PATENT TROLLS AND THE MARKET FOR ACQUISIITIONS

by

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A DISSERTATION

Presented to the Department of Finance
and the Graduate School of the University of Oregon
in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy

June 2020

DISSERTATION APPROVAL PAGE

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Title: Patent Trolls and the Market for Acquisitions

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Degree awarded June 2020

DISSERTATION ABSTRACT

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Doctor of Philosophy

Department of Finance

June 2020

Title: Patent Trolls and the Market for Acquisitions

Frivolous patent-infringement claims increase the cost of innovation for small businesses and force them to exit via premature and discounted acquisitions. This study investigates the effect of abusive patent-infringement claims by patent trolls on acquisitions of small firms. I exploit the staggered adoption of anti-patent troll laws in 35 states as a quasi-natural experiment and find that the laws have two effects on acquisitions. First, the number of acquisitions of small businesses by large firms declines after these laws are passed. Second, the anti-troll laws increase the acquisition price for large firms. I find that the market reflects the increased cost of acquisition following the passage of anti-troll laws as measured by the lower acquisition announcement returns. Moreover, I find evidence that large firms increase R&D expenditure after the adoption of state laws. Using a sample of acquisitions that are plausibly unaffected by the state laws, I disentangle alternative explanations such as local economic shocks, industry-wide changes and merger waves. Overall, the findings suggest that the anti-patent troll laws increase the value of small innovative firms.

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ACKNOWLEDGMENTS

I am indebted to my advisor and committee chair, Brandon Julio, for his unsparing support throughout my doctoral studies and being an inspiration in professional life. I would like to extend my sincere appreciation and gratitude to my committee members, Steve Mckeon, Jay Wang, and Jeremy Piger for their guidance and constructive comments that were very helpful in advancing my dissertation. I am also thankful to the faculty of University of Oregon's Department of Finance, the PhD program, and the staff at Lundquist College of Business for their support.

I dedicate this dissertation to my family. My wife, Atoosa, has provided me unwavering support and encouragement during the past five years of my journey. She has cheered me on when I was discouraged and has always been confident in my ability to get this done! To my parents, Mehdi and Azam, who instilled in me the love of learning from early age. Thank you, mom and dad, for always believing in me. Without you, I would never be the person I am today.

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CHAPTER I

INTRODUCTION

Patent litigation has historically been the last resort for companies to protect their products and services against patent infringement by their competitors. In recent years, however, entities who do not have an economic interest in the technology underlying a patent have been purchasing large quantities of patents with the sole purpose of asserting patent rights against other companies. As a result, the majority of patent lawsuits today are filed by these non-practicing entities (patent trolls) whose core business is not production, but patent litigation and licensing (Cohen, Gurun, and Kominers, 2019).[1]

Using stock market reaction to patent lawsuit filings from 1990 through 2010, Bessen, Ford, and Meurer (2011) find that patent troll lawsuits are associated with $500 billions of lost wealth to public companies.

Yet survey evidence suggests that lawsuits filed against public companies is only a small fraction of patent trolls' activities (Chien, 2013). Patent trolls primarily target small firms because they have neither the financial resources nor sufficient legal knowledge to defend themselves in a lengthy lawsuit should they be targeted by trolls. Moreover, Chien (2013) documents that most small firms prefer to quickly reach a settlement and pay a simple "go-away" fee regardless of actual infringement simply because settlement is less costly than the litigation process. Chien (2013) reports that startups that entered settlements have paid $340,000 on average, whereas those that fought the demands in court have faced average expenses of $857,000.

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[1] The surge of patent troll activities has led to a nearly threefold increase in the number of patent-related lawsuits from 2005 to 2013

As a result, patent trolls often send demand letters to thousands of (mostly) small firms, offering them licensing deals in lieu of litigation. One of the most notorious patent trolls, MPHJ, sent demand letters to over 16,000 small firms between 2012 and 2013, but never filed a single lawsuit.

Chien (2013) documents that small firms that are frequently targeted by patent trolls' demand letters experience significant losses in valuation, shutting down, and/or selling out to well-funded firms at heavily impaired prices. In this paper, I investigate the effect of patent trolls' frivolous infringement claims on the economics of acquisitions involving small targets. My empirical strategy exploits the staggered adoption of state-level anti-troll laws in 35 states that limit the ability of patent trolls to target local firms with bad-faith assertions of patent infringement via demand letters.[2]

I begin my investigation by examining the effect of state anti-troll laws on the number of acquisitions involving small targets. When facing the decision to accept an acquisition offer, a small target compares the value of independent operation with the acquisition offer. The adoption of anti-troll laws increases the value of operations for small firms by reducing the exposure to patent trolls. As a result, small firms are less likely to agree to acquisitions. Consistent with this conjecture, I find that the number of acquisitions involving independent companies in the tech industries declines by 8.3% after the adoption of anti-troll laws. In contrast, the adoption of state laws has no effect on the acquisitions of non-tech firms. Moreover, I find that the effect of anti-troll laws is stronger for small businesses. These findings are consistent with the existing evidence that patent trolls target firms in the tech industries and mainly small firms. For example,

[2] Examples of bad faith are letters with intentionally vague information and ambiguous claims, letters with unreasonable licensing fees, and payment demands in an unreasonably short period of time.

The Washington Post describes patent trolls and their litigation as: "The preponderant economic picture is that patent litigation now imposes substantial costs, particularly on small and innovative firms."

Next, I turn to investigate the effect of anti-troll laws on the acquisition price the targets receive. A target agrees to an acquisition if the offer is higher than the value of operating independently. Given that the adoption of state laws increases the value of operating independently, small businesses located in the adopting states only agree to more lucrative offers that exceed the now-increased value of their stand-alone operation. Thus, the adoption of anti-troll laws is expected to lead to larger payments to targets. The empirical evidence supports this hypothesis. I define price ratio as the value of the acquisition deal scaled by book value of the target's assets and find that the adoption of anti-troll laws increases acquisition price ratios in the tech industries significantly and that this effect is magnified among small firms. This finding suggests that, after the adoption of state laws, tech targets receive a larger payoff for their investment in the firm (total assets) if they choose to be acquired by a larger firm.

Identifying the effect of the adoption of anti-troll laws as a policy change is empirically challenging due to the potential endogeneity concern that a confounding factor, such as a regional economic shock, might affect both the adoption of the laws and the acquisition market. However, the staggered nature of the adoption of the state laws helps mitigate this concern in that any potential confounding factor is less likely to coincide with the laws in the same staggered nature. Moreover, I perform a series of analysis to tests the validity of the identification assumptions. I find that both patent troll litigation at U.S. district courts as well as Google searches related to patent trolls decline in states that adopt the laws, providing further support that the anti-troll laws are effective in limiting the activities of patent trolls.

I also find that the adoption of anti-troll laws has no effect on the acquisitions of targets that are part of a larger firm. These targets, which I refer to as non-independent targets, benefit from the financial resources, legal expertise, and overall protection of their large, well-funded parent companies. Hence, the protection provided by anti-troll laws is less valuable to them. The finding that non-independent targets are not affected by the state laws is consistent with the notion that I capture the effect of anti-troll laws on acquisitions and not other economic or political confounding shocks that may affect a broader set of acquisitions.

Finally, consistent with the parallel trend assumption, I find that the acquisition market in treated and control states do not differ up to four years before the adoption of the laws, providing comfort that the control states are valid counterfactuals for how the acquisition market would have evolved in the treated states in the absence of anti-troll legislation. Moreover, in contrast to a local shock that is likely to affect multiple states in the same region, the effect of state laws stops at the state borders and does not spill over to the neighboring states. Collectively, these findings help mitigate the identification concerns and thus provide further support that the effect of state anti-troll laws on the acquisition market is properly identified.

An additional hypothesis involves the choice of payment in the acquisitions of small targets. The diversification hypothesis posits that an entrepreneur should always prefer cash payments to mitigate her idiosyncratic risk exposure since stock ownership ties her wealth more closely to her innovation. However, stock ownership in the acquirer becomes appealing to the entrepreneur as the value of the innovation increases, and thus the returns to monetizing the innovation justifies the risk exposure. Choice of payment thus reveals an entrepreneur's desire to remain engaged with her innovation. Since the adoption of anti-troll laws increases the entrepreneur's incentives to remain engaged (the value of innovation increases with state laws), the probability of receiving an equity payment should increase after the adoption of state laws. I find empirical evidence supporting this hypothesis: conditional on an acquisition, the likelihood of accepting an equity-like payment increases by 3%-4% for tech targets after the adoption of anti-troll laws.

I next investigate the effect of anti-troll laws on how investors in acquiring firms react to acquisition announcements. Because the laws increase the payment to targets, the value that acquirers can extract from an acquisition declines, which should have a negative impact on the market reaction to such acquisitions. In a sample of acquisitions involving public acquirers[3], I find significant evidence that acquirers have lower abnormal returns during the announcement period if the acquisition involves targets that are affected by the adoption of anti-troll laws. Specifically, the firms that acquire tech targets affected by the laws have 1.4% lower cumulative abnormal returns during the 3-day acquisition announcements relative to non-tech targets. In terms of economic magnitude, this lower announcement return is associated with $267 million lower value accrued to an average public acquirer given that the average market value of the public acquirers is $19 billion.

[3] The market reaction to an acquisition announcement can be defined only for public firms.

Finally, I examine the effect of anti-troll laws on the innovation activities of public firms. Anti-troll laws impact public firms through two channels. First, the adoption of anti-troll laws protects public firms as well, though to a lesser extent, and thus provides them with more incentives to engage in innovation. Second, the passage of anti-troll laws shrinks the pool of potential targets that supply innovation to larger firms. Therefore, acquiring innovation becomes more expensive for large, public firms in states with these laws in place. Public firms in these states, then, are more likely to turn to alternative methods to meet their innovation needs: internal research and development (R&D). The empirical evidence is consistent with the positive effects of anti-troll laws on public firms' R&D. I find that R&D expenditures increase in public tech firms by 7% after the adoption of anti-troll laws in their state. Moreover, to achieve an estimate of the effect on the intensive margin, I limit the sample to firms with positive R&D at the beginning of the sample and find that the effect is magnified at 15.8%.

Although there exists a large body of research on innovation in economics and finance, little attention has been paid to the effects of patent trolls. Using stock market reactions to patent lawsuit filings, Bessen, Ford, and Meurer (2011) find that patent troll lawsuits are associated with $500 billions of lost wealth to defendants from 1990 through 2010. Feldman and Frondorf (2015) report that significant proportion of recently public companies received patent demands either shortly before or after their IPO, one of the most public and vulnerable periods of a company's development. Cohen, Gurun, and Kominers (2019) show that patent trolls opportunistically target firms that are flush with cash and firms that are busy dealing with other patent-unrelated litigation. They find that firms that lose to patent trolls (either in court or through settlement) reduce their research and development expenditures by roughly 20% going forward, relative to ex ante identical firms. Smeets (2014) also documents significant decreases in innovation activities in public firms targeted by patent trolls.

This study adds to this growing literature in two distinct ways. First, I examine the effect of patent trolls' abusive patent claims on small businesses as opposed to large, public firms. Investigating small firms is necessary given that survey evidence in Chien (2013) indicates that firms with $100 millions of annual revenue or less comprise at least 66% of firms targeted by trolls and at least 55% of unique defendants in troll suits make $10 millions or less per year. Second, While the studies above examine the effect of being sued in court by a patent troll, I focus on the contingent threat of being targeted by a troll with frivolous mass demand letters. This distinction is crucially important because the existing evidence both in academic research and in the media suggests that patent lawsuits are just the tip of the iceberg and the significant majority of patent disputes are settled outside of the courtrooms. Therefore, investigating the burden patent trolls and their abusive demand letters put on small, innovative companies is of great interest.

This paper also provides additional evidence on the effect of state anti-troll laws, which have been a source of controversy among patent experts, policy makers, and regulators since their adoptions. On one hand, the laws are aimed to hamper the frivolous patent demands by increasing the costs of bad-faith patent assertion. On the other hand, critics argue that the laws might create impediments for the whole class on non-practicing entities in helping small firms monetize their innovation and bring legitimate patent claims to court. Therefore, careful examinations of the state laws seem warranted. Appel, Farre-Mensa, and Simintzi (2019) show that the adoption of state anti-troll legislation leads to an average 2% increase in employment at small high-tech firms, which are precisely the firms that patent trolls tend to target with demand letters. My study is different from their work in that I directly examine the wealth effects of the laws on the owners of small firms. I document that small businesses extract more value through higher acquisition prices should they choose to be acquired. Overall, it appears that the laws help small firms to avoid premature or discounted acquisitions as an exit outcome.

CHAPTER II

METHODOLOGY

In this chapter, I briefly discuss the institutional background of the state anti-troll laws, and then, I lay out the empirical methodology I employ to capture the effect of state-level anti-troll laws on the economics of acquisitions of small targets.

State Anti-patent Troll Laws

While both the number of patent-related litigation and the share of cases brought by patent trolls have increased alarmingly in recent years[4], litigation is not the only tool that patent trolls use to make patent infringement allegations. When a patent-related lawsuit is filed by a patent troll, both the small firm (defendant) and the patent troll (plaintiff) must supply documents to demonstrate how the allegedly infringing product is made during the lawsuit discovery phase. Since patent trolls do not make products, the discovery phase is extremely costly for the defendant while it is far less costly for the plaintiff. A significant number of patent trolls take advantage of this cost asymmetry and the resulting lawsuit aversion of their targets by sending demand letters, with the intentions of making a licensing offer and threatening litigation unless a royalty fee is paid. Patent trolls hope that their small targets will be coerced into paying settlements because of their significant aversion to lawsuits and their lack of financial resources and experience with the patent system (AIPLA, 2013). For example, one of the most notorious patent trolls, MPHJ, sent demand letters to over 16,000 small firms between 2012 and 2013, but never filed a single lawsuit.

[4] According to a report by RPX in 2015, the share of cases brought by non-practicing entities has increased from 35% in 2010 to 69% in 2015.

Unfortunately, the number of businesses targeted by frivolous demand letters is not observable. However, survey evidence indicates that, particularly among small firms, the number of targets is substantially larger than the number of firms that are sued in court. Chien (2013) reports that 35% of the surveyed startups have been targeted by demand letters. Startups that ended up entering into a settlement with a patent trolls reported the average expenses of $340,000 (13% of annual revenue). Moreover, in a survey of IPO firms during 2007 and 2012, Feldman and Frondorf (2015) report that nearly half of respondents reported receiving demand letters in the period surrounding the IPO. The survey evidence in Chien (2013) and Feldman and Frondorf (2015) shows that small businesses bear significant costs when they are targeted by demand letters and that these costs are crippling to their core business in most cases.

The significant amount of evidence that reveals the value decreasing behavior of patent trolls calls for new regulations aimed at protecting inventors. As a result, several bills have been introduced in Congress since 2012 to limit the activities of patent trolls, and in particular their ability to send abusive demand letters.[5] However, as of today, none of them have become law because the proponents of patent trolls contend that they encourage innovation as an efficient intermediary. Patent trolls allow individual inventors, who lack the capacity to commercialize their patents, to monetize them through a more efficient licensing entity. Moreover, patent trolls provide a mechanism to enforce patents against infringers that the individual inventor may not be able to afford.

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[5] These include the Targeting Rogue and Opaque Letters (TROL) Act (H.R. 2045), the Patent Transparency and Improvements Act (S. 1720), the Saving High-tech Innovators from Egregious Legal Disputes (SHIELD) Act (H.R. 845), the Innovation Act (H.R. 3309), the Stopping the Offensive Use of Patents (STOP) Act (H.R. 2766), the Transparency in Assertion of Patents Act (S. 2049), and the Demand Letter Transparency Act (H.R. 1896).

Nonetheless, the empirical evidence in the law, economics, and finance literature is inconsistent with the intermediary role of patent trolls (e.g., Cohen, Gurun, and Kominers, 2019; Chien, 2013; Feldman, 2013; Feldman and Frondorf, 2015; Smeets, 2014; Tucker, 2014; and Cortropia, Kesan, and Schwartz, 2014).

In response to lack of federal legislation, a number of states, beginning with Vermont in 2013, have adopted patent reforms that protect local businesses from bad-faith demand letters. For example, the Vermont's anti-troll law is intended to increase the potential costs of sending out mass demand letters for patent trolls. Specifically, if a court finds that a firm has been the target of a bad-faith demand letter, then the court may award it the following: (1) equitable relief; (2) damages; (3) costs and fees, including reasonable attorney's fees; and (4) exemplary damages in an amount equal to $50,000 or three times the total of damages, costs, and fees, whichever is greater. As of the beginning of 2018, 35 states have passed a version of the anti-troll laws. Figure 1 depicts states that have passed the anti-troll laws as of the beginning of 2018. Also, Appendix C reports the signing dates for each state anti-troll law.

The political economy surrounding the laws have varied across states. In some states, including Vermont, the legislation was pushed by small businesses. In others, the law was initiated by financial institutions. While anti-troll laws spread quickly, California, as one of the largest and most innovative states, has yet to pass such a law. Although, an anti-troll law was introduced in the California State Senate in February 2015 with the support of key Senators as well as the Silicon Valley Leadership Group, it was not passed due to disagreements on specific amendments. In my analysis, I show that the results are robust to exclusion of California from the control group. Overall, the fact that lobbying for the laws was initiated by either small firms or a non-high-tech industry group (such as financial institutions) mitigates reverse-causality concerns.

Empirical Design

The passage of anti-troll laws across the states has been staggered over the 2013-2017 period. This staggered adoption provides me with a clean setting to investigate the effect of patent trolls' behavior on small businesses and innovation activities by utilizing a difference-in-difference methodology with staggered treatment events. Specifically, to identify the effect of anti-troll laws on state-level outcomes such as the number of troll lawsuits and number of acquisition transactions, I estimate the following difference-in-difference specification at the state-quarter level:

$$y_{s,t} = \alpha_s + \lambda_t + \beta . Anti\ Troll\ Law_{s,t-1} + \Gamma . X_{s,t-1} + \epsilon_{s,t} \qquad (1)$$

where *s* denotes state and *t* denotes quarter. *Anti-Troll Law* is a dummy equal to 1 if the state *s* has passed the anti-patent troll law at any time before *t* and zero otherwise. *X* is a vector of control variables at the state level, including state GDP, state per capita income, and an indicator variable that takes a value of 1 if the state has adopted another initiative to promote small businesses at any time before quarter *t*. λ_t is a set of year-quarter fixed effects to control for macroeconomic shocks that affect all acquisition activities in all states. α_s is a set of state fixed effects to control for time-invariant differences across states.

To identify the effect of anti-troll laws on deal-level outcomes, including acquisition price ratio, method of payment, and market reactions to deals involving public acquirers, I estimate the following difference-in-difference specification at the deal level:

$$y_{i,s,t} = \alpha_s + \lambda_t + \beta . Anti\ Troll\ Law_{s,t-1} + \Gamma . X_{s,t-1} + \Delta . M_i + \epsilon_{i,s,t} \qquad (2)$$

where all variables are the same as in Equation (1) except for an array of deal-level variables, M, that I include as a new set of controls. Most of the dependent variables are positively serially correlated and the anti-troll laws do not change in any state once they are adopted. Therefore, in all specifications, I report robust standard errors that are clustered at the state level (Bertrand, Duflo, and Mullainathan, 2004).

This empirical design has two advantages. First, one potential concern is that an omitted variable coinciding with adoption of anti-troll laws could be the true underlying cause of changes in acquisition activities. Due to the staggered nature of the adoption of the anti-troll laws, an omitted variable would need to fluctuate every time (or even most of the time) an anti-troll law is adopted. Therefore, this approach mitigates omitted variables concerns. Second, the staggered passage of the anti-troll laws means that the control group is not restricted to states that never pass a law. In fact, Equation (1) can be estimated even if all states eventually passed a law. It takes as the control group at quarter t all firms located in states that do not pass a law as well as states that will pass the law after quarter t.

CHAPTER III

DATA AND DESCRIPTIVE STATISTICS

Data

I obtain acquisition transactions data from Capital IQ. The rationale for using Capital IQ is twofold. First, Capital IQ reports a broader sample of transactions in comparison to other data sources such as SDC and thus includes acquisition transactions that involve very small targets, which are the main focus of this study. Second, the financial records of private target firms are better populated in Capital IQ's database. Since the state anti-troll laws apply to firms based on the location of operation, I require target firms to be located in the United States. The original sample of all transactions with a U.S. target from the first quarter of 2010 to the first quarter of 2018 includes 83,462 observations.

Capital IQ reports acquisitions of independent as well as non-independent targets. For example, if IBM acquires Vision Solutions Inc., it will be reported in Capital IQ regardless of whether the target is an independent business entity or is sold to IBM by Microsoft. In the latter case, Capital IQ reports Microsoft as the seller. The type of seller in the data varies from private firms and investment firms to public companies. I exclude all deals for which a seller is reported since this study is concerned only with independent small businesses. This restriction reduces our sample size to 55,902 observations. The non-independent acquisitions, however, are used in robustness tests.

Because patent trolls mostly tend to target firms in the tech industries (AIPLA, 2013; Chien, 2013), I mainly focus on transactions within the tech industries. Specifically, I follow Bureau of Labor Statistics (BLS) to identify tech industries. The

BLS used data from Occupational Employment Statistics survey and Current Population survey to determine the share of jobs in each industry that are held by STEM workers. I use the 4-digit SIC codes of the target to determine whether the target is in one of the tech industries.[6]

The final sample of M&A transactions comprises 12,631 deals in tech industries and 30,267 deals in non-tech industries. I aggregate the transactions data to state-quarter observations for both tech and non-tech acquisitions using the geographic location of the target and the announcement date of the deal.

Calculating the traditional acquisition premium, defined as the ratio of offered stock price and target's stock price at announcement date, is impossible in my setting given that almost all target firms in my sample are private firms. Therefore, as a measure of the payoff to the target's shareholders, I define *Price Ratio*. Specifically, I define price ratio as the ratio of the deal value over the book value of the firm. This measure incorporates both the future investment opportunities of the firm and the premium the acquirer is willing to pay.

While different from the commonly-used acquisition premium, this measure captures the value the acquirer is willing to pay for a given dollar of investment by the target firm (total assets). Thus, using this measure, I investigate whether tech targets receive higher payoffs after the adoption of anti-troll laws. Deal value is reported for 9,504 observations (22% of the sample). As the book value of the target, I use *Total Assets* which is reported for only 2,821 deals. The overlap of the two groups gives me 1,367 deals.

[6] When the SIC code for the target is not reported, I use the SIC code of the buyer to determine whether the deal is a tech deal.

I collect data on patent-related litigation using RPX Corporation, a company that tabulates information on patent trolls and patent litigation. Specifically, I use the list of all patent-related litigation cases in the U.S. and assign the cases to states according to the district court the case is brought to. I aggregate the litigation data to the state-quarter level. This data is manually obtained from RPX website.

In all state-level analyses, I control for the following state-level macroeconomic variables: the state quarterly real GDP growth rate and natural logarithm of per capita income from the Bureau of Economic Analysis. Following Appel, Farre-Mensa, and Simintzi (2019), I also control for contemporaneous state laws aimed at promoting small businesses and startups. Appel, Farre-Mensa, and Simintzi (2019) provide the list of such legislation in their study. I use the list they complied up to the end of their sample. To update the list of initiatives, I use the Council for Community and Economic Research State Business Incentive database, which collects information on state-level business incentive programs.

Descriptive Statistics

Table 1, Panel A reports descriptive statistics for the whole sample. On average, there are 24.96 acquisition deals, 7.23 tech deals, and 17.73 non-tech deals in each state in a given quarter. The median of acquisition deals in a state-quarter is significantly lower than the average, suggesting that the distribution of deals in a state-quarter is skewed. Thus, I use the log transformation of the number of deals as the dependent variable in all of my analyses. The sample contains 42,631 acquisition deals. The size of the deal is reported in the sample for only 9,504 deals and it averages $257 million. Although there are a number of very large deals that increase the average, the majority of the deals are very small with a median of $15.6 million.

Due to data limitations, I can only measure price ratio for 1,367 deals, 461 of which are tech deals and 960 are non-tech deals. The average price ratio is 4.47 for tech deals and 3.05 for non-tech deals, implying that acquirers, on average, pay $4.47 for a single dollar of assets in tech targets while pay only $3.05 for a single dollar of assets in non-tech targets. In other words, acquirers are willing to pay tech targets 46% more for a single dollar of assets in comparison to non-tech targets. Also, 27% of tech deals involve non-cash payments to the target while 22% of non-tech deals involve non-cash payments.

Panel B reports the number of acquisition deals according to their size and industry. The value of 2,175 deals is less than $50 million, which are identified as small deals; 619 deals are larger than $50 million; and 9,570 are missing size. In non-tech industries, there are 30,267 deals, 5,007 of which are small, 1,655 are large, and 23,605 have missing size. Not surprisingly, the non-independent deals are larger, and their size is reported with greater frequency.

Figure 2 shows the distribution of acquisition deals across the states over the sample period. The acquisition activity is fairly similar across the states with a few exceptions. California has the highest number of deals in both tech and non-tech industries by more than 3,500 tech deals and 2,400 non-tech deals. Texas, Florida, and New York have high acquisition activities as well. For robustness, I show that all the results reported in this study hold after excluding the states with abnormally high acquisition activity. Moreover, Figure 3 shows the distribution of acquisitions over the years. The acquisitions show a steady level over the years with little variation in both tech and non-tech industries. 2018 reports a smaller level of acquisition activities because the sample only includes the first quarter of the year.

CHAPTER IV

ANTI-TROLL LAWS AND THE NUMBER OF ACQUISITIONS

In this section, I begin my analysis by examining the effect of anti-troll laws on the number of acquisitions. Then, I provide some evidence in support of the parallel-trend assumption that is necessary in a differences-in-differences setting. Last, I check whether the results are robust to different sets of specifications and assumptions.

Baseline Results

When facing the decision to sell to a larger firm, a small target compares the expected value of continuing independent operation with the acquisition offer. Adoption of anti-troll laws increases patent trolls' expected cost of trolling small businesses, reducing their activities in states that adopt the laws. The reduced exposure to patent trolls lowers the cost of operating independently for small firms and increases their value. A small firm with higher expected value of operation is less likely to agree to be acquired simply because fewer acquisitions beat the now-increased value of independent operation. Therefore, I expect anti-troll laws to cause the number of acquisitions to drop.

I examine how anti-troll laws affect acquisition activities that involve a large, well-funded acquirer bidding on a small target and report the results in Table 2. Column 1 shows that the adoption of anti-troll laws in a state reduces the number of acquisitions by 5.4% with statistical significance at 10% level. Given that the average number of acquisitions of independent targets in a quarter is 25, a 5.4% decrease is equal to 1.3 fewer transactions per quarter in an average state.

In columns 2 and 3, I report results for tech and non-tech deals separately. As expected, the number of acquisitions in the tech industries declines by 8.3% with stronger statistical significance, whereas the change in the number of deals among non-tech industries is not significant. These findings support the notion that patent trolls are more active in the tech industries where patents are vaguer and more complex (AIPLA, 2013; Appel, Fare-Mensa, and Simintzi, 2019; Chien, 2013).

Moreover, Figure 5 shows how the number of acquisitions changes around the adoption of anti-troll legislation. The figure plots the estimated average difference in acquisitions at treated states relative to control states from year *t-3* to year *t+3* and beyond, where for each treated state, quarter *t* is the quarter when the anti-troll law is signed into law. Panel (a) plots the estimated differences for tech industries. While the difference between treated and control states is not significantly different from zero, the number of tech acquisitions are significantly lower in treated states in years following the adoption of state laws. Panel (b) plots the estimated differences for non-tech industries. The number of non-tech acquisitions do not differ significantly in treated states following the adoption of state laws.

Small businesses often do not have legal expertise and thus are more likely to be forced into unfair settlements with patent trolls. Hence, I expect anti-troll laws to have a larger impact on small businesses. In columns 4 and 5, I limit the sample to acquisition deals in which the target is small in tech and non-tech industries respectively. I define a small deal as one in which the valuation of the target is less than $50 million based on the value of the deal. The effect of anti-troll laws on acquisitions is significantly larger for smaller targets. Acquisition of small businesses declines by 9.7% in tech industries and by 10.9% in non-tech industries.

The findings in Table 2 support the hypothesis that a contingent threat of patent trolls (i.e., in form of demand letters) is costly to businesses in the tech industry and forces them to sell to deep-pocketed firms that have significant knowledge and resources to combat trolls and their frivolous claims. In non-tech industries, however, the effect is only observed among small firms where a lack of legal knowledge and experience is most likely to be present. Thus, it appears that the protection that comes with the adoption of anti-troll laws reduces the frequency with which small firms choose to exit via an acquisition, especially in tech industries. The evidence is consistent with the typical targets of patent trolls' demand letters: small firms especially in tech industries.

If the decrease in the number of acquisitions is related to the protections small businesses gain after the adoption of anti-troll laws, then the adoption of such laws should not have an impact on acquisitions when an alternative form of protection against trolls is present. I test this conjecture by examining the effect of anti-troll laws on a sample of acquisitions that involve non-independent targets. I define non-independent targets as firms that are owned and operated by larger firms such as private investment firms and other public companies. These non-independent firms are protected from patent trolls by the legal expertise of their parent companies. Therefore, passage of anti-troll laws is expected not to have as large of an effect on these acquisitions. To test this hypothesis, I re-estimate the regressions in Table 2 for non-independent acquisitions and report the results in Table 3. Consistent with my conjecture, the effect of anti-troll laws on non-independent acquisitions is not statistically significant.

One potential concern is that the anti-troll laws might coincide with other economic and/or regulatory changes that tend to affect mergers and acquisitions. A large strand of literature on mergers and acquisitions has empirically documented that mergers and acquisitions occur in waves and strongly cluster by industry due to industry-specific economic, regulatory and technological shocks (see e.g., Mitchell and Mulherin, 1996; Andrade, Mitchell, and Stafford, 2001; Harford, 2005). The analysis of acquisitions of non-independent targets helps rule out the potential role of industry trends in acquisitions. Specifically, it is unclear how an industry-specific shock that triggers mergers would affect non-independent and independent acquisitions differently. Moreover, Netter, Stegemoller, Wintoki (2011) show that the clustering of mergers appears to be driven largely by the clustering of acquisitions of public firms by public firms and inclusion of smaller and private deals appears to substantially attenuate the evidence for merger waves. The significant majority of the deals in this study involves small private targets with median size of $15 million, mitigating the concern involving merger waves.

Identification Assumptions

An important concern is that an omitted variable could be driving both the adoption of anti-troll laws and the reduction in acquisition activity. For instance, suppose that the local economy in one state is booming. As a result, the small businesses in the state may find it more attractive to stay in business rather than agreeing to acquisitions. At the same time, legislators might adopt anti-troll laws because of lobbying by local small businesses. In this case, finding a negative treatment effect would be a spurious outcome. Furthermore, a reverse causality concern may arise if the states' innovation intensities and business activities trigger the new state-level regulation. In this section, I provide several different pieces of evidence that help alleviate omitted variable and reverse causality concerns.

First, Figure 4 shows that the number of lawsuits filed by patent trolls has declined since 2013, the beginning of the state-level anti-troll laws. Using the staggered adoption of the state laws in a difference-in-difference methodology Equation (1), I find that the national decline in the number of lawsuits is mostly due to the treated states. Specifically, column 1 of Table 4 shows that the number of patent-related lawsuits filed in U.S. district courts in the treated states declines by 8.8% after the adoption of the state laws. In columns 2-4, I provide evidence that the decrease in the patent litigation activity after the adoption of anti-troll laws is robust to the exclusion of California and Texas as well as a weighted regression that accounts for the initial litigation activity in each state.

Further, similar to Appel, Farre-Mensa, and Simintzi (2019), I find that the google searches that are related to patent trolls decline in treated states after the adoption of the laws. Specifically, I collect data on Google's Search Volume Index for the term "patent troll" for each state-quarter and estimate Equation (1) where the dependent variable is the natural logarithm of one plus the Search Volume Index. Search Volume Index of Google Trends has often been used a good measure of attention in several prior studies (see e.g., Da, Engelberg, and Gao, 2014; Da, Engelberg, and Gao, 2014; Drake, Roulstone, and Thornock, 2012). Column 5 of Table 4 reports that the adoption of the state laws is associated with a 4.8% decrease in patent troll-related google searches. In columns 6-7, I show that this finding is robust to the exclusion of District of Columbia from the sample (DC has the highest index for most years in the sample) as well as an annual regression.

Such findings altogether suggest that anti-troll laws are effective in reducing the activities of patent trolls and thus their impact on small firms. These findings also corroborate the identifying assumption that the difference-in-difference methodology in this study captures the effects of state anti-troll laws and not of some other confounding factors that are unlikely to affect patent litigation and troll-related Google searches.

Second, I show in Table 2 that the effect of anti-troll laws is higher for small businesses as well as businesses in the tech industries. Moreover, Table 3 shows that the laws do not have a significant impact on the acquisitions of non-independent firms. Therefore, to drive the results, an omitted variable not only needs to coincide with all (or at least most) of the adoptions of the law in the same staggered manner, it also needs to differ in how it affects firms in different industries, firms of different sizes, and firms with different ownership structures. Moreover, the bulk of the effect relates to small businesses which are unlikely to have access to lobbying power. These findings work together to help mitigate identification concerns.

I also perform a wide variety of tests related to endogeneity in Table 5. In columns 1, 2, and 3, I examine the dynamics of acquisition activities before the adoption events by including *T-i* indicator variables that take the value of one *i* years before the adoption of anti-troll laws. For example, *T-1* takes a value of 1 in the year preceding the adoption of the anti-troll law in that state and zero for the rest of the years. If reserve causality is indeed present, I should observe changes in acquisition activities prior to adoption of the anti-troll laws. However, the coefficient estimates on all *T-i* variables are insignificant, indicating that the acquisition activities do not differ between the treatment and control states up to four years before the adoption of anti-troll laws. Also, the effect of anti-troll laws remains virtually unchanged after including the *T-i* variables.

Moreover, Figure 5 plots the estimated average difference in acquisition activity at treated states relative to control states for both tech and non-tech acquisitions. Consistent with the parallel-trends assumption, I find no significant difference in the evolution of employment at treated and control states prior to the passage of anti-troll legislation.

To further validate the assumption of parallel trends, I perform a placebo test where I assume the laws are passed three years before the actual laws are passed in each state. Specifically, I include $\textit{Anti-troll Law}_{t-12}$, which takes a value of 1 from 12 quarters before the law is passed in a state to the end of the sample. Columns 4, 5, and 6 of Table 5 report the results. Consistent with the parallel-trend assumption, none of the coefficients on $\textit{Anti-troll Law}_{t-12}$ are statistically significant. In sum, the acquisition activities in treatment and control states exhibit a similar trend, providing support for the parallel trend assumption that is crucial in the difference-in-difference methodology in this study.

Lastly, I investigate the possibility of a regional shock as a confounding factor. If a regional shock drives both the adoption of anti-troll laws and the decline in acquisitions in a state, then it is likely that the same shock reduces acquisitions in neighboring states even though no anti-troll law is adopted. However, if the decline in acquisitions is a causal effect of anti-troll laws, then the effect of the adoption of anti-troll laws should not affect neighboring states where the law does not apply. I investigate the effect of the adoption of anti-troll laws on neighboring states in columns 7 to 12 of Table 5 by including *Neighbor Law*, which is a dummy that takes a value of 1 if a state has not passed an anti-troll law at time t but at least one neighboring state has. First, the anti-troll laws have no effect on acquisition activities in neighboring states, mitigating the concern that a regional shock might have driven both effects. Second, the effect of anti-troll laws on acquisition activities in the states where they are adopted maintains its economic and statistical significance after inclusion of *Neighbor Law*.

Robustness Tests

So far, I have shown that anti-troll laws negatively impact the number of acquisitions in states that adopt these laws and that the effect is not likely to be driven by omitted variables. In this section, I present several tests that examine if the preceding results are robust to different model specifications and sub-sample analyses.

First, in all tests in Table 2, I aggregate acquisition activities at the quarterly level. Acquisitions are not very frequent, and it is possible for states with smaller economies such as North Dakota or non-tech states such as Alaska to have numerous quarters with zero observations. Therefore, it is important to examine whether the results hold if I aggregate the acquisitions at larger intervals. In Table 6, I investigate the effect of anti-troll laws on the number of acquisitions that occur in a given year rather than a quarter. Interestingly, not only does the effect of anti-troll laws maintain its statistical significance, but it also increases in economic magnitude. Specifically, column 2 reports that acquisitions of tech targets decrease by 14% after the adoption of anti-troll laws.

Second, some of the states that have adopted anti-troll laws such as Wyoming, Alaska, and North Dakota are among the states with the fewest number of acquisitions. On the contrary, some states with large economies and substantial acquisition activities such as California have yet to pass an anti-troll law. This fact raises the concern that the findings may be driven by low-acquisition states. If so, the conclusions may not be extendable to high-acquisition states that adopt similar laws in the future. To investigate this question, I estimate Equation (1) using a Weighted OLS regression that employs the number of acquisitions in the first quarter of 2010 as weights. Column 5 estimates a 6.6% decrease in the number of acquisitions after anti-troll laws are passed. This estimate has the same statistical significance but is smaller in economic magnitude than the 8.3% decrease I estimated in column 2 of Table 2 with simple OLS. Therefore, I conclude that the degree to which the findings are disproportionately driven by low-acquisition states is not very significant and thus the adoption of anti-troll laws is likely to impact acquisitions in high-acquisition states when they adopt similar laws.

To further test if the results in Table 2 are disproportionately driven by certain states, I investigate whether exclusion of California and Texas from the sample affects my findings. California has the highest number of acquisitions in the sample with more than 3,500 non-tech acquisitions and more than 2,300 tech acquisitions and yet it has not passed an anti-troll law. Column 8 reports 8.3% decrease in acquisitions after anti-troll laws are passed, which is virtually the same as column 2 of Table 2. Also, among the states that have passed anti-troll laws, Texas leads in acquisition activities with 2,500 non-tech acquisitions and 950 tech acquisitions. In column 11, I exclude both California and Texas and find the same 8.3% decrease in acquisitions after anti-troll laws are passed.

Overall, these findings suggest that my identification strategy captures the effect of anti-troll laws on acquisition activities and not other confounding variables such as regional economic shocks, other regulations, etc. Moreover, they suggest the results are pervasive in all states and are not driven by states with abnormally high or low acquisitions.

CHAPTER V

ANTI-TROLL LAWS AND ACQUISITION PRICE RATIOS

The adoption of anti-troll laws increases the net present value of monetizing innovation by operating as an independent business since the law curbs the patent trolls to the side. Therefore, when facing an acquisition offer, a small target agrees to sell if the offer is higher than the now-increased net present value of operating independently. The results in Chapter IV reveals that small businesses agree to fewer acquisition offers after they receive protection against patent trolls from anti-troll laws, indicating that fewer acquisition offers exceed the new net present value. If this mechanism is true, then the acquisition offers to which the targets agree should have a higher value after anti-troll laws are adopted. In other words, after a state passes an anti-troll law, small businesses located in the state only agree to more lucrative offers. Thus, the adoption of anti-troll laws leads to larger payments to targets.

The literature on mergers and acquisitions most often uses acquisition premiums as a measure of over- or under-payment to targets (see e.g., Harford, 1999; Officer, 2003; Malmendier and Tate, 2008; Bargeron, Schlingemann, Stulz, and Zutter, 2008; Jenter and Lwellen, 2015; and others).

The premium in this literature is defined as the ratio of the offer price to the market price of a common share at the time of the offer. In contrary to almost all previous studies where targets are public and thus market price of a share is known, the targets in my sample are all private companies and very small in size. As a result, not only is share price unknown, other financial information about them is unavailable.

Nevertheless, I tackle these two problems to a certain extent. First, I obtain financial data such as book value of assets, book value of equity, net income, etc. on target firms from Capital IQ. However, since the targets are usually small private companies, the financial data is sparsely populated. Out of 42,631 independent acquisitions in the sample, only 1,501 deals have reliable financial data. Nonetheless, it is still a very large number when compared to sample sizes in the prior studies on acquisitions that are limited to public targets (see e.g., Harford, 1999; Officer, 2003; Malmendier and Tate, 2008; Bargeron, Schlingemann, Stulz, and Zutter, 2008; Jenter and Lwellen, 2015; and others).

Second, I define a proxy for payoff to the targets as the ratio of the deal value to book value of assets. I call this measure *Price Ratio*. The only difference between price ratio and the commonly-used measure of premium in the literature is that I use the book value of assets rather than market value of the firm to scale the value of the acquisition. As a result, my measure captures the premium the acquirer is willing to pay for the target as well as the investment and growth opportunities of the target firm. Given the question I ask, however, such distinction between the two sources is irrelevant. Specifically, I examine acquisition price ratios to assess whether the adoption of anti-troll laws transfers wealth from deep-pocketed acquirers to small targets in the tech industries. A higher price ratio means that the target receives a larger payoff (acquisition value) for her investment (book value of assets). In other words, with higher price ratios, well-funded firms have to pay more to acquire the same assets, indicating that small firms better monetize their innovations via an acquisition after the adoption of anti-troll laws.

To investigate the effect of anti-troll laws on acquisition price ratios, I estimate different variations of Equation (2) where the dependent variable, *Price Ratio*$_{i,s,t}$ is equal to the ratio of deal value to book value of assets in firm *i* which is located in state *s* at the time of the acquisition *t*. Table 7 reports the results. In column 1, I only include the *Anti-troll Law* indicator in the regression. Interestingly, the acquisition price ratios for the whole sample of deals are not affected by the state laws. In column 2, I interact *Tech* with the treatment variable to examine whether the effect differs between the two groups of industries. The interaction term enters the equation with a positive and significant coefficient, suggesting that acquisition price ratios for the tech targets go up by 1.869 relative to non-tech targets after the adoption of the laws which is statistically significant at 1% level. Considering that the standard deviation of the price ratio is 4.03, this suggests that the anti-troll laws increase acquisition price ratios by 0.46 standard deviations, which is economically large.

The new state laws are expected to mostly benefit small firms. To investigate the effect of anti-troll laws on the acquisition price ratios of small targets, I replicate the analysis in the first two columns expect I limit the sample to small deals. As expected, the coefficient on the interaction is positive and significant at 1% level, and substantially larger that the coefficient in column 2. Therefore, consistent with the notion that demand letters are sent mostly to small tech businesses, the acquisition price ratios increase the most for small tech targets after the adoption of the anti-troll laws.

As discussed earlier, the anti-troll laws provide small businesses with protection against patent trolls and their dubious letters. Hence, the laws should have a lower effect on non-independent targets in that small firms already owned by larger firms benefit from an alternative form of protection from their well-funded parent companies. I therefore examine whether the adoption of state laws have a similar impact on acquisition price ratios in non-independent deals. As expected, Table 8 reports insignificant effects, both statistically and economically, in non-independent deals. This supports the hypothesis that non-independent targets, who are already protected against trolls by their parents, do not benefit from the adoption of anti-troll laws to the same extent as small independent tech businesses do.

In Table 9, I perform a series of identification and robustness tests. Overall, I show that the results are robust to exclusion of California from the sample: the effect on acquisition price ratios that tech targets and small tech targets receive has the same economic magnitude. Moreover, I show that the results remain virtually the same after inclusion of *Neighbor Law*. Lastly, consistent with the parallel trend assumption, I show that the coefficients on $\textit{Anti-Troll Law}_{t-12}$ is insignificant suggesting no differences prior the adoption of the state laws.

Overall, the evidence shows that passage of anti-troll laws transfers wealth from deep-pocketed firms to small independent businesses, especially those in the tech industries. First, small businesses prefer to remain in business rather than accept acquisition deals to independently monetize their innovation. This evidence is consistent with findings in Appel, Farre-Mensa, and Simintzi (2019) that small tech businesses increase their employment after the passage of anti-troll laws, raise more financing through Venture Capitals and private loans, and generate more innovation outputs. Second, small businesses appear to be paid more when they agree to sell to a larger firm after these laws are passed. In other words, with state anti-troll laws in place, small businesses are better off even if they decide to monetize their innovation via acquisitions.

CHAPTER VI

METHOD OF PAYMENT AND MARKET REACTION

The empirical findings in Chapter IV and Chapter V show that the state laws affect the target's decision of whether accepting acquisitions. The state laws, however, also affect the terms of the acquisition the target accepts. In this Chapter, I examine the effect of anti-troll laws on the acquisition's method of payment (cash versus non-cash), and then investigate the market reaction to acquisition announcements for a sample of public acquirers.

Method of Payment

I posit that the underlying reason behind small business' decision not to accept acquisition offers after state anti-troll laws are adopted is that they prefer to stay in business and keep their wealth tied to their innovation. Whereas, when they sell to an acquirer, they insulate themselves from the risks of further monetizing their innovation. This conjecture, however, can only be true for deals where the target receives a specified amount of cash. When targets receive non-cash payments, mainly in the form of the acquirer's equity, their wealth still depends on the monetization of their innovation. Since the adoption of anti-troll laws increases the returns to innovation, I hypothesize that small businesses are more likely to agree to non-cash payments when accepting an acquisition offer after the state anti-troll laws are adopted.

To examine the effect of anti-troll laws on method of payment, I define a dummy variable, *Combination*, that indicates whether the acquisition's method of payment involves a non-cash form. In other words, *Combination* is equal to 0 if the target receives only a cash payment, whereas it is equal to 1 if the target either receives only non-cash payments or a combination of the two forms. I estimate Equation (2) using a Linear Probability Model where the dependent variable is *Combination*.

Table 10 reports the estimation results. In column 1, I include all deals in the regression (there is no restriction on size or industry) and find no evidence that state anti-troll laws have any effect on the method of payments in an average acquisition. In column 2, I include an interaction term between *Tech* and *Anti-troll Laws* to examine whether the law impacts the two groups of industries differently. The coefficient on the interaction term has a positive sign and it is statistically significant at the 5% level, providing evidence that state anti-troll laws affect terms of tech deals.

The effect of anti-troll laws on targets' form of payment should be stronger in acquisitions that involve small targets. Thus, I investigate the effect of anti-troll laws on small targets by limiting the sample to deals in which the targets are smaller than $50 million. In column 3, I do not differentiate between tech and non-tech deals and as a result, I do not find a significant effect on small targets' form of payment. In column 4, I include the interaction term and find evidence, again at 5% statistical significance. The findings in columns 2 and 4 support the notion that that targets in tech acquisitions (especially small ones) are more likely to receive non-cash payments after the adoption of anti-troll laws.

Overall, while the statistical significance is low, the coefficients are consistent with the hypothesis that tech businesses are less likely to cash out after the adoption of state anti-troll laws even at times when they accept an acquisition offer.

Market Reaction to Acquisition Announcements

So far, I have shown that the adoption of anti-troll laws changes the acquisition market in favor of tech targets. Specifically, the state laws make it harder and more expensive for well-funded firms to acquire small targets, and they are more likely to offer equity in the transaction. Therefore, given the evidence that the adoption of state laws transfers more wealth from acquirers to targets, the natural question that arises is whether the acquisitions of targets that are affected by anti-troll laws are less valuable than the acquisitions of targets that are not affected.

Estimating the value added by an acquisition in my sample is challenging for several reasons. First, the majority of the acquirers in the sample are large private firms for which long time series of financial information is unavailable. Second, when the data is available (i.e., for public acquirers), the financial information is reported at the aggregate level, which includes all other contemporaneous investments of the firm. Therefore, I am unable to pinpoint the value of the acquisitions. To tackle these concerns, prior literature has extensively used the acquirer stock returns around the announcement of an acquisition as a measure of acquisition value for the acquirer (see e.g., Travlos, 1987; Lang, Stulz, and Walking, 1989; Harford1999; Moeller, Schlingemann, and Stulz, 2005; Lehn and Zhao, 2006; Masulis, Wang, and Xie, 2007; Malmendier and Tate, 2008; and others). The market reaction to the announcement of an acquisition reflects an assessment of the value a particular acquisition has for the firm.

To test whether the adoption of anti-troll laws shifts more value to the target in an acquisition, I examine the acquirer's cumulative abnormal returns to acquisition announcements that involve public firms. The announcement period is defined as days *t-1* to *t+1* where the acquisition deal is announced at day *t*. I estimate the cumulative abnormal returns, *CAR*, as the daily returns in excess of the market model. The estimation period for the market model is days *t-250* to *t-20*. I estimate a variation of Equation (2) where the dependent variable is *CAR*, and I include a set of control variables for the acquirer that are drawn from the acquisition literature. Specifically, I include a dummy that is equal to 1 if the acquirer's cash to asset ratio is above the median of its industry. I also include the acquirer's past annual returns, natural logarithm of market value, leverage, book-to-market ratio, and free cash flow.

Table 11 reports the results. Column 1 shows that the passage of state-level laws has no significant effect on the announcement returns for the total sample of both tech and non-tech acquisitions. In column 2, I limit the sample to cash acquisitions. The rationale for this condition is twofold. First, prior literature shows the market reacts negatively to the use of equity in an acquisition because it provides a signal that the equity is over-valued. Therefore, focusing on cash deals removes the effect of the method of payment. Second, as laid out in Chapter V receiving a higher payoff is more likely in acquisitions in which the target cashes out of the market. Nonetheless, the results in column 2 continue to provide no evidence that anti-troll laws affect the returns.

The findings in the first two columns of Table 11 are not surprising because they include all acquisition transactions, the majority of which may be too small to attract the market's attention. The median deal size in my sample is $15 million, significantly smaller that the median deal size in notable prior studies that examine public-public mergers. For example, some prior studies require a minimum deal value (e.g., 1% of the acquirer's market value of equity) in their data screening process, leading them to filter out small acquisitions.

In column 3, I limit the sample to acquisitions that are larger than $50 million to focus on deals that are more likely to have a visible impact on the respective acquirers. As expected, the coefficient on *Anti-troll Law* remains insignificant but the coefficient on the interaction term is negative and significant. The results in column 3 show that the market perceives the acquisitions of tech businesses that are treated by the anti-troll laws as lower-value acquisitions, as reflected by their significantly lower *CARs*. To this end, acquirers of tech targets that are affected by the anti-troll laws experience 1.4% lower returns in their 3-day announcement windows relative to acquirers of non-tech targets that are affected by the anti-troll laws. I limit the sample to cash deals in column 4 and find similar evidence. Acquisitions in tech industries generate significantly smaller market reactions (1.3% lower returns) if the target is located in a state with anti-troll laws. To be in line with the previous literature on mergers and acquisitions, I limit the sample of acquisitions to those with value of greater than 1% of the acquirer's market value of equity. columns 5 and 6 report the estimates for all deals and cash deals respectively. Similarly, acquirers of treated tech targets experience 1.2%-1.3% lower *CARs*. In terms of economic magnitude, this lower announcement return in acquisitions of treated tech targets is associated with $228-$267 million lower value accrued to an average public acquirer given that the average market value of the public acquirers in my sample is $19 billion.

In untabulated robustness checks, I re-estimate the effect of anti-troll laws on acquirer cumulative abnormal returns around acquisition announcement dates using a variety of announcement windows, up to a window of *t-9* to *t+9*. The results are robust to the choice of announcement window.

Overall, the evidence in Table 11 shows that the adoption of state anti-troll laws significantly affects acquirer announcement returns in tech deals, whereas they do not appear to affect non-tech deals. The evidence is consistent with the findings in previous sections that the state laws more strongly affect tech firms. Overall, the negative market reaction to the acquisition of treated tech targets (targets in states with anti-troll laws) is consistent with the wealth transfer hypothesis. After the adoption of state anti-troll laws, the targets are paid more, as shown in Table 7, and thus more value goes to the targets. This is undesirable for the investors of the acquirer firm, and favorable to the owners of the target firm.

CHAPTER VII

ANTI-TROLL LAWS AND PUBLIC FIRMS' R&D EXPENDITURES

The purpose of state anti-troll laws is to protect innovative firms from patent trolls and their demand letters. So, a natural question to ask is whether the state laws actually promote innovation. Appel, Farre-Mensa, and Simintzi (2019) report a significant increase in the number of patents that are granted to small firms located in treated states after the adoption of anti-troll laws.[7]

I add to their findings by examining the effect of anti-troll laws on public firms' R&D expenditures. First, studying R&D is important in that patents only showcase innovation output while R&D expenditures measures innovation input. This distinction is important in that innovation output, while having its own merits, partially captures innovation by ignoring innovative projects that fail. Moreover, R&D expenditure is a timelier measure of innovation because the legal process of patenting an innovation takes between 2 and 3 years while the expenditures are reported at the end of the quarter. Given the fact that the first law is passed in 2013 (5 years before the last observation in my sample) and the last is passed in 2017 (1 year before the last observation in my sample), using a timely measure to examine the effect of state laws on innovation is crucial.

One problem with R&D expenditure is that they are not available for private firms. As a result, my tests of the effect of state laws on R&D are limited to public firms. While it is mute on the effect of anti-troll laws on small-firm innovation, public R&D provides insight on two ways that the laws affect the innovation market. First, the anti-troll laws treat all demand letters that are sent with bad faith the same way, independent

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[7] They define small firms in their tests related to patent grants as firms with less than 500 employees.

of the size and ownership structure of the recipient. The difference between how the anti-troll laws affect small and large firms in the state is driven by the higher frequency with which the letters are sent to small firms as well as their lack of legal expertise. Therefore, the adoption of anti-troll laws promotes innovation among large firms as well, although to a lesser extent.

Second, large public firms resort to internal R&D to meet their needs for innovation when the cost of acquiring external innovation increases after the adoption of anti-troll laws in their state. This argument, however, ignores cross-state acquisitions; for example, it assumes that all targets in the state of Colorado are acquired by public companies in Colorado, so only public firms in Colorado increase their R&D to substitute for the acquisition of innovation after the adoption of the law in Colorado. While this assumption appears unrealistic, it is not far from reality in my sample. Specifically, the fraction of acquisitions (especially small ones) that involve acquirers and targets that are located in the same state is not small. Moreover, Grote and Umber (2006) show that there is a strong and consistent home bias for M&A transactions in the U.S. As a result, I expect public firms located in a treated state to increase their R&D expenditures after anti-troll laws are passed. However, I cannot rule out the possibility that public firms in control states might engage in the same substitution behavior in that they might be the potential acquirers who would buy the targets in the treated state had the law not been passed. This would lead to an underestimation of the effect of anti-troll laws on public firms' R&D expenditure.

To examine the effect of the adoption of anti-troll laws on the R&D expenditure of public firms located in the adopting state, I estimate a variation of Equation (2) where the dependent variable is the natural logarithm of R&D expenditure. I include a set of firm-level control variables drawn from the literature on innovation. Specifically, I include the natural logarithm of market value and age, leverage, profitability (measured as the ratio of EBIT to assets), tangibility (measures as the ratio of net PPE to assets), free cash flow, and Herfindahl Index (calculated for each 3-digit SIC code).

Table 12 reports the results. Column 1 shows that the adoption of anti-troll laws reduces the R&D expenditures of non-tech firms but significantly increases the R&D expenditures of tech firms located in an adopting state. In column 2, I tighten the specification and replace state fixed effects with firm fixed effects to absorb fixed unobservables at the firm level. The coefficient on the interaction between *Tech* and *Anti-troll Law* remains positive and significant, tough it declines in magnitude. Specifically, I find that public firms in tech industries increase their R&D by 5.5% after the adoption of anti-troll laws.

Similar to small private firms, small public firms benefit significantly from the protection of the state laws. Therefore, their incentives to increase R&D expenditure is higher than those of larger public firms. In column 3 and 4, I test this hypothesis by limiting the sample to firms with less than $100 million in Sales and $200 million in assets respectively. These two cutoffs are close to 30% percentile of the distributions of sales and assets during the sample period. I find strong evidence that smaller firms in the public sector increase their innovation input after the adoption of state laws.

In column 5, I limit the sample to firms with positive R&D expenditures at the beginning of the sample to reduce the statistical noise that accompanies with firms with constant zero R&D. This condition also helps me estimate the effect of the anti-troll laws on the intensive margin. The results increase in magnitude after exclusion of no R&D firms. A comparison of Columns 1 and 5 suggests that the laws encourage firms to engage in innovation more aggressively both on the intensive and extensive margins.

The evidence in Table 12 supports the hypothesis that anti-troll laws boost innovation by protecting firms against the patent trolls. The evidence is consistent with the hypothesis that deep-pocketed firms have to increase their internal innovation as a substitute for external innovation that is now more expensive to acquire.

CHAPTER VIII

CONCLUSION

Patent trolls and their impact on small businesses have been the center of debate in the media, among politicians and legislators, and among academics. Small businesses are responsible for a significant part of innovation output and thus the economic growth in the U.S. Hence, any limitations or frictions on their innovative activities are crucial to study. In this paper, I examine the effect of patent trolls on small businesses' decision to stay in business or sell to well-funded firms.

To examine the effect of patent trolls on small firms, I exploit the staggered adoption of state-level anti-troll laws in 35 states that limit the abusive activities of patent trolls. I find that acquisitions of small, independent targets decline after the adoption of anti-troll laws, whereas acquisitions of non-independent firms (large firms' subsidiaries and divisions) are not affected by the state laws. Moreover, I show that the effect is mostly driven by tech firms, which are the typical targets of patent trolls. I also find evidence consistent with the hypothesis that, after adoption of the laws, tech targets and small targets receive higher payoffs (higher acquisition price ratios) when they agree to sell. The higher payment to tech targets is reflected in lower acquirer abnormal returns around the acquisition announcements for public acquirers. Lastly, I show that public firms increase their R&D spending after the adoption of state laws to substitute for the now-more expensive acquisition of innovation.

Overall, the evidence in this study points to a transfer of innovation value from well-funded, large acquirers to the small businesses that are typically the true producers of the innovation and the true founders of innovative businesses. The results also suggest that the abusive behavior of patent trolls transfers wealth from small innovators to large firms, which is in contrast to the intermediary role that proponents of non-practicing entities (patent trolls) contend. Given the positive effects of the anti-troll laws at state level and the benefits they provide to small businesses, it is warranted to explore potential new pieces of legislation that are aimed at curbing the activities of patent trolls at the federal level.

APPENDIX A

FIGURES

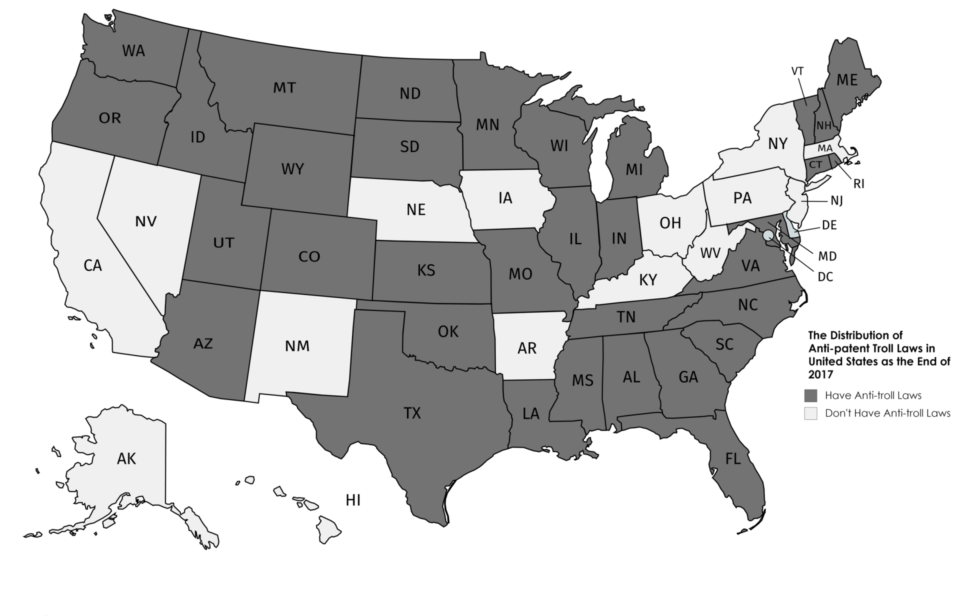


Figure 1: Anti-patent troll laws across the States.
Note: This map shows the distribution of anti-patent troll laws across the States as of first quarter of 2018.

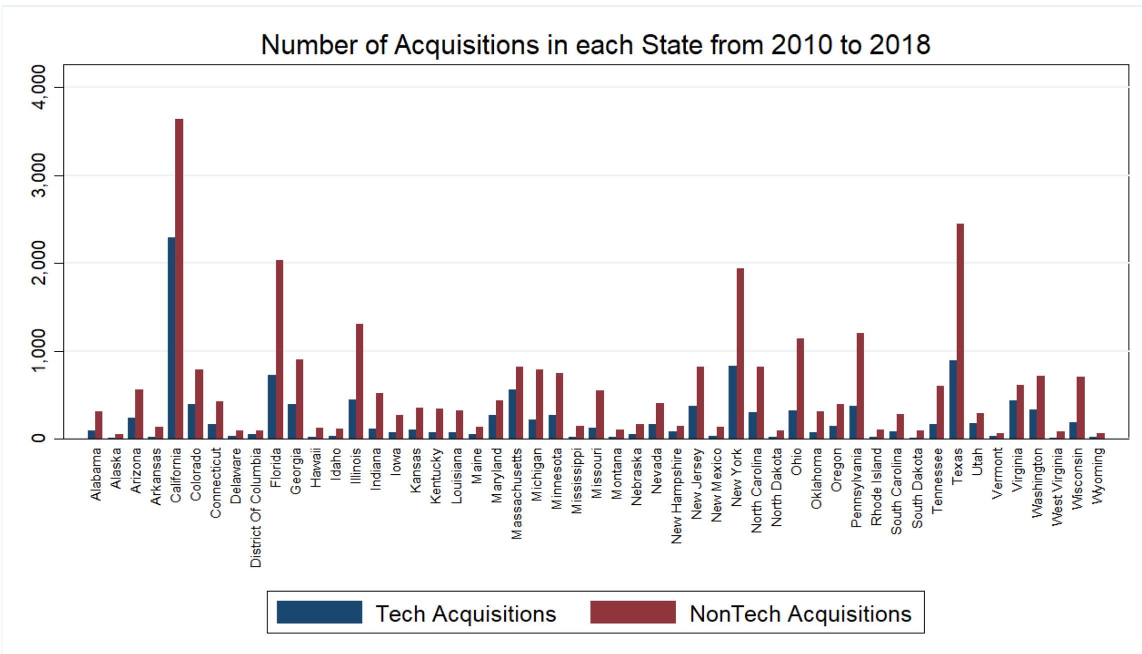


Figure 2: Number of acquisitions across the States.
Note: This figure plots the number of tech (in blue) and non-tech (in red) acquisitions in 50 US States and District of Columbia between the first quarter of 2010 and the first quarter of 2018. Acquisitions in the sample are assigned to the states based on the location of the target.

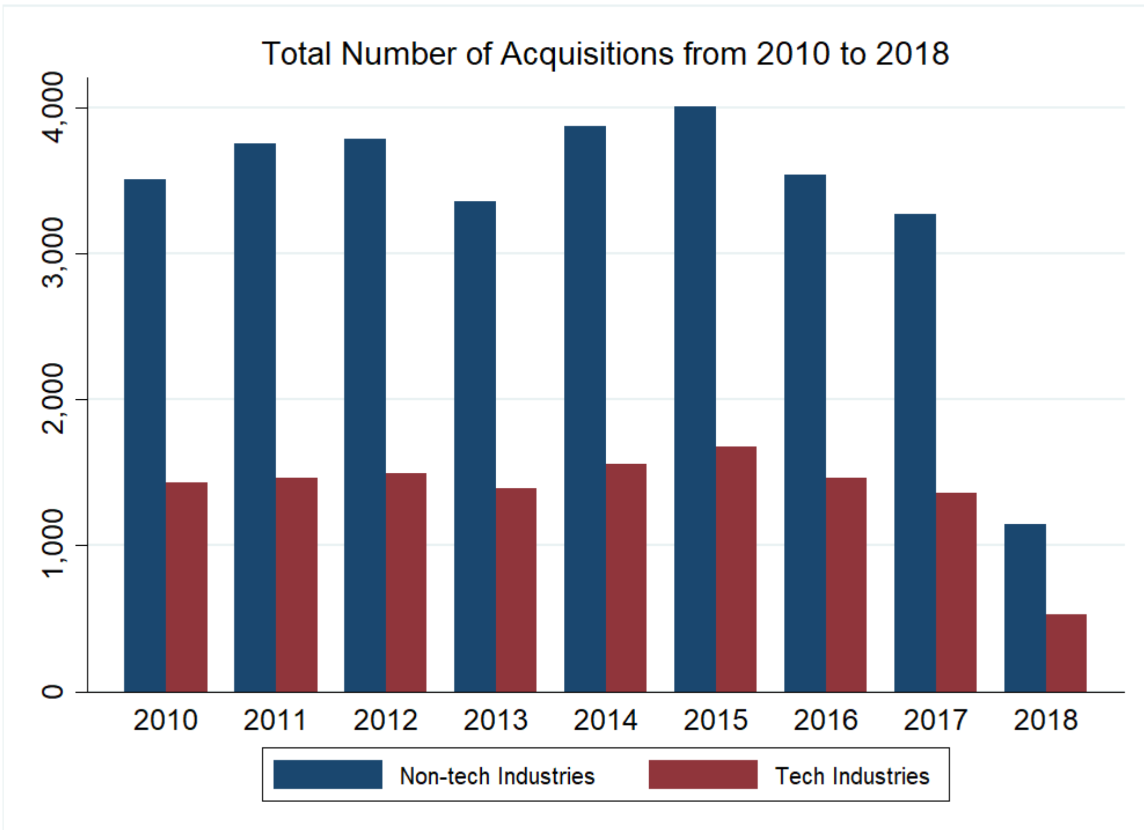


Figure 3: Number of acquisitions across years.
Note: This figure plots the number of tech (in red) and non-tech (in blue) acquisitions between the first quarter of 2010 and the first quarter of 2018. Acquisitions in the sample are assigned to a given year based on the date of the acquisition announcement. 2018 only includes acquisitions in the first quarter.

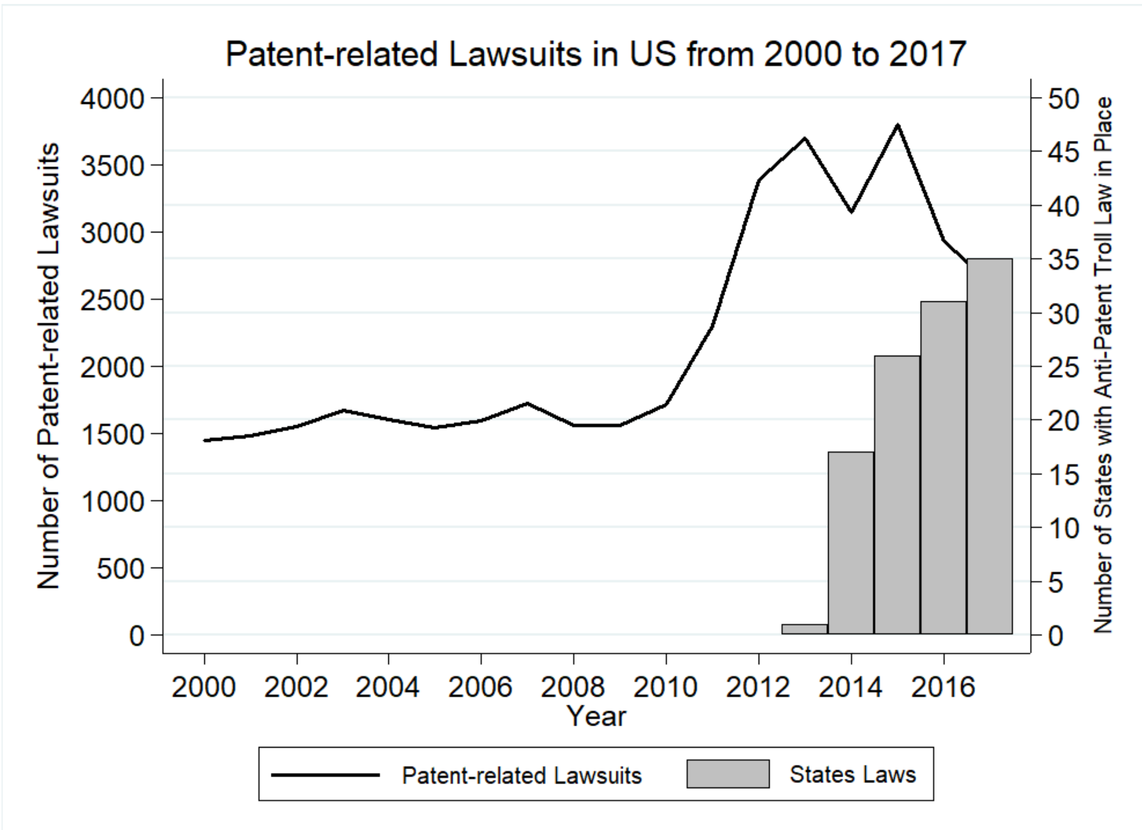
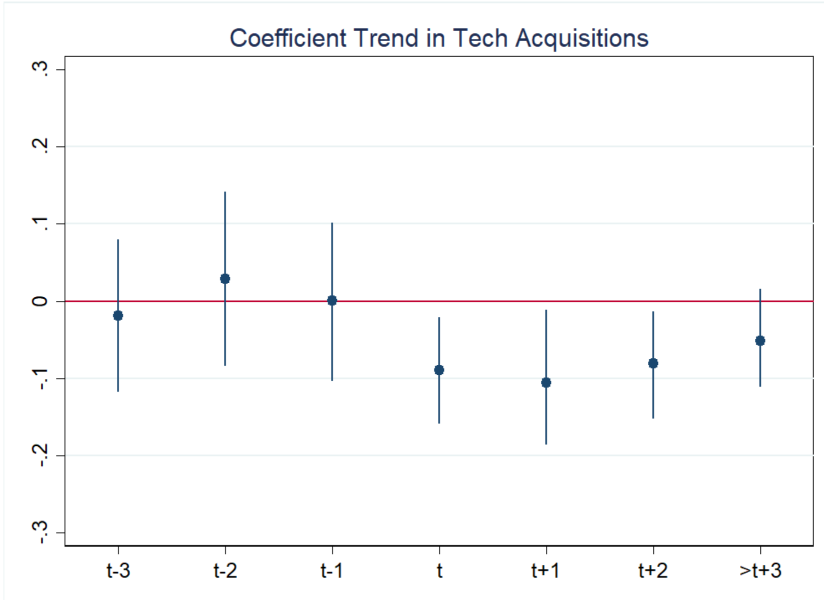
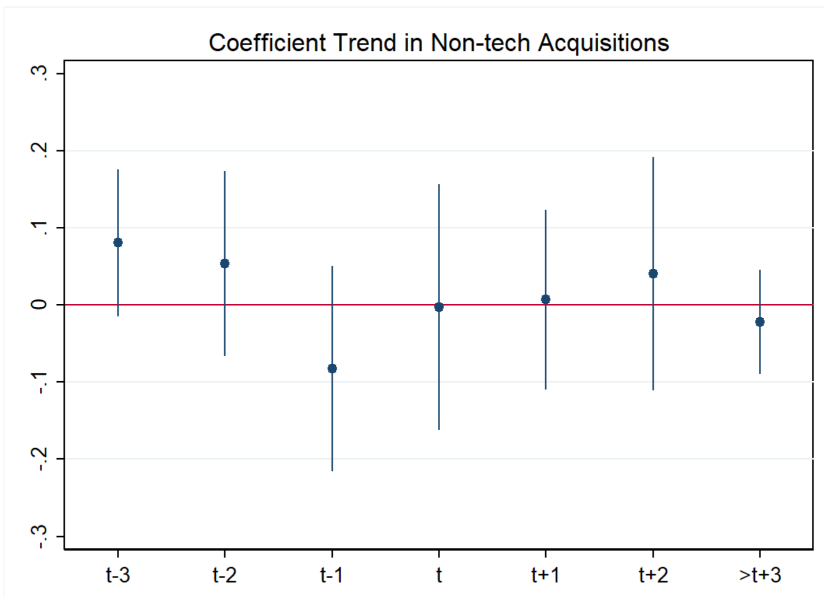


Figure 4: Time series of patent-related litigation and Anti-troll Laws.
Note: This figure plots the number of patent-related lawsuits filed at U.S. District Courts (right axis) and the number of states that have anti-troll laws in place (left axis) from the beginning of 2000 to the end 2017.



Panel a



Panel b

Figure 5: Anti-troll Laws' Coefficient Trends.
Note: This figure plots the evolution of acquisition activity in tech industries (Panel a) and non-tech industries (Panel b) in states with anti-troll laws relative to states without such laws. I estimate Equation (2), except that I replace the *Anti-Troll Law* indicator with indicators that identify quarters *t-3, t-2, t-1, t, t+1, t+2,* and *>t+3* for states that pass an anti-troll law, where quarter *t* is the quarter the anti-troll law is signed. The graph shows the point estimates associated with each of these indicators along with the 95\% confidence interval where robust standard errors are clustered by state.

APPENDIX B
TABLES

Table 1
Descriptive Statistics

Panel A	Observation	Mean	Median	SD
All Independent Acquisitions *(per qtr.)*	1,683	24.96	12	17.67
Tech Independent Acquisitions *(per qtr.)*	1,683	7.23	4	11.28
Non-tech Independent Acquisitions *(per qtr.)*	1,683	17.73	11	21.04
All Non-Independent Acquisitions *(per qtr.)*	1,683	15.14	6	12.54
Tech Non-Independent Acquisitions *(per qtr.)*	1,683	4.79	2	10.1
Non-tech Non-Independent Acquisitions *(per qtr.)*	1,683	10.35	6	14.05
Patent-related Litigation *(per qtr.)*	1,683	20.46	4	60.27
Target Size *(All Deals)*	9,504	257.43	15.6	2,412
Target Size *(Tech Deals)*	2,816	239.57	14.7	2,299
Target Size *(Non-tech Deals)*	6,688	264.96	16.12	2,459
Price Ratio *(All Deals)*	1,367	3.53	2.05	3.9
Price Ratio *(Tech Deals)*	461	4.47	2.97	4.03
Price Ratio *(Non-tech Deals)*	906	3.055	1.66	3.74
Combination *(All Deals)*	16,707	0.23	0	0.42
Combination *(Tech Deals)*	4,809	0.27	0	0.44
Combination *(Non-tech Deals)*	11,898	0.22	0	0.41
R&D *(All public firms)*	12,932	145.98	16.1	563.55
R&D *(Tech public firms)*	5,795	195.89	30.14	642.51
R&D *(Non-tech public firms)*	7,137	105.45	5.86	486.46

Panel B	Small	Large	Missing	Total
Independent Tech	2,175	619	9,570	12,364
Independent Non-tech	5,007	1,655	23,605	30,267
Non-Independent Tech	1,781	2,066	4,597	8,159
Non-Independent Non-tech	4,295	4,493	9,000	17,603

Note: Panel A reports the summary statistics of all the variables used in the study. *Target Size* is reported in $million. *Price Ratio* is the ratio of deal value to the target's book value of assets. *Combination* is an indicator variable taking the value of one if the target receives a non-cash payment (fully or partially) and zero otherwise. Independent Acquisitions are those in which the target is an independent entity. Non-independent Acquisitions are those in which the target is owned by a larger entity such as an investment firm or a larger corporation. Panel B reports the breakdown of number of acquisitions by ownership, size, and industry.

Table 2
The Effect of Anti-troll Laws on Acquisitions of Independent Targets

	(1) All Firms	(2) All Tech	(3) All Non-Tech	(5) Small Tech	(6) Small Non-Tech	(8) Large Tech
Anti-Troll Law	-0.054*	-0.083**	-0.023	-0.097**	-0.109**	-0.039*
	(0.093)	(0.044)	(0.447)	(0.037)	(0.026)	(0.096)
Constant	2.644***	1.608***	2.309***	0.735***	1.221***	0.173***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	42631	12364	30267	2243	5184	570
# of Observations	1,683	1,683	1,683	1,683	1,683	1,683
R-squared	0.914	0.851	0.893	0.665	0.718	0.468
State FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
SD Cluster	State	State	State	State	State	State

Note: The dependent variable, $Ln(1+numberofdeals)_{st}$, is equal to one plus the natural log of the number of M&A deals in state *s* during quarter *t*. Geographic location is determined based on the location of the target and the quarter is determined by the announcement date of the deal. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t*. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. Column 1 includes both tech and non-tech deals in the sample. Column 2 includes only tech deals and column 3 includes only non-tech deals. Columns 4, 5, and 6 repeat the first three columns respectively except only for small deals. A small transaction is defined as deals involving targets that are smaller than $50 million. Column 7 reports the results for only large tech deals. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3
The Effect of Anti-troll Laws on Acquisitions of Non-Independent Targets

	(1) All Firms	(2) All Tech	(3) All Non-Tech	(5) Small Tech	(6) Small Non-Tech	(8) Large Tech
Anti-Troll Law	-0.034	-0.042	-0.006	-0.024	0.034	0.000
	(0.293)	(0.355)	(0.857)	(0.602)	(0.490)	(0.997)
Constant	2.166***	1.076***	1.949***	0.442***	1.036***	0.337***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	25762	8159	17603	1537	4329	2021
# of Observations	1,683	1,683	1,683	1,683	1,683	1,683
R-squared	0.895	0.844	0.862	0.634	0.681	0.717
State FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
SD Cluster	State	State	State	State	State	State

Note: The dependent variable, $Ln(1+numberofdeals)_{st}$, is equal to one plus the natural log of the number of M&A deals in state *s* during quarter *t*. Geographic location is determined based on the location of the target and the quarter is determined by the announcement date of the deal. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t*. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. Column 1 includes both tech and non-tech deals in the sample. Column 2 includes only tech deals and column 3 includes only non-tech deals. Columns 4, 5, and 6 repeat the first three columns respectively except only for small deals. A small transaction is defined as deals involving targets that are smaller than $50 million. Column 7 reports the results for only large tech deals. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 4
The Effect of Anti-troll Laws on Patent-related Litigation in U.S. District Courts and Troll-related Google Searches

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dep. Variable: Litigation				Dep. Variable: Google Search Index		
	Quarterly	Weighted	CA Excluded	TX&DE Excluded	Quarterly	Quarterly (DC Excluded)	Annual
Anti-troll Law	-0.088*	-0.122*	-0.089*	-0.090*	-0.048*	-0.067**	-0.093**
	-0.095	-0.052	-0.099	-0.094	-0.073	-0.046	-0.019
Constant	1.642***	3.138***	1.584***	1.536***	0.623***	0.579***	1.568***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Lawsuits	35,619	35,619	35,619	35,619			
# of State-Quarters (years)	1,683	1,518	1,650	1,617	1,836	1,800	459
R-squared	0.896	0.923	0.885	0.871	0.851	0.862	0.855
State FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State	State	State

Note: The dependent variable in column 1-4, *Litigation*, is equal to one plus the natural log of the number of patent-related lawsuits in state *s* during quarter *t.* Geographic location is determined based on the location of the district court and quarter is determined by the filing date of the lawsuit. The dependent variable in column 5-7, *Search Index*, is equal to one plus the natural log of Google Search Volume Index for "Patent Troll". *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t.* State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 5
Identification Assumptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Trend			T-12			Neighbor Law			Neighbor Law		
	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech
Anti-Troll Law	-0.062*	-0.101**	-0.022							-0.088**	-0.113**	-0.047
	(0.081)	(0.042)	(0.516)							(0.029)	(0.045)	(0.233)
Anti-Troll Law t-12				-0.009	-0.070	0.036						
				(0.816)	(0.177)	(0.292)						
Neighbor Law							-0.005	0.017	-0.012	-0.056	-0.049	-0.039
							(0.891)	(0.660)	(0.720)	(0.178)	(0.335)	(0.359)
T-1	-0.016	-0.016	-0.009									
	(0.819)	(0.852)	(0.913)									
T-2	-0.109	-0.046	-0.09									
	(0.165)	(0.613)	(0.191)									
T-3	0.042	0.028	0.045									
	(0.453)	(0.716)	(0.457)									
T-4	0.011	-0.165	0.074									
	(0.842)	(0.103)	(0.122)									
Constant	2.644***	1.608***	2.309***	2.644***	1.608***	2.309***	2.644***	1.608***	2.309***	2.644***	1.608***	2.309***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	42,631	12,364	30,267	42,631	12,364	30,267	42,631	12,364	30,267	42,631	12,364	30,267
# of State-Quarters	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683	1,683
R-squared	0.914	0.851	0.893	0.913	0.85	0.893	0.913	0.85	0.893	0.914	0.851	0.893
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table 5 (continued)
Identification Assumptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Trend			T-12			Neighbor Law			Neighbor Law		
	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State	State	State	State	State	State	State	State

Note: The dependent variable, *Ln(1+numberofdeals)*$_{st}$, is equal to one plus the natural log of the number of M&A deals in state *s* during quarter *t*. Geographic location is determined based on the location of the target and quarter is determined by the announcement date of the deal. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t*. *Anti-Troll Law*$_{t-12}$ is a dummy variable that assumes the law in each state was passed 12 quarters earlier. *Neighbor Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has not passed the law but has at least neighbor state that has passed the law at any time before t. *T-i* is a dummy variable taking a value of 1 *i* quarters before the law is passed in each state. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 6
Robustness Tests for Number of Acquisitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Annual Regressions			Weighted Regressions			California Excluded			California and Texas Excluded		
	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech	All Deals	All Tech	All Non-Tech
Anti-Troll Law	-0.062*	-0.140**	-0.026	-0.038	-0.065**	-0.018	-0.056*	-0.083**	-0.026	-0.053	-0.083*	-0.023
	(0.095)	(0.032)	(0.509)	(0.161)	(0.018)	(0.599)	(0.092)	(0.049)	(0.399)	(0.108)	(0.053)	(0.469)
Constant	3.997***	2.704***	3.685***	3.784***	2.757***	3.447***	2.593***	1.557***	2.262***	2.554***	1.521***	2.222***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	42,631	12,364	30,267	42,631	12,364	30,267	42,631	12,364	30,267	42,631	12,364	30,267
# of State-Quarters	459	459	459	1,650	1,518	1,584	1,650	1,650	1,650	1,617	1,617	1,617
R-squared	0.974	0.938	0.967	0.96	0.914	0.94	0.904	0.828	0.882	0.896	0.816	0.872
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State	State	State	State	State	State	State	State

Note: The dependent variable, *Ln(1+numberofdeals)$_{st}$*, is equal to one plus the natural log of the number of M&A deals in state *s* during quarter *t*. Geographic location is determined based on the location of the target and quarter is determined by the announcement date of the deal. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t*. The number of acquisitions in the first quarter of the sample in each state is used as the weights in the weighted OLS regressions in columns 4-6. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 7
The Effect of Anti-troll Laws on Acquisition Price Ratios of Independent Targets

	(1) All Targets	(2) All Targets	(3) Small Targets	(4) Small Targets
Anti-Troll Law	-0.881	-1.480***	-0.969	-1.711***
	(0.104)	(0.003)	(0.119)	(0.007)
Anti-Troll Law*Tech		1.869***		2.742***
		(0.002)		(0.002)
Tech		1.162***		0.863***
		(0.000)		(0.002)
Constant	3.359***	3.001***	3.041***	2.790***
	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	1,367	1,367	690	690
R-squared	0.101	0.131	0.158	0.187
State FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
SD Clusters	State	State	State	State

Note: The dependent variable, *Price Ratio$_{ist}$*, is the value of the deal divided by the target's book value of assets. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t. Tech* is a dummy variable indicating that the target belongs to a tech industry. *Small* is a dummy variable indicating the size of the target is below $50 million. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 8
The Effect of Anti-troll Laws on Acquisition Price Ratios of Non-Independent Targets

	(1) All Targets	(2) All Targets	(3) Small Targets	(4) Small Targets
Anti-Troll Law	-0.145	-0.095	-0.153	0.039
	(0.460)	(0.642)	(0.481)	(0.871)
Anti-Troll Law*Tech		-0.021		-0.458
		(0.943)		(0.176)
Tech		1.197***		1.397***
		(0.000)		(0.000)
Constant	2.752***	2.348***	2.967***	2.562***
	(0.000)	(0.000)	(0.000)	(0.001)
# of Deals	2,674	2,674	1,434	1,434
R-squared	0.062	0.094	0.090	0.187
State FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
SD Clusters	State	State	State	State

Note: The dependent variable, *Price Ratio*$_{ist}$, is the value of the deal divided by the target's book value of assets. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t. Tech* is a dummy variable indicating that the target belongs to a tech industry. *Small* is a dummy variable indicating the size of the target is below $50 million. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 9
Identification Tests for Acquisition Price Ratios

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	California Excluded				Neighbor Law				Treatment T-12			
	All	All	Small	Small	All	All	Small	Small	All	All	Small	Small
Anti-Troll Law	-0.708	-1.296***	-1.025	-1.696**	-1.383**	-1.905***	-1.195*	-1.905***	-0.512	-1.047**	-0.554	-1.089*
	(0.199)	(0.009)	0	(0.023)	(0.042)	(0.003)	(0.062)	(0.003)	(0.285)	(0.018)	(0.351)	(0.094)
Neighbor					-0.723	-0.671	-0.347	-0.671				
					(0.152)	(0.128)	(0.537)	(0.128)				
Anti-Troll Law*Tech		1.845***		2.647***		1.916***		1.916***		1.542*		1.670*
		(0.003)		(0.004)		(0.002)		(0.002)		(0.054)		(0.070)
Neighbor Law*Tech						0.250		0.250				
						(0.609)		(0.609)				
Tech		1.160***		0.959**		1.099***		1.099***		1.073***		0.621**
		(0.000)		(0.013)		(0.000)		(0.000)		(0.000)		(0.012)
Anti-Troll Law t-12									-0.945**	-1.013**	-0.884	-1.131*
									(0.022)	(0.032)	(0.117)	(0.064)
Anti-Troll Law t-12*Tech										0.419		1.366
										(0.551)		(0.110)
Constant	3.741***	3.408***	3.406***	3.172***	3.358***	3.020***	3.032***	3.020***	3.381***	3.050***	3.074***	2.869***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	1,124	1,124	565	565	1,367	1,367	690	1,367	1,367	1,367	690	690
R-squared	0.111	0.144	0.177	0.211	0.102	0.132	0.158	0.132	0.104	0.135	0.161	0.193
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State	State	State	State	State	State	State	State

Note: The dependent variable, *Price Ratio$_{ist}$*, is the value of the deal divided by the target's book value of assets. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t*. *Neighbor Law* is a dummy variable taking value of 1 at time *t* for a given state if the state has not passed the law but has at least one neighboring state that has passed the law at any time before *t*. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 10
The Effect of Anti-troll Laws on the Method of Payment to Independent Targets

	(1) All Deals	(2) All Deals	(3) Small Deals	(4) Small Deals
Anti-Troll Law	-0.001	0.010	-0.013	-0.002
	(0.949)	(0.422)	(0.551)	(0.925)
Anti-Troll Law*Tech		0.040**		0.036**
		(0.036)		(0.046)
Tech		0.050***		0.049***
		(0.000)		(0.000)
Constant	0.276***	0.261***	0.244***	0.228***
	(0.000)	(0.000)	(0.000)	(0.000)
# of Deals	16,707	16,707	7,381	7,381
R-squared	0.034	0.036	0.021	0.023
State FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES
SD Clusters	State	State	State	State

Note: The dependent variable, $Combination_{ist}$, is a dummy variable taking a value of 1 if the target accepts a payment method that involves a non-cash payment (partially or fully) and takes a value of 0 if payment is only cash. Each column reports a Linear Probability Model. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before *t. Tech* is a dummy variable indicating that the target belongs to a tech industry. *Small* is a dummy variable indicating the size of the target is below $50 million. Control variables are state GSP, state per capita income, and a dummy variable for other state initiatives to promote innovation and small businesses. Control variables are included in the regressions but not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 11
The Effect of Anti-troll Laws on Acquirers' Cumulative Abnormal Returns around Acquisition Announcements

	(1)	(2)	(3)	(4)	(5)	(6)
	All Deals	Cash Deals	All Deals > $50m	Cash Deals > $50m	All Deals > 1%	Cash Deals > 1%
Anti-Troll Law	0.005	-0.003	0.013	-0.003	0.002	-0.004
	(0.254)	(0.527)	(0.306)	(0.783)	(0.733)	(0.542)
Anti-Troll Law *Tech	0.019	-0.007	-0.014**	-0.013**	-0.012*	-0.013**
	(0.461)	(0.184)	(0.037)	(0.034)	(0.068)	(0.031)
Constant	0.064**	0.023***	0.139*	0.052***	0.098**	0.028***
	(0.014)	(0.002)	(0.088)	(0.008)	(0.031)	(0.000)
# of Observations	7,380	3,961	2,437	1,650	3,636	2,470
R-squared	0.027	0.030	0.126	0.115	0.044	0.054
State FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State	State

Note: The dependent variable, *CAR(-1,+1)*, is calculated as the daily returns in excess of the market model in a 3-day window around the announcement of the acquisition. The parameters for the market model are estimated using daily returns from *t-250* to *t-20*. Column 1 reports estimation for all deals, column 2 for all cash deals, column 3 for all deals larger than $50m, column 4 for all cash deals larger than $50m, column 5 for all deals larger than %1 of acquirer market value, and column 3 for all cash deals larger than %1 of acquirer market value. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before t. *Tech* is a dummy variable indicating the target belongs to a tech industry. Control variables are a dummy variable indicating that the firm is cash rich, past annual returns, natural logarithm of market cap, leverage ratio, boot-to-market ratio, and free cash flow. Control variables are not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Table 12
The Effect of Anti-troll Laws on Public Companies' R&D Expenditures

	(1)	(2)	(3)	(4)	(5)
	All Firms	All Firms	Sales < $100m	Assets < $200m	R&D > 0
Anti-Troll Law	-0.213***	-0.061***	-0.299***	-0.094	-0.074
	(0.001)	(0.001)	(0.000)	(0.182)	(0.138)
Anti-Troll Law *Tech	0.338**	0.055*	0.282**	0.176*	0.157*
	(0.011)	(0.061)	(0.044)	(0.086)	(0.082)
Tech	1.406***		0.658***	0.680***	0.680***
	(0.000)		(0.000)	(0.000)	(0.000)
Constant	-0.931***	0.229	0.913***	0.965***	-1.136***
	(0.000)	(0.289)	(0.000)	(0.000)	(0.000)
# of Observations	12,932	12,932	3,434	4,193	8,954
R-squared	0.468	0.986	0.462	0.461	0.719
State FE	YES	NO	YES	YES	NO
Firm FE	NO	YES	NO	NO	YES
Time FE	YES	YES	YES	YES	YES
SD Clusters	State	State	State	State	State

Note: The dependent variable, $R\&D_{ist}$, is the natural log of R&D expenditure for company *i* located in state *s* at year *t* as reported in Compustat. *Anti-Troll Law* is a dummy variable taking a value of 1 at time *t* for a given state if the state has passed the law at any time before t. *Tech* is a dummy variable indicating the target belongs to a tech industry. Control variables are natural logarithm of sales, firm age, leverage ratio, profitability, tangibility, free cash flow, and Herfindahl index. Control variables are not reported for brevity. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state. State and year-quarter fixed effects are included in all tests. Standard errors are clustered by state.***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

APPENDIX C

ANTI-PATENT TROLL LAWS SIGNING DATES

STATE	DATE
AL	4/2/2014
AZ	3/24/2016
CO	6/5/2015
CT	5/8/2017
FL	6/2/2015
GA	4/15/2014
ID	3/26/2014
IL	8/26/2014
IN	5/5/2015
KS	5/20/2015
LA	5/28/2014
ME	4/14/2014
MD	5/5/2014
MI	1/6/2017
MN	4/29/2016
MS	3/28/2015
MO	7/8/2014
MT	4/2/2015
NH	7/11/2014
NC	8/6/2014
ND	3/26/2015
OK	5/16/2014
OR	3/3/2014
RI	6/4/2016
SC	6/9/2016
SD	3/26/2014
TN	5/1/2014
TX	6/17/2015
UT	4/1/2014
VT	5/22/2013
VA	5/23/2014
WA	4/25/2015
WI	4/24/2014
WY	3/11/2016

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