conduct a supplementary analysis using CFO state of origin. We identify CFO state of origin for roughly 60% of the sample.

[18] For ease of exposition, we call both true private targets and subsidiary targets "private" targets.

[19] Abnormal returns are estimated using the market model.

TABLE 1
Summary Statistics

Table 1 reports summary statistics for the sample used in the analysis and described in Section II. The sample includes 5,505 deals (Panel A), 23,718 firm-year observations (Panel B), and 1,209,618 firm-state-year observations (Panel C) from 1992 to 2014. Panel A reports summary statistics for deal characteristics, Panel B reports summary statistics for firm-year characteristics, and Panel C reports summary statistics for firm-year-state characteristics. All variables denoted in US$ are adjusted for inflation and expressed in 2014 constant US$. All variable definitions are reported in the Appendix.

Variable	*N*	Mean	Q1	Median	Q3	Std. Dev.
Panel A. Deal Observations						
BIDDER_CAR	5,505	0.011	−0.021	0.006	0.039	0.090
HOME_STATE_DEAL	5,505	0.105	0.000	0.000	0.000	0.307
IN_STATE_DEAL	5,505	0.213	0.000	0.000	0.000	0.410
HOMETOWN_DEAL	5,505	0.036	0.000	0.000	0.000	0.187
DEAL_VALUE ($millions)	5,505	755.478	35.987	112.873	382.697	3,724.530
RELATIVE_DEAL_SIZE	5,505	0.174	0.029	0.068	0.180	0.281
CASH_DEAL	5,505	0.591	0.000	1.000	1.000	0.492
STOCK_DEAL	5,505	0.150	0.000	0.000	0.000	0.357
PUBLIC_TARGET	5,505	0.205	0.000	0.000	0.000	0.404
DIVERSIFYING_DEAL	5,505	0.391	0.000	0.000	1.000	0.488
FRIENDLY_DEAL	5,505	0.991	1.000	1.000	1.000	0.092
TARGET_PREMIUM	1,122	0.461	0.219	0.382	0.609	0.389
Panel B. Firm-Year Observations						
DEAL_DUMMY	23,718	0.1476	0.0000	0.0000	0.0000	0.3547
HOME_STATE_DEAL	23,718	0.0172	0.0000	0.0000	0.0000	0.1299
IN_STATE_DEAL	23,718	0.0336	0.0000	0.0000	0.0000	0.1801
FIRM_SIZE	23,718	7.4319	6.3406	7.2846	8.4369	1.6179
BM	23,718	0.6209	0.4212	0.6117	0.8042	0.2667
LEVERAGE	23,718	0.2173	0.0508	0.2000	0.3296	0.1845
CASH	23,718	0.1529	0.0243	0.0823	0.2225	0.1746
ROA	23,718	0.0365	0.0155	0.0530	0.0918	0.1208
INVESTMENT	23,718	0.0595	0.0235	0.0429	0.0756	0.0545
G_INDEX	12,803	9.2324	7.0000	9.0000	11.0000	2.6349
CEO_OWNERSHIP	23,718	0.0274	0.0042	0.0113	0.0289	0.0395
INSTITUTIONAL_OWNERSHIP	20,137	0.7162	0.5813	0.7414	0.8427	0.1969
AGENCY_INDEX	11,813	0.5114	0.3602	0.5117	0.6599	0.2047
E_INDEX	11,612	2.2531	1.0000	2.0000	3.0000	1.2733
CEOHMTENURE	13,830	31.9698	19.0000	22.0000	50.0000	17.2578
Panel C. Firm-Year-State Observations						
DEAL_DUMMY	1,209,618	0.0034	0.0000	0.0000	0.0000	0.0583
PRIVATE_DEAL_DUMMY	1,209,618	0.0027	0.0000	0.0000	0.0000	0.0515
PUBLIC_DEAL_DUMMY	1,209,618	0.0007	0.0000	0.0000	0.0000	0.0278
LN_DEAL_VALUE	1,209,618	0.0174	0.0000	0.0000	0.0000	0.3126
CEOCOLLSTATE	705,330	0.0205	0.0000	0.0000	0.0000	0.1418
CEOBOARDSTATE	866,898	0.0192	0.0000	0.0000	0.0000	0.1373
CEORESISTATE	669,477	0.0271	0.0000	0.0000	0.0000	0.1623

deals, it is negative (−1.2%) for public target deals, which is consistent with the M&A literature (e.g., Moeller et al. (2004), Betton et al. (2008), Billett and Qian (2008), and Ishii and Xuan (2014)).[20]

Panel B of Table 1 reports the summary statistics of acquirer characteristics and acquisition behavior based on firm-year observations. The statistics indicate that acquisitions happen in 14.8% of firm-years. Home-state acquisitions occur in about 1.7% of firm-years, and in-state acquisitions occur during 3.4% of firm-years.[21]

[20] The common explanation for this empirical finding is that there is more competition in acquiring public targets since the public market price can be viewed as a default competing bidder (e.g., Billett and Qian (2008)).

[21] It is important to note that if the bidder CEO grew up in the state where the bidder's headquarters is located (i.e., local CEO), then home-state deals are also in-state deals. As we find later, CEOs exhibit home bias *only* in cross-state deals. We, therefore, introduce a variable *hometown deal* to capture those cross-state home-state acquisitions.

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In our analysis in Section III, we employ a firm-state pair model to investigate whether firms have an abnormally high propensity to acquire targets from their CEOs' home states. This analysis is conducted using firm-state-year observations. In other words, each firm (in Panel B of Table 1) is paired with each of the 50 states and Washington, DC, each year. Panel C reports the summary statistics for this sample. The mean of the deal dummy in this specification is 0.34%.

III. CEO Hometown Bias in Acquisitions

We begin our analysis by investigating whether CEOs exhibit a hometown bias when making acquisition decisions. That is, are they more likely to acquire targets from their home states? Both *home advantages* and *agency conflicts* suggest that they should.

A. Baseline Results

Similar to Pool et al. (2012) and Tate and Yang (2015), we use a firm-state pair model to test this hypothesis. Specifically, we use firm-state-year observations (i.e., each firm is paired with each of the 50 states and Washington, DC, each year) and estimate the following regression:[22]

$$Y_{i,j,r,s,t} = \delta_{j,t} + \gamma_{r,s,t} + \beta_h \text{CEO_HOME_STATE}_{i,s,t} + \Gamma' \text{Controls}_{i,s,t-1} + \varepsilon_{i,s,t}. \quad (1)$$

The dependent variable $Y_{i,j,r,s,t}$ measures the acquisition activity of firm i in state s during year t. We use two measures to capture this: DEAL_DUMMY, which is 1 if firm i acquires at least one firm in state s during year t, and 0 otherwise, and LN_DEAL_VALUE, which is the natural logarithm of the total value of firm i's acquisitions in state s during year t. The additional subscripts j and r indicate the industry and state of headquarters of firm i, respectively. The variable of interest is CEO_HOME_STATE, which is an indicator variable that is equal to 1 if the CEO of firm i in year t grew up in state s. If CEOs exhibit a hometown bias in acquisitions, then we expect the estimate of β_h to be positive.

There are two important sources of heterogeneity for which our model controls. First, since it is well documented that M&A occur in "waves" (Harford (2005)), we control for differences in the propensity of firms in different industries to engage in M&A over time. We do so by including industry-year fixed effects based on the Fama–French (1997) 48-industry classification, $\delta_{j,t}$. The inclusion of these effects controls for unobserved time-varying heterogeneity at the industry level. The second source of heterogeneity is geographic in nature. Firms located in certain states will differ in their propensity to acquire firms in other states due to the effects of geographic distance or other linkages across states, such as those induced by industry agglomeration or co-agglomeration. We also allow for these

[22] Alternatively, we use a methodology similar to Yonker (2017a) to test whether there is CEO hometown bias in acquisitions, and we find similar results. Specifically, we test whether the observed proportion of home-state acquisitions is greater than the expectation, where the expected proportion of home-state targets is based on the actual number of potential targets in the home state.

effects to vary over time to account, for example, for changing state-level economic conditions or legal environments. We do so by including bidder and target headquarters (HQ) state-pair-year fixed effects, $\gamma_{r,s,t}$, where r is firm i's state of headquarters and s is the state of potential acquisition target. Note that these fixed effects absorb all bidder and (potential) target location effects, including, for example, distances between bidder and target states, differences in the populations between bidder and target states, and similarity in culture between bidder and target states.

We also control for a set of firm characteristics that are likely to influence firms' acquisition decisions. Specifically, we control for FIRM_SIZE, BM, LEVERAGE, CASH, ROA, and INVESTMENT. All of these variables are lagged 1 year. Additionally, we control for the firm's headquarters state either by the inclusion of bidder and target headquarters state-pair fixed effects or the dummy variable, HQ_STATE, which is a dummy variable that equals 1 if firm i's headquarters is located in state s. Both Kang and Kim (2008) and Kedia, Panchapagesan, and Uysal (2008) report evidence that the information advantage arising from geographic proximity makes acquisitions more likely to occur within a state. Additionally, since many CEOs are local (Yonker (2017a)), we control for the interaction effect of the headquarters state and the CEO home state in order to isolate the effect of the CEO home state on M&A activities. Definitions of all independent variables are described in the Appendix. In all regressions, t-statistics are computed based on robust standard errors clustered at the firm level.

Table 2 reports the regression results. In Panel A, the dependent variable is DEAL_DUMMY, and in Panel B, it is LN_DEAL_VALUE. Each panel reports various regressions that differ by their fixed effects. In column 1, we report results with only industry-year fixed effects so that we can gauge the relative effects of the CEO home state and the firm headquarters state. The coefficient estimate on CEO_HOME_STATE is positive with a t-statistic of over 9, suggesting that firms are more likely to make acquisitions in their CEOs' home states. The coefficient estimate implies that the incremental effect of CEO_HOME_STATE on the propensity to acquire a target in a given state is 0.0085. Given that the unconditional probability of making an acquisition in a given state is 0.0034, this implies firms are 2.5 times (0.0085/0.0034) *more* likely to acquire firms headquartered in states where their CEOs grew up.

The coefficient estimate on HQ_STATE is also significantly positive, which is consistent with Kang and Kim (2008) and Kedia et al. (2008) and confirms that in-state acquisitions occur much more frequently than out-of-state deals. Moreover, the relative magnitudes suggest that the incremental CEO effect is 25% as large as that of the headquarters location. The estimate on the interaction term between these two effects is negative. This negative estimate could arise for at least two reasons. First, it could be that local CEOs (i.e., CEOs who grew up in their firms' headquarters states) generally engage in fewer deals than non-local CEOs. Second, when the CEO is local, it is possible that knowledge of local targets by the rest of the management team prevents local CEOs from engaging in "pet" home-state acquisitions. Regardless of the interpretation, the estimates imply that the CEO hometown bias in acquisitions is driven by cross-state deals.

In column 2 of Table 2, we include HQ state-pair fixed effects. Note that the HQ_STATE dummy is absorbed by this set of fixed effects. In this regression, the

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TABLE 2
Acquisition Hometown Bias

Table 2 reports results of various forms of the following regression equation (1) outlined in Section III.A:

$$Y_{i,j,r,s,t} = \delta_{j,t} + \gamma_{r,s,t} + \beta_h \text{CEO_HOME_STATE}_{i,s,t} + \Gamma' \text{Controls}_{i,s,t-1} + \varepsilon_{i,s,t},$$

where the dependent variable $Y_{i,j,r,s,t}$ measures the acquisition activity of firm i in state s during year t. In Panel A, this is measured using DEAL_DUMMY, which is 1 if firm i acquires at least one firm in state s during year t, and is 0 otherwise. Panel B uses LN_DEAL_VALUE, which is the natural logarithm of the total value of firm i's acquisitions in state s during year t. The additional subscripts j and r indicate the industry and state of headquarters of firm i, respectively. The variable of interest is CEO_HOME_STATE, which is an indicator variable that is equal to 1 if the CEO of firm i in year t grew up in state s. $\delta_{j,t}$ is an industry-year fixed effect based on the Fama–French (1997) 48-industry classification and is included in all models. $\gamma_{r,s,t}$ is an HQ state-pair-year fixed effect based on firm i's state of headquarters, r, and the state of acquisition activity, s. Models 3–5 include these fixed effects, while model 2 includes the time-invariant version. The models in columns 4 and 5 additionally include firm fixed effects and firm-state fixed effects, respectively. Definitions of the independent variables are described in the Appendix. Robust standard errors are clustered at the firm level and robust t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Variable	1	2	3	4	5
Panel A. DEAL_DUMMY as Dependent Variable					
CEO_HOME_STATE	0.00849***	0.00453***	0.00437***	0.00430***	0.00285**
	(9.23)	(5.24)	(5.06)	(5.00)	(2.27)
HQ_STATE	0.03198***				
	(16.48)				
CEO_HOME_STATE × HQ_STATE	−0.01299***	−0.00741**	−0.00696**	−0.00669**	−0.00601
	(3.97)	(2.27)	(2.09)	(2.03)	(1.22)
FIRM_SIZE	0.00015***	0.00015***	0.00015***	−0.00031**	−0.00031**
	(2.70)	(2.72)	(2.65)	(2.15)	(2.15)
BM	−0.00072**	−0.00072**	−0.00066*	−0.00288***	−0.00288***
	(2.03)	(2.06)	(1.84)	(5.75)	(5.75)
LEVERAGE	−0.00031	−0.00034	−0.00027	−0.00438***	−0.00438***
	(0.65)	(0.72)	(0.57)	(6.70)	(6.88)
CASH	0.00011	0.00008	0.00007	0.00581***	0.00581***
	(0.19)	(0.14)	(0.13)	(6.74)	(6.74)
ROA	0.00281***	0.00277***	0.00280***	0.00417***	0.00417***
	(5.42)	(5.31)	(5.24)	(6.74)	(6.74)
INVESTMENT	−0.00867***	−0.00861***	−0.00846***	−0.00187	−0.00187
	(4.61)	(4.53)	(4.38)	(0.93)	(0.93)
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
HQ state-pair fixed effects	No	Yes	No	No	No
HQ state-pair-year fixed effects	No	No	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	No
Firm-state fixed effects	No	No	No	No	Yes
No. of obs.	1,209,618	1,209,618	1,209,618	1,209,618	1,209,618
R^2	0.0074	0.0180	0.0507	0.0556	0.1701
Panel B. LN_DEAL_VALUE as Dependent Variable					
CEO_HOME_STATE	0.04222***	0.02209***	0.02109***	0.02074***	0.01317**
	(8.93)	(4.98)	(4.75)	(4.68)	(1.99)
HQ_STATE	0.17310***				
	(15.24)				
CEO_HOME_STATE × HQ_STATE	−0.07436***	−0.04533**	−0.04275**	−0.04130**	−0.03058
	(4.12)	(2.53)	(2.34)	(2.28)	(1.18)
Firm controls	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
HQ state-pair fixed effects	No	Yes	No	No	No
HQ state-pair-year fixed effects	No	No	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	No
Firm-state fixed effects	No	No	No	No	Yes
No. of obs.	1,209,618	1,209,618	1,209,618	1,209,618	1,209,618
R^2	0.0074	0.0176	0.0508	0.0550	0.1620

coefficient estimate on CEO_HOME_STATE is roughly half of that in column 1. This shows the importance of controlling for geography-related heterogeneity.

In all of the remaining specifications, we include HQ state-pair-year fixed effects. Column 3 of Table 2 serves as our "baseline" estimate of the CEO

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hometown acquisition bias and implies that firms are about 1.3 times more likely to acquire firms from their CEOs' home states than from other states.

In column 4 of Table 2, we add firm fixed effects to control for any time-invariant firm-level factors that affect firms' propensity to make acquisitions. The inclusion of these effects has little effect on our estimates.

Finally, in column 5 of Table 2, we report our most stringent regression model, in which we include firm-state-pair fixed effects. In this model, there are 51 fixed effects for each firm. This allows us to exploit within-firm variation in CEO home states. The estimates in this model are driven by the acquisition behavior of CEOs from different states running the same firm at different points in time. Even in this extremely conservative specification, the coefficient estimate suggests that firms are about 83% more likely to acquire firms from their CEOs' home states.

Turning to Panel B of Table 2, which uses the LN_DEAL_VALUE as the dependent variable, we find qualitatively similar results. On average, firms spend more in acquiring targets in their CEOs' home states than in other states.

As previously discussed, the negative coefficient estimate on the interaction term between CEO_HOME_STATE and HQ_STATE in Table 2 suggests that the hometown bias in acquisitions is driven by cross-state deals. To focus on these cross-state deals, in Table 3, we estimate the regressions in Table 2 excluding the in-state sample (acquisitions of targets located in the bidder's headquarters state).[23] As in Panel A of Table 2, the dependent variable is DEAL_DUMMY. The coefficient estimates on CEO_HOME_STATE are nearly identical to those in Panel A of Table 2.[24] In light of this, we focus on the cross-state sample in the rest of this section to investigate the role of CEO characteristics, target type, and bidder governance in the CEO home bias in acquisitions.

TABLE 3

Cross-State Acquisitions and the Acquisition Hometown Bias

Table 3 reports the results of regressions analogous to those in Panel A of Table 2 using the sample of cross-state acquisitions (those targets not headquartered in the bidder's headquarters state). The dependent variable is DEAL_DUMMY and the variable of interest is CEO_HOME_STATE. All regressions control for firm characteristics included in the models in Panel A of Table 2, but the coefficient estimates are suppressed for brevity. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. * and ** indicate significance at the 5% and 1% levels, respectively.

Variable	1	2	3	4	5
CEO_HOME_STATE	0.00849**	0.00453**	0.00436**	0.00430**	0.00285*
	(9.22)	(5.23)	(5.05)	(4.99)	(2.26)
Firm controls	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes
HQ state-pair fixed effects	No	Yes	No	No	No
HQ state-pair-year fixed effects	No	No	Yes	Yes	Yes
Firm fixed effects	No	No	No	Yes	No
Firm-state fixed effects	No	No	No	No	Yes
No. of obs.	1,185,900	1,185,900	1,185,900	1,185,900	1,185,900
R^2	0.0021	0.0113	0.0462	0.0512	0.1599

[23] In particular, a firm's headquarters state is excluded from the 50 states and Washington, DC.

[24] In unreported results, we confirm that using LN_DEAL_VALUE as the dependent variable yields qualitatively similar results. We also conduct our analysis on only in-state deals and find that the coefficient estimate on CEO_HOME_STATE is not statistically different from 0, confirming that the hometown bias is driven by cross-state deals.

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B. Robustness of Baseline Results

1. Placebo Analysis

While the inclusion of headquarters state-pair-year fixed effects should control for any time-varying or time-invariant omitted variables that could drive our estimates on CEO home state, we run placebo tests using data in which we randomize both CEO home states and target headquarters states to ensure that our coefficient estimates are not capturing some other aspect of the joint distributions of these variables. We then estimate regressions analogous to those in columns 1 and 3 of Table 2 (Panel A) and harvest the coefficient estimates on CEO home state. The regressions are of the form DEAL_DUMMY = CEO_HOME_STATE + HQ_STATE + CEO_HOME_STATE × HQ_STATE and other fixed effects. These regressions include no other controls.

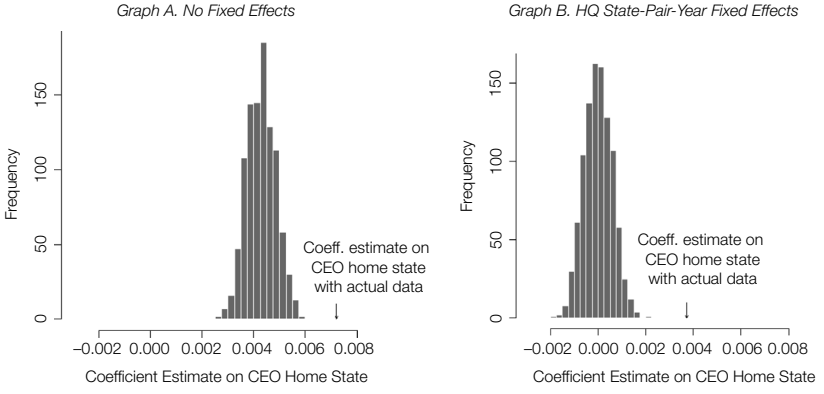
Graphs A and B of Figure 1 plot the distributions of coefficient estimates on CEO home state from the placebo regressions. The randomization procedure maintains the original data structure but reshuffles the CEO home states and target headquarters states, maintaining the original geographic distributions of both by drawing target headquarters states and CEO home states from the original distributions without replacement. Each histogram plots 1,000 simulations and indicates the estimate obtained using the actual data. The model estimated in Graph A does not include fixed effects. The average estimate is upward biased at 0.0043, which is not surprising since more CEOs and more targets come from more populous states. However, the estimate of CEO home state using the actual data is 0.0072, which is over 5 standard deviations from the mean estimate. Moreover, this estimate is larger than any of the 1,000 placebo estimates.

In Graph B of Figure 1, the estimated model includes headquarters state-pair-year fixed effects as in our baseline model. Immediately, it is apparent that the

FIGURE 1

Acquisition Hometown Bias: Placebo Tests

Graphs A and B of Figure 1 plot the distributions of coefficient estimates on CEO home state from placebo regressions analogous to those in columns 1 and 3 from Panel A of Table 2, respectively. The placebo estimates use simulated data in which both CEO home states and target headquarters states are randomized across CEOs and targets. The randomization procedure maintains the original data structure but reshuffles the CEO home states and target headquarters states, maintaining the original geographic distributions of both by drawing target headquarters states and CEO home states from the original distributions without replacement. Each histogram plots 1,000 simulations and indicates the estimate obtained using the actual data.



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fixed effects correct for the upward bias exhibited in our simulations in Graph A. The mean coefficient estimate from the simulated data is 0.000 and the coefficient estimate using the actual data is 0.0037, which is 6.35 standard deviations from the mean. The coefficient estimate using the actual data is again larger than any estimate using the simulated data.

2. Possibility of Reverse Causality

One concern of the analysis is that the results could be driven by reverse causality. It is plausible that CEOs are hired by firms specifically to make acquisitions in their home states. For example, a firm may hire a CEO from Michigan if the firm plans to invest in the automobile industry. Since many auto and auto parts firms are located in Michigan, this might explain why this CEO is more likely to acquire firms from Michigan. To address this concern, we exclude observations if CEO tenure is less than 3 years and repeat the analysis. The result presented in column 1 in Appendix Table IA in the Supplementary Material shows that the coefficient estimate on CEO_HOME_STATE continues to be statistically different from zero. Reverse causality does not drive the results.

3. CEOs' Social Connections and Industry Experience

We want to see whether our results are driven by known social connections due to CEOs' education or board positions. We repeat the analysis in Table 3 by including an additional dummy variable that equals 1 if the state is where the CEO went to college. The result is reported in column 2 in Appendix Table IA.1 in the Supplementary Material. We find that CEO hometown bias still exists after controlling for CEOs' college state. Similarly, we include a dummy variable that equals 1 if a CEO sits on the board of a firm in that state, and our result continues to hold (column 3 in Appendix Table IA.1).

Firms in some industries tend to cluster in certain states (e.g., auto firms cluster in Michigan). If a CEO has experience and expertise about such an industry, she might be more likely to acquire targets from those certain states. To control for the effect of CEO industry experience on the propensity to acquire from a state, we include a variable that captures the percentage of public firms in a state that match the industry (2-digit Standard Industrial Classification (SIC) code) experience of a CEO. The regression result is reported in column 4 in Appendix Table IA.1 in the Supplementary Material, and our main result still holds.

4. CEO Overconfidence

We check whether our finding is driven by CEO overconfidence. The alternative story is that overconfident CEOs tend to overestimate their information advantage for targets in their home states, hence leading to a hometown acquisition bias, which is stronger for private acquisitions due to less attention and scrutiny. Following Hirshleifer, Low, and Teoh (2012), we define a dummy variable OVERCONFIDENT_CEO to be equal to 1 if the CEO postpones the exercise of vested options that are at least 67% in the money, and 0 otherwise. We then repeat the analysis in Table 3 by including two additional variables: OVERCONFIDENT_CEO and the interaction term between CEO_HOME_STATE and OVERCONFIDENT_CEO. The results are reported in Appendix Table IA.2 in the Supplementary Material.

Consistent with the literature (e.g., Malmendier and Tate (2008)), we observe a significantly positive coefficient on OVERCONFIDENT_CEO, suggesting that overconfident CEOs make more acquisitions. Nonetheless, the coefficient on the interaction term is insignificant for both private and public deals. Further, the coefficient on CEO_HOME_STATE remains positive. This suggests that CEO overconfidence does not drive the hometown bias in acquisitions.[25]

5. Excluding CEOs from New York, California, and Illinois

Another concern is that our results are driven by the fact that many CEOs grew up in a few large states that also house a large proportion of firms that are potential acquisition targets. Our empirical model using state-pair-year fixed effects should address this problem to a large degree. Nevertheless, to further ensure our results are not driven by this effect, we repeat the analysis by removing CEOs who grew up in the top three states where CEOs are from: New York (NY), California (CA), and Illinois (IL). About 30% of CEOs in our sample are from those three states. The results are reported in Appendix Table IA.3 in the Supplementary Material, and our results continue to hold.

6. Influence of CFOs

Our study focuses on CEO behavior because there is little doubt that the CEO is the most important decision maker of a firm. Especially when it comes to M&A, CEOs tend to retain a high degree of control and are less likely to delegate the decision (Graham, Harvey, and Puri (2015)). Nonetheless, there is a growing interest in understanding the role of CFOs in firm decision-making (e.g., Malmendier et al. (2016)). Therefore, we conduct a supplementary analysis on the role of CFOs' home states on acquisition decisions. We repeat the analysis in Table 3 with the inclusion of a dummy variable that captures the CFO home state. The results, reported in Appendix Table IA.4 in the Supplementary Material, show that there is also an acquisition bias toward the CFO's home state that is distinct from the CEO effect; however, in most specifications, the magnitude of the effect is about half that of the CEO effect.

7. Robustness to the CEO Birth County

While identifying CEOs' geographic origin based on their Social Security number has the advantage of being widely available for most CEOs, it is also a coarse measure of "home" for large states such as California. To alleviate this concern, we use an alternative measure of "home." In particular, we manually collect CEOs' birth county information following the method in Bernile, Bhagwat, and Rau (2017). We identify birthplaces for roughly one third of our original sample.[26] We then create the variable LOG_DISTANCE_TO_CEO_BIRTH_COUNTY, calculated as the natural logarithm of the population-weighted center of each state to the CEO's birth county, and test whether longer distances to the CEO's birth

[25] In unreported results, we also use the media-based overconfidence measure following Malmendier and Tate (2008) for a subsample of 605 CEOs and find similar results.

[26] Among 23,718 original firm-year observations, we are able to identify 8,844 observations with CEO birth county information. Of these observations, there are 992 unique firms and 1,300 unique CEOs.

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county imply a lower propensity to engage in deals. The results, reported in Appendix Table IA.5 in the Supplementary Material, show that the coefficient estimates on LOG_DISTANCE_TO_CEO_BIRTH_COUNTY are indeed significantly negative, corroborating our earlier findings.

8. Robustness to the Inclusion of Financial and Utility Firms

The analysis excludes both financial and utility firms. As a robustness check, we conduct our main tests adding firms in the financial and utility industries to our sample. Our results are unaltered (see Appendix Table IA.6 in the Supplementary Material).

C. CEO Home-State Ties and the Hometown Bias in Acquisitions

We would expect that CEOs with stronger ties to their home states will be more likely to make hometown acquisitions. This is true under either the *home advantage* or the *agency conflict* hypothesis. We measure the strength of a CEO's home-state tie in several ways: how long she has lived in her home state, whether she went to college in her home state, whether she holds a board seat at another firm in her home state, and whether she currently owns a residence in her home state. Specifically, similar to Pool et al. (2012), we define CEOHMTENUREGTMED as a dummy variable that is equal to 1 if a CEO's home tenure is greater than the median in the sample, where home tenure is based on how long the CEOs lived in their home states (see the Appendix). CEOCOLLSTATE is a dummy variable that is equal to 1 if the CEO attended college in state *s*. CEOBOARDSTATE is a dummy variable that is equal to 1 if the CEO holds a board position at a firm located in state *s*. CEORESISTATE is a dummy variable that is equal to 1 if the CEO owns a residence in state *s*.

To test whether the hometown bias is more pronounced when the home-state ties are stronger, we extend the empirical model used in Table 3 by including the CEO-specific measures of home-state ties and their interaction with CEO_HOME_STATE. If stronger ties lead to greater bias, then the coefficient estimates on the interaction terms should be positively estimated. The results are reported in Table 4. Consistent with our hypothesis, the coefficients on three of the four interaction terms are significantly positive.[27] These results suggest that the hometown bias is more pronounced when CEOs lived longer in their home state, went to college there, or currently hold a board position there.[28] In short, Table 4 provides evidence that CEOs' hometown bias is more pronounced when they have stronger ties to their home states.

[27]The exception is that the coefficient is not significant on the interaction term between CEORESISTATE and CEO_HOME_STATE. However, the coefficient on CEORESISTATE is large and highly significant. This suggests that firms are more likely to buy targets in states where their CEOs own second homes. So, the hometown bias will be larger if the CEO owns a residence in her home state, but that extra propensity is not magnified because the property is located in her home state.

[28]Since CEO age and gender are likely to affect CEO acquisition behavior in general (Yim (2013), Huang and Kisgen (2012)), we also explore whether certain types of CEOs are more likely to be biased in their acquisition behavior. The results are reported in Appendix Table IA.7 in the Supplementary Material. None of the coefficient estimates on interaction terms are statistically different from 0. The CEO hometown bias in acquisitions does not vary with these characteristics.

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TABLE 4

CEO Home-State Ties and the Acquisition Hometown Bias

Table 4 reports results of the impact of CEO characteristics on the acquisition home bias. The table reports coefficient estimates on CEO_HOME_STATE as well as its interactions with various CEO characteristics using the models outlined in Table 2 and the sample of cross-state acquisitions used in Table 3. The dependent variable is DEAL_DUMMY. CEOHMTENUREGTMED is a dummy variable equal to 1 if the CEO home tenure is greater than the median in the sample (see the Appendix). CEOCOLLSTATE is a dummy variable equal to 1 if the CEO attended college in state *s*. CEOBOARDSTATE is a dummy variable equal to 1 if the CEO sits on the board of a firm in state *s*. CEORESISTATE is a dummy variable that is equal to 1 if the CEO owns a residence in state *s*. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4
CEO_HOME_STATE	0.00206**	0.00093	0.00305***	0.00318***
	(2.00)	(0.71)	(3.10)	(2.84)
CEO_HOME_STATE × CEOHMTENUREGTMED	0.00790***			
	(2.60)			
CEOHMTENUREGTMED	−0.00007			
	(0.37)			
CEO_HOME_STATE × CEOCOLLSTATE		0.00538**		
		(2.26)		
CEOCOLLSTATE		0.00048		
		(0.51)		
CEO_HOME_STATE × CEOBOARDSTATE			0.01553*	
			(1.95)	
CEOBOARDSTATE			0.00188	
			(1.39)	
CEO_HOME_STATE × CEORESISTATE				0.00116
				(0.26)
CEORESISTATE				0.00532***
				(3.29)
Firm controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
HQ state-pair-year fixed effects	Yes	Yes	Yes	Yes
No. of obs.	691,500	691,500	849,900	656,350
R^2	0.0727	0.0726	0.0603	0.0729

D. Agency Conflicts and the Hometown Bias

To disentangle the *agency conflicts* from the *home advantage* hypothesis, we examine the effect of corporate governance on hometown bias. If the hometown bias stems from agency conflicts, then weaker governance should intensify the ability of CEOs to acquire home-state targets. In contrast, if home advantages drive the hometown bias, we should find no relationship between corporate governance and the intensity of the bias. We directly investigate this prediction by testing whether lower-quality corporate governance increases the hometown bias. We run these tests conditional on target type because, as we note in Section I, we would expect that agency conflicts would be more prevalent among smaller, private deals, while home advantages driven acquisitions should be among the highly visible, large, public acquisitions.

We choose to focus on this private/public partition because the M&A literature shows that acquisitions of public and private targets differ in their frequency, deal size, public attention, and acquisition performance (Fuller et al. (2002), Officer (2007), and Betton et al. (2008)). However, we note that partitioning on deal size gives qualitatively similar results. In our sample, private target deals represent over 80% of all M&A; however, public target deals represent about 72% of

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total dollars spent on acquisitions.[29] The average deal size of public acquisitions is 10 times that of the private acquisitions in our sample. The public acquisitions are associated with more disclosure (e.g., both the bidder and the target need to file with the U.S. Securities and Exchange Commission about the acquisition) and receive much more attention and, therefore, scrutiny from investors, media, and regulators.

We follow Duchin and Sosyura (2013) to construct an agency index that combines the following three measures of governance variables: i) the Gompers, Ishii, and Metrick (2003) governance index (G_INDEX), ii) the percentage of shares held by institutional investors, and iii) the fraction of shares held by the top managers.[30] In particular, the AGENCY_INDEX averages a firm's percentile ranking in the sample according to each measure (for the latter two, the reverse ranking is used). We then scale the index to range from 0 (low) to 1 (high). High values of the agency index indicate weak corporate governance and, therefore, severe agency problems. If home-state acquisitions are made for CEOs' private benefits, then weak governance should intensify the hometown bias. If this is true, then there should be a positive coefficient for the interaction term between CEO_HOME_STATE and AGENCY_INDEX.

The results of the tests are reported in Table 5. Columns 1 and 3 report estimates of the hometown bias for the samples of private and public targets, respectively, using the baseline model from column 3 in Table 3. Columns 2 and 4 include the interaction term between CEO_HOME_STATE and AGENCY_INDEX. Note that the limited availability of the governance measures reduces the sample size by half.

The estimates indicate that firms exhibit a hometown bias when acquiring both private and public targets. For private targets, the coefficient estimate on CEO_HOME_STATE is highly significant and its magnitude is similar to that reported in Table 3. The unconditional probability of acquiring a private (public) target in a particular state is 0.0027 (0.0007) (see Panel C of Table 1). The coefficient estimate in column 1 of Table 5, thus, implies that firms are about 1.3 times more likely to acquire private targets in their CEOs' home states than in other states. For public targets, the coefficient estimate on CEO_HOME_STATE in column 3 of Table 5 is 0.00064 and indicates that firms are about 92% more likely to acquire public firms in their CEOs' home states. However, the significance of this coefficient estimate is weaker, which may not be surprising due to the limited number of public targets from a certain state.

The result in column 2 of Table 5 shows that for private deals, the interaction term between CEO_HOME_STATE and AGENCY_INDEX is positive and statistically significant, suggesting there is indeed a stronger hometown bias for firms with weak governance. The magnitude of the effect is nontrivial: Moving from the first to third quartile of AGENCY_INDEX increases the home-state acquisition bias by 0.0024 (the unconditional probability of making a private acquisition

[29] In our sample, the aggregate deal value in deals with private targets is $1.18 trillion and the aggregate deal value in deals with public targets is $2.97 trillion.

[30] In Appendix Table IA.8 in the Supplementary Material, we run analogous tests using each of the components of the agency index as well as the E_INDEX of Bebchuk, Cohen, and Ferrell (2009). Both the G_INDEX and E_INDEX produce significant moderating effects on the hometown bias.

TABLE 5

Impact of Corporate Governance on the Acquisition Hometown Bias

Table 5 reports results on the impact of corporate governance on the acquisition hometown bias. Columns 1 and 2 report coefficient estimates from linear probability regressions using the cross-state private target sample, and columns 3 and 4 report coefficient estimates using the cross-state public target sample. The dependent variable is a dummy equal to 1 if DEAL_DUMMY = 1 and the deal is a private (public) deal for columns 1–2 (columns 3–4). AGENCY_INDEX combines the following three measures of governance variables: i) the Gompers et al. (2003) governance index, ii) the percentage of shares held by institutional investors, and iii) the fraction of shares held by top managers. Details on these variables are provided in the Appendix. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. * and ** indicate significance at the 10% and 1% levels, respectively.

	Private Targets		Public Targets	
Variable	1	2	3	4
CEO_HOME_STATE	0.00362**	0.00048	0.00064*	0.00062
	(4.75)	(0.16)	(1.68)	(0.74)
CEO_HOME_STATE × AGENCY_INDEX		0.00797*		0.00041
		(1.69)		(0.14)
AGENCY_INDEX		0.00004		0.00009
		(0.87)		(0.32)
Firm controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
HQ state-pair-year fixed effects	Yes	Yes	Yes	Yes
No. of obs.	1,185,900	590,650	1,185,900	590,650
R^2	0.0437	0.0574	0.0425	0.0518

in a given state is 0.0027). In contrast, there is no statistical difference in the bias due to corporate governance among public target deals.

Overall, Table 5 suggests that poor corporate governance exacerbates CEO hometown bias in acquiring private targets (but not public targets). This is consistent with the notion that private home-state acquisitions, due to their small size and lack of public attention, are likely to suffer from agency problems. Public home-state deals, on the other hand, are not driven by agency problems.

One potential source of agency conflicts in private deals is that CEOs may possess strong bonds with residents of their home states and could use shareholder funds to help those residents during difficult times (Yonker (2017b)). Since financial data for private targets are unavailable, we test this motivation by examining the economic conditions of target states prior to acquisitions. In particular, we focus on the target state's unemployment rate, gross domestic product (GDP) growth rate, and per capita income growth rate in the year prior to each acquisition.

In Table 6, we examine whether CEOs exhibit stronger hometown bias for private targets when their home states are distressed using each of our three proxies for local economic conditions. In column 1, we repeat the regression in Table 3 by including two additional variables: UNEMPLOYMENT_RATE, the unemployment rate of the target state, and the interaction of UNEMPLOYMENT_RATE and CEO_HOME_STATE. The coefficient estimate on UNEMPLOYMENT_RATE is negative, indicating that, in general, a CEO is less likely to acquire targets from a state with a high unemployment rate. However, when it comes to CEO home state, a high unemployment rate is associated with a higher acquisition likelihood, as indicated by the significantly positive coefficient on CEO_HOME_STATE × UNEMPLOYMENT_RATE. In columns 2 and 3 of Table 6, we examine the effects of the state's GDP and per capita income growth rates. We find that a lower per capita income growth rate intensifies

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TABLE 6

Impact of State-Level Economic Conditions on the Acquisition Hometown Bias

Table 6 reports results of the impact of state-level economic conditions on the acquisition hometown bias. Columns 1–3 report coefficient estimates from linear probability regressions using the cross-state private target sample, and columns 4–6 report coefficient estimates using the cross-state public target sample. The dependent variable is DEAL_DUMMY, as outlined in the previous tables. The variables of interest include CEO_HOME_STATE and its interactions with various state-level economic condition measures, including the UNEMPLOYMENT_RATE (columns 1 and 4), GDP_GROWTH_RATE (columns 2 and 5), and PER_CAPITAL_INCOME_GROWTH_RATE (columns 3 and 6) of the target state. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Private Deals			Public Deals		
Variable	1	2	3	4	5	6
CEO_HOME_STATE	0.00326*** (4.61)	0.00359*** (4.55)	0.00367*** (4.69)	0.00051 (1.49)	0.00066* (1.71)	0.00065* (1.66)
CEO_HOME_STATE × UNEMPLOYMENT_RATE	0.00166** (2.02)			0.00050 (1.58)		
UNEMPLOYMENT_RATE	−0.00019*** (3.56)			−0.00010*** (4.04)		
CEO_HOME_STATE × GDP_GROWTH_RATE		−0.00044 (1.20)			−0.00009 (0.50)	
GDP_GROWTH_RATE		0.00001 (0.47)			0.00002* (1.93)	
CEO_HOME_STATE × PER_CAPITAL_INCOME_GROWTH_RATE			−0.00097* (1.85)			−0.00023 (1.01)
PER_CAPITAL_INCOME_GROWTH_RATE			0.00002 (0.91)			0.00002 (1.23)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
HQ state-pair fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	1,185,900	1,185,900	1,185,900	1,185,900	1,185,900	1,185,900
R^2	0.0092	0.0092	0.0092	0.0068	0.0068	0.0068

the CEO hometown bias, which is consistent with hometown deals being more pervasive when CEOs' home-state economies are suffering. When performing the same tests for public targets (columns 4–6), we do not find similar effects. Overall, the evidence is consistent with private home-state deals being driven by agency conflicts and that one likely source of these conflicts is CEOs' desire to help their hometown communities using shareholder money. This motivation is similar to the evidence found in Yonker (2017b).

IV. CEO Hometown Bias and Acquisition Performance

To further disentangle the cause of the observed CEO hometown bias in acquisitions, we investigate the stock market reaction of bidders around the deal announcements. If agency conflicts induce CEOs to acquire home-state firms, then we expect to see an unfavorable market reaction to home-state deals. If CEOs engage in these deals to exploit their home advantages, then the acquisition performance will be better for home-state deals than for other deals. We continue to conduct our tests conditional on the type of target, public or private.

A. Acquirer Announcement Returns

For each acquisition, we use the market model to calculate the CAR of the acquirer during the 3 trading days surrounding the announcement of the acquisition. We estimate the market model using the CRSP value-weighted market index

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return as the benchmark and data over the 200-day period ending 11 days before the announcement date (Masulis et al. (2007), Bouwman, Fuller, and Nain (2009), and Deng, Kang, and Low (2013)). The unit of observation in this analysis is at the deal level. We define a HOMETOWN_DEAL as a cross-state home-state acquisition.[31]

Panel A of Table 7 reports means of announcement returns, deal characteristics, and acquirer characteristics for hometown deals and non-hometown deals, as well as the results of difference tests between these groups. The samples are split between private and public deals.

The univariate statistics on announcement returns reveal sharp contrasts between the performance of hometown and non-hometown deals for public and private acquisitions.[32] For private acquisitions, the average CAR of hometown deals is essentially 0, while the mean of other private deals is a significantly positive 1.78%. The difference is significant at the 1% level. That is, while private acquisitions, on average, create value for acquirers (consistent with, e.g., Fuller et al. (2002)), private hometown deals do not. In contrast, while public

TABLE 7

Bidder Announcement Returns and the Acquisition Hometown Bias

Table 7 reports summary statistics split between hometown deals and other deals for private and public targets (Panel A) as well as results of ordinary least squares regressions of the acquirers' 3-day cumulative abnormal returns on HOMETOWN_DEAL and relevant controls (Panel B). The sample of acquisitions consists of 5,505 completed U.S. M&A between 1992 and 2014, as described in Table 1. The dependent variable in Panel B is the acquirer's 3-day cumulative abnormal returns centered on the acquisition announcement. The variable of interest is HOMETOWN_DEAL, which equals 1 if the deal is a cross-state home-state acquisition, and 0 otherwise. Definitions of the independent variables are described in the Appendix. All regressions control for year fixed effects and industry (Fama–French 48-industry) fixed effects. *t*-statistics based on standard errors adjusted for firm clustering are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Summary Statistics

	Private Targets			Public Targets		
Variable	Hometown Deals $N=158$	Other Deals $N=4,218$	Diff.	Hometown Deals $N=41$	Other Deals $N=1,088$	Diff.
BIDDER_CAR	0.04%	1.78%	***	0.93%	−1.27%	**
LARGE_LOSS_DEAL1	13.29%	7.09%	**	7.32%	20.96%	***
LARGE_LOSS_DEAL2	1.90%	0.66%		0.00%	9.56%	***
LARGE_LOSS_DEAL3	4.43%	1.97%		2.44%	15.17%	***
DEAL_VALUE	289.34	269.05		1,912.56	2,665.36	
FIRM_SIZE	7.27	7.15		8.52	8.38	
MB	2.39	2.33		2.29	2.59	
LEVERAGE	0.19	0.20		0.18	0.20	
ROA	0.04	0.05		0.08	0.06	*
CASH	0.19	0.17		0.14	0.17	
RELATIVE_DEAL_SIZE	0.12	0.14		0.32	0.33	
CASH_DEAL	0.65	0.63		0.51	0.43	
STOCK_DEAL	0.09	0.12		0.24	0.29	
DIVERSIFYING_DEAL	0.41	0.40		0.51	0.33	**
FRIENDLY_DEAL	0.99	0.99		0.95	0.97	

(continued on next page)

[31] This definition excludes in-state acquisitions from home-state deals. This exclusion is consistent with our findings in Section III that the hometown bias is driven by cross-state acquisitions, and it allows us to disentangle the firm headquarters effect from the CEO effect.

[32] To ensure the findings are not driven by outliers, we winsorize the bidder's CAR at the 1st and 99th percentiles. We also exclude deals that are announced within ±1 trading day of firm earnings releases.

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TABLE 7 (continued)
Bidder Announcement Returns and the Acquisition Hometown Bias

Panel B. Regression Analysis

Variable	Private Targets 1	Public Targets 2	All Deals 3	Exclude CEO <3 Years' Tenure 4	All Deals 5
HOMETOWN_DEAL	−0.0152** (2.38)	0.0212** (2.33)	−0.0159*** (2.69)	−0.0165** (2.39)	−0.0199*** (3.41)
PUBLIC			−0.0294*** (7.21)	−0.0311*** (6.85)	−0.0292*** (7.92)
HOMETOWN_DEAL × PUBLIC			0.0409*** (3.78)	0.0434*** (3.52)	0.0250* (1.87)
TOP_SIZE_QUARTILE					0.0011 (0.16)
HOMETOWN_DEAL × TOP_SIZE_QUARTILE					0.0274* (1.83)
FIRM_SIZE	−0.0055*** (2.90)	−0.0022 (1.24)	−0.0048*** (3.05)	−0.0055*** (2.88)	−0.0050*** (4.60)
MB	0.0019 (1.15)	−0.0018 (0.77)	0.0006 (0.45)	0.0005 (0.40)	0.0006 (0.46)
LEVERAGE	−0.0060 (0.63)	0.0077 (0.42)	−0.0057 (0.62)	−0.0064 (0.63)	−0.0055 (0.59)
ROA	−0.0101 (0.42)	−0.0073 (0.21)	−0.0151 (0.69)	0.0165 (0.83)	−0.0146 (0.67)
CASH	−0.0119 (0.96)	−0.0366** (2.02)	−0.0154 (1.38)	−0.0189 (1.48)	−0.0156 (1.41)
LN_FIRM_AGE	−0.0007 (0.38)	−0.0016 (0.52)	−0.0002 (0.10)	0.0007 (0.36)	−0.0002 (0.11)
RELATIVE_DEAL_SIZE	0.0836*** (3.25)	−0.0219** (2.52)	0.0362** (2.31)	0.0394** (2.20)	0.0356* (1.86)
CASH_DEAL	0.0021 (0.55)	0.0239*** (4.04)	0.0074** (2.18)	0.0071** (2.02)	0.0074** (2.25)
STOCK_DEAL	−0.0039 (0.73)	−0.0018 (0.27)	−0.0070* (1.66)	−0.0079* (1.69)	−0.0070* (1.65)
DIVERSIFYING_DEAL	−0.0022 (0.72)	−0.0024 (0.50)	−0.0013 (0.48)	−0.0018 (0.59)	−0.0012 (0.47)
FRIENDLY_DEAL	0.0003 (0.05)	−0.0021 (0.19)	−0.0013 (0.12)	0.0024 (0.17)	−0.0008 (0.07)
IN_STATE_DEAL	0.0077 (1.56)	0.0095* (1.88)	0.0073* (1.68)	0.0082* (1.65)	0.0073* (1.67)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
No. of obs.	4,376	1,129	5,505	4,524	5,505
R^2	0.073	0.153	0.063	0.072	0.063

acquisitions, on average, destroy acquirer shareholder value, public hometown deals actually create value.[33] Among public acquisitions in our sample, hometown deals have an average CAR of 0.93%, whereas other public deals have an average CAR of −1.27%. The difference is significant at the 5% level. Panel A of Table 7 also gives some hint of why public hometown deals outperform. The table shows that public hometown deals are less likely to be "large loss deals," suggesting that CEOs avoid extremely bad deals in their home states, which is consistent with the mechanism in Van Nieuwerburgh and Veldkamp (2009). The rest of Panel A shows that there are very few differences in acquirer and deal characteristics between hometown deals and other deals.

[33]Most studies find negative announcement returns for public targets, on average (e.g., Fuller et al. (2002), Moeller et al. (2004), Moeller, Schlingemann, and Stulz (2005), and Betton et al. (2008)).

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Next, we estimate multiple regressions of the acquirer's announcement return on HOMETOWN_DEAL, controlling for other deal and firm characteristics (Masulis et al. (2007), Deng et al. (2013)). The results are displayed in Panel B of Table 7. We first run a subsample analysis. Columns 1 and 2 report results for private and public target acquisitions, respectively. Consistent with the univariate results in Panel A, we observe a significantly negative coefficient of −1.52% on HOMETOWN_DEAL for deals with private targets. This translates into $22 million in shareholder value destruction for the average-size bidder in our sample.

In contrast, in public hometown deals, acquiring firms experience significantly higher announcement returns. After controlling for a variety of firm and deal characteristics, we find that acquirers in public hometown deals experience 2% higher abnormal announcement returns. This translates into about $100 million in enhanced shareholder value for the average-size bidder in our public deal sample.

In column 3 of Table 7, we use the full sample and include PUBLIC, a dummy for public target deals, and an interaction term between HOMETOWN_DEAL and PUBLIC as an alternative specification to gauge the effect of hometown deal on acquisition performance. The results point to the same conclusions. The coefficient on HOMETOWN_DEAL is significantly negative, suggesting private hometown deals underperform otherwise similar private deals by that much. The coefficient on PUBLIC is negative, meaning that public deals, on average, underperform. However, the coefficient on HOMETOWN_DEAL × PUBLIC is significantly positive, suggesting that public hometown deals outperform other public deals.

1. Robustness

In order to rule out the reverse causality story that CEOs are hired specifically to make home-state acquisitions, we repeat the analysis but remove deals where the CEO's tenure is less than 3 years. The results are displayed in column 4 of Panel B in Table 7 and show that this filter has little effect on the estimates. The abnormal announcement return results do not seem to be driven by reverse causality.

We conduct three additional tests to ensure the robustness of the results. First, it is possible that CEOs making hometown acquisitions have different skill levels than other CEOs. To address this concern, in column 1 of Appendix Table IA.9 in the Supplementary Material, we repeat our analysis within the subsample of CEOs who make *both* hometown and non-hometown deals. This effectively holds skill constant but greatly reduces the sample size. Nonetheless, private hometown deals continue to significantly underperform non-hometown deals, and public hometown deals continue to significantly outperform other public deals.

Second, we repeat the analysis by removing deals conducted by CEOs who grew up in three large states: New York (NY), California (CA), or Illinois (IL). The result is reported in column 2 of Appendix Table IA.9 in the Supplementary Material, and our main result continues to hold.

Third, we match each hometown deal to non-hometown deals based on target state, deal announcement year, public status, and deal size, and repeat the analysis within the matched sample. In particular, we require that the targets in matched

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non-hometown deals are from the same states and share the same public status, that the deals are announced during the same year, and that the deal size is in the range of 50% and 200% of the hometown deal. The result is reported in column 3 of Appendix Table IA.9 in the Supplementary Material; our conclusion still holds.

Overall, the underperformance of private hometown deals is consistent with these deals being driven by agency conflicts. In contrast, the favorable market reaction toward public hometown deals is more consistent with the *home advantage* hypothesis; it is unlikely these deals are driven by agency problems.

2. Effect of Deal Size

One natural follow-up question is what exactly drives the difference in performance between public and private hometown deals. Public and private acquisitions differ in many aspects, including deal size, public attention and scrutiny, and required disclosure, and these aspects are intertwined. Deal size in public deals is about 10 times that in private deals, making the costs (of conducting a bad acquisition) and incentives (to make the deal work) for public acquisitions much larger. It is reasonable to expect that CEOs are more likely to use corporate funds to help hometown firms when the money at stake is small, but have a lot more incentives to pull their home advantages when the deal is important enough to affect their career. We, therefore, expect the performance difference is at least partially due to the deal size difference. We explore to what extent the difference can be explained by deal size.

We first conduct univariate analysis by sorting deals into quartiles based on the deal size. We examine whether the unfavorable market reactions in private hometown deals are driven by smaller deals and the favorable market reactions in public hometown deals are driven by larger deals. The results are reported in Appendix Table IA.10 in the Supplementary Material. For private target acquisitions, we find that hometown deals indeed underperform only in quartiles 1–3, but not in the top size quartile. This is consistent with the conjecture that small private hometown deals are likely to be driven by agency issues. For public target acquisitions, first of all, there is no hometown deal in the bottom size quartile, suggesting none of the public hometown deals are very small. Moreover, the number of deals increases with the size quartile. We only observe significant outperformance for hometown deals in the top quartile, which is consistent with the conjecture that large public hometown deals are driven by home advantages, but the lack of significant outperformance in quartiles 2–3 can also be due to the very small numbers of hometown deals.

We then conduct regression analysis to test if deal size *fully* explains the difference in performance between public and private hometown deals. We define TOP_SIZE_QUARTILE as a dummy variable equal to 1 if the deal size is in the top quartile of the sample. We repeat our regression analysis including TOP_SIZE_QUARTILE and the interaction of this variable with HOMETOWN_DEAL. The result is reported in column 5 of Panel B in Table 7. We find that the interaction term between HOMETOWN_DEAL and TOP_SIZE_QUARTILE is positive and statistically significant, confirming that the difference in deal size helps explain the difference in performance between private and public hometown deals. Yet, we continue to observe a positive and significant coefficient for

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the interaction term between HOMETOWN_DEAL and PUBLIC, suggesting that other factors, such as public scrutiny and the extent of disclosure, may also contribute to the difference in performance between public and private hometown deals.

B. Agency Conflicts and Acquirer Announcement Returns

The previous results suggest that private hometown deals are driven by agency conflicts, in that hometown bias for private acquisitions is exacerbated by weak corporate governance, and that they underperform other private deals. If these deals are driven by agency conflicts, we also expect that the underperformance in private hometown deals will be stronger among bidders with weaker governance. We test this by including in the performance regression two additional variables: AGENCY_INDEX and the interaction term between HOMETOWN_DEAL and AGENCY_INDEX. We expect to see a negative coefficient on HOMETOWN_DEAL × AGENCY_INDEX for private acquisitions. In contrast, we expect a non-significant coefficient on the interaction term for public acquisitions.

Table 8 reports the regression results. For private deals, reported in column 1, the interaction term between HOMETOWN_DEAL and AGENCY_INDEX is negative and statistically significant, suggesting more negative reactions for private hometown deals if the bidders are weakly governed.[34] Column 2 reports coefficient estimates for public deals. We find no evidence that firm governance affects the performance of hometown deals in public targets. These results provide further support that private hometown deals are related to agency conflicts.

TABLE 8

Corporate Governance and Bidder Announcement Returns

Table 8 reports results on the impact of corporate governance on the bidder announcement returns. Column 1 reports coefficient estimates using the private target sample, and column 2 reports coefficient estimates using the public target sample. The dependent variable is the acquirer's 3-day cumulative abnormal returns centered on the acquisition announcement. The variable of interest is HOMETOWN_DEAL × AGENCY_INDEX. HOMETOWN_DEAL equals 1 if the deal is a cross-state home-state acquisition, and 0 otherwise. AGENCY_INDEX combines the following three measures of governance variables: i) the Gompers et al. (2003) governance index, ii) the percentage of shares held by institutional investors, and iii) the fraction of shares held by top managers. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. ** indicates significance at the 5% level.

	Private Targets	Public Targets
Variable	1	2
HOMETOWN_DEAL	0.0326	0.0434**
	(1.51)	(2.05)
HOMETOWN_DEAL × AGENCY_INDEX	−0.0929**	−0.0031
	(2.13)	(0.64)
AGENCY_INDEX	−0.0062	−0.0138
	(0.41)	(0.65)
Controls (Table 7)	Yes	Yes
Industry fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. of obs.	2,068	618
R^2	0.110	0.245

[34] We report in Panel B of Table IA.8 in the Supplementary Material that when looking at individual governance variables, high E_INDEX, low CEO ownership, and low institutional ownership are associated with lower performance on private hometown deals.

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C. Large Loss Deals

The implication from Van Nieuwerburgh and Veldkamp (2009) is that there should be less information noise about hometown deals. If public hometown deals are driven by home information advantage, then this could imply that CEOs should be able to avoid extremely bad public deals when acquiring from their home states. We test whether this is the case.

The summary statistics from Panel A of Table 7 suggest that CEOs are better at avoiding large losses in public deals when acquiring in their home states. We define a "large loss" based on the percentage loss (the CAR) or dollar loss (the acquirer's market cap times the CAR). LARGE_LOSS_DEAL1 is a dummy variable equal to 1 if the acquirer's CAR is in the bottom decile of the sample (less than −5.94%). By this definition, 10% of all deals experience large losses, but the frequencies are different for public and private deals (20.46% vs. 7.31%). LARGE_LOSS_DEAL2 is a dummy variable equal to 1 if the dollar loss is at least $1.0 billion, based on the 3-day CAR. There are 135 such deals (2.45% of the sample): 104 (9.21%) for public deals and 31 (0.71%) for private. Finally, LARGE_LOSS_DEAL3 is a dummy variable equal to 1 if the dollar loss is at least $500 million.

Although public acquisitions are more likely than private acquisitions to experience large losses, the large losses are concentrated in public non-hometown deals. Under all three definitions of large loss deals, the probability of public hometown deals experiencing large losses is always much smaller than that of public non-hometown deals: 7.32% vs. 20.96% for the first definition, 0 vs. 9.56% for the second, and 2.44% vs. 15.17% for the third. In contrast, among private acquisitions, hometown deals are more likely to experience large losses than non-hometown deals: the respective probabilities are 13.29% vs. 7.09%, 1.90% vs. 0.66%, and 4.43% vs. 1.97%.

We next examine whether these results hold when controlling for differences in deal and acquirer characteristics. Table 9 presents the regression results of linear probability models. The dependent variables are the three dummy variables of large loss deals. The main explanatory variable is HOMETOWN_DEAL. The first three columns are for the subsample of private acquisitions, and the last three columns are for public acquisitions.

The results in Table 9 show that among public acquisitions, CEOs tend to avoid large loss deals when acquiring targets from their home states. The coefficient on HOMETOWN_DEAL is significantly negative under all three definitions of large loss deals. Moreover, the coefficients are economically significant. For example, the coefficient estimate in column 4 indicates that acquiring in the CEO's home state decreases the large loss likelihood by 11.10 percentage points, which is about half of the unconditional probability of transacting in a large loss deal (20.46%). The coefficient estimate of −9.41% in column 5 implies that a hometown deal nearly wipes out the chance of losing $1 billion or more (unconditional probability is 9.21%), which is consistent with the univariate results in Panel A of Table 7.

For private acquisitions, the coefficient on HOMETOWN_DEAL is always positive but is only statistically significant in column 1 of Table 9. So, there is some evidence that CEOs are more likely to engage in extremely bad private deals

TABLE 9

Large Loss Deals and the Acquisition Hometown Bias

Table 9 reports results of regression analysis of the probability that a bidder makes a large loss deal. The table reports coefficient estimates from linear probability model regressions. LARGE_LOSS_DEAL1 (columns 1 and 4) is a dummy variable equal to 1 if the bidder's 3-day cumulative abnormal return is in the bottom decile of the sample, and 0 otherwise. LARGE_LOSS_DEAL2 (columns 2 and 5) is a dummy variable equal to 1 if the bidder experiences at least a $1 billion loss based on 3-day cumulative abnormal returns. LARGE_LOSS_DEAL3 (columns 3 and 6) is a dummy variable equal to 1 if the bidder experiences at least a $500 million loss based on 3-day cumulative abnormal returns. The variable of interest is HOMETOWN_DEAL. Models 1–3 report results for the sample of acquisitions for private targets, and models 4–6 report results for the sample of acquisitions for public targets. All regressions control for year fixed effects and industry (Fama–French 48-industry) fixed effects. Definitions of control variables are described in the Appendix. *t*-statistics based on standard errors adjusted for firm clustering are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Private Targets			Public Targets		
Variable	1	2	3	4	5	6
HOMETOWN_DEAL	0.0637**	0.0102	0.0199	−0.1110**	−0.0941***	−0.1279***
	(2.29)	(0.75)	(1.16)	(2.47)	(4.16)	(3.59)
FIRM_SIZE	0.0002	0.0111***	0.0268***	0.0047	0.0729***	0.0987***
	(0.06)	(5.47)	(8.78)	(0.54)	(8.98)	(12.24)
MB	0.0061	0.0019	0.0047**	0.0086	0.0176**	0.0084
	(1.60)	(1.04)	(2.01)	(0.80)	(2.09)	(1.02)
LEVERAGE	−0.0425	−0.0149**	−0.0225	−0.1119	−0.1117**	−0.1534***
	(1.52)	(1.97)	(1.53)	(1.27)	(2.35)	(2.59)
ROA	−0.1581**	−0.0171	−0.0151	−0.1018	−0.1579	−0.0249
	(2.49)	(0.83)	(0.39)	(0.58)	(1.36)	(0.19)
CASH	0.0620*	0.0020	0.0068	0.2376**	0.0277	0.0974
	(1.81)	(0.16)	(0.40)	(2.38)	(0.39)	(1.27)
LN_FIRM_AGE	−0.0126**	−0.0011	−0.0031	0.0080	0.0036	0.0033
	(2.58)	(0.74)	(1.13)	(0.52)	(0.35)	(0.27)
RELATIVE_DEAL_SIZE	0.0512**	0.0197***	0.0364***	0.2238***	0.0519**	0.0974***
	(2.33)	(3.18)	(3.47)	(5.53)	(2.10)	(3.51)
CASH_DEAL	−0.0073	−0.0073**	−0.0108**	−0.1558***	−0.0274	−0.0480**
	(0.75)	(2.15)	(2.11)	(4.97)	(1.33)	(2.07)
STOCK_DEAL	0.0206	−0.0023	0.0018	0.0044	0.0006	0.0008
	(1.07)	(0.37)	(0.21)	(0.12)	(0.03)	(0.03)
DIVERSIFYING_DEAL	0.0069	0.0002	0.0019	0.0197	0.0310*	0.0396*
	(0.84)	(0.09)	(0.46)	(0.80)	(1.78)	(1.77)
FRIENDLY_DEAL	−0.0023	−0.0301	−0.0056	0.0467	−0.0319	0.0457
	(0.03)	(0.65)	(0.12)	(0.65)	(0.49)	(0.68)
IN_STATE_DEAL	−0.0099	0.0038	0.0004	−0.0348	−0.0007	−0.0049
	(0.95)	(1.05)	(0.08)	(1.15)	(0.03)	(0.19)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	4,376	4,376	4,376	1,129	1,129	1,129
R^2	0.063	0.065	0.111	0.203	0.265	0.302

in their home states. We also conduct the full sample analysis and report the results in Appendix Table IA.11 in the Supplementary Material. The results are similar.

In short, we find that among public deals, acquirers are *less* likely to experience large losses if the target is from the CEO's home state; whereas among private deals, there is weak evidence that acquirers are *more* likely to experience large losses if the target is from the CEO's home state.

D. Additional Sources of the Value Gain in Public Hometown Deals

Our analysis so far suggests that CEOs create value when buying public targets from their home states. One reason for the value gain is that they are able to avoid extremely bad deals when making such acquisitions. Given the importance of public deals in M&A studies, we conduct additional tests to gain more insight regarding the value creation in public hometown deals.

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1. Role of CEO Home-State Residence in Hometown Deals

First, we explore CEO resident information to shed some light on the channel through which CEOs create value in public hometown deals. If it is indeed home advantage that drives the results in public hometown deals, then we would expect the value enhancement in such deals to be stronger when CEOs actually own a residence (i.e., a second home) in their home states surrounding the hometown deal.

To test this, we use data on executive residences from Bernile, Bhagwat, and Yonker (2018). For the subsample for which we have CEO-residence information, we define HMSTATE_RES as an indicator variable that is equal to 1 if the CEO is a resident in the home state during the 2 years leading up to the deal announcement or the 2 years after deal completion. If home advantages indeed drive our findings for public hometown deals, we expect the performance of hometown deals to be even better when CEOs own a home-state residence. In other words, we expect to see a positive coefficient estimate for the interaction term between HOMETOWN_DEAL and HMSTATE_RES. The results reported in column 1 of Table 10 show evidence supporting this conjecture. Column 2 reports coefficient estimates for the private-target sample, but we find no evidence that HMSTATE_RES affects the performance of hometown deals in private targets.

We recognize HMSTATE_RES can be a noisy measure for greater home advantage, especially if the home state is large. For example, a CEO who has a second home in San Francisco, California, may not necessarily have advantages if the target headquarters is in San Diego.[35] To mitigate this concern, we directly investigate the effect of distance between CEO residence and target headquarters on market reactions. We use model 2 in Panel B of Table 7, but we also include variables that indicate whether the distance is less than 100 miles, between 100

TABLE 10

CEO Home-State Residence and Bidder Announcement Returns

Table 10 reports results on the impact of CEO home-state residence on bidder announcement returns. Column 1 reports coefficient estimates using the private target sample, and column 2 reports coefficient estimates using the public target sample. The dependent variable is the acquirer's 3-day cumulative abnormal returns centered on the acquisition announcement. The variable of interest is HOMETOWN_DEAL × HMSTATE_RES. HOMETOWN_DEAL equals 1 if the deal is a cross-state home-state acquisition, and 0 otherwise. HMSTATE_RES is an indicator variable that is equal to 1 if the CEO resides in his home state 2 years leading up to the deal announcement and/or 2 years post deal completion, and 0 otherwise. Robust standard errors are clustered at the firm level. Robust *t*-statistics are in parentheses. * and ** indicate significance at the 10% and 5% levels, respectively.

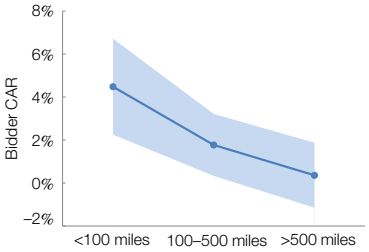
	Public Targets	Private Targets
Variable	1	2
HOMETOWN_DEAL	0.0033	−0.0150**
	(0.28)	(2.12)
HOMETOWN_DEAL × HMSTATE_RES	0.0351*	−0.0023
	(1.94)	(0.06)
HMSTATE_RES	0.0005	−0.0029
	(0.10)	(0.81)
Controls	Yes	Yes
Industry fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. of obs.	560	2,660
R^2	0.279	0.057

[35] The distance from San Francisco to San Diego is about 500 miles.

and 500 miles, or greater than 500 miles of the zip code. The coefficient estimates on these indicators, interacted with HOMETOWN_DEAL, are plotted in Figure 2 along with their standard errors. The figure shows that market reactions increase with shorter distances between CEO residence and target headquarters.

FIGURE 2
Target Distance from CEO Home-State Residence and Bidder Announcement Returns

Figure 2 depicts the coefficient estimates from a regression analysis that investigates the effect of distance between CEO residence and target headquarters on bidder announcement returns. We use model 2 in Panel B of Table 7 but also include indicator variables that indicate whether the distance between CEO residence and target headquarters is within 100 miles of the zip code, from 100 to 500 miles of the zip code, or greater than 500 miles of the zip code. The coefficient estimates on these indicators, interacted with HOMETOWN_DEAL, are plotted along with their standard errors.



2. Takeover Premium

One potential way that acquirers can create value for their shareholders is to negotiate better acquisition prices. We, therefore, investigate whether CEOs, on average, pay less to public hometown targets. We test this by investigating differences in the premium paid to hometown and non-hometown target firms.

We define premium paid to the target as the offer price relative to the market price of the target 4 weeks prior to the announcement of the deal (PPM4WK from SDC). In column 1 of Table 11, we report the estimated coefficient for HOMETOWN_DEAL from a regression of target premium on HOMETOWN_DEAL and firm and deal controls (same control variables as in column 2 of Panel B in Table 7; unreported for brevity). The coefficient is insignificant, suggesting that the premium paid to hometown targets is no lower than that paid to non-hometown public targets. Similarly, in column 2 of Table 11, we find that the 3-day announcement return to the target is also not significantly different in public hometown deals. This finding implies that the favorable acquirer market reactions in public hometown deals are not driven by lower premiums paid to hometown targets.

3. Combined Announcement Returns and Post-Acquisition Operating Performance

Next, we test whether hometown deals have greater synergies than other deals. We measure the synergy of a deal as the value-weighted CAR of the acquirer and the target. Column 3 of Table 11 reports the regression of the combined firm CAR on HOMETOWN_DEAL. The coefficient estimate on HOMETOWN_DEAL is significantly positive and suggests that, all else being equal, public hometown deals create higher synergies worth 1.91% of the combined firm value. Given the average combined firm value of $12.3 billion (median $8.1 billion),

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TABLE 11

Sources of Value Creation in Public Hometown Deals

Table 11 reports results of ordinary least squares regressions of the target premium, targets' 3-day cumulative abnormal returns, and combined firm announcement returns. The sample of acquisitions consists of 1,129 deals with public targets. The dependent variable in model 1 is the premium of offer price to target trading price 4 weeks prior to the deal announcement date from the SDC. The dependent variable in model 2 is the target's 3-day cumulative abnormal returns centered on the acquisition announcement. The dependent variable in model 3 is the combined 3-day cumulative abnormal returns centered on the acquisition announcement. The dependent variable in model 4 is ΔROA, the change in industry-adjusted ROA from 1 year before the deal announcement to 3 years after the deal completion. The variable of interest is HOMETOWN_DEAL, which equals 1 if the deal is a cross-state home-state acquisition, and 0 otherwise. The control variables are the same as in Table 7 (column 2 of Panel B), and the definitions are described in the Appendix. All regressions control for year fixed effects and industry (Fama–French 48-industry) fixed effects whose coefficients are suppressed for brevity. *t*-statistics based on standard errors adjusted for firm clustering are reported in parentheses. * and ** indicate significance at the 10% and 5%, respectively.

	Target Premium	Target CAR	Combined CAR	ΔROA
Variable	1	2	3	4
HOMETOWN_DEAL	0.0489	0.0311	0.0191**	0.0196*
	(1.09)	(0.93)	(2.17)	(1.76)
Controls	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. of obs.	1,122	1,129	1,129	992
R^2	0.133	0.168	0.182	0.247

a typical public hometown deal creates $235 million (median $155 million) worth of more synergies. Higher synergies in hometown deals are consistent with both more suitable targets and anticipated better integration of the firms *ex post*.

If public hometown deals truly create more synergies, then these synergies should be reflected in post-acquisition operating performance. We use ROA as our measure of operating performance and follow Healy, Palepu, and Ruback (1992) to benchmark ROA against industry median (e.g., Cai and Sevilir (2012), Duchin and Schmidt (2013)). To measure the pre-merger ROA, we calculate the value-weighted average of the industry-adjusted ROA of the acquirer and target in the fiscal year preceding the deal announcement, where the weights are based on the asset values of the two firms. We then track each acquisition for 3 years after the deal completion year and calculate the 3-year average of the combined firm's industry-adjusted ROA as our measure of post-merger ROA. Finally, we calculate the change in operating performance of the combined company, ΔROA, as the difference between post-merger ROA and pre-merger ROA.

To see whether public hometown deals are associated with higher post-acquisition operating performance, we estimate a regression of ΔROA on HOMETOWN_DEAL in column 4 of Table 11. We include the same control variables as those included in column 2 of Panel B in Table 7 (unreported for brevity). The results indicate that ROA increases by two percentage points more for hometown deals than for other deals in the post-merger period. This suggests that hometown deals are associated with higher post-acquisition operating performance.

V. Conclusions

It is well documented that investors, both individual and institutional, exhibit home bias when making their portfolio choices. Our study is the first to document that corporate managers also exhibit home bias when making real investment decisions in M&A. We show that firms are about twice as likely to acquire

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targets located in states where their CEOs grew up than similar targets domiciled elsewhere. For private targets, the hometown bias is stronger when acquirer governance is lax. Unlike typical private acquisitions, private hometown deals do not create value for acquiring firms. The evidence, thus, suggests that CEOs engage in these deals for their private benefits.

In contrast, when it comes to public target acquisitions, where the stakes are high and public attention is focused, the motivation for acquiring a target from the CEO's home state seems to be different. Unlike typical public acquisitions that tend to destroy bidders' shareholder value, hometown deals create value. Public hometown deals are less likely to experience large losses, suggesting that CEOs are able to avoid extremely bad deals in their home states. Further investigation reveals that the value created in public hometown deals is not because targets are underpaid. Rather, the evidence points to these deals creating value through higher synergies. Consistent with this evidence, acquirers of public hometown deals experience more positive changes in operating performance than acquirers of non-hometown deals during the post-merger period.

Appendix. Variable Definitions

Variables	Definitions
Main Variables	
CEO_HOME_STATE	Dummy variable equal to 1 if a state matches a firm CEO's state of origin.
HQ_STATE	Dummy variable equal to 1 if a state matches a firm's state of headquarters.
HOME_STATE_DEAL	Dummy variable equal to 1 if the target is headquartered in the acquiring CEO's home (grew-up) state. Note that a home-state deal is also an in-state deal if the bidder CEO grew up in the bidder's headquarters state.
IN_STATE_DEAL	Dummy variable equal to 1 if the bidder's headquarters state matches the state in which the target is headquartered.
HOMETOWN_DEAL	Dummy variable equal to 1 if the deal is a cross-state acquisition and the target is headquartered in the acquiring CEO's home (grew-up) state (i.e., a cross-state home-state deal).
BIDDER_CAR	Three-day cumulative abnormal return calculated using the market model. Source: CRSP.
LARGE_LOSS_DEAL1	Dummy variable equal to 1 if the bidder's 3-day cumulative abnormal return is in the bottom decile of the sample, and 0 otherwise.
LARGE_LOSS_DEAL2	Dummy variable equal to 1 if the bidder experiences at least a $1 billion loss based on 3-day cumulative abnormal returns.
LARGE_LOSS_DEAL3	Dummy variable equal to 1 if the bidder experiences at least a $500 million loss based on 3-day cumulative abnormal returns.
TARGET_PREMIUM	Premium of offer price to target trading price 4 weeks prior to the original announcement date (PPM4WK). *Source:* SDC.
Firm Characteristics	
LN_FIRM_SIZE	Natural logarithm of a firm's market cap. *Source:* CRSP.
ASSETS	Total assets. *Source:* Compustat.
MB	(Total Assets − Book Equity + Market Value of Equity)/Total Assets. *Source:* Compustat.
LEVERAGE	Sum of long-term debt and debt in current liabilities over book value of total assets. *Source:* Compustat.
ROA	Operating income before depreciation (EBITDA) over book value of total assets. *Source:* Compustat.
CASH	Cash and cash equivalent holdings over book value of total assets. *Source:* Compustat.
INVESTMENT	Capital expenditure over book value of total assets. *Source:* Compustat.
LN_FIRM_AGE	Natural logarithm of the number of years a firm has been listed. *Source:* CRSP.

(continued on next page)

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Variables	Definitions
Deal Characteristics	
DEAL_VALUE	Deal value from SDC, adjusted to 2014 dollars. *Source:* SDC.
RELATIVE_DEAL_SIZE	Transaction value over acquirer market value of equity. *Source:* SDC.
PUBLIC_TARGET	Dummy variable equal to 1 for public target. *Source:* SDC.
PRIVATE_TARGET	Dummy variable equal to 1 for private or subsidiary target. *Source:* SDC.
DIVERSIFYING_DEAL	Dummy variable equal to 1 if the target and the acquirer have the same 2-digit SIC code. *Source:* SDC.
CASH_DEAL	Dummy variable equal to 1 for deals paid for 100% by cash. *Source:* SDC.
STOCK_DEAL	Dummy variable equal to 1 for deals paid for 100% by stock. *Source:* SDC.
Other Measures	
G_INDEX	Governance index based on 24 antitakeover provisions, taken from Gompers et al. (2003).
E_INDEX	Governance index based on 6 antitakeover provisions, taken from Bebchuk et al. (2009).
INSTITUTIONAL_OWNERSHIP	Percentage of shares held by institutional investors.
MANAGERIAL_OWNERSHIP	Acquirer CEO's percentage ownership of the firm, including both stock and stock options. *Source:* ExecuComp.
AGENCY_INDEX	An index that combines the following three measures of governance variables: i) the Gompers et al. (2003) governance index (G-index), ii) the percentage of shares held by institutional investors, and iii) the fraction of shares held by the top managers. In particular, the agency index averages a firm's percentile ranking in the sample according to each measure (for the latter two, the reverse ranking is used). We then scale the index to range from 0 (low) to 1 (high).
CEO_AGE	Age of acquirer CEO. *Source:* ExecuComp.
CEO_GENDER	Dummy variable equal to 1 if the acquirer CEO is a male, and 0 otherwise. *Source:* ExecuComp.
CEO_TENURE	Number of years the acquirer CEO has been in office upon the deal announcement. *Source:* ExecuComp.
CEOCOLLSTATE	Dummy variable equal to 1 if the CEO attended college in state *s*. *Source:* BoardEx.
CEOBOARDSTATE	Dummy variable equal to 1 if the CEO holds a board position in firms from state *s*. *Source:* BoardEx.
CEOHMTENUREGTMED	Dummy variable that is 1 if the number of years that the CEO lived in his home state is greater than the median in the sample. A particular CEO's home tenure is equal to his age if the CEO's home state matches the state in which the firm is headquartered. If the two states do not match, then, if the CEO attended college in the same state as his home state, the age at which the CEO graduated from his degree program is considered the CEO's home tenure. If the CEO did not attend college in his home state and does not work for a firm headquartered in his state, then the CEO is assumed to have left the state 4 years prior to obtaining a degree at an institution outside his home state (Pool et al. (2012)). *Source:* BoardEx.
OVERCONFIDENT_CEO	Following Hirshleifer et al. (2012), this dummy variable equals 1 if a CEO postpones the exercise of vested options that are at least 67% in money, and 0 otherwise.

Supplementary Material

Supplementary Material for this article is available at https://doi.org/10.1017/S002210901800131X.

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