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Patent Trolls and the Market for Acquisitions

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Abstract

I study the effect of patent-infringement claims by patent trolls on acquisitions of small firms. Exploiting staggered adoption of state anti-patent troll laws, I find that the laws have two effects. First, the number of acquisitions of small firms declines after these laws are adopted. Second, the anti-troll laws increase the acquisition price for acquirers. The market reflects the increased cost of acquisition as measured by lower acquisition announcement returns. Large firms increase R&D after the adoption of state laws, replacing external innovation. Using a sample of acquisitions that are plausibly unaffected by the laws, I disentangle alternative explanations.

I. Introduction

Patent litigation has historically been the last resort for companies to protect their products and services against patent infringement. In recent years, however, entities with no 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 nonpracticing entities (NPEs or often known as "Patent Trolls") whose core business is patent litigation and licensing (Cohen, Gurun, and Kominers (2019)).

Yet, litigation is only a small fraction of patent trolls' activities. Patent trolls primarily send *demand letters* with vague patent-infringement claims to a large number of firms, offering questionable licensing agreements in lieu of litigation (American Intellectual Property Law Association (AIPLA) (2013)).[1] Small firms are trolls' main target because they have neither the financial resources nor sufficient legal knowledge to defend themselves in a lengthy lawsuit and often choose to quickly reach a settlement and pay a simple "go away" fee regardless of actual infringement. Thus, small firms that are frequently targeted by patent trolls' demand

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[1]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.

letters experience significant cash outflows and declines in valuation that often leave them with no choice other than selling out to well-funded firms at heavily impaired prices (Chien (2013)).

In this article, I investigate the effect of patent trolls' frivolous infringement claims on the economics of acquisitions of small firms. 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 acquisition volume. I find that the number of acquisitions of independent targets declines by 5.4% after the adoption of the state laws, suggesting that, by reducing exposure to patent trolls and thus supplying legal protection, anti-troll laws eliminate a group of acquisitions where the target mainly seeks protection against trolls from a larger acquirer. The effect of laws is more pronounced in the tech industries: Acquisitions in the tech industries decline by 8.3%. In contrast, the adoption of state laws has no effect on the acquisitions of nontech firms. Moreover, the effect of anti-troll laws is stronger for acquisitions when the target firm is small. These findings support existing evidence that patent trolls target firms in the tech industries and mainly small firms.

Next, to investigate the effect of anti-troll laws on the proceeds the targets receive, I define PRICE_RATIO as the value of the acquisition deal scaled by book value of the target's assets.[3] I find that the adoption of anti-troll laws significantly increases acquisition price ratios in the tech industries and that this effect is magnified among small target 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. I perform a series of analyses to test the validity of the identification assumptions. First, using a sample of acquisitions in which the targets are unlikely to be threatened by patent trolls, I show that the adoption of anti-troll laws affects neither acquisition volume nor acquisition price ratios. These targets, which I refer to as nonindependent targets, are subsidiaries of larger firms, and thus 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. Second, I find that patent litigation brought to U.S. district courts by patent trolls declines after the adoption of anti-troll laws. More importantly, this decrease is completely concentrated in the group of lawsuits that target small and private defendants. In contrast, patent-related lawsuits filed by plaintiffs other than patent trolls do not change after the adoption of the state laws. These findings suggest that the anti-troll laws are effective in limiting the activities

[2]Examples of bad faith are providing vague information and ambiguous claims, as well as demanding unreasonably high fees in a short period of time.

[3]Given the market valuation of the private targets is not available, I cannot calculate the acquisition premium and thus use price ratio.

of patent trolls, especially against small and private firms. Finally, consistent with the parallel trend assumption, I find that the acquisition market in treated and control states do not differ up to 4 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.

In addition, recent advancements in econometric theory reveal that the standard Two-Way Fixed Effects (TWFE) estimation may be biased when the treatment timing and dynamic treatment effects are heterogeneous (Callaway and Sant'Anna (2020), De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), and Sun and Abraham (2021)). Given that anti-troll laws were enacted recently and during a short period of time, and given that 15 states never passed the laws (acting as clean controls), estimating their effect with a TWFE estimator is less prone to this potential bias. Nonetheless, I estimate the effect of anti-troll laws using 2 alternative proposed estimators that correct for such bias (Stacked Regression Estimator and Callaway and Sant'Anna (2020)) and find qualitatively similar results. The coefficients have the same sign and statistical significance as the TWFE estimator with slight variation in economic magnitudes.

Next, I examine the effect of anti-troll laws on other notable aspects of the acquisitions. First, the additional protection against patent trolls reduces the risk of stand-alone operation for small firms and thus enhances the targets' positions in deal negotiations. This enhanced negotiation power may linger the negotiation process and may lead to increases in the likelihood of failure in reaching an agreement. Furthermore, the choice of payment in the acquisition reveals an entrepreneur's desire to remain engaged with her innovation: Cash payment mitigates her idiosyncratic risk while equity ownership in the acquirer ties her wealth more closely to her innovation. By mitigating litigation risk, the state laws increase the entrepreneur's incentives to remain engaged and thus lead to a higher probability of equity payments in acquisitions of small firms. I find empirical evidence supporting these two hypotheses. First, the completion rate of tech acquisitions is lower after the anti-troll laws are signed. Second, among completed acquisitions, the time to completion is significantly longer for tech acquisitions after the signing of anti-troll laws. Lastly, the adoption of anti-troll laws increases the probability of noncash payments in tech acquisitions.

The effect of anti-troll laws on the acquisition proceeds to targets raises an interesting question. Do targets receive a larger pay because the laws tilt the division of gains in their favor or because the laws increase the total value gain in the deal? The former implies that the value that acquirers can extract from an acquisition declines after the adoption of the laws while the latter appeals to an increase in the acquirer's gain. Supporting the former hypothesis, I find that in a sample of acquisitions involving public acquirers,[4] acquirers of treated tech targets have

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

1.4% lower announcement cumulative abnormal returns, leading to $29.4 million lower value accrued to the median public acquirer.[5] My final test examines the effect of anti-troll laws on the innovation activities of public firms as potential acquirers. I find that R&D expenditure increases in public tech firms after the adoption of anti-troll laws, suggesting that public firms are more likely to turn to internal innovation after the anti-troll laws are passed.

Although there exists a large body of research on corporate innovation, less attention has been paid to the effects of patent trolls. Cohen et al. (2019) show that patent trolls target firms that are flush with cash and firms that are busy dealing with other patent-unrelated litigation and document significant decline in R&D in targeted firms.[6] Bessen, Ford, and Meurer (2011) find that patent troll lawsuits are associated with $500 billion of lost wealth to defendants from 1990 to 2010. Feldman and Frondorf (2015) report that significant proportion of IPOs received patent demands either shortly before or after their IPO. My study adds to this growing literature in 2 distinct ways. First, I examine the effect of patent trolls' abusive patent claims on small businesses as opposed to large, public firms. This is crucial given small firms comprise the dominant majority of recipients of demand letters and the majority of defendants in patent troll lawsuits (Chien (2013)). Second, while the studies above examine the effect of actual litigation, I focus on the contingent threat of being targeted by a troll via demand letters. This contribution is important given the significant majority of patent disputes are settled outside of courtrooms.

This article also provides additional evidence on the effect of state anti-troll laws. 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 of nonpracticing 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 anti-troll legislation improves employment and access to VC funding at small tech firms. My study documents 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.

While I focus on (the threat of) patent litigation, my work is related to a broader literature that documents adverse effects of the U.S. litigation system on innovative firms. Lin, Liu, and Manso (2021) show that the adoption of universal demand laws, which reduces the risk of shareholder derivative lawsuits, increases firms' innovation activities. Mezzanotti (2021) finds improvements in patent enforcement lead to general increase in corporate innovation activities by reducing some of the distortions caused by patent litigation. Kempf and Spalt (2022) suggest that securities class action lawsuits constitute an obstacle to valuable corporate innovation. My work compliments these studies in highlighting the adverse effects of the patent litigation system on innovative firms.

[5]Market value of the median public acquirer in my sample is $2.1 billion.

[6]Smeets (2014) also documents significant decreases in innovation activities in public firms targeted by patent trolls.

The fact that litigation threat is a driving decision for small firms to seek an acquiring partner is relevant to our understanding of motives for mergers and acquisitions. Mergers provide an efficient and value-increasing response for firms to industry-side technological or regulatory shifts (Mitchell and Mulherin (1996), Andrade, Mitchell, and Stafford (2001), Jovanovic and Rousseau (2001), and Harford (2005)). Firms also merge to exploit synergy value, efficiency gains, and economies of scale (Schoar (2002), Lambrecht (2004), Hoberg and Phillips (2010), and Maksimovic, Phillips, and Prabhala (2011)). Exploiting firm misvaluation is another motive for an acquisition (Shleifer and Vishny (2003), Rhodes-Kropf, Robinson, and Viswanathan (2005)). Target firms, especially small firms, enjoy supplied liquidity by their acquiring partners, suggesting that easing financial constraint is an important motive for mergers and acquisitions (Fuller, Netter, and Stegemoller (2002), Officer (2007), Erel, Jang, and Weisbach (2015), and Greene (2017)). This study contributes to this large strand of research by highlighting litigation risk as a new motivation for mergers.

II. Background on State Anti-Patent Troll Laws

When a patent-infringement lawsuit is filed by a patent troll, both the target firm (defendant) and the patent troll (plaintiff) must supply documents to demonstrate how the alleged patent-infringing product is made during the *discovery phase*. Since patent trolls do not make products, the discovery phase is far less costly for them while extremely costly for the defendant. Moreover, the discovery phase, the court's assessment of the parties' claims, and its subsequent ruling often take multiple years, requiring significant time and monetary commitment.

Taking advantage of targets' aversion to a long and expensive litigation, patent trolls send mass demand letters to small firms with the intention of making a licensing offer and threatening litigation unless a royalty fee is paid. Patent trolls hope that the recipients' lack of experience with litigation, lack of knowledge with the patent system, and lack of financial resources will coerce them into licensing agreements (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.[7] Another example is Lodsys. Between 2011 and 2013, Lodsys sent thousands of demand letters to small iOS app developers.[8] Chien (2013) reports that firms being targeted by patent trolls spent significant founder time and 5%–24% of annual revenue in costs. The majority of targeted firms reported distraction from their core business, significant impact on their operations, loss of firm value, and even having to eliminate a business line or the business altogether.

The significant amount of evidence on the adverse effects of patent trolls has resulted in introduction of several bills in Congress since 2012 to limit the activities

[7]The F.T.C Complaint regarding MPHJ's aggressive behavior can be found at: https://www.ftc.gov/enforcement/cases-proceedings/142-3003/mphj-technology-investments-llc-matter. Also, coverage of the case by the New York Times can be found at: https://www.nytimes.com/2014/11/07/business/ftc-settles-first-case-targeting-patent-troll.html.

[8]See https://trollingeffects.org/ letters for examples of seemingly abusive demand letters sent by NPEs.

of patent trolls, and in particular, their ability to send abusive demand letters.[9] However, as of today, none of them have become law. Proponents of patent trolls contend that they allow individual inventors, who lack the capacity to commercialize their patents, to monetize them through a more efficient licensing entity. Nonetheless, the empirical evidence in law, economics, and finance literature is inconsistent with the intermediary role of patent trolls (e.g., Chien (2013), Feldman (2013), Cortropia, Kesan, and Schwartz (2014), Smeets (2014), Tucker (2014), Feldman and Frondorf (2015), and Cohen et al. (2019)).

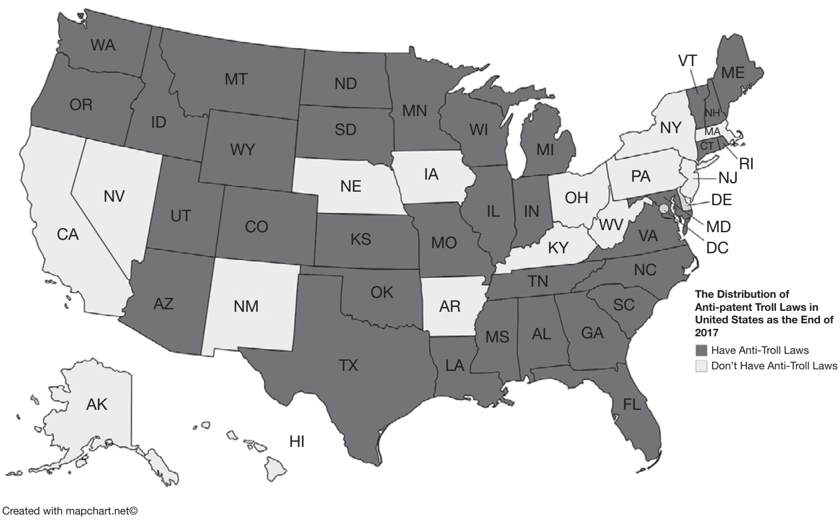
In response to the lack of federal legislation, a number of states, beginning with Vermont in 2013, have adopted legislation that protects *local businesses* from bad-faith demand letters. As of the beginning of 2018, 35 states have passed a version of the anti-troll laws. Figure 1 depicts states that have passed the anti-troll laws as of the beginning of 2018. Also, Table IA1 in the Supplementary Material reports the signing dates for each state anti-troll law.

The anti-troll laws aim to curtail bad-faith demand letters by allowing courts to impose penalties on the senders of such letters. In Vermont, for example, if a court rules that the patent troll (plaintiff) has sent a bad-faith demand letter to a firm *located* in Vermont, then the law allows the court to award it the following: "i) equitable relief; ii) damages; iii) costs and fees, including reasonable attorney's

FIGURE 1

Anti-Troll Laws Across the States

Figure 1 shows the distribution of anti-troll laws across the States as of the first quarter of 2018. Table IA1 in the Supplementary Material reports the signing date of each state laws.



[9]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).

fees; and iv) exemplary damages in an amount equal to $50,000 or 3 times the total of damages, costs, and fees, whichever is greater." Moreover, the law allows the court to completely dismiss the case prior to the discovery phase, and subsequent evaluation of the patents and infringement claims.

An important feature of the anti-troll laws is that they cover any target firm located in the state, regardless of where the firm is incorporated or where the sender of the letter is located or incorporated. This is achieved by framing the anti-troll legislation as consumer protection laws, which have state jurisdiction and thus circumventing federal jurisdiction of patent laws.[10]

The political economy surrounding the laws has 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 Feb. 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. Overall, the fact that lobbying for the laws was initiated by either small firms or a nonhigh-tech industry group (such as financial institutions) mitigates reverse-causality concerns.[11]

III. Empirical Design and Data

A. Empirical Design

The passage of anti-troll laws across the states has been staggered over the period of 2013 to 2017. 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 standard TWFE difference-in-differences estimator with staggered treatment events. Specifically, to identify the effect of anti-troll laws on state-level aggregate outcomes, I estimate the following difference-in-differences specification at the state-quarter level:

$$Y_{s,t} = \alpha_s + \lambda_t + \beta \times \text{ANTI\_TROLL\_LAW}_{s,t} + \Gamma \times X_{s,t-1} + \varepsilon_{s,t}, \tag{1}$$

where s denotes state and t denotes calendar 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 0 otherwise.[12] $X_{s,t-1}$ is a vector of control variables at the state level, including state GDP, state per capita income, and an indicator variable that takes 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.

[10]For a comprehensive legal discussion of the anti-troll laws, see DeSisto (2015).

[11]For a more comprehensive description of the political economy around the state laws, see Appel et al. (2019).

[12]To investigate the dynamic effects of the laws, I replace ANTI_TROLL_LAW with a set of relative time-to-treatment indicators t_k for $-5 < k < 5$.

To identify the effect of anti-troll laws on deal-level outcomes, I estimate the following difference-in-differences specification at the deal level:

$$(2)\quad Y_{i,s,t} = \alpha_s + \lambda_t + \beta \times \text{ANTI\_TROLL\_LAW}_{s,t} + \Gamma \times X_{s,t-1} + \delta \times M_{i,s,t} + \varepsilon_{i,s,t},$$

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. 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.

However, while this design offers advantages with regards to concerns about contemporaneous trends and confounding factors, recent advancements in econometric theory reveals that the standard TWFE estimation may be biased when the treatment timing and dynamic treatment effects are heterogeneous (Callaway and Sant'Anna (2020), De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), and Sun and Abraham (2021)).

Goodman-Bacon (2021) demonstrates that the TWFE estimator in equations (1) and (2) is a weighted average of all possible canonical 2×2 difference-in-differences estimators that compare three different timing groups (Treated vs. Never Treated, Earlier Treated vs. Later Treated, and Later Treated vs. Earlier Treated). In the presence of heterogeneous and dynamic treatment effect, the estimators from the last group (Later Treated vs. Earlier Treated) may be biased since the control units' outcome variable at the time when the later-treated units receive the treatment is contaminated with the effect of their earlier treatment. In general, Baker, Larcker, and Wang (2022) argue that the bias due to dynamic treatment effect is strong when the timing of treatment stretches across a long time, and when all (or a dominant majority) of units eventually receive treatments. Therefore, estimating the effect of anti-troll laws with a TWFE estimator is less prone to this potential bias given that they are enacted recently and during a short period of time, and given that 15 states never passed the laws and thus serve as the cleanest effective control units.

Nonetheless, I explore the extent of this bias in my setting using Goodman-Bacon (2021) decomposition. As a diagnostic test for identifying the potential bias, Goodman-Bacon (2021) proposes to plot the constituent 2×2 estimates by each constituent comparison's implicit assigned weight and constituent comparison's type (e.g., Earlier vs. Later Treated).

I then investigate the robustness of the TWFE estimates from equations (1) and (2) to alternative proposed estimators that correct for such bias: Callaway and

Sant'Anna (2020) and Stacked Regression Estimator.[13] Callaway and Sant'Anna (2020) estimate treatment effect in multiple time periods with the presence of variation in both treatment effect and treatment timing. CS first estimates the average treatment effect for each treated group, where a group is defined by when units are first treated (i.e., states first treated in 2014-Q1 are one group), and in each time period separately, denoted $\text{ATT}_{g,t}$, "group-time average treatment effects." Then, adjusting the weights for these estimates, it averages the treatment effect across groups and ultimately across time periods to arrive at an estimate analogous to the TWFE coefficent estimate on ANTI_TROLL_LAW in equations (1) and (2).[14] I use CS estimation while allowing never-treated as well as not-yet-treated states to be valid controls.[15]

Next, I estimate the effect of anti-troll laws using the Stacked Regression Estimator.[16] Specifically, I create an event-specific data set for each treatment group, defined as a single or multiple states that adopt the law in a certain quarter, which includes all observations for the treated state(s) and all other clean control states for an 11-quarter window ($t=-5$ to $t=5$). A clean control state is one that either never adopts an anti-troll law or does not adopt the law until 6 quarters after the adoption for the treatment group. Then, I stack these event-specific data sets based on quarter relative to treatment ($t=-5$ to $t=5$) instead of calendar time. Finally, I calculate an average effect across all events using the following specification:

$$(3) \qquad Y_{s,t,g} = \alpha_{s,g} + \lambda_{t,g} + \beta \times \text{ANTI\_TROLL\_LAW}_{s,t,g} + \Gamma \times X_{s,t-1,g} + \varepsilon_{s,t,g},$$

where s denotes state, t denotes quarter relative to treatment for each group instead of a calendar quarter, and g denotes a treatment group. ANTI_TROLL_LAW is posttreatment indicator. $\alpha_{s,g}$ is a set of state-group fixed effects and $\lambda_{t,g}$ is a set of relative time-group fixed effects. The difference between this functional form and the standard TWFE is that the state and time-fixed effects are group specific. As a result, by stacking and aligning events in event-time, this approach is equivalent to a setting where the treatment events happen contemporaneously, and it prevents using past treated units as effective comparison units, which may occur with a staggered design.

[13]There are two other estimators to my knowledge that correct for the potential biases in TWFE estimation. First is Sun and Abraham (2021) which is numerically similar to Callaway and Sant'Anna (2020) and thus for brevity, I focus only on the latter. Second is De Chaisemartin and d'Haultfoeuille (2020) which mainly focuses on single-period treatment which is not applicable in my setting.

[14]For a more detailed description of CS estimator and applied examples, refer to Callaway and Sant'Anna (2020) and Baker et al. (2022).

[15]Note that the CS estimator is asymptotically unbiased in both cases. However, using not-yet-treated control states drops fewer observations and presumably has higher power to detect treatment effects. Nonetheless, the results are qualitatively similar when only never-treated states are used as control groups.

[16]Baker et al. (2022) recommend this estimation as a versatile and credible approach in lieu of TWFE. Deshpande and Li (2019) is a published study that utilizes such estimation to estimate the effect of application costs on the targeting of disability programs using the closings of Social Security Administration field offices.

B. Data

I obtain M&A transaction data from Capital IQ. The rationale for using Capital IQ is twofold. First, Capital IQ reports a broader sample of transactions including 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 M&A 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 nonindependent targets. In the latter case, Capital IQ reports the target's parent firm 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 nonindependent acquisitions, however, are used in additional identification tests.

Because patent trolls mostly tend to target firms in the tech industries (AIPLA (2013), Chien (2013)), I follow Bureau of Labor Statistics (BLS) to identify tech industries[17] and investigate the effect of anti-troll laws on tech and nontech acquisitions separately. I use the 4-digit SIC codes of the target to determine whether the target is in one of the high-tech industries.[18] After removing deals with unknown SIC codes, the final sample of M&A transactions comprises 12,631 deals in tech industries and 30,267 deals in nontech industries. I aggregate the transaction data to state-quarter observations using the geographic location of the target and the announcement date of the deal.

To study acquisition prices, I define PRICE_RATIO as a measure of the payoff to the target. 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.[19] While different from the commonly-used acquisition premium, this measure captures the payoff to the target shareholders for a given dollar of investment by the target firm (total assets). 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 provides me with price ratio for 1,367 deals. Scaling transaction value with other common measures such as book value of equity or annual sales leads to qualitatively similar results; however, they are less populated in my sample.

I collect data on patent-related litigation from The Stanford Non-Practicing Entity (NPE) Litigation Database, a publicly available data set that comprehensively

[17]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.

[18]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.

[19]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 the overwhelming majority of target firms in my sample are private firms.

tracks patent-related litigation in the United States. I use the Database's detailed categorization of the plaintiff to determine whether the lawsuit is filed by a patent troll or a practicing entity. To aggregate litigation data at the state-quarter level, I use the location of the district court the case is brought to and the filing date.

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 et al. (2019), I also control for contemporaneous state laws aimed at promoting small businesses and startups.[20]

Panel A of Table 1 reports descriptive statistics for the whole sample from the first quarter of 2010 to the first quarter of 2018. On average, there are 24.96 acquisition deals, 7.23 tech deals, and 17.73 nontech 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. Out of 42,631 acquisition deals in the sample, deal size is reported 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. Furthermore, tech acquisitions are significantly smaller than nontech deals. Due to data limitations, I can only measure PRICE_RATIO for 1,367 deals, 461 of which are tech deals and 960 are nontech deals. The average PRICE_RATIO is 4.47 for tech deals and 3.05 for nontech deals, implying that acquirers, on average, pay $4.47 for a single dollar of assets in tech targets while they pay only $3.05 for a single dollar of assets in nontech targets. 96% of announced tech deals and 93% of nontech deals successfully complete with an average of 37 days for tech deals and 42 days for nontech deals to complete. Also, 27% of high-tech deals involve noncash payments to the target while 22% of nontech deals involve noncash payments. For the sample of acquisitions with public acquirers, the acquirer's 3-day CAR is 90 basis points for the tech deals and 95 basis points for nontech deals. Lastly, R&D expenditure is $145 million for the whole sample with tech firms having higher R&D at $196 million and nontech firms averaging only $105 million in R&D.

Panel B of Table 1 reports the breakdown of acquisitions according to their size, industry, and ownership of the target. There are 42,631 independent targets 21,935 of which have missing size. Out of the remainder, 7,182 are smaller than $50 million while 2,274 are larger than $50 million. Out of the 42,631 independent acquisitions, 12,364 are within the tech industries and 30,267 are in the nontech industries. The sample also includes 25,762 nonindependent targets with 8,159 in the tech and 17,603 in the nontech industries. In contrast to independent acquisitions, the size distribution of nonindependent targets slightly tilts toward larger deals. Specifically, 52% of nonindependent acquisitions are valued more than $50

[20]Appel et al. (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.

TABLE 1
Descriptive Statistics

Panel A of Table 1 reports the summary statistics of all the variables used in the study. Independent Acquisitions are those in which the target is an independent entity. Nonindependent Acquisitions are those in which the target is owned by a larger entity such as an investment firm or a larger corporation. Tech industries are defined following Bureau of Labor Statistics (BLS) classifications. ACQUISITION_SIZE is reported in $million. PRICE_RATIO is the ratio of deal value to the target's latest available book value of assets. COMPLETION is a dummy indicating whether a deal is completed or withdrawn. TIME_TO_COMPLETE is the number of days between completion and announcement dates. NONCASH is an indicator variable taking the value of 1 if the target receives a noncash payment (fully or partially) and 0 otherwise. CAR acquirer's cumulative abnormal returns in a 3-day window around the deal announcement date. R&D is reported in $millions. Panel B reports the breakdown of number of acquisitions by ownership, size, and industry.

Panel A. Summary Statistics

	All Acquisitions				Tech Acquisitions				Non-Tech Acquisitions			
	No. of Obs.	Mean	Median	Std. Dev.	No. of Obs.	Mean	Median	Std. Dev.	No. of Obs.	Mean	Median	Std. Dev.
INDEPENDENT *per state/quarter*	1,683	24.96	12	17.67	1,683	7.23	4	11.28	1,683	17.73	11	21.04
NONINDEPENDENT *per state/quarter*	1,683	15.14	6	12.54	1,683	4.79	2	10.1	1,683	10.35	6	14.05
ACQUISITION_SIZE	9,504	257.43	15.6	2,412	2,816	239.57	14.7	2,299	6,688	264.96	16.12	2,459
PRICE_RATIO	1,367	3.53	2.05	3.9	461	4.47	2.97	4.03	906	3.05	1.66	3.74
COMPLETION	41,219	0.94	1	0.23	12,365	0.96	1	0.19	28,854	0.93	1	0.25
TIME_TO_COMPLETE	38,922	40.58	0	89.45	11,870	37.14	0	88.5	26,834	42.62	0	89.67
NONCASH	16,707	0.23	0	0.42	4,809	0.27	0	0.44	11,898	0.22	0	0.41
CAR	7,580	0.93	0.12	16.38	2,973	0.9	0.03	21.13	4,785	0.95	0.18	12.55
R&D	12,936	145.98	16.1	565.55	5,795	195.89	30.14	642.51	7,137	105.45	5.86	486.46

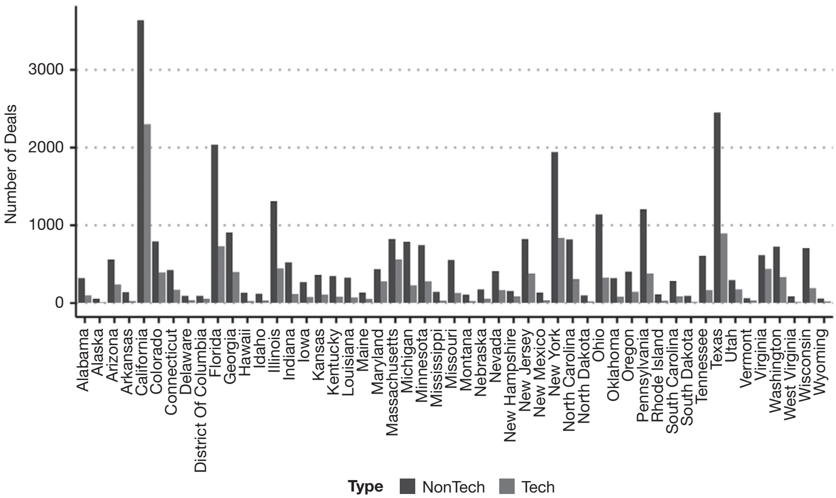
Panel B. Sample Breakdown

	All Acquisitions				Tech Acquisitions				Non-Tech Acquisitions			
	Total	Small	Large	Missing	Total	Small	Large	Missing	Total	Small	Large	Missing
INDEPENDENT	42,631	7,182	2,274	21,935	12,364	2,175	619	9,570	30,267	5,007	1,655	23,605
NONINDEPENDENT	25,762	6,076	6,559	13,597	8,159	1,781	2,066	4,597	17,603	4,295	4,493	9,000

FIGURE 2

The Number of Acquisitions Across the States

Figure 2 plots the number of tech and nontech acquisitions in 50 U.S. States and the 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.



million whereas only 24% of independent targets are valued at more than \$50 million.

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. For example, California has the highest number of deals in both tech and nontech industries with more than 3,500 tech deals and 2,400 nontech deals. For robustness, I show that all the results reported in this study hold after excluding the states with abnormally high acquisition activity. Moreover, Figure IA1 in the Supplementary Material shows the distribution of acquisitions over the sample period. The acquisitions show a steady level over the years with little variation in both high-tech and nontech industries. The lack of time series variation in the number of acquisitions is consistent with Netter, Stegemoller, and Wintoki (2011), documenting that the merger waives are driven by large acquisitions and the presence of merger waives significantly attenuates with the inclusion of small/private target firms in the sample.

IV. Anti-Troll Laws and Number of Acquisitions

A. Main Results

In Table 2, I examine how anti-troll laws affect acquisition activities using the TWFE estimator in equation (1). 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 the

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TABLE 2

The Effect of Anti-Troll Laws on Acquisitions of Independent Targets

The dependent variable in Table 2, ln(1+NUMBER_OF_DEALS)$_{s,t}$, is equal to the natural log of one plus the number of acquisition 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 GDP, 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. A small transaction is defined as deals involving targets that are smaller than $50 million. 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% levels, respectively.

	All Deals	All Tech	All Non-Tech	Small Tech	Small Non-Tech	Large Tech
	1	2	3	4	5	6
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)
No. of deals	42,631	12,364	30,267	2,243	5,184	570
No. of state-quarters	1,683	1,683	1,683	1,683	1,683	1,683
R^2	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

average state. In columns 2 and 3, I repeat the analysis for tech and nontech 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 nontech industries is not significant. These findings support the notion that patent trolls are more active in the tech industries where patents are more vague and complex (AIPLA (2013), Chien (2013), and Appel et al. (2019)).

To shed light on the dynamics of the laws' effect, Figure 3 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 the control states from quarter $t-5$ to quarter $t+5$ and beyond, where for each treated state, quarter t is the quarter when the anti-troll law is signed into law. Graph A plots the estimated differences for tech industries. While the difference between treated and control states is not significant prior to the adoption of the anti-troll laws, the number of tech acquisitions is significantly lower in treated states following the adoption of state laws. Graph B plots the estimated differences for nontech industries. The number of nontech acquisitions do not differ significantly in treated states following the adoption of state laws.

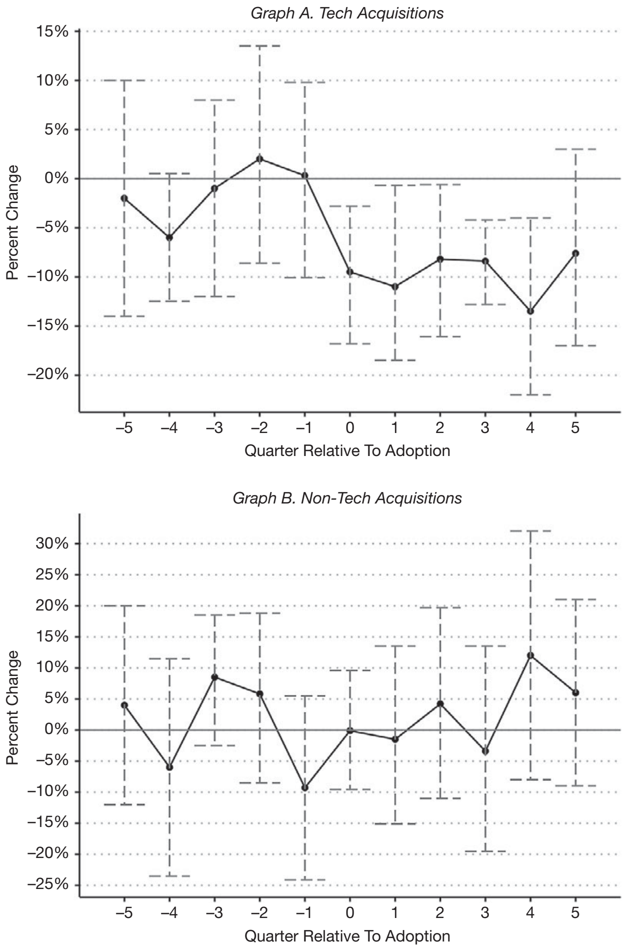
Small businesses, lacking legal expertise and financial resources, are more vulnerable to patent trolls, making the effect of anti-troll laws more profound for them. In columns 4 and 5, I limit the sample to acquisition deals in which the target is small in tech and nontech industries, respectively. 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 nontech industries. Lastly, I examine the effect of anti-troll laws on the acquisition of large, tech deals in column 6. The acquisition of large tech targets drops by 3.9% after the adoption of anti-troll laws.

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, especially small firms and firms in the tech industries, and forces them to more frequently sell to

FIGURE 3

The Dynamic Effect of Anti-Troll Laws on Acquisition Activities

Figure 3 plots the evolution of acquisition activity in tech industries (Graph A) and nontech industries (Graph B) in states with anti-troll laws relative to states without such laws. I estimate equation (1) as in Table 2, except that I replace the ANTI_TROLL_LAW indicator with indicators that identify quarters $t-5$, to $t+5$ 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.



deep-pocketed firms that have significant knowledge and resources to combat trolls and their frivolous claims. It appears that the legal protection the adoption of anti-troll laws provides reduces the frequency with which small firms choose to exit via an acquisition. The evidence is consistent with the typical targets of patent trolls' demand letters: small firms, especially in the tech industries.

B. Identification and Robustness Tests

At the heart of the difference-in-differences research design lie concerns around potential endogeneity of the state-level anti-troll laws. One possibility is

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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 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 et al. (2001), and Harford (2005)). I address this possibility by studying the acquisitions of nonindependent targets. I define nonindependent targets as firms that are owned and operated by larger firms such as private investment firms and other public companies. These targets are great counterfactuals in that they operate in the same state, at the same time, and in the same industry as the independent targets, and thus are exposed to the same potential regional, technological, and regulatory shocks. However, nonindependent targets are protected from patent trolls by the legal expertise and financial resources of their larger parent companies. Therefore, anti-troll laws have no effect on these acquisitions. In Table 3, I estimate the effect of anti-troll laws on nonindependent acquisitions and find that the effect is not statistically significant. The analysis of acquisitions of nonindependent targets helps rule out the potential role of confounding variables such as regional shocks and industry trends in acquisitions. Moreover, Netter, Stegemoller, and 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, mitigating the concern involving merger waves.

Furthermore, the heterogeneity of the effect of state laws on acquisitions is extremely consistent with patent trolls' operation, lending further support to the view that the effect of laws is properly identified. In other words, 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

TABLE 3

The Effect of Anti-Troll Laws on Acquisitions of Nonindependent Targets

The dependent variable in Table 3, ln(1+NUMBER_OF_DEALS)$_{s,t}$, is equal to the natural log of one plus the number of acquisition 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 GDP, 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. Non-independent targets are subsidiaries of other companies sold in the transaction. A small transaction is defined as deals involving targets that are smaller than $50 million. 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% levels, respectively.

	All Deals	All Tech	All Non-Tech	Small Tech	Small Non-Tech	Large Tech
	1	2	3	4	5	6
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)
No. of deals	25,762	8,159	17,603	1,537	4,329	2,021
No. of state-quarters	1,683	1,683	1,683	1,683	1,683	1,683
R^2	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

TABLE 4

The Effect of Anti-Troll Laws on Patent-Related Litigation in U.S. District Courts and Troll-Related Google Searches

The dependent variable in columns 1–6 of Table 4, ln(1+LITIGATION)$_{s,t}$, is equal to the natural log of one plus the number of patent-related lawsuits filed 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 columns 7–8, GOOGLE_SEARCH_INDEX$_{s,t}$, is equal to 1 plus the natural log of Google Search Volume Index for "Patent Trol" in state s during quarter t, excluding DC. 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 GDP, 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% levels, respectively.

	Patent Litigation						Google Search Index	
	Patent Troll All Targets	Patent Troll Public Targets	Patent Troll Private Targets	Patent Troll Small Targets	Nonpatent Troll All Targets	Nonpatent Troll Private Targets	Quarterly DC Excluded	Annual DC Excluded
	1	2	3	4	5	6	7	8
ANTI_TROLL_LAW	−0.101**	−0.031	−0.094**	−0.097**	−0.025	−0.016	−0.067**	−0.091**
	(0.024)	(0.195)	(0.024)	(0.022)	(0.117)	(0.223)	(0.046)	(0.017)
No. of lawsuits	32,413	4,630	27,783	2,230	29,597	27,976		
No. of state-quarters	1,836	1,836	1,836	1,836	1,836	1,836	1,800	450
R^2	0.833	0.811	0.825	0.827	0.880	0.874	0.862	0.855
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

affects firms in different industries, firms of different sizes, and firms with different ownership structures.

Next, I provide further evidence supporting the view that the adoption of anti-troll laws had a significant effect on patent troll activities. First, Figure IA2 in the Supplementary Material reports the number of patent-related lawsuits filed in U.S. district courts from 2005 to 2017 by both patent trolls and nonpatent trolls.[21] The figure shows that while the number of lawsuits filed by nonpatent trolls has stayed flat during the sample, the number of lawsuits filed by patent trolls exhibited a sharp increase starting in 2010 and has declined steadily since 2013, the beginning of the state-level anti-troll laws.

State-level analysis in Table 4 further shows that the national decline in the number of patent lawsuits is completely driven by patent trolls in treated states and mainly focused on private litigation targets. Specifically, column 1 shows that the number of patent troll lawsuits in the treated states declines by 10.1% after the adoption of the state laws. In column 2, I limit the sample to lawsuits in which a patent troll sues a public firm and finds that the adoption of anti-troll laws has no impact. However, column 3 shows that the adoption of the laws leads to 9.4% decrease in patent troll lawsuits against private firms. In column 4, I examine the effect of anti-troll laws on lawsuits filed against small public firms and find a significant decrease in the number of lawsuits after the signing of the law. In columns 5 and 6, I repeat my analysis using the sample of nonpatent troll lawsuits where the plaintiff is a company with products and/or services. Both columns show

[21]Stanford NPE Litigation Database categorizes product companies and their IP subsidiaries as nonpatent trolls.

that the adoption of anti-troll laws has no impact on the number of lawsuits brought to court by product companies. Overall, the findings in Table 4 suggest that lawsuits with higher likelihood of legitimate claims – those filed by product companies and those brought against public and large firms – are not affected by the passage of anti-troll laws, and thus the drop in patent litigation activity is concentrated among patent-troll lawsuits brought against private and small businesses.

Second, similar to Appel et al. (2019), I find that Google searches that are related to patent trolls decline in treated states after the adoption of the laws.[22] Column 7 of Table 4 reports that the adoption of the state laws is associated with a 6.7% decrease in patent troll-related Google searches.[23] In column 8, I repeat my analysis with annual regression and find a larger effect at 9.1% drop in Google search activity. These findings altogether suggest that anti-troll laws effectively curb patent trolls' activities, corroborating the identifying assumption that 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.

Another concern is reverse causality: The states' innovation intensities and business activities may be the triggering force of the new state-level regulation. To address this concern, I perform a wide variety of classic identification tests in Table 5. In columns 1–3, 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. 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 4 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 3 plots the estimated average difference in acquisition activity at treated states relative to control states for both tech and nontech acquisitions. Consistent with the parallel-trends assumption, I find no significant difference in the evolution of acquisitions at treated and control states prior to the passage of anti-troll legislation. In columns 4–6, I perform a placebo test where I assume the laws are passed 3 years before the actual laws are passed in each state. Specifically, I include $\text{ANTI\_TROLL\_LAW}_{t-12}$, which takes a value of 1 from 12 quarters before the law is passed in a state to the end of the sample. Consistent with the parallel-trend assumption, none of the coefficients on $\text{ANTI\_TROLL\_LAW}_{t-12}$ 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

[22]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., Engelberg and Gao (2011), Da, Engelberg, and Gao (2012), Drake, Roulstone, and Thornock (2012), and Da, Engelberg, and Gao (2014)).

[23]I exclude the District of Columbia from the sample because DC has the highest index for all years in the sample by a significant margin.

TABLE 5

Identification Assumptions

The dependent variable in Table 5, ln(1+NUMBER_OF_DEALS)$_{s,t}$, is equal to the natural log of one plus the number of acquisition 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 one neighboring 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 GDP, 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% levels, respectively.

	Trend			$T-12$			Neighbor Law			Neighbor Law		
	All Deals	Tech	Non-Tech	All Deals	Tech	Non-Tech	All Deals	Tech	Non-Tech	All Deals	Tech	Non-Tech
	1	2	3	4	5	6	7	8	9	10	11	12
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	−0.090									
	(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)									
No. 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
No. 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^2	0.914	0.851	0.893	0.913	0.850	0.893	0.913	0.850	0.893	0.914	0.851	0.893
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

decline in acquisitions in a state, then it is likely that the same shock reduces acquisitions in neighboring states even though no anti-troll laws are adopted. In columns 7–12, I include 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.

Furthermore, in Table IA2 in the Supplementary Material, I examine if the preceding results are robust to different model specifications and subsample analyses. First, in columns 1–3, I investigate the effect of anti-troll laws on the number of acquisitions that occur in a given year using annual regressions instead of quarterly. 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 are among the states with the fewest number of acquisitions. On the contrary, California with substantial acquisition activities has yet to pass an

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anti-troll law. This fact raises the concern that the findings may be driven by low-acquisition states. Column 5, estimates a 6.6% decrease in the number of acquisitions after anti-troll laws are passed, using a weighted OLS regression that employs the number of acquisitions in the first quarter of 2010 as weights. Furthermore, in columns 8 and 11, I find that exclusion of California and Texas leads to virtually the same 8.3% decrease in acquisitions after anti-troll laws are passed.

C. Alternative Estimators

In the presence of dynamic treatment effects, the TWFE estimator may be biased (Callaway and Sant'Anna (2020), De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), and Sun and Abraham (2021)). However, estimating the effect of anti-troll laws with TWFE estimators is less prone to this potential bias given that they are enacted recently and during a short period of time, and given that 15 states never passed the laws and thus serve as the cleanest effective control groups (Baker et al. (2022)). These 2 features of the anti-troll laws are visually depicted in Graph A of Figure IA3 in the Supplementary Material.

As a diagnostic test for identifying this potential bias, Goodman-Bacon (2021) proposes to plot the constituent 2×2 estimates by each constituent comparison's implicit assigned weight and constituent comparison's type (e.g., earlier vs. later treated, later vs. earlier treated, and treated vs. never treated). Graph B of Figure IA3 in the Supplementary Material reports the diagnostics plots for the estimation of the effect of anti-troll laws on tech targets (column 2 of Table 2).[24] The decomposition alleviates the concern around the bias due to dynamic treatment effect. First, problematic comparisons in which later treated states are the treatment states and earlier treated states are effective comparison carry only 9% weight and thus account for only less than one-tenth of the average estimated effect. Second, all 3 estimated treatment effects (i.e., earlier treated vs. later treated, later treated vs. earlier treated, and treated vs. never treated) have the same sign and point toward a decrease in the number of acquisitions in the tech industries. Specifically, the estimated coefficients are -0.05, -0.07, and -0.09, which weighted average to -0.08, reported in column 2 of Table 2.

Although Goodman-Bacon (2021) diagnostics test alleviates the concern for bias in this setting, I investigate the robustness of the TWFE estimates reported earlier to the Stacked Regression Estimator and Callaway and Sant'Anna (2020) as 2 alternative estimators that correct for such bias.

Panel A of Table 6 reports the effect of anti-troll laws on acquisition activities estimated with the Stacked Regression Estimator (equation (3)). Column 1 shows that the adoption of the laws reduces the acquisition of tech targets by 15.6%, whereas column 2 shows the laws do not have an effect on acquisitions of nontech targets. Columns 3 and 4 repeat the same analysis while limiting the sample to acquisitions of small targets and report similar results. Specifically, the Stacked Regression Estimator estimates a 14% decrease in acquisition of small tech firms

[24]This decomposition can only be performed on balanced panels and without covariates. So, the results in Figure IA3 in the Supplementary Material differ from Table 2 in that they do not have the covariates. However, the exclusion of covariates does not change the TWFE estimated treatment effect in Table 2.

TABLE 6

Alternative Estimation of the Effect of Anti-Troll Laws on Acquisition of Independent Targets

The dependent variable in Table 6, ln(1+NUMBER_OF_DEALS)$_{s,t}$, is equal to the natural log of one plus the number of acquisition 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*. Panel A estimates the effect of Anti-Troll laws on acquisition activities using the Stacked Regression Estimator. The stacked regression estimator stacks cohort-specific (Group) data sets that include observations from states that adopt the law in a certain quarter, and all states that do not adopt within 10 quarters. The stacked regressions include the interaction of cohort-specific event date with both calendar date and states as two sets of Fixed Effects. These FEs are analogous to state and time FEs in TWFE specification. Control variables are state GDP, 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. Panel B estimates the effect of Anti-Troll laws on acquisition activities using Callaway and Sant'Anna (2020). The effective comparison observations (control group) are not-yet-treated states. No control variables are included in Panel B. A small transaction is defined as deals involving targets that are smaller than $50 million. Standard errors are clustered by state. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	All Tech	All Non-Tech	Small Tech	Small Non-Tech
	1	2	3	4
Panel A. Stacked Regression Estimator				
ANTI_TROLL_LAW	−0.156**	−0.024	−0.140**	−0.097
	(0.028)	(0.666)	(0.021)	(0.129)
No. of obs.	1,518	1,518	1,518	1,518
R^2	0.839	0.911	0.655	0.724
State × Group FE	Yes	Yes	Yes	Yes
Time × Group FE	Yes	Yes	Yes	Yes
Panel B. Callaway and Sant'Anna (2020) Estimator				
ANTI_TROLL_LAW	−0.163*	−0.067	−0.133**	−0.012
	(0.084)	(0.367)	(0.042)	(0.884)
No. of obs.	1,683	1,683	1,683	1,683

that is statistically significant and estimates no significant effect on acquisitions of small nontech firms. Panel B of Table 6 reports Callaway and Sant'Anna (2020) estimates of the effect of anti-troll laws on the acquisition activities. The adoption of the law decreases the acquisitions of tech targets by 16.3% (column 1) which is significant at 10% and has insignificant impact of the acquisition of nontech firms (column 2). Furthermore, the laws reduce acquisitions of small tech firms by 13.3% and have no effect of the acquisition of small nontech firms.

Figure IA4 in the Supplementary Material depicts the dynamics of the effect of anti-troll laws on the acquisition activities in the tech industries. Specifically, the figure plots the estimated average difference in acquisitions at treated states relative to the control states from quarter $t-5$ to quarter $t+5$ and beyond, where for each treated state, quarter t is the quarter when the anti-troll law is signed into law. Graph A plots the estimated differences using the Stacked Regression Estimator and Graph B plots the estimates from Callaway and Sant'Anna (2020). Similar to the TWFE estimates in Figure 3, the difference between treated and control states is not significant prior to the adoption of the anti-troll laws but the number of tech acquisitions is significantly lower in treated states in years following the adoption of state laws.

Overall, the effect of anti-troll laws on the acquisition activities in the tech industries is robust to alternative bias-free estimation methods proposed in the literature, lending further credence to the traditional TWFE estimates of the effect in Section IV.A.

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V. Anti-Troll Laws and Acquisition Price Ratios

A. Main Results

In this section, I examine the effect of anti-troll laws on the acquisition payoff to the target firms. 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), Bargeron, Schlingemann, Stulz, and Zutter (2008), Malmendier and Tate (2008), Jenter and Lewellen (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. 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 mostly 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 2 problems to a certain extent. First, I obtain financial data such as book value of assets, book value of equity, net income, and so forth on target firms from Capital IQ. However, since the targets are usually small private companies, the financial data are sparsely populated. Out of 42,631 independent acquisitions in the sample, only 1,501 deals have reliable financial data. Nonetheless, it is still a large sample when compared to sample sizes in the prior studies on acquisitions that are limited to public targets (see, e.g., Harford (1999), Officer (2003), Bargeron et al. (2008), Malmendier and Tate (2008), Jenter and Lewellen (2015), and others).

Second, as a proxy for payoff to the targets, I use 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. 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 2 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 nontech 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

TABLE 7

The Effect of Anti-Troll Laws on Acquisition Price Ratios of Independent Targets

The dependent variable in Table 7, PRICE_RATIO$_{i,s,t}$, is the value of the deal divided by the target's latest available 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 high-tech industry. A small transaction is defined as deals involving targets that are smaller than $50 million. Control variables are state GDP, 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% levels, respectively.

	All Targets		Small Targets	
	1	2	3	4
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)
No. of deals	1,367	1,367	690	690
R^2	0.101	0.131	0.158	0.187
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

increase acquisition price ratios by 0.46 standard deviations, which is economically large. I replicate this analysis for the sample of 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, the acquisition price ratios increase the most for small, tech targets, suggesting that the new state laws benefit small firms in their states.

I also examine whether the adoption of state laws has a similar impact on acquisition price ratios in nonindependent deals. Table 8 reports insignificant effects, both statistically and economically, in nonindependent deals. This supports the hypothesis that nonindependent targets, who are already protected against trolls by their parents, do not benefit from the adoption of anti-troll laws to the same extent independent businesses do. Moreover, it lends further support to the view that the results are not due to regional, economic, technological, and regulatory shocks that are expected to treat independent targets as well.

B. Robustness Tests and Alternative Estimators

In this section, I perform a series of identification tests related to the TWFE estimated effect. Then, I estimate the effect of anti-troll laws on acquisition prices using the Stacked Regression Estimator (equation (3)). Table IA3 in the Supplementary Material reports the identification tests. I show that i) the effect of state laws on price ratios is robust to exclusion of California (columns 1–4), ii) the results remain virtually the same after inclusion of NEIGHBOR_LAW (columns 5–8), and iii) the coefficients on ANTI_TROLL_LAW$_{t-12}$ is insignificant, suggesting no differences prior the adoption of the state laws (columns 9–12). These findings help alleviate the concern that the estimated effect in Table 7 may be driven by confounding factors such as regional economic shocks, other regulations, and so forth.

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TABLE 8

The Effect of Anti-Troll Laws on Acquisition Price Ratios of Nonindependent Targets

The dependent variable in Table 8, PRICE_RATIO$_{i,s,t}$, is the value of the deal divided by the target's latest available 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 high-tech industry. A small transaction is defined as deals involving targets that are smaller than $50 million. Nonindependent targets are subsidiaries of other companies sold in the transaction. Control variables are state GDP, 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% levels, respectively.

	All Targets		Small Targets	
	1	2	3	4
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)
No. of deals	2,674	2,674	1,434	1,434
R^2	0.062	0.094	0.090	0.132
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

Next, I reestimate the effect of anti-troll laws on acquisition prices with the Stacked Regression Estimator in equation (3).[25] Table IA4 in the Supplementary Material reports the estimation results. Column 1 shows the effect of anti-troll laws on acquisition prices of all deals. Similar to TFWE estimation in Table 7, the estimated effect is indistinguishable from zero. In column 2, I interact the TECH indicator with the ANTI_TROLL_LAW indicator to examine whether the adoption of the law affects deals in the tech and nontech industries differently. Similar to TWFE results, the interaction term enters the equation with a positive and significant coefficient, suggesting that acquisition price ratios for the tech targets go up by 2.997 relative to nontech targets after the adoption of the laws. The estimated effect here is even larger than the TWFE estimated effect of 1.869 and implies that the anti-troll laws increase acquisition price ratios by 0.74 standard deviations. In columns 3 and 4, I repeat this analysis for the sample of small deals. The coefficient estimate on the interaction term in column 4 is positive and significant at the 1% level, and slightly larger than the TWFE estimated effect in Table 7.

VI. Implications for Other Aspects of Acquisitions

The results in Sections IV and V demonstrate that the adoption of anti-troll laws leads to a decrease in the volume of acquisitions and an increase in payouts to targets, especially in the tech industries. These 2 effects are mainly due to the protection the laws offer small firms against patent trolls' activities, reducing the risk of stand-alone operation for the small businesses and thus enhancing their

[25]The deal-level data set used to estimate the effect of anti-troll laws on acquisition prices (Table 7) is an unbalanced panel and thus it is not possible to conduct a Goodman-bacon diagnostics test. Moreover, Callaway and Sant'Anna (2020) do not allow for interaction terms and thus are not suitable in this section.

positions during the acquisition negotiations. Hence, the acquisitions of small targets that are protected by the anti-troll laws may have lower completion rates and longer time to completion.

Moreover, the underlying reason behind small firms' decisions 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 cash deals. When targets receive noncash 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, through reductions in troll risk, small businesses are more likely to agree to noncash acquisition offers after the state anti-troll laws are adopted.

To examine the effect of anti-troll laws on these three aspects of acquisitions, I define three variables, COMPLETION, which indicates whether an announced deal is completed or withdrawn, TIME_TO_COMPLETE, as the natural logarithm of one plus the number of days between an announcement and completion,[26] and NONCASH, that indicates whether the acquisition's method of payment involves a noncash form.

Table 9 reports the estimation results. The first 3 columns investigate the effect of anti-troll laws for the full sample of acquisitions. In column 1, the sample includes both 38,903 completed acquisitions and 2,316 withdrawn deals, and the dependent variable is COMPLETION. The results indicate that acquisitions of targets in the tech industries are 1.1% less likely to be completed.[27] Given that the unconditional likelihood of an announced acquisition in my sample to be withdrawn is 5.2%, a 1.1% increase in the likelihood of a withdrawal as a consequence of the anti-troll laws is economically meaningful. In column 2, the sample includes only completed acquisitions and the dependent variable is TIME_TO_COMPLETE. The estimated results show that the time to completion is significantly longer for tech acquisitions after the signing of anti-troll laws. Further, the results in column 3, where the dependent variable is NONCASH, indicate that the adoption of anti-troll laws increases the probability of noncash payments in tech acquisitions, 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.

The effect of anti-troll laws on acquisition targets should be stronger in acquisitions that involve small targets. The last 3 columns of Table 9 investigate the effect of anti-troll laws on targets that are smaller than $50 million. Column 4 shows that small tech acquisitions that are announced after the adoption of anti-troll laws are 2.1% less likely to be completed, which is significantly stronger than the effect on the whole sample of tech acquisitions in column 1. However, as reported in columns 5 and 6, the effect of anti-troll laws on small

[26]I use one plus log number of days because a significant number of deals in the sample, especially small ones, are completed on the same day they are announced.

[27]I arrive at this number by adding two estimated coefficients of -0.004 and -0.007 and multiplying by 100.

TABLE 9

The Effect of Anti-Troll Laws on Other Aspects of Acquisitions

The dependent variables in Table 9 are COMPLETION$_{i,s,t}$, a dummy variable taking a value of 1 if an announced acquisition is completed and zero if withdrawn, TIME_TO_COMPLETE$_{i,s,t}$, the natural log of one plus the number of days between announcement and completion of the acquisition, and NONCASH$_{i,s,t}$, a dummy variable taking value of 1 if the target accepts a payment method that involves a noncash payment (partially or fully) and takes a value of 0 if payment is only cash. All columns with binary dependent variables are Linear Probability Models. 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 high-tech industry. A small transaction is defined as deals involving targets that are smaller than $50 million. Control variables are state GDP, 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% levels, respectively.

	All Deals			Small Deals		
	Completion	Time to Complete	Noncash	Completion	Time to Complete	Noncash
	1	2	3	4	5	6
ANTI_TROLL_LAW	−0.004	2.282	0.010	0.001	4.344*	−0.002
	(0.384)	(0.119)	(0.422)	(0.918)	(0.066)	(0.925)
ANTI_TROLL_LAW × TECH	−0.007*	3.803**	0.040**	−0.021**	2.564*	0.036**
	(0.092)	(0.043)	(0.036)	(0.044)	(0.084)	(0.046)
TECH	0.006**	1.837	0.050***	0.014**	−0.125	0.049***
	(0.017)	(0.507)	(0.000)	(0.016)	(0.920)	(0.000)
No. of deals	41,219	38,903	16,707	6,538	5,941	7,381
R^2	0.015	0.087	0.042	0.027	0.032	0.032
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

tech acquisitions is fairly similar in magnitude to that of the whole sample of tech acquisitions.

Next, I examine whether the adoption of state laws have a similar impact on acquisition completion characteristics and payment method of nonindependent deals. Table IA5 in the Supplementary Material reports insignificant effects, both statistically and economically, on completion rate, time to completion, and the payment method in acquisition of nonindependent targets. This supports the hypothesis that nonindependent targets, who are already protected against trolls by their parents, are unaffected by the anti-troll laws. Furthermore, I reestimate the effect of anti-troll laws on completion rates, time to completion, and the method of payment using the Stacked Regression Estimator in equation (3). Table IA6 in the Supplementary Material reports the estimation results. The coefficient estimates from the Stacked Regression Estimator are all similar to TFWE estimates in Table 9 with slight differences in economic magnitudes, providing assurance that the implications of anti-troll laws on acquisitions' completion and payment method are robust to an alternative bias-free estimation approach.

Another potential implication involves the value of acquisition deals to acquirers. Sections IV and V show that the state anti-troll laws make acquisitions of small targets harder and more expensive for well-funded firms, raising the question of 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 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 Walkling (1989), Harford (1999), Moeller, Schlingemann, and Stulz (2005), Lehn and Zhao (2006), Masulis, Wang, and Xie (2007), Malmendier and Tate (2008), and others). With its own caveats, the market reaction to the announcement of an acquisition reflects an assessment of the value a particular acquisition has for the firm.

In a sample of public acquirers, I examine the acquirer's cumulative abnormal returns around acquisition announcements, 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 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 10 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 nontech 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 the method of

TABLE 10

The Effect of Anti-Troll Laws on Acquirers' Cumulative Abnormal Returns Around Acquisition Announcements

In Table 10, 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 $t-20$. 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 high-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% levels, respectively.

	All Deals	Cash Deals	All Deal > \$50 m	Cash Deal > \$50 m	All Deal > 1%	Cash Deal > 1%
	1	2	3	4	5	6
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)
TECH	0.002	−0.002**	−0.002	−0.042***	−0.003	−0.022***
	(0.642)	(0.057)	(0.591)	(0.004)	(0.194)	(0.000)
No. of obs.	7,380	3,961	2,437	1,650	3,636	2,470
R^2	0.027	0.030	0.126	0.115	0.044	0.054
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

payment may have. Second, as laid out in Section 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 2 columns of Table 10 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. 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 1.4% lower returns in their 3-day announcement windows relative to acquirers of nontech targets that are affected by the same anti-troll laws. I limit the sample to cash deals in column 4 and find similar evidence. To be in line with the previous literature on mergers and acquisitions, I limit the sample of acquisitions to those with a value 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 $25.2–$29.4 million lower value accrued to the median public acquirer given that the median market value of the public acquirers in my sample is $2.1 billion.

I also estimate the effect of anti-troll laws on acquirers' CARs using the Stacked Regression Estimator in equation (3) and report the estimation results in Table IA7 in the Supplementary Material. Similar to the results in Table 10, the acquirers of tech targets experience lower CARs after the anti-troll laws are signed. However, the economic magnitude of the effect is stronger when estimated with the Stacked Regression Estimator. Lastly, in untabulated robustness checks, I reestimate 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 $t+9$. Moreover, I re-estimate CARs using Fama and French 3-factor model. The results are robust to both the choice of announcement window and the choice of Risk-adjustment model. Overall, the negative market reaction to the acquisition of treated tech targets (targets in states with anti-troll laws) is consistent with the view that the targets are paid more after the adoption of state anti-troll laws, which is undesirable for the investors in the acquiring firm.

The purpose of state anti-troll laws is to protect innovative firms from patent trolls. My last test investigates whether anti-troll laws promote innovation in public firms. These laws may impact public firms' innovation in 2 ways. First, the laws provide protection to large firms' innovation,[28] Second, large public firms, as

[28]Although patent trolls primarily target small, private firms, public firms regularly receive demand letters from patent trolls and often face troll litigation (Cohen Gurun, and Kominers (2016), Cohen et al. (2019)).

potential acquirers, may resort to internal innovation, when the cost of acquiring external innovation increases after the adoption of anti-troll laws. For a sample of public acquirers, I estimate a variation of equation (2) where the dependent variable is the natural logarithm of R&D expenditure and report the results in Table IA8 in the Supplementary Material.[29] I find that the singing of the anti-troll laws increases innovation among public firms, supporting the hypothesis that deep-pocketed firms substitute external innovation with increased internal innovation after the adoption of the anti-troll laws.

VII. Discussion and Conclusion

Patent trolls' impact on small businesses has been the center of debate in the media, among politicians and legislators, and among academics. Anti-troll laws are the first regulatory action aiming to curb the activities of patent trolls. The significant decrease in the number of patent infringement lawsuits by patent trolls after the adoption of anti-troll laws indicates that the laws are effective in curbing abusive patent infringement claims. The anti-troll laws' legal protection against trolls seems to have positive effects on small firms' welfare. First, Appel et al. (2019) show that small tech firms increase employment, raise more financing, and output more innovation after anti-troll laws are enacted. My findings indicate that small firms that choose to accept acquisition offers are better off after the signing of anti-troll laws by receiving higher payouts. In other words, with state anti-troll laws in place, small businesses are better off regardless of whether they decide to monetize their innovations independently or via acquisitions.

Overall, the results suggest that the abusive behavior of patent trolls transfers wealth from small innovators to larger firms via impaired acquisition prices, which is in contrast with the intermediary role that proponents of nonpracticing entities (patent trolls) contend. Given the positive effects of the anti-troll laws at the 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.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000078.

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