Chilling Effects from Anti-SLAPP Laws

Schaufele, Brandon

Ivey Business School, Western University

27 June 2022

Chilling Effects from Anti-SLAPP Laws

Brandon Schaufele*

June 27, 2022

Abstract

Anti-SLAPP legislation has proliferated across the US and Canada. SLAPPs are "strategic lawsuits against public participation," private claims whose objective is to chill opposition by limiting parties' ability to participate in public debate. SLAPPs involve a complementarity between a private harm, typically the tort of defamation, and an extra-judicial project, often a real estate development. This paper incorporates SLAPPS into a standard model of frivolous litigation, demonstrating that the economic implications of SLAPPs are narrower than frequently portrayed. A staggered adoption difference-in-differences research design is applied to empirically estimate the chilling effects of anti-SLAPP laws on construction investment and new home starts in Canada. Results demonstrate that anti-SLAPP laws do chill construction investment by roughly $80 million per month within Canadian cities. New starts of single family homes also decline by 120 per month relative to a counterfactual scenario where anti-SLAPP laws do not exist.

Keywords: Anti-SLAPP laws, chilling effects, civil procedure, frivolous lawsuits, real estate development, tort.

JEL codes: K41, K15, R39.

*Associate Professor, Business, Economics and Public Policy, Ivey Business School, Western University, London, Ontario, Canada, bschaufele@ivey.ca. Valuable comments and feedback were provided by Yonathan Arbel, Christian Dippel, Joshua Foster, Brad Hackinen, Cherie Metcalfe, Nouri Najjar and attendees at several conferences and seminars. All errors are mine.

"Everyone loves to hate SLAPPs." – Beatty (1997, pg.108)

On September 10, 2020, the Supreme Court of Canada released its decision in 1794604 Ontario Limited versus Pointes Protection Association ("Pointes Protection") (2020 SCC 22). The Pointes Protection decision represented the Canadian Court's first guidance on anti-SLAPP legislation, laws that have proliferated across North America. SLAPP refers to "strategic lawsuits against public participation."[1] SLAPPs are meritless defamation lawsuits filed by businesses against ordinary citizens who, on public interest grounds, oppose the companies' activities (Beatty, 1997).[2,3] A SLAPP is private claim, one where the purported objective is to quell opposition and limit a defendant's ability to participate in public debate. The canonical example, characterized by the Pointes Protection case, is when a developer sues a local, grassroots organization in an attempt to intimidate and silence opposition to a real estate project.[4]

Anti-SLAPP laws are designed to distinguish meritorious lawsuits that *arise* from public participation from frivolous suits designed to *suppress* public participation. In the US, 31 states have enacted anti-SLAPP provisions as have the Canadian provinces of British Columbia, Ontario and Quebec (Rasmussen, 2011; anti slapp.org, 2021).[5] Anti-SLAPP advocates argue that defamation lawsuits are malicious and designed to limit expression on valid public policy issues (Ecojustice, 2010). Plaintiffs, as expected, adopt the opposite perspective. They maintain that many legitimate claims are mischaracterized as SLAPPs. SLAPPs are civil litigatino and civil litigation, as a general rule, involves disagreements that are factually or legally ambiguous and most jurisdictions have mechanisms to expeditiously dismiss frivolous suits.[6] The unique fea-

[1]The term SLAPP, or strategic lawsuit against public participation, originates with Canan and Pring (1988). They classify a series of what they view as "intimidation lawsuits," civil tort litigation strategically designed to chill public participation in policy and economic dialogue (Pring and Canan, 1996).

[2]The Pointes Protection decision defines SLAPPs as "lawsuits initiated by plaintiffs who use litigation not as a direct tool to vindicate a *bona fide* claim, but rather as an indirect tool to limit the expression of others" (at para. 2, 2020 SCC 22).

[3]While the large majority of SLAPP claims are for defamation, they may also include breach of contract or a range of torts such as nuisance, conspiracy, process abuse or other business torts. However, since implementing anti-SLAPP legislation, the Canadian province of Ontario has had only 3 out of 52 anti-SLAPP motions claim a tort other than defamation (as of November 2020).

[4]Claims brought by developers against community groups represent the standard example of SLAPP actions, but real estate disputes are not the most common case where anti-SLAPP motions are brought in Canada. Between 2016 and 2021, in Ontario, Canada, 52 unique "s.137.1 motions" (i.e., anti-SLAPP motions) were considered under the province's anti-SLAPP legislation. Developers were involved in only five of these. A large majority were defamation claims between individuals, often those associated with blogs or political publications. Importantly, however, all five suits initiated by developers were dismissed as being SLAPPs, even as only 15 out of the 47 of the remaining claims were viewed as a SLAPP under Ontario's balancing test.

[5]The Canadian province of British Columbia originally had anti-SLAPP legislation for several months in 2001, a law was that almost immediately repealed following the election of a new government. The province reinstated (modified) anti-SLAPP rules in 2019.

[6]Indeed, the dominant theme of the economics of civil procedure involves studying mechanisms that trade-off direct litigation costs versus error costs (Klerman, 2015).

ture of anti-SLAPP legislation is the interaction of a *private* legal proceeding with a *public* policy debate. Anti-SLAPP laws seek to thaw the alleged chilling effect of litigation on public expression. This was the presumptive motivation underlying the Pointes Protection action: a grassroots conservation organization, seeking to protect an ecologically senstive area, may have been reluctant to protest the proposed new development if were not protected from costly, uncertain and frivolous legal action.

The economics of anti-SLAPP laws combines research into chilling effects with the instruments of civil procedure. The nature of most anti-SLAPP legislation is a balancing test where courts sort meritorious from frivolous lawsuits. Anti-SLAPP balancing tests add a unique dimension to the standard rules of civil procedure: they incorporate the prospective value of public expression. Public expression and the ability to participate in policy and regulatory debates is viewed as foundational to participatory government. As such, these provisions are often viewed as pro-democratic. Yet, while courts can interpret and apply balancing tests, they are not in a position to address broader policy implications of anti-SLAPP legislation. Because of the interaction of private claims with public acts, these laws have the potential to ripple throughout the economy, influencing behaviour outside of the courtroom. The contribution of this paper is to model and measure these ripples.

My focus is the prospective chilling effect of anti-SLAPP laws on construction investment and physical real estate development.[7] The phrase "chilling effect," especially when translated into legal rules, is frequently contested (Kendrick, 2012). Hence, I begin by modelling litigants' behaviour under two scenarios: with and without anti-SLAPP laws. The model offers a precise, if stylized, definition of chilling effects for a real estate developer. Importantly, by bringing SLAPPS within the purview of canonical models of frivolous lawsuits (e.g., Bone, 1997), the model highlights that prospective SLAPP problems are narrower than conventionally believed (e.g., Pring and Canan, 1996).

After modelling chilling effects, I estimate the economic implications of anti-SLAPP legislation on construction investment and new home starts in Canada. Measuring the effects of anti-SLAPP laws presents several empirical challenges. Principally, anti-SLAPP laws are not randomly assigned to jurisdictions. Interventions may be confounded with unobserved province-level factors such as the propensity to engage in litigation. To address this, my main research design is based on staggered adoption difference-in-differences assumptions. Using data for 35 municipalities and exploiting cross-provincial variation in the enactment of legislation, I find that anti-SLAPP laws reduce average city-level construction investment by an economically notable $60 million per month. This chilling effect on investment is driven by reduced physical construction

[7] Bradshaw (2021) describes how land development projects are a source of endless controversy, where groups interact to block, approve or stall projects. Indeed, she outlines how public stakeholders can impose increasing costs on developers, pushing otherwise profitable projects into negative expected values. As a result, real estate is viewed as a particularly important sector to evaluate the implications of SLAPPs.

of single family home and is concentrated in large cities. The results demonstrate that 120 fewer single family homes are started per month and less investment flows to commercial and industrial projects compared with a counterfactual scenario where anti-SLAPP legislation was not implemented. Declines in single family homes are partially offset by an increase in the number of multi-unit, apartment starts.

While anti-SLAPP legislation has captured the attention of legal scholars, economic literature on the topic is scant.[8] Hurley and Shogren (1997) and Hurley (1995) model SLAPPs as a game of asymmetric and incomplete information with endogenous timing. Yet, it is not obvious that SLAPPs involve asymmetric information and, indeed, there are persuasive arguments against promulgating anti-SLAPP laws based on asymmetric information problems. Courts have mechanisms for discovery and a wide array of tools to minimize asymmetries. Judges should therefore be reluctant to pursue early dismissal on the grounds of one side's informational advantage (notwithstanding the costs of the discovery process). More directly, Hurley (1995) and Hurley and Shogren (1997) avoid evaluating SLAPPs as an independent phenomenon. Rather they treat suits as instrumental, revealing an agent's type, overlooking many interesting legal and policy implications of anti-SLAPP rules.

To the best of my knowledge, no empirical estimates exist within the law and economics literature on anti-SLAPP laws. Despite this, economics has addressed chilling effects arising from rule changes. Antonovics and Sander (2013), for example, study alleged chilling effects of California's Proposition 209, a 1996 vote that prohibited California's UC system from basing admission decisions on race, sex or ethnicity. Employing a before-after design combined with data on all freshmen applicants between 1995 and 2000, they find, contrary to expectations, a thawing effect on minority college admission yield rates. Altindag et al. (2021) show how loss of immunity chilled the political activities of opposition Turkish Members of Parliament. Without immunity, opposition members drafted fewer pieces of legislation and curtailed criticism of the governing party. Applying a difference-in-differences design, Galasso and Luo (2022) study how increased upstream product liability chills innovation in the medical device industry. Knock-on liability claims caused upstream suppliers to foreclose input markets, avoiding the risk of future lawsuits, but also chilling new product development.

The next sections proceed as follows. Section 1 presents a model of SLAPPs. Section 2 describes the empirical methodology and data. The main results are in section 3. Section 4 concludes.

[8]SLAPPs are a category of frivolous lawsuits. Frivolous lawsuits have received substantial attention in the law and economics literature, with contributions from Bebchuk (1988, 1996); Cooter and Rubinfeld (1989); Katz (1990); Rosenberg and Shavell (1985) among many others. Much of this literature has focused on the ability to solicit settlement or the roles of cost shifting and court sanctions in deterring nuisance suits. Few papers empirically link the merits of a suit to a broader public policy dialogue, thus this analysis adds a new dimension to the literature on frivolous suits.

1 Chilling Effects of SLAPPs in a Model of Frivolous Lawsuits

Chilling effects have a long track record in policy debate. Yet, as an economic concept, defining what is being chilled and by how much is often difficult. In this paper, chilling effects emerge from a model of frivolous lawsuits, one that incorporates SLAPPs. Unfortunately, using frivolous lawsuits as a starting point poses its own challenge. This is because defining frivolous lawsuits has proved difficult.[9] Conventionally, frivolous lawsuits are classified as cases with a negative expected monetary value for the plaintiff (Bone, 2003; Miceli, 2009; Cooter and Ulen, 2008). While intuitive, this definition overlooks a critical access to justice function of the courts: often plaintiffs pursue cases for reasons other than monetary compensation (Kalajdzic, 2018). Establishing jurisprudence, for instance, is viewed as a meritorious motive, even in circumstances where monetary damages are small or unlikely to be recovered. Defamation cases, the most common form of anti-SLAPP claim, provide another example. In the course of a public proceeding, a plaintiff may believe that they have been libeled. Further, the plaintiff may have full information and know that defamation suits almost always fail (Bezanson, 1986; Young, 2017). Nonetheless, the plaintiff may pursue a case on non-monetary grounds in an effort to restore their reputation, perhaps by obtaining something as simple as an apology. Thus, defining all suits that yield negative expected values as frivolous is a restrictive. Notwithstanding important non-monetary motives, the remainder of this section keeps the discussion conceptually simple by adhering to the conventional definition. Frivolous cases are lawsuits with negative expected monetary payoffs.

This paper's empirical predictions come from a model with two players, stylized as a Developer and a Conservation Group. Figure 1 illustrates their interactions in an extensive form game of symmetric, complete but imperfect information. This game contains five prospective outcomes and the model proceeds sequentially with two stages. The game starts after the Developer proposes a real estate project. In the first stage, the Conservation Group observes the proposal and decides whether to PROTEST or ACCOMMODATE. PROTEST involves fighting the development, while ACCOMMODATE entails letting the project proceed unopposed. In the second stage, if the Conservation Group plays PROTEST, the Developer moves, choosing whether to SUE the Conservation Group. Alternatively, it may choose DO NOT SUE and avoid the courtroom battle. The outcome from this game is determined by nature randomly selecting whether the real estate project is approved or not. The equilibrium is subgame perfect, solved backwards.

The game described by Figure 1 is analyzed under two states of the world. In one scenario, player strategies, equilibria and payoffs are evaluated in the game as presented in Figure 1. This is referred to as the business-as-usual (BAU) state of the world, one where anti-SLAPP laws are not enacted. The second scenario is the anti-SLAPP environment. Anti-SLAPP laws operate by restricting the set of actions – and hence strategies – available to the Developer. Anti-SLAPP

[9] Indeed, Miceli (2009, pg.181) argues that "it is virtually impossible to offer data on ... suits that are in fact frivolous".

laws eliminate the SUE action, forcing the Developer to play DO NOT SUE. Chilling effects are defined by comparing equilibrium strategies in the BAU to the anti-SLAPP versions of the model. The theory has the advantage of mirroring the paper's difference-in-differences research design which compares counterfactuals without anti-SLAPP laws (i.e., BAU states) to those that introduce these laws during the period of analysis.

Two initial points should be addressed at the outset. First, the plaintiff's payoffs are assumed to be primarily related to the project, not the purported harm propagated by the tort-feasor. The exposition uses an example where the Conservation Group defames the Developer, potentially harming its reputation. The claim is brought because this defamation, but the suit is only a SLAPP because the developer's payoffs depend on both the remedy for the (potential) defamation and the outcome of the project. By definition, SLAPPs are the product of a complementarity between reducing opposition to a project and the potential harm inflicted by the defendant. Indeed, this is how and why SLAPPs are distinct from other nuisance lawsuits. If the defamation suit is bought, but has no influence on the project, then it is dubious to classify the suit as a SLAPP; instead, it is a conventional claim, even if a frivolous one. To restate, SLAPPs occur because of the complementarity between the legal proceeding and outcomes related to, but independent from, that proceeding. The spillover to outside projects is the characteristic that differentiates SLAPP from vanilla lawsuits.

Second, the chilling effects of anti-SLAPP statutes exist because of the complementarity *and* because the new law changes equilibrium play. Both public expression and the developer's decision to pursue projects are the objects influenced by anti-SLAPP laws. The suits themselves are secondary. In fact, I do not empirically study SLAPPs directly. I focus on how anti-SLAPP laws change the incentives to develop real estate projects *relative to a counterfactual scenario where the laws did not exist*. It is the change in equilibrium play by the defendant Conservation Group and plaintiff Real Estate Developer that yields testable predictions.

As stated, the model is solved backwards and I consider two subgames labelled I and II in Figure 1.[10] These are reviewed, respectively, in sections 1.1 and 1.2. There are two chilling effects. The first is on public expression in the BAU scenario. The second is on project development in the anti-SLAPP scenario. I start by presenting the plaintiff's decision to file suit, recognizing the complementarity between the real estate project's payoff and the potential chilling effect of choosing to SUE. It is the plaintiff's decision to issue a claim that influences a defendant's choice of whether to protest the project. If SLAPPs have any meaningful effect, anti-SLAPP legislation should thaw public discourse – or, at least, present a more meaningful threat of protest.[11] Section 1.3 relates the model and anti-SLAPP laws to the rules of civil procedure.

[10] Payoffs in Figure 1 refer to the real estate project only (i.e., costs are net of defamation awards).

[11] That is, observable protests and SLAPPs are merely necessary, not sufficient, for there to be a measureable chilling effect. A mere threat of protests, or of SLAPPs, can change behaviour, regardless of whether actual protests or lawsuits are observable in the data.

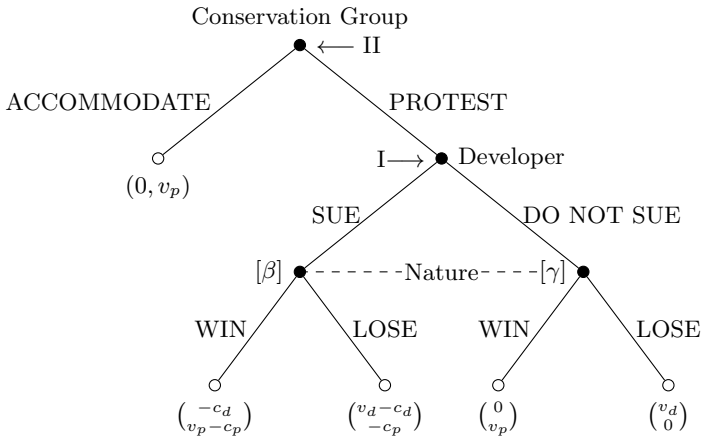

Figure 1: Game Tree for Interactions between the Developer and Conservation Group

1.1 Developer's Decision to File a SLAPP Suit (Subgame I)

A plaintiff Developer's decision to file suit is subgame I in Figure 1. This node shows the plaintiff's decision depends on two factors. First, the Developer's lawsuit hinges on the alleged harm from the Conservation Group's defamation (or similar tort), which may, for example, include damage to reputation. Suing exclusively over damage to reputations is a vanilla defamation claim, one covered by conventional models of civil procedure. Suing only based on defamation also does not make the case a SLAPP. The case becomes a SLAPP once the implications of the Conservation Group's public actions, actions for which the defamation suit is filed, interact with the real estate project. SLAPPs are defined by the complementarity between the payoffs of the external real estate project and the alleged harm from the defamation. Both motivations, the alleged harm plus the spillover to the project, factor into the plaintiff's strategy.

Because the plaintiff's strategy depends on two elements, I outline each and then describe how anti-SLAPP laws alter the actions available to the Developer. I start by looking at the plaintiff's decision to file suit based on the purported defamation. Following this, I introduce the SLAPP dimension of the game. Finally, these elements are combined providing the complete strategy for the plaintiff.

First, the Developer alleges it was defamed by the Conservation Group. The Developer, as plaintiff, must decide whether to seek a legal remedy for this alleged harm. It bases its decision to file suit on a rule with three terms (Miceli, 2009). The first term captures the costs of filing a claim, c_p. (Lower case index "p" refers to the Developer "plaintiff", while lower case "d" is for the defendant Conservation Group.) These costs include all legal and court costs associated with the case.[12] Second is the claimed damages, D, inflicted by the defendant on the plaintiff. Finally,

[12]Some models decompose these costs into fixed and variable portions to explore the roles of timing and sunk costs on a plaintiff's attempt to extract settlement from a defendant (e.g., Rosenberg and Shavell, 1985).

there is the probability, α, that the plaintiff's suit is successful. Uncertainty, even with symmetric information, arises from the prospect of legal error or idiosyncratic judge-specific shocks (Kaplow and Shavell, 1994; Kaplow, 1994). Combining the three terms, a rational plaintiff files a suit on defamation grounds if the expected value of the suit is positive:

$$\alpha D - c_p > 0 \tag{1}$$

where the payoff for the plaintiff from not pursuing the claim is set to zero. Critically, the decision to file suit, reflected in (1), is not associated with the economics of SLAPPs. Damages D and probability of success α represent the parameters of a conventional civil suit. Missing is the interaction between the defamation case and the public opposition to the real estate project that defines a SLAPP.

The Developer only arrives at its decision node if the Conservation Group, whose strategies are discussed below, chooses PROTEST in subgame II. In the BAU state, a developer chooses between two actions. It must decide between SUE and DO NOT SUE. SUE refers to filing a lawsuit with respect to the alleged defamation. It incorporates the terms in (1), but also the spillover to the real estate project. DO NOT SUE means no claim for defamation is made in response to the Conservation Group's protest.

Let β represent the probability that the Developer's real estate project proceeds given it chooses to SUE. γ represents the probability the project proceeds when the Developer chooses DO NOT SUE. Assume that $\beta \geq \gamma$. For the claim to be considered a SLAPP, there must be some interaction between the defamation claim and the real estate project (otherwise, the scenario reduces to one of conventional litigation). Moreover, the Developer's decision to file suit must (weakly) increase the probability that the project will be approved. Therefore, for SLAPPs to be a meaningful concept in a game of complete information, β must be at least as big as γ.[13] To make this point clearer, consider the value of the lawsuit for the real estate project, exclusive of the defamation. The value of the project is denoted v_p. The Developer's *defamation-exclusive* decision-rule to SUE is given by the condition:

$$(\beta - \gamma)v_p - c_p > 0 \tag{2}$$

Equation (2) says that the Developer should SUE if the expected increase in the value of the real estate project exceeds the costs of litigation (net of any defamation award). The increase in the project's approval is governed by the difference in approval probabilities $(\beta - \gamma)$, where β is the project's probability of success with SUE and γ is the probability of success with DO NOT SUE.

[13] Even if the real estate project was independent of the alleged defamation, the Developer may still file suit. In this case, because the lawsuit is independent of the project's expected outcome, the case should not be classified as a SLAPP as there are no chilling effects on public participation. Rather, it is a vanilla defamation claim.

The next step connects the defamation claim with the effect on the real estate project. Figure 2 shows the Developer's strategy including the complementarity between the real estate project and the alleged harm. The vertical axis of Figure 2 represents the expected payout to the Developer from its defamation suit. The horizontal axis is the probability that the Conservation Group will be found liable for defamation. Two lines represent the Developer's strategies in an environment with and without anti-SLAPP legislation.[14,15]

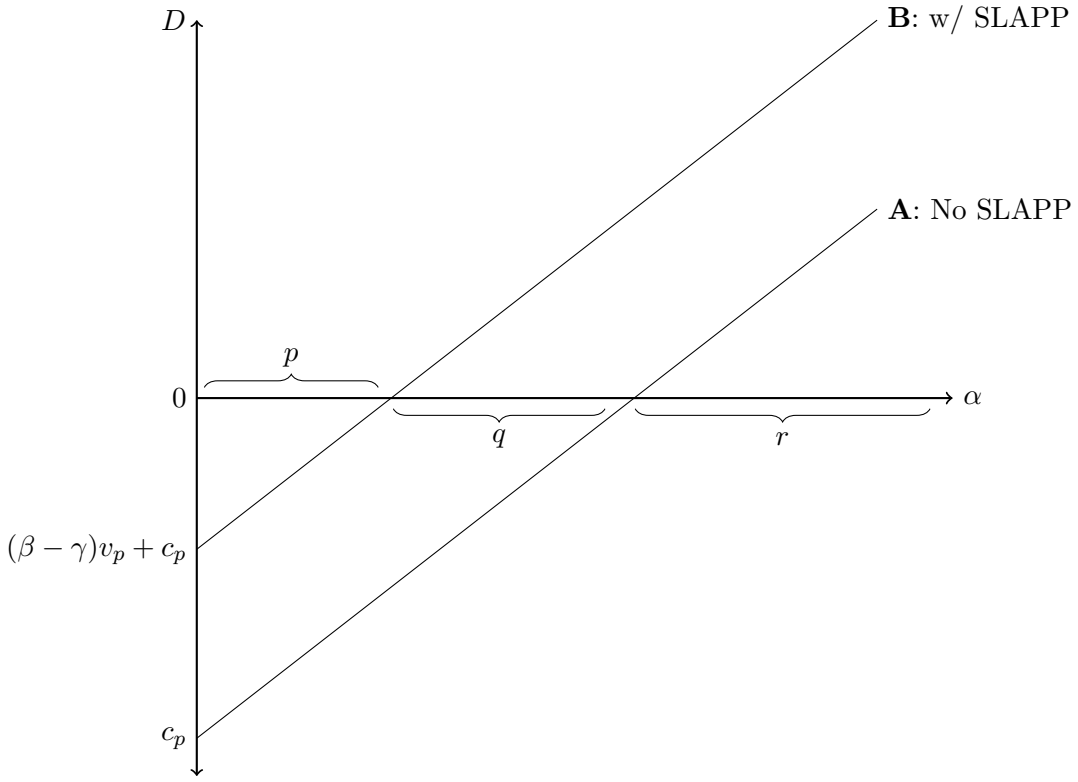

Figure 2: Illustration of a Plaintiff's Subgame Decision to File a Claim

The curve labelled **A** represents the plaintiff's decision to file a defamation suit, ignoring complementarities. That is, curve **A** represents the decision rule for a defamation claim on its merits as shown in (1). Litigation is costly. If either α, the probability that the defamation suit

[14]A completely frivolous suit implies either $\alpha = 0$ or $D = 0$ and, hence, the decision to file suit is a horizontal curve at $(\beta - \gamma)v_p + c_p$.

[15]It is straightforward to add features to Figure 2 to, say, explore various cost allocation rules or court sanctions. For example, consider the difference between the so-called "American Rule", illustrated in the main text, where all parties pay their own costs, and the "English Rule", where the losing party pays both the plaintiff's and defendant's costs. Switching from the American to the English rule has two main effects. First, it shifts the intercept downward. In Figure 2, the intercept for curve **A** is given by c_p, representing the plaintiff's legal costs. Under the English rule, this equals the sum of plaintiff and defendant costs, $c_p + c_d$. Second, the slope of the curve becomes steeper, because the the plaintiff's payoff increases with her probability of success. As the probability of success approaches one, expected legal costs approach zero.

is successful, or D, the payoff from the defamation suit, are small relative to costs, c_p, then the case is "frivolous" because it has a negative expected value. The point where curve **A** crosses the horizontal axis defines a threshold, delineating meritorious from frivolous suits. Cases to the left of this point are frivolous and should not be filed (or should be dismissed at an early stage). Suits to the right are credible, because the Developer's expected recovery is positive.

The second line in Figure 2, curve **B**, represents the plaitiff's decision rule including a complementarity with the real estate project. The combined decision rule gives the Developer's strategy in the BAU environment. Under BAU, the Developer's strategy is play:

$$\begin{cases} \text{SUE} & \text{if} \quad \alpha D + (\beta - \gamma) v_p - c_p > 0 \\ \text{DO NOT SUE} & \text{otherwise} \end{cases} \tag{3}$$

The complementarity between the defamation suit and the real estate project reduces the net costs of filing a lawsuit. Stated differently, filing a suit against the Conservation Group increases the likelihood that the real estate project will be approved. This, in turn, increases the incentive to sue, holding α and D fixed. Indeed, if $(\beta - \gamma) v_p > c_p$, the Developer will always file a lawsuit, even if there is no compensable defamation (i.e., either $\alpha = 0$ or $D = 0$).

Comparing curves **A** and **B** in Figure 2 illustrates the goals of anti-SLAPP legislation. In Figure 2, the BAU scenarios shows that SLAPPs lead to a vertical shift in expected value of the lawsuit curve from **A** to **B**. The point where **B** cuts the horizontal axis defines the threshold given by (3). Suits to the left of this threshold lead to DO NOT SUE. Suits to the right yield SUE. Anti-SLAPP laws restrict the Developer's strategies, by eliminating the SUE action. Anti-SLAPP laws constrain the Developer to curve **A**.

Anti-SLAPP laws decrease the density of lawsuits filed by an amount represented by area q in Figure 2. To understand why, it is useful to characterize three regions of the probability distribution, defined by the two thresholds where the curves **A** and **B** cross the horizontal axis. First, consider region r and a prospective suit to the right of the point where **A** crosses the horizontal axis. This case has a positive expected payoff for the plaintiff irrespective of the complementary with the real estate project. A plaintiff will always pursue this case wholly on its merits. Likewise, cases in region p, shown to the left of where curve **B** cuts the horizontal axis, have negative expected values. These claims are never pursued, even after incorporating interactions. Only claims in region q, between curves **A** and **B**, defamation claims that are frivolous on their merits but have a positive expected payoff because of their complementarity with the real estate project can be defined as SLAPPs. The horizontal distance q is the increase in nuisance suits that arise from the interaction of the alleged defamation damages and the project. It is these suits that supply the *necessary conditions* for a chilling effect on public participation.

Unambiguously defining the conditions under which SLAPPs exist has proved elusive in many

legislative discussions (e.g., Ontario, 2010). Routinely, SLAPPs are taken to be any lawsuit brought by a business against citizens. Figure 2 shows that this characterization is too broad. SLAPPs are cases for which a defamation award (or other tort) is too small on it merits to warrant court proceedings, but one where, after incorporating the change in the expected value of a complementary project, a lawsuit yields a positive net expected value for the plaintiff. SLAPPs are constrained to region q in Figure 2. Defamation claims falling into region r are not SLAPPs as these cases would proceed on their merits even in a but-for scenario with no change in project approval probabilities. Similarly, there are disputes, as shown in area p, with protests and perceived defamation, but no lawsuits.

Anti-SLAPP laws chill real estate development because they limit the plaintiff's choices with respect to alleged defamation. Restricting the Developer's strategies with respect to lawsuits, in turn, results in a lower probability that proposed real estate projects are approved. In Figure 1, anti-SLAPP laws reduce (i.e., chill) observed real estate investment by $(\gamma - \beta)v_p$, an amount proportional to the difference in project approval probabilities. Whether this magnitude is economically meaningful is an empirical question, one that is studied in section 3; however, it is possible to provide some initial intuition. Consider average monthly construction investment in the City of Montreal, Quebec. Between 2011 and 2021, more than $1.4 billion was spent on new construction in the city. Assuming a 5% reduction in project approval probabilities implies a chilling effect on real estate investment equal to $70 million. Similarly, the Pointes Protection project included 91 lots valued at approximately $400,000 per lot (see Appendix B for a map of the project). Halting this project yielded greater than $35 million in foregone development.

1.2 Chilling Effect of SLAPPs on Public Expression (Subgame II)

Working backward, the Conservation Group's strategy for whether to protest or accommodate a proposed real estate project depends on whether anti-SLAPP legislation exists. Subgame II illustrates the choices and stylized payoffs at the Conservation Group's decision node.

The available actions for the Conservation Group are: ACCOMMODATE and PROTEST. If the Conservation Group chooses ACCOMMODATE, their payoff, as illustrated in Figure 1, is set to zero and the project is guaranteed to proceed.

To start, consider an environment that has enacted anti-SLAPP rules. Developers, in this scenario, are forced to play DO NOT SUE. Given the Developer's strategy, the Conservation Group does not incur extraneous costs from meritless civil litigation, irrespective of their action. The Conservation Group's expected payoff from playing PROTEST is $(1 - \gamma)v_d > 0$ (i.e., the Developer "loses", with probability $(1 - \gamma)$). Therefore, when anti-SLAPP laws exist, the Conservation Group's pure strategy is to always play PROTEST.[16] The subgame's equilibrium, in the

[16]Even if the Developer wins in the anti-SLAPP state of the world, or, alternatively, as $\gamma \to 1$, the Conservation Group has not incurred costs, so they are indifferent between accommodating and protesting.

anti-SLAPP scenario, is, therefore, the Conservation Group plays PROTEST, obtaining payoffs of $(1-\gamma)v_d$, while the Developer plays DO NOT SUE, receiving an expected payoff of γv_p.

In the BAU environment, the Conservation Group's strategy must consider the both actions available to the Developer. To start, if the Developer plays DO NOT SUE, then the Conservation Group's strategy is identical to the scenario with anti-SLAPP rules. They should play PROTEST, obtaining a payoff of $(1-\gamma)v_d$.

If the Developer plays SUE, then the Conservation Group should play:

$$\begin{cases} \text{PROTEST} & \text{if} \quad -\alpha D + (1-\beta)v_d - c_d > 0 \\ \text{ACCOMMODATE} & \text{otherwise} \end{cases} \tag{4}$$

In words, the Conservation Group plays PROTEST if the expected benefits from halting the real estate project exceed the expected defamation penalty plus legal expenses: $(1-\beta)v_d > \alpha D + c_d$.

All strategies in the BAU setting generate equilibria in pure strategies. But, unlike in the anti-SLAPP scenario, the equilibria of BAU environment depend on the magnitudes of the parameters. Equation (3) shows that the Developer will play SUE if $\alpha D + (\beta - \gamma)v_p - c_p > 0$. Given this, there are three cases to evaluate. (To show the prospective equilibria as simply as possible, define c_d as net of defamation damages costs.[17])

Begin with the simple case. Assume that $(\beta - \gamma)v_p < c_p$. In this case, the Developer plays DO NOT SUE and the Conservation Group plays PROTEST. Payoffs are $\{\gamma v_p, (1-\gamma)v_d\}$, respectively, for the Developer and Conservation Group.

Next, in the second case, let $(\beta - \gamma)v_p > c_p$, so the Developer plays SUE in subgame I. The Conservation Group's action depends on a comparison of its legal costs versus the expected value of halting the project. If $(1-\beta)v_d < c_d$, the the Conservation Group will play ACCOMMODATE and equilibrium payoffs are $\{v_p, 0\}$.

Finally, for the third case, let $(\beta - \gamma)v_p > c_p$, so the Developer plays SUE in subgame I but $(1-\beta)v_d > c_d$. On this equilibrium path, the Conservation Group plays PROTEST, the Developer SUEs and expected payoffs are $\{\beta v_p - c_p, (1-\beta)v_d - c_d)\}$.

Crucially, the chilling effects of SLAPPs on public participation depend on which equilibrium manifests. Even with its simplicity, the model delivers clear intuition for the scope of SLAPPs and their observable effects on economic outcomes. Table 1 summarizes the predictions. First, when anti-SLAPP laws exist, the Conservation Group always plays PROTEST. Prospective chilling effects evince by comparing this scenario to the three more nuanced BAU equilibria as shown in the bottom rows of Table 1. Under BAU, the Conservation Group's choice of action depends

[17]Recall that αD represents the expected damages that the Conservation Group needs to pay the Developer for the alleged defamation. Most defamation claims fail, so α is likely small. Further, much of the SLAPP literature assumes minimal monetary damages for defamation, implying that D is also small. Nonetheless, the Conservation Group may be held liable if a judge finds the protest to be injurious to the Developer's reputation (or other tort).

on a comparison of the costs and benefits of protesting. In the first case, the Developer plays DO NOT SLAPP and the Conservation Group PROTEST. This equilibrium is represented by region p in Figure 2. No chilling of public expression materializes because there is no credible threat of legal action. Next, the second case shows an equilibrium where public debate is chilled in BAU (thawed in the anti-SLAPP scenario). In this equilibrium, the Conservation Group's strategy is to play ACCOMMODATE, and not voice opposition to the project, because the prospect of fighting the defamation suit is more costly than the benefits of preventing the development. Because the Conservation Group acquiesces in the face of credible legal action, there is a chilling effect on participation in public debate. This situation, where the Conservation Group remains silent due to the threat of legal action, is what anti-SLAPP laws are designed to mitigate. Finally, in the third equilibrium, public participation proceeds and there is no chilling effect because the Conservation Group plays PROTEST. Importantly, however, even though public expression is not chilled in this equilibrium, the Conservation Group is forced to engage in costly litigation and their equilibrium payoffs are smaller than in the anti-SLAPP scenario. Likewise, anti-SLAPP laws do not chill real estate investment in this equilibrium because the laws have no effect on the Developer's equilibrium strategy.

	Anti-SLAPP Scenario	*Potential Equilibria under Business as Usual*		
Equilibrium Strategy				
Conservation Group	PROTEST	PROTEST	ACCOMMODATE	PROTEST
Developer	DO NOT SUE	DO NOT SUE	SUE	SUE
Payoffs				
Public expression	$(1-\gamma)v_d$	$(1-\gamma)v_d$	0	$(1-\beta)v_d - c_d$
Real estate development	γv_p	γv_p	βv_p	$\beta v_p - c_p$
Change caused by enacting anti-SLAPP laws:				
Public expression		No change	Thawing	No change
Real estate development		No change	Chilling	No change

Table 1: Summary of Chilling Effects due to Enacting anti-SLAPP Laws

Straightforward predictions come from a standard model of litigant behaviour that includes SLAPPs. The model predicts that switching from a BAU to an anti-SLAPP scenario chills real estate investment and thaws public expression. Yet, both effects are proscribed to specific equilibria that depend on the magnitudes of the game's parameters. The extent to which these laws matter for economic outcomes, therefore, is an empirical question.[18]

[18]Empirically consistent generalizations could also be added to the model. For example, a proposal stage could be included prior to the Conservation Group's decision node. If sunk costs are required to propose a project, then a Conservation Group's threat of protest may be sufficient to chill real estate investment, even if protests are unobserved. That is, if the threat of protests is sufficiently credible, switching from a BAU to anti-SLAPP scenario may reduce real estate investment while yielding no change (or even a reduction) in observed public expression.

1.3 Anti-SLAPP Laws and Codes of Civil Procedure

Several additional points require comment as the model presented in Figure 1 necessarily simplifies important features of civil procedure.

First, factual or legal ambiguity, judicial conservatism or the prospect for legal error are required for SLAPPs – or any lawsuit – to exist. Some uncertainty about how a judge (or regulator) will determine an outcome must be involved, otherwise both the Developer and Conservation Group would immediately settle. This is true for both anti-SLAPP and BAU scenarios. Because of this uncertainty, each player requires some subjective probability distribution over potential outcomes. In other words, both players have incomplete information with respect to how the court will treat the plaintiff's defamation suit. (All suits would be foregone conclusions without this uncertainty.) As with the definition of frivolous suits, there is disagreement on the precise causes for incomplete information (e.g., differing perceptions, legal error, etc.). I avoid taking a position and merely accept some case-specific randomness. More directly, Figure 1 adopts a standard trick for this class of game (Harsanyi, 1967). Figure 1 does not illustrate a game of incomplete information. Rather, it shows an extensive form game of complete but imperfect information by introducing Nature as a player that randomly determines the outcome of both the real estate project (as illustrated by β and γ in Figure 1) and, correspondingly, the defamation suit (as shown with α in Figure 2). The purpose of this analysis is to compare scenarios with and without anti-SLAPP laws, so these modelling short-cuts are viewed as innocuous.

Second, this set-up relies a second implicit assumption, namely that, after passing anti-SLAPP legislation, courts are able and willing to distinguish between prospective SLAPPs and otherwise legitimate tort claims. Prior to enacting anti-SLAPP laws, the large majority of codes of civil procedure already contained provisions for the early dismissal of frivolous lawsuits. As an example, the Canadian Province of Ontario's Rules of Civil Procedure (O. Reg. 575/07, 2020) contain rule 2.1.01 (1) that states: "The court may, on its own initiative, stay or dismiss a proceeding if the proceeding appears on its face to be frivolous or vexatious or otherwise an abuse of the process of the court."[19] Independent of whether anti-SLAPP statutes exists, judges are expected to evaluate claims on their merits, ignoring irrelevant facts. An absence of anti-SLAPP laws in BAU does not grant frivolous suits free-pass as pre-existing rules *should require* judges to evaluate the case facts based on curve **A** in Figure 2.

This redundancy of anti-SLAPP legislation is the most persuasive counter-argument against anti-SLAPP laws. It is also the most convincing reason to think these rules matter. Cases in region q in Figure 2, as an example, are frivolous according to the conventional definition. Judges already have tools for their early dismissal if dismissal is warranted. Therefore, the argument is, anti-SLAPP laws should not offer additional scope for lawsuits to credibly chill public expression

[19] Appendix A also reviews the two-part legal test, used to appraise anti-SLAPP motions, as articulated in the Supreme Court's Pointes Protection decision.

and additional rules, if enacted, should not change courtroom outcomes. Anti-SLAPP rules, according to their opponents, are purely symbolic.

Despite this critique, advocates continue to lobby for the inclusion of anti-SLAPP rules in codes of civil procedure. In fact, prior to enacting its law, the Province of Ontario convened a panel to evaluate the merits of proposed anti-SLAPP legislation. Among received comments, "most of the submissions (27 out of 31) supported the introduction of special legislation against SLAPPs" (Ontario, 2010, at para. 6). Moreover, as the Pointes Protection example highlights, courts appear to have internalized tests of public participation and are paying greater attention to extra-case facts when determining whether early dismissal is warranted. This suggests that anti-SLAPP legislation does influence court behaviour – and likely portends a harsher overall policy and regulatory environment for real estate development, one the goes beyond courtroom manoeuvring. Debate over the merits of legislation hinges on how courts and zoning authorities respond to anti-SLAPP rules. The empirical question in this research is on their effect on real estate development. Yet, this variable is connected to anti-SLAPP laws via a series of behaviours, the mechanism through which these rules influence economic outcomes. Some real or perceived change in judicial decision-making or shift in investor expectations, as examples, are needed for their to be an observed effect on economic outcomes.

Finally, anti-SLAPP laws may do little to encourage public expression if it is the threat of legal action, rather than the act of filing a suit, that quells opposition. Cheap talk about prospective legal action may be sufficient, even if the threat is not credible (i.e., actions on an off equilibrium path). Uncertainty due to legal error means that meritless threats may mute some opposition and chill public debate, even if it is mere posturing. Of course, the counter-argument equally applies: many government and regulatory bodies provide explicit and protected opportunities for public consultation. Because public debate is has extra protection in these venues, anti-SLAPP motions only add legal costs to plaintiffs' already expensive processes. Moreover, defamation law already involves a balance between freedom of expression and protection of reputation. Any legislation that protects "expression under the name of public participation would distort that balance and create undue harm to reputation" (Ontario, 2010, at para. 11).

2 Research Design

Anti-SLAPP laws were introduced at staggered intervals across jurisdictions. This differential timing alongside a parallel trends assumption is exploited to estimate chillings effects via a staggered adoption design. Because of the differential timing of the enactment of anti-SLAPP statutes, conventional two-way fixed effects estimators can yield negative weights for observations, diluting treatment effects and making interpretation difficult (Goodman-Bacon, 2021; Borusyak et al., 2021). Staggered adoption designs adjust for observations that are treated at different times and

for different durations. Specifically, I apply Gardner's (2021) two-stage difference-in-differences model as my main approach. Section 2.1 describes the main econometric specification. Section 2.2 reviews the data.

2.1 Econometric Models

Identifying variation for the difference-in-differences estimate of anti-SLAPP laws on construction investment comes from the differential timing of the laws' implementation across Canadian provinces. To address interpretation issues that arise when two-way fixed effects models are combined with staggered timing of treatment exposure, I use the estimator developed by Gardner (2021) (see also, Butts and Gardner, 2021).[20]

Gardner's (2021) method invokes the Frisch-Waugh-Lovell Theorem and the common trends assumption. Gardner's estimator involves two stages. First, excluding the treatment variable, regress the outcome variable (construction investment, as an example) on unit- and time-specific parameters (and any additional covariates). This first-stage regression includes all never-treated units – provinces that never enacted anti-SLAPP laws – plus not-yet treated units – jurisdictions that will, but have yet to, implement the rules. Using the estimates from this first-stage, it is possible to construct an "adjusted" dependent variable for both control and treatment units that is purged of the differential timing problem. By applying the Frish-Waugh-Lovell Theorem, the problematic correlation between the treatment dummy and the fixed effects has been partialled out in the construction of the adjusted dependent variable. This adjusted dependent variable can then be regressed, in the second stage, on the treatment variable to recover the parameter of interest, the population-averaged treatment effect on the treated of anti-SLAPP laws.

The parallel trends assumption facilities this procedure. Absent treatment, under parallel trends, treated units should experience the same evolution as untreated units. Gardner (2021) demonstrates that, because untreated potential outcomes are linear in group and period effects, the Frisch-Waugh-Lovell Theorem can be invoked to partial out these factors. It is through formulating the first-stage that the sample selection problem, where the heterogeneous treatment

[20]To get intuition for why this is important, consider a simplified estimator of the average treatment effect on the treated (ATT). ATTs are simply weighted comparisons of units. Goodman-Bacon (2021) demonstrated how the conventional two-way fixed effect difference-in-difference ATT actually recovers a parameter that involves weighted averages of three different comparisons. First, treated units are differenced from never treated units. This comparison is valid and accords with the standard intuition of difference-in-differences. Next, treated units are differenced from yet-to-be-treated units. Again, this comparison matches what researchers are attempting to recover, the effect of switching the treatment on. Finally, the standard two-way fixed effect estimator subtracts (i.e., differences) a weighted average of earlier treated units from later treated units. This final difference is odd and involves applying a negative weight to treated observations. Because later treated units are subtracted in the calculation of the ATT, the true treatment effect is diluted by an unknown amount if there are heterogenous responses over time. Another way to state this is, we do not know whether the conditional parallel trends assumption holds when comparing late versus early treated units, precisely because, when there is the prospect for a heterogeneous treatment effect, we do not know how much the treatment affected the units. That is, there may be bias due to heterogeneity in time when comparing early to late treated units (Cunningham, 2021).

effect is correlated with the fixed effects, is avoided and the second-stage recovers the parameter of interest.

More formally, the two-staged difference-in-differences procedure starts with the following first-stage regression:

$$y_{it}(0) = \lambda_i + \tau_t + \varepsilon_{it}$$

where λ_i is the group-specific fixed effect that controls for group-specific, time invariant unobservables and τ_t is the time fixed effect, capturing shocks common to all units at a particular time. $y_{it}(0)$ is the dependent variable for the untreated obsevations. The estimates for the fixed effects, $\hat{\lambda}_i$ and $\hat{\tau}_t$, are then used to impute the missing untreated outcome:

$$\tilde{y}_{it} = y_{it} - \hat{\lambda}_i - \hat{\tau}_t$$

where y_{it} is the observed outcome (e.g., real estate investment) and $\tilde{y}_{it}$ is the adjusted dependent variable. The second-stage then regresses $\tilde{y}_{it}$ on the variable of interest:

$$\tilde{y}_{it} = \alpha_1 + \delta_{2SDD} D_{it} + u_{it} \tag{5}$$

where D_{it} takes a value of one if jurisdiction i has anti-SLAPP legislation in period t and zero otherwise (i.e., $D_{it} = \mathbb{1}\{\text{anti-SLAPP}_{it}\}$). δ_{2SDD} is the parameter of interest, the two-stage difference-in-differences estimate of the chilling effect of anti-SLAPP laws. δ_{2SDD} can be interpreted as the average treatment effect on the treated. Gardner (2021) and Butts and Gardner (2021) describe a general method of moments procedure which recovers the correct standard errors as $\tilde{y}_{it}$ is an estimate. For each specification, I also calculate randomization p-values by simulating a series of placebo models.

The identifying assumption for two-staged difference-in-differences is, in the absence of anti-SLAPP laws, construction investment for treated and untreated units would follow the same trend. Threats to identification come from simultaneous province-month shocks that are coincident with the enactment of anti-SLAPP laws. This same identifying assumption is invoked in conventional difference-in-differences designs. I probe the validity of this assumption by using the event study estimators developed by Callaway and Sant'Anna (2021) and De Chaisemartin and D'Haultfœuille (2020). Appendix C also applies Dettmann et al.'s (2020) variant of the semi-parametric method of Heckman et al. (1997, 1998), invoking a selection-on-observables assumption.[21]

[21] Similar to Callaway and Sant'Anna (2021) and Sun and Abraham (2021), Heckman et al. (1997, 1998) is an aggregation difference-in-difference estimator. In essence, a series of group-by-time treatment effects are estimated and then aggregated according to some weighting. Matching plus difference-in-differences combines the conditional independence assumption with parallel trends, so the underlying causal assumption is "conditional parallel trends"

2.2 Data

Anti-SLAPP laws are studied using city-month observations on construction investment and housing starts as the variables of interest. Data on new Canadian construction investment were retrieved from Statistics Canada for 2011-2021 (Statistics Canada, 2021). These are monthly data, measured in Canadian dollars, for 35 Canadian census metropolitan areas (CMA), referred to as cities. Information on physical housing starts is obtained from the Canadian Mortgage and Housing Corporation (CMHC) for 1990-2021.[22] Appendix Figure B.1 provides an example of a typical Canadian CMA. CMAs are large geographical regions usually comprised of several independent municipalities. The example in the appendix shows the CMA of London, Ontario. This CMA is comprised of one mid-sized Canadian city (i.e, London), two smaller cities plus several rural municipalities. Importantly, zoning responsibility and local development regulations vary across governments within CMAs as well as across CMAs.

Total construction investment is used as aggregate within city investment. Investment is categorized as residential, industrial, commercial or institutional construction. The latter three categories are combined into a non-residential variable. For both residential and non-residential, investment is further narrowed into new, greenfield expenditure, representing approximately half of total investment. New investment excludes funds allocated to renovation and maintenance. My main focus is on total investment and, especially, new residential expenditures as these are the variables most closely associated with SLAPPs. As an example, the Pointe Estates, the development associated with the *Pointes Protection* decision, was proposed as a new residential neighbourhood. (A map of this project is illustrated in Appendix Figure B.2.) As with new versus maintenance investment, the housing starts data are comprised of two series, single family units and apartments, where apartments includes row housing. These data are for, roughly, the same set of cities as the investment data. Finally, data on population and employment are also retrieved from Statistics Canada.

Anti-SLAPP laws exist in the Canadian provinces of Quebec, Ontario and British Columbia. Quebec's law received assent in 2009, so, while treatment turns on for all cities in the housing starts data, there is no variation in treatment status for the Quebec cities in the investment data. In other words, the cities of Gatineau, Montrèal, Quèbec City, Saguenay, Sherbrooke and Trois-Rivièes are "always treated" units in the investment analysis. (Because of the housing starts in-

(Callaway and Sant'Anna, 2021). Common support and irreversibility of treatment are also required (Dettmann et al., 2020; Callaway and Sant'Anna, 2021). The two estimators, the primary two-staged difference-in-differences (Gardner, 2021) and the group aggregation estimator (Dettmann et al., 2020; Heckman et al., 1997, 1998), make slightly different comparisons and recover distinct estimands. They are, therefore, not directly comparable. Conditional on satisfying the identifying assumptions, both do, however, recover the causal effect of anti-SLAPP laws on the outcome.

[22] CMHC is a state-owned company (crown corporation) that reports to the Canadian Parliament and receives its mandate from the federal government. The CMHC "exists for a single reason: to make housing affordable for everyone in Canada" (CMHC, 2022b).

formation begins well before the law's implementation, this is not an issue for the CMHC series.) Ontario's anti-SLAPP laws came into effect in January 2016, while British Columbia's legislation passed in March 2019. Ultimately, 18 out of 35 cities experienced a change in their treatment status during the sample period of the investment data, while 23 out of 34 were treated in the housing starts data. British Columbia's legislation is widely viewed as a replica of Ontario's.[23]

Table 2 contains summary statistics for the main variables. Average monthly construction investment in Canadian cities equals $282 million, but the range is wide, spanning from $3.8 million to $3.4 billion. This wide range is echoed in the population data. The smallest city in the data has approximately 54,000 inhabitants, two orders of magnitude smaller than, Toronto, Canada's largest, which has over 5.5 million residents.

Average new residential construction investment equals $97.6 million per city-month. The average city sees approximately 137 new homes started per month, while work commences on almost 150 apartment units per month. Dividing average monthly residential construction by average monthly starts (houses plus apartments), single family plus apartments, gives a sense of the average cost of a new Canadian residential unit. This equals $343,179. Finally, average new non-residential investment equals $45.6 million per month.

	Avg	StdDev	Min	Max
Construction investment ($000,000)	282.4	518.6	3.8	3,436.7
New residential investment ($000,000)	97.6	177.2	0.3	1,395.2
New non-residential investment ($000,000)	45.6	80.0	1.2	500.0
Single family housing starts	136.5	227.6	0	2,524
Apartment unit starts	147.9	381.5	0	5,043
Population (000)	615.8	998.2	54.3	5,540.9
Unemployment rate (%)	6.7	1.9	1.8	16.7

Table 2: Summary Statistics for Construction Investment and Housing Starts

3 Empirical Results

Results on the effect of anti-SLAPP laws on construction investment are presented in section 3.1. Estimates for housing starts are in section 3.2. Section 3.3 summarizes several extensions and robustness checks with results presented in the Appendix.

[23] In fact, a Supreme Court of British Columbia decision, in *Cheema v. Young* (2021 BCSC 461), applied the Supreme Court of Canada's reasoning in *Pointes Protection*, even though the *Pointes Protection* case was based on Ontario's statute.

3.1 Chilling Effect on Construction Investment

Table 3 shows that anti-SLAPP laws chill construction investment. The point estimates are economically meaningful. Table 3 contains two columns. Columns (1) shows a regression that includes province- and time-specific indicators. Shocks that are province-specific but time-invariant, such as the stock of judges and the propensity to engage in litigation, and time-varying but common across provinces, such as the overall state of the Canadian economy, are captured by these fixed effects. Column (2) adds finer grained fixed effects, using interacted province-year fixed effects. Identifying variation in column (2) is within province-year controlling for other province-level changes that vary over time.[24]

Column (1) shows that enacting anti-SLAPP legislation leads to a $61 million reduction in real estate investment. To give a sense for this magnitude, the 60-40 interquartile range of the dependent variable equals roughly $65 million. Thus, implementing anti-SLAPP laws has an effect equivalent to a city moving from approximately 60th percentile of the investment distribution to the 40th percentile, a notable change. This coefficient is precisely estimated and has a randomization inference p-value equal to 0.02.[25] The corresponding estimate in column (2) is larger (in absolute value) suggesting that anti-SLAPP laws reduce monthly city-level construction investment by $99 million. This estimate is mildly less precise, but corroborates the main conclusion. Additional results, using logged dependent variable (Table C.2) and an alternative estimator (Table C.1), are contained in Appendix C, with these supplementary models supporting the main findings.

Table 3 suggests that anti-SLAPP legislation meaningfully influences real estate development. Construction investment is chilled by roughly $60 million per month relative to a counterfactual scenario without anti-SLAPP statutes. Changing civil procedure to restrict developers' ability to use private law does spillover into the real estate market.

Two-staged difference-in-differences and the results in Table 3 hinge on parallel trends. Figure 3 explores this assumption with a stacked event study model. By probing the dynamic effects of anti-SLAPP provisions, the evolution of the effect of anti-SLAPP legislation in the pre- and post-treatment periods can be examined. Figure 3 is generated via a stacked event study regres-

[24] Corresponding results using city-level fixed effects are contained in Appendix C. Province-level fixed effects are preferred in the main text as these correspond to the level of treatment and, as mentioned in section 2.2, the unit of observation is a CMA which frequently contains several distinct municipal planning authorities, so city fixed effects do not uniquely capture time invariant factors within a local planning region.

[25] Randomization p-values are calculated by running 500 placebo models, with treatment randomized across observations. Of those 500 regressions, only 2% yielded an estimate more extreme than the $61 million reduction in construction investment.

	(1)	(2)
Anti-SLAPP Law ($000,000)	-61.76	-99.25
	(17.06)	(18.51)
Randomization p-value	0.02	0.12
Observations	4,111	4,111

Column (1) contains year and province fixed effects. Column (2) uses province-year fixed effects. Standard errors, in parentheses, are clustered on city-year. Randomization p-values are calculated as the share of point estimates more extreme than the main specification out of 500 placebo replications.

Table 3: Effect of Anti-SLAPP Laws on Total Construction Investment

sion of the form (Callaway and Sant'Anna, 2021):

$$y_{pt} = \alpha_3 + \sum_{m=-l, m\neq 0}^{M} \delta_m \mathbb{1}\{SLAPP_p = t - m\} + \gamma_p + \tau_t + u_{it}$$

where $SLAPP_p$ is the first period that province p enacts anti-SLAPP laws. This model regresses construction investment on province and time fixed effects and relative time indicators. The variable $\mathbb{1}\{SLAPP_p = t - m\}$ equals 1 if province p has anti-SLAPP laws m periods ago. For For $m \geq 1$, $\hat{\delta}_m$ captures the cumulative effect of the $m + 1$ treatment periods. Similarly, for $m \leq -1$, $\hat{\delta}_m$ represents the a placebo coefficient, intended as a test of the parallel trends assumption (de Chaisemartin and D'Haultfoeuille, 2022). These pre-trend estimates compare estimates for jurisdicitons that will and will not pass anti-SLAPP legislation in m periods. The estimator of De Chaisemartin and D'Haultfœuille (2020) is used to generate bootstrapped standard errors for the pre-treatment period.

Figure 3 graphs two main results. First, importantly for the Gardner (2021) methodology, few notable pre-trends in construction investment are evident prior the law's enactment. Coefficients in the "pre" periods hover around, and are statistically indistinguishable from, zero. This suggests that common trends is a credible identifying assumption and two-staged difference-in-differences is a valid estimator for this research.

Second, Figure 3 illustrates a clear decrease in construction investment, one that is stable but delayed by roughly three months. The cummulative effect of anti-SLAPP laws on investment is roughly $80 million per month, matching the estimates in Table 3. The dynamic treatment effects represented in Figure 3 bolster the estimates from Table 3. Construction investment is chilled by anti-SLAPP laws.

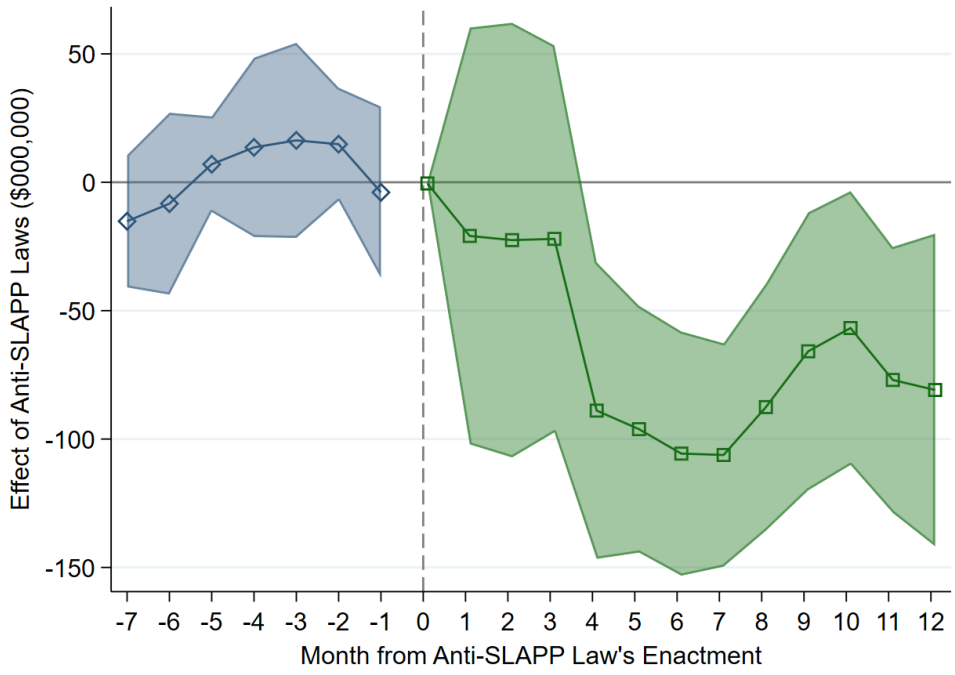

Figure 3: Dynamic Effect of Anti-SLAPP Laws on Construction Investment

Effect of Anti-SLAPP Laws on New Residential and Non-residential Investment

Table 4 builds on Table 3 and Figure 3 by investigating the effect of anti-SLAPP laws on new residential and non-residential investment. Whereas Table 3, includes total construction investment, conventional belief is that new developments are more likely to attract opposition and, hence, are more sensitive to anti-SLAPP legislation. Chilling new real estate development is not an explicit objective of anti-SLAPP laws, but these bills are designed to support public expression. Classic illustrations involve grassroots organizations fighting to stall or foreclose specific projects, protecting wetlands and open spaces from the incursion of the built environment. Therefore, understanding the implications on new development is particularly interesting.

Table 4 shows that anti-SLAPP laws chill both new residential and non-residential investment and that the effect is driven by big cities. Column (1) shows how anti-SLAPP laws affect new residential investment relative to a counterfactual scenario without anti-SLAPP statutes. Column (2) contains comparable values for non-residential construction. Three panels are included. Panel A displays estimates for the full sample of cities. Anti-SLAPP laws cause a $47 million per month decline in residential investment and a $51 million reduction in non-residential investment. Both parameters are precisely estimated. These estimates are large relative to the sample-wide average construction investment values. Panels B and C, therefore, decompose this

average effect into the part driven by large cities and small municipalities, respectively. Panel B shows the effect of anti-SLAPP laws on the 20 biggest cities in the sample, those with a population of greater than 250,000 people. Estimated magnitudes are slightly larger than the coefficients in Panel A, but are not statistically different. New residential investment declines by $58 million compared to a no anti-SLAPP counterfactual. New non-residential investment falls by a corresponding $69 million. Both effects are precisely estimated. These effects can be contrasted with the results in Panel C. Panel C presents the estimates from the sample of small cities. Effects sizes are dramatically attenuated. Anti-SLAPP laws lead to a small, but imprecisely estimated, *increase* in new residential investment paired with a small decrease in non-residential investment.

By comparing Panels B and C in Figure 4, a clear pattern is evident. The effect of anti-SLAPP laws are concentrated in larger cities, a conclusion that is further corroborated in Figure C.3 in Appendix C. Figure C.3 shows the effect of anti-SLAPP laws on total building investment across quartiles of the investment distribution. Applying a quantile regression variant of the two-stage difference-in-dfferences design, this graph illustrated that the effect of anti-SLAPP laws are concentrated on upper end of the distribution (i.e., larger cities).

	(1)	(2)
	Residential	Non-residential
Panel A: All Cities		
Anti-SLAPP Law ($000,000)	-47.34	-51.77
	(15.28)	(6.62)
Randomization p-value	0.00	0.01
Observations	5,148	5,148
Panel B: Large Cities		
Anti-SLAPP Law ($000,000)	-58.19	-69.46
	(23.34)	(10.18)
Randomization p-value	0.09	0.00
Observations	3,146	3,146
Panel C: Small Cities		
Anti-SLAPP Law ($000,000)	2.83	-4.27
	(0.77)	(0.39)
Randomization p-value	0.79	0.02
Observations	2,002	2,002

Both columns include interacted province-year fixed effects.
Standard errors are clustered on city-year.

Table 4: Effect of Anti-SLAPP Laws on New Residential and Non-residential Construction Investment

Taken together, the results of Tables 3 and 4 demonstrate how civil procedure influences the construction industry. Anti-SLAPP laws reduce real estate investment in Canadian cities with magnitudes that are economically meaningful. Decreases in investment are also concentrated in larger urban centres. More generally, this analysis offers insight into how the tort system affects incentives in the broader economy, suggesting that rules designed to weigh the trade-offs associated with speech, protest and development have real consequences. These consequences should factor in to how laws are drafted and applied (Galasso and Luo, 2018).

Finally, there is some question about whether the estimates in Tables 3 and 4 are too large. Straightforward additions to the rules of civil procedure, the argument is, cannot plausibly induce effect sizes as shown in these tables. On one hand, this critique seems warranted. The inability to engage in litigation should not introduce a large impediment to real estate investment. Yet, as an initial counterpoint, consider the Pointe Estates as shown in Figure B.2. This proposed development, the first challenged by Ontario's newly enacted anti-SLAPP law, contained 91 waterfront lots in a rural area of Ontario. A conservative estimate is that the average lot would sell for $400,000. Stopping this project, thus, yielded at least a $36 million dollar reduction in development in a CMA solidly in the bottom quartile of the investment distribution. More generally, anti-SLAPP laws are likely a harbinger of a more adversarial approach to real estate development. As described in section 1, changing legal rules changes in incentives both inside and outside of the courtroom. Often, these incentives alter economic behaviour even when key actions are unobserved. For example, the threat of a protest, rather than any observable action, may be sufficient to chill investment. Planning and regulatory proceedings are costly for developers. Eliminating the ability to protect investments via the courts may have large effects on the willingness to commence projects.[26] It is not immediately obvious that seemingly modest changes to legal procedure should necessarily rule out large effects on economic outcomes.

3.2 Heterogeneous Effect on New Housing Starts

As a next step, the effect of anti-SLAPP rules on physical construction is evaluated using a longer time series of data from CMHC. Focusing on housing starts offers a distinct unit of analysis. A "start" is defined as "the beginning of construction work on the building where the dwelling unit will be located" (CMHC, 2022b).[27] New single family housing developments often change the landscape in ways that provoke local backlash. Apartment starts are also evaluated. Apartment starts reflect the number of units expected in a given building, so, for example, if construction on a single 10 unit apartment complex was initiated in March 2017, 10 apartment starts are recorded for that period.

[26] To this point, a report by the CMHC (2022a, pg.7) recently argued that Canada faces "long-standing challenges of progressing through the regulatory system to get new housing built" so small changes to processes may have large implications.

[27] Pre-fabricated homes are omitted.

Table 5 shows that, relative to a counterfactual scenario without the enactment of anti-SLAPP laws, there is a striking decline in single family house starts combined with an offsetting increase in apartment starts. Column (1) shows that new single family housing starts decline by 119 per month following the passage of anti-SLAPP statutes. The standard error on this estimate is small. Column (2), in contrast, highlights that anti-SLAPP laws actually led to an increase in new apartment units. An additional 288 apartment units are started relative to a scenario without anti-SLAPP rules.

	(1)	(2)
	Single Family	Apartment
Anti-SLAPP Law	-119.0	287.6
	(12.5)	(47.2)
Randomization p-value	0.00	0.00
Observations	12,168	12,168

All models include interacted province-year fixed effects. Standard errors are clustered on city-year.

Table 5: Effect of Anti-SLAPP Laws on New Single Family and Apartment Starts

The stark contrast in these estimates suggests that developers may have responded to the new legislative regime by reallocating capital and attention in response to anti-SLAPP rules. New single family houses usually involve encroaching on previously undeveloped landscapes. New roads, sewers and electrical infrastructure are needed, activities that fundamentally alter the environment. To the extent that changing the natural landscape is more salient to local residents, greenfield development may invite grassroots opposition. Apartments, in contrast, tend to increase density in existing neighbourhoods. Rather than trying to develop larger, neighbourhood scale projects, developers appear to have refocused on these denser developments.

Figure 4 adds context to Table 5. Figure 4 replicates the stacked event study plot in Figure 3 for new single family housing starts. This graph illustrates the cumulative treatment effect and probes violations of the identifying assumption. There are two main results. First, common trends appears reasonable as an identifying assumption. Few meaningful deviations between treated and untreated cities are observed in the months prior to the enactment of anti-SLAPP laws. The bands illustrate 95% confidence intervals. Second, the dynamic effect of anti-SLAPP laws on new housing starts is immediate but mitigates over time. After 10 months the cumulative effect of anti-SLAPP legislation on new single family housing starts is negative, yet it is smaller and noisier than the immediate response. While speculative, this hints that developers may adapt to the new rules after an early learning phase or that the new rules are useful at stalling, but not fully eliminating, the creation of new neighbourhoods.

The results in Table 5 and Figure 4 provide evidence that there is a chilling effect from anti-

SLAPP laws and that the effect is concentrated in single family residential housing. New residential developments are often suburb-style projects on the periphery of cities. Frequently, these developments encroach on farm land, add traffic and density to otherwise sparsely populated areas and may alter environmentally sensitive regions. These may be precisely the developments most at risk of local opposition. In contrast, large, dense apartment complexes often re-purpose existing, but under-valued, land within urban boundaries. To the extent that developers are able to predict where they may confront opposition, they appear to have reallocated activity from new single family houses toward denser multi-unit complexes.

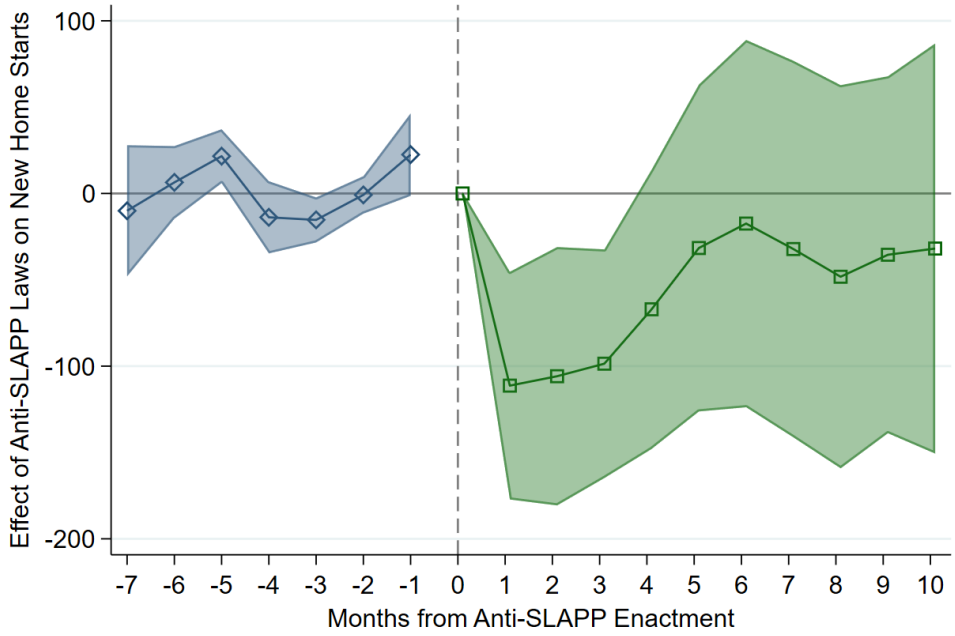

Figure 4: Dynamic Effect of Anti-SLAPP Laws on Single Family Home Starts

3.3 Extensions and Robustness Checks

Overall, the results show an economically meaningful and statistically significant reduction in real estate development following the enactment of anti-SLAPP laws. This is consistent with the idea that anti-SLAPP rules chill developer activity.

Appendix C contains a series of robustness checks bolstering the main conclusions. The results are robust to using a different, matching plus difference-in-differences estimator(Table C.1) and to using logged construction investment and housing starts (Tables C.2, C.3 and C.4). Table C.5 shows that the investment results are robust to including finer city-level fixed effects and that chilling effects are driven by larger cities, defined as those with a population greater than

250,000 residents. The results are robust to including dummies for Ontario's Bill 139.[28] Finally, the results hold after dropping all observations after March 2020, accounting for covid-related effects (Table C.7).

Next, Appendix D uses US data to explore two additional dimensions of anti-SLAPP laws. Anti-SLAPP legislation has been enacted in 31 US states. Table D.8, using data from Zillow, shows the effect of anti-SLAPP laws on house prices in US cities. House prices are an imperfect measure of the effect of anti-SLAPP laws on real estate as they conflate both new and existing housing stock plus average rents. Still, assuming that anti-SLAPP laws reduce the supply of new construction and that residential demand remains unchanged, a reasonable prediction is that prices of a "typical" or median home would increase following the enactment of anti-SLAPP laws. Stable demand growth, met with diminished supply growth, implies higher prices. This is precisely what Table D.8 shows. Relative to a counterfactual scenario where a state did not enact anti-SLAPP laws, the typical city-level price of a home increased. Moreover, the magnitude of the increase is larger in states that implemented "strong" anti-SLAPP laws as judged by anti slapp.org (2021).

Beyond their effects on real estate development, anti-SLAPP laws also affect public expression. The close connection between anti-SLAPP laws and defamation suggests that it is not merely whether a particular remark was expressed, but what precisely was said. Table D.9 explores the effect of anti-SLAPP laws on the emotional tone of public expression. As described in the appendix, US state-level data on the emotional tone of online public expression is collected from the Global Database of Events, Language, and Tone (GDELT) for the years 1997-2021. GDELT applies an algorithm to calculate the emotional "tone" of a sample of online expressions. These values are then averaged at the state-month level. Tonality is coded as a binary variable. Positive average tonality is defined as a one. Negative tone (or neutrality) is coded as a zero. Table D.9 shows that, relative to a counterfactual scenario without the provisions, online expression is more likely to have a negative tone after a state enacts anti-SLAPP laws. Moreover, states with "strong" anti-SLAPP laws see an even more negativity of expression.

4 Conclusion

Confronted with potentially expensive obstruction and delay, developers may use the courts to intimidate opponents. Protesters, faced with the prospect of costly and drawn-out legal battles, may self-censor and remain silent rather than voicing dissent with respect to new development. Anti-SLAPP laws are designed to eliminate this outcome. Anti-SLAPP laws attempt to remove the threat of bad faith suits, enabling citizens to freely and publicly express their views on eco-

[28] Bill 139 led to the *Local Planning Tribunal Act, 2017*. This bill updated and repealed the previous *Ontario Municipal Board Act*. It established a series of protocols for municipal zoning and planning, created a series of local appeal bodies and revamped the development appeal process. The law received Royal Assent in December 2017.

nomic activity. Yet, anti-SLAPP laws involve a trade-off. By protecting opposition to economic development, fewer projects will be planned and economic activity will be chilled.

SLAPPs rely on a complementarity between a purported harm, caused by the tortfeasor, and an outside project. Incorporating SLAPPs into a standard model of frivolous lawsuits demonstrates that the definition of a SLAPP is narrower than often contended. For a lawsuit to be classified as a SLAPP, the suit must both be frivolous on its merits and, in equilibrium, increase the probability that the business's project will be approved. Meritorious claims are not SLAPPs because they should proceed irrespectively. Likewise, if a protest does not influence the probability that a project will be approved, businesses will not pursue meritless cases as this yields additional legal costs with no commensurate benefit. SLAPPs must be frivolous and there must be an interaction between the purported harm and an outside project.

Empirically, this paper demonstrates that anti-SLAPP laws chilled construction investment and new home starts in Canada. The magnitude of estimated effects are notable. Relative to a counterfactual scenario without anti-SLAPP laws, investment declines by roughly $60 million per city per month. Similarly, 120 fewer new single family houses are started. The behavioural response is concentrated in more populous, urban centres. These results add to a scarce body of research on the effects of civil procedure on the broader economy. Anti-SLAPP laws, and civil procedure, more generally, have the potential to influence many areas of economic behaviour. Appreciating the effects of torts and legal rules on public activities warrants greater attention, especially as what occurs in the courtroom – and disagreements that fail to reach the courtroom – can have far-reaching implications for communities and businesses.

References

Abadie, A. and Imbens, G. W. (2006). Large sample properties of matching estimators for average treatment effects. *econometrica*, 74(1):235–267.

Altindag, D. T., Mocan, N. H., and Zhang, J. (2021). Freedom of speech, deterrence, and compellence in the parliament. Technical report, National Bureau of Economic Research.

anti slapp.org, P. P. P. (2021). Your state's free speech protect. Technical report.

Antonovics, K. L. and Sander, R. H. (2013). Affirmative action bans and the "chilling effect". *American law and economics review*, 15(1):252–299.

Beatty, J. W. (1997). The legal literature of slapps: A look behind the smoke nine years after pring and canan first yelled fire. *U. Fla. JL & Pub. Pol'y*, 9:85.

Bebchuk, L. A. (1988). Suing solely to extract a settlement offer. *The Journal of Legal Studies*, 17(2):437–450.

Bebchuk, L. A. (1996). A new theory concerning the credibility and success of threats to sue. *The Journal of Legal Studies*, 25(1):1–25.

Bezanson, R. P. (1986). The libel suit in retrospect: what plaintiffs want and what plaintiffs get. *Calif. L. Rev.*, 74:789.

Bone, R. G. (1997). Modeling frivolous suits. *University of Pennsylvania Law Review*, 145(3):519–605.

Bone, R. G. (2003). The economics of civil procedure. *The Oxford Handbook of Law and Economics*, 3.

Borusyak, K., Jaravel, X., and Spiess, J. (2021). Revisiting event study designs: Robust and efficient estimation. *arXiv preprint arXiv:2108.12419*.

Bradshaw, K. (2021). Stakeholder dynamics in land development projects. *The Journal of Legal Studies*, 50(S2):S53–S70.

Butts, K. and Gardner, J. (2021). Did2s: Two-stage difference-in-differences. *arXiv preprint arXiv:2109.05913*.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Canan, P. and Pring, G. W. (1988). Strategic lawsuits against public participation. *Social Problems*, 35(5):506–519.

CMHC, C. (2022a). Canada's housing supply shortages: Estimating what is needed to solve canada's housing affordability crisis by 2030. Technical report, https://www.cmhc-schl.gc.ca/en/professionals/housing-markets-data-and-research/housing-research/research-reports/accelerate-supply/housing-shortages-canada-solving-affordability-crisis.

CMHC, C. (2022b). What does cmhc do? *https://www.cmhc-schl.gc.ca/en/about-us/cmhcs-story (accessed January 14, 2022*.

Cooter, R. and Ulen, T. (2008). Law and economics.

Cooter, R. D. and Rubinfeld, D. L. (1989). Economic analysis of legal disputes and their resolution. *Journal of economic literature*, 27(3):1067–1097.

Cunningham, S. (2021). *Causal Inference*. Yale University Press.

De Chaisemartin, C. and D'Haultfœuille, X. (2020). Difference-in-differences estimators of intertemporal treatment effects. *Available at SSRN 3731856*.

de Chaisemartin, C. and D'Haultfoeuille, X. (2022). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. Technical report, National Bureau of Economic Research.

Dettmann, E., Giebler, A., and Weyh, A. (2020). Flexpaneldid: A stata toolbox for causal analysis with varying treatment time and duration. Technical report, IWH Discussion Papers.

Ecojustice (2010). Breaking the silence: The urgent need for anti-slapp legislation in ontario. Technical report.

Galasso, A. and Luo, H. (2018). When does product liability risk chill innovation? evidence from medical implants. Technical report, National Bureau of Economic Research.

Galasso, A. and Luo, H. (2022). When does product liability risk chill innovation? evidence from medical implants. *American Economic Journal: Economic Policy*, 14(2):366–401.

Gardner, J. (2021). Two-stage differences in differences.

GDELT (2022). The gdelt project. Technical report.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*.

Harsanyi, J. C. (1967). Games with incomplete information played by "bayesian" players, i–iii part i. the basic model. *Management science*, 14(3):159–182.

Heckman, J. J., Ichimura, H., Smith, J. A., and Todd, P. E. (1998). Characterizing selection bias using experimental data.

Heckman, J. J., Ichimura, H., and Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The review of economic studies*, 64(4):605–654.

Hurley, T. M. (1995). Private participation in public policy: the economics of strategic lawsuits against public participation.

Hurley, T. M. and Shogren, J. F. (1997). Environmental conflicts and the slapp. *Journal of Environmental Economics and Management*, 33(3):253–273.

Imai, K., Kim, I. S., and Wang, E. (2019). Matching methods for causal inference with time-series cross-sectional data. In *Center for the Study of American Politics— Yale University—ISPS CSAP Quantitative Research Methods Workshop*.

Kalajdzic, J. (2018). *Class actions in Canada: The promise and reality of access to justice*. UBC Press.

Kaplow, L. (1994). The value of accuracy in adjudication: An economic analysis. *The Journal of Legal Studies*, 23(S1):307–401.

Kaplow, L. and Shavell, S. (1994). Accuracy in the determination of liability. *The Journal of Law and Economics*, 37(1):1–15.

Katz, A. (1990). The effect of frivolous lawsuits on the settlement of litigation.

Kendrick, L. (2012). Speech, intent, and the chilling effect. *Wm. & Mary L. Rev.*, 54:1633.

Klerman, D. (2015). The economics of civil procedure. *Annual Review of Law and Social Science*, 11:353–371.

Leetaru, K. and Schrodt, P. A. (2013). Gdelt: Global data on events, location, and tone, 1979–2012. In *ISA annual convention*, volume 2, pages 1–49. Citeseer.

Miceli, T. J. (2009). *The economic approach to law*. Stanford University Press.

O. Reg. 575/07, s. . (2020). Rules of civil procedure. Technical report.

Ontario, A. G. (2010). Anti-slapp advisory panel report to the attorney general.

Pring, G. W. and Canan, P. (1996). *SLAPPs: Getting sued for speaking out*. Temple University Press.

Rasmussen, K. (2011). Slapp stick: Fighting frivolous lawsuits against journalists. *News Media and the Law*, 35:S1–S16.

Rosenberg, D. and Shavell, S. (1985). A model in which suits are brought for their nuisance value. *International Review of Law and Economics*, 5(1):3–13.

Statistics Canada, C. (2021). Investment in building construction. table 34-10-0175-01. *DOI: https://doi.org/10.25318/3410017501-eng*.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Young, H. (2017). The canadian defamation action: An empirical study. *Can. B. Rev.*, 95:591.

A Ontario's Two-part Anti-SLAPP Test: Interpretation based on the Pointes Protection Decision

Anti-SLAPP provisions add legal tests to existing rules of civil procedure. As an example, I outline Ontario's law and its interpretation in the Supreme Court of Canada's *Pointes Protection* decision.

Ontario's law was drafted via a multi-year process. In 2010, Ontario's Attorney General convened an Advisory Panel to investigate the prospect of implementing anti-SLAPP legislation (Ontario, 2010) and consider what that law might include. The Government had previously, in 2008, attempted to pass an anti-SLAPP bill, but this initial attempt failed on first reading. After extensive consultation, the Attorney General's Advisory Panel recommended that Ontario adopt a "two-part test" to determine whether a claim should be dismissed as a frivolous SLAPP case. In November 2015, Ontario introduced Bill-52, the *Protection of Public Participation Act*. This law largely embraced the Panel's recommendation by adding section 137 to the *Courts of Justice Act* ("*CJA*"). Notably, there Act created the "s.137 motion", as it became known, whereby a moving defendant seeks to dismiss a respondent plaintiff's civil claim on grounds that the suit is a SLAPP. The Supreme Court of Canada's first guidance on this law is the *Pointes Protection* decision.

The two-part test in Ontario's law is as follows. First, defendants, those moving to dismiss a suit on grounds that it is a SLAPP, must satisfy the "threshold burden". The threshold burden requires that the moving party to establish on a "balance of probabilities, that the proceeding arises from an expression ... relates to a matter of public interest" (2020 SCC 22). This threshold burden, itself, requires two steps. The moving party must demonstrate that the underlying issue arises from an "expression made by the moving party" and "that the expression relates to a matter of public interest" (at para. 21, 2020 SCC 22). Ontario's *CJA* defines "expression" in s. 137.1(2). An expression is "any communication, regardless of whether it is made verbally or non-verbally, whether it is made publicly or privately, and whether or not it is directed at a person or an entity" (CJA, 2021). The Supreme Court of Canada stressed that both "expression" and "public interest" should be interpreted expansively.

If the defendant, the moving party, meets the threshold burden, onus shifts to the plaintiff as respondent for the second part of the test. To prevent the proceeding from being dismissed, the plaintiff respondent must satisfy both a "merits-based hurdle" and a "public-interest hurdle". The merits-based hurdle states that the plaintiff's claim has "substantial merit" *and* the defendant "has no valid defence". The language used by the Supreme Court when interpreting substantial merit is "something more than mere suspicion but less than proof on the balance of probabilities" (at para. 40). The plaintiff's initial claim, which is usually a defamation claim, must have a "real prospect of success". The Court also offered clarity on how it interprets as no

valid defense. Finally, plaintiffs must also satisfy the public-interest hurdle as described in the *CJA*. The public-interest hurdle states that the plaintiff respondent must demonstrate, on a balance of probabilities, that the harm suffered as a result of the defendant's expression "is sufficiently serious that the public interest in permitting the proceeding to continue outweighs the public interest in protecting that expression" (at para. 126).

B Background Maps

B.1 Canadian Census Metropolitan Areas

Figure B.1 is an example of a typical CMA in Canada. CMAs tend to include an major urban centre plus a range of proximate smaller municipalities. Often the geographies include substantial rural and agricultural land. Figure B.1 shows the CMA of London, Ontario. The CMA has a population of 543,551 according to the 2021 population census.

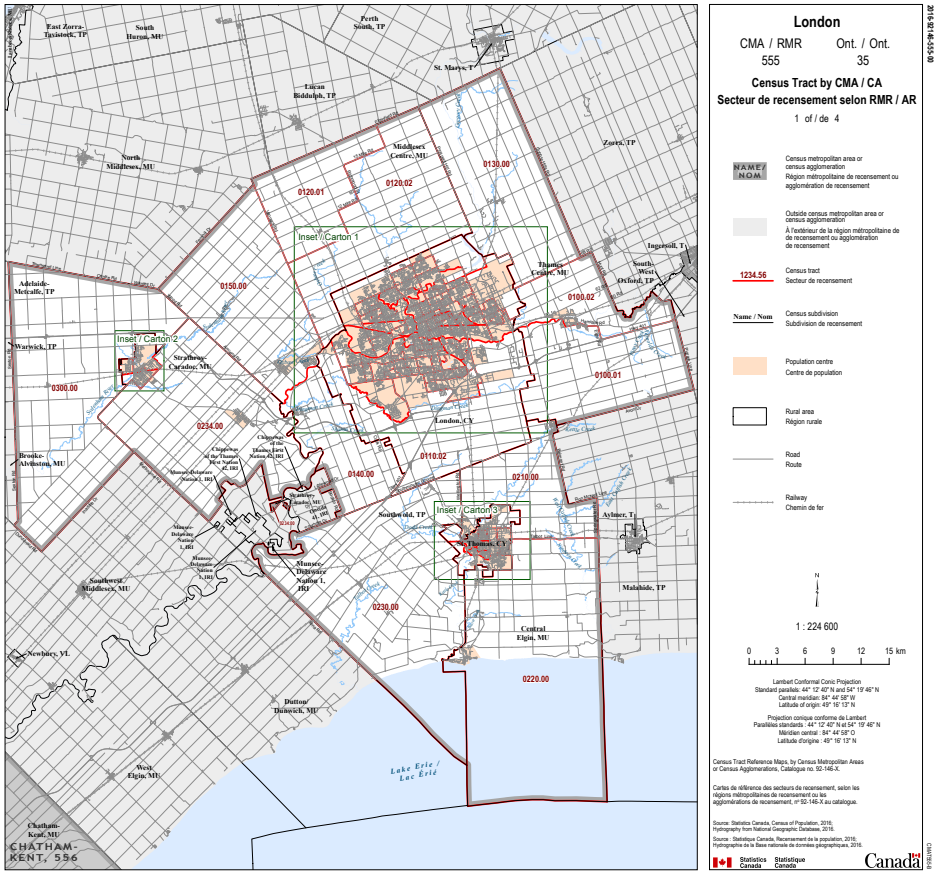

Figure B.1: Example of a Canadian Census Metropolitan Area, London

B.2 Pointe Estates

The *Pointes Protection* decision related to a proposed residential real estate development in Sault Ste Marie, Ontario. Figure B.2 illustrates the proposed waterfront development. In total, the Developer intended to sell 91 lots.

Figure B.2: Proposed Pointe Estate Development

C Additional Results and Robustness Checks

C.1 Replicating Main Results with a Different Estimator

The results in the main text are supported by applying a matching-plus-difference-in-differences estimator, initially developed by Heckman et al. (1997, 1998) and adapted to a staggered adoption designs by Dettmann et al. (2020) and Imai et al. (2019).

The Heckman et al. (1997, 1998) estimator takes the form:

$$\begin{aligned}\delta_{MATCH} &= \mathbb{E}\left[Y_{i1}(1) - Y_{i1}(0) | X_i, SLAPP_i = 1\right] \\ &= \frac{1}{N_1} \sum_{j \in I_1} \left\{ \left(Y_{j1}(1) - Y_{j0}(0) \right) - \sum_{k \in I_0} \omega_{jk}(X_j, X_k) \cdot (Y_{k1}(0) - Y_{k0}(0) \right\} \end{aligned} \tag{6}$$

where I_1 refers to cities that are treated with anti-SLAPP legislation and I_0 are matched cities in untreated provinces. Cities subject to anti-SLAPP legisliation are indexed with j, while control cities are indexed with k.

Implementation, like with two-staged difference-in-differences, proceeds in two stages. First, matching is completed. The weights used in the counterfactual matching exercise are ω_{jk}, where matching is nearest neighbour, with a radius caliper. These weights are selected to provide a counterfactual estimate of the treated units in an untreated counterfactual scenario. I use a parsimonious matching strategy and match treated to control cities based on population and unemployment rates. This allows me to verify covariates are balanced between treated and control units. I do not match on pre-treatment trends. It is through this matching process that the problem of differential timing is solved. The data are processed to align units based on treatment history, avoiding the inappropriate comparisons that render conventional two-way fixed effects questionable. Implementation, in fact, avoids both the second and third comparisons in Goodman-Bacon (2021), by selecting weights, ω_{jk} for untreated compared to treated units, which may equal zero. The second step then recovers the parameter of interest from the following regression:

$$(Y_{j,t} - Y_{j,2011}) - (Y_{k,t} - Y_{k,2011}) = \alpha_2 + \delta_{MATCH} D_{it} + \nu_{jt} \tag{7}$$

where D_{it} is defined as above and δ_{MATCH} is the estimand of interest, the average treatment effect on the treated of anti-SLAPP laws.

Table C.1 shows the results for aggregate construction investment in both levels and logs. A clear negative effect, as in the main text, is apparent, although less precisely estimated. Because different estimands are generated, the parameter of interest in Table C.1 is not directly comparable to those in, say, Table 3. Still, the qualitative pattern supports the main conclusion that anti-SLAPP laws suppress real estate development.

Table C.1: Effect of Anti-SLAPP Laws on Construction Investment

	(1)	(2)
Effect of Anti-SLAPP Law ($000,000)	-22.51	-0.13
	(22.64)	(0.11)
Transformation of DV	Levels	Logs
Treated cities	19	24
Matched controls	9	10

Matches based on population and the unemployment rate. Standard errors are from Abadie and Imbens's (2006) bias-corrected estimator for matching.

C.2 Robustness Checks

The following five tables present a series of robustness checks for results presented in the main text.

C.2.1 Logged Dependent Variables

Three tables – Tables C.2, C.3 and Table C.4 – replicate Tables 3 from the main text, using logged dependent variables. Results are consistent with those presented above.

	(1)	(2)
	Logs	Logs
Anti-SLAPP Law	-0.30	-0.56
	(0.10)	(0.21)
Randomization p-value	0.00	0.01
Observations	4,111	4,111

The dependent variable is logged millions of CAD. Column (1) contains year and province fixed effects. Column (2) uses province-year fixed effects. Standard errors, in parentheses, are clustered on city-year. Randomization p-values are calculated as the share of point estimates more extreme than the main specification out of 500 placebo replications.

Table C.2: Effect of Anti-SLAPP Laws on Total Construction Investment, Logged Dependent Variable

	(1)	(2)
	Residential	Non-residential
Anti-SLAPP Law	-0.75	-1.28
	(0.11)	(0.10)
Randomization p-value	0.00	0.00
Observations	5,148	5,148

The dependent variable is logged millions of CAD. Both columns include interacted province-year fixed effects. Standard errors, in parentheses, are clustered on city-year.

Table C.3: Effect of Anti-SLAPP Laws on New Residential and Non-residential Construction Investment

	(1)	(2)
	Single Family	Apartment
Anti-SLAPP Law	-0.64	2.24
	(0.10)	(0.17)
Randomization p-value	0.00	0.00
Observations	12,168	12,168

The dependent variable is logged starts. All models include interacted province-year fixed effects. Standard errors, in parentheses, are clustered on city-year.

Table C.4: Effect of Anti-SLAPP Laws on New Single Family and Apartment Starts, Logged DV

C.2.2 Quartiles of the Building Investment Distribution

Figure C.3 illustrates the effect of anti-SLAPP legislation on building investment at quartiles of the investment distribution. These regressions were constructed by applying a modified version of Gardner's (2021) two-stage difference-in-differences estimator. To start, I replicated Gardner's (2021) first-stage, applying the Frisch-Waugh-Lovell theorem to removed province and year effects. Second, as with the models in the main text, an "adjusted" dependent variable was constructed. The data using these adjusted dependent variables were then re-ordered to match the distribution of the raw building investment data. Finally, quantile regressions were estimated using the adjusted and reordered data.

Figure C.3 demonstrates that the majority of the effect of anti-SLAPP laws on real estate investment in concentrated at the higher end of the building investment distribution. In fact, the estimated effect in the first quartile is negative but not economically meaningfully different from zero. In contrast, the effect in the upper quartile is economically large and statistically different

from zero.[29]

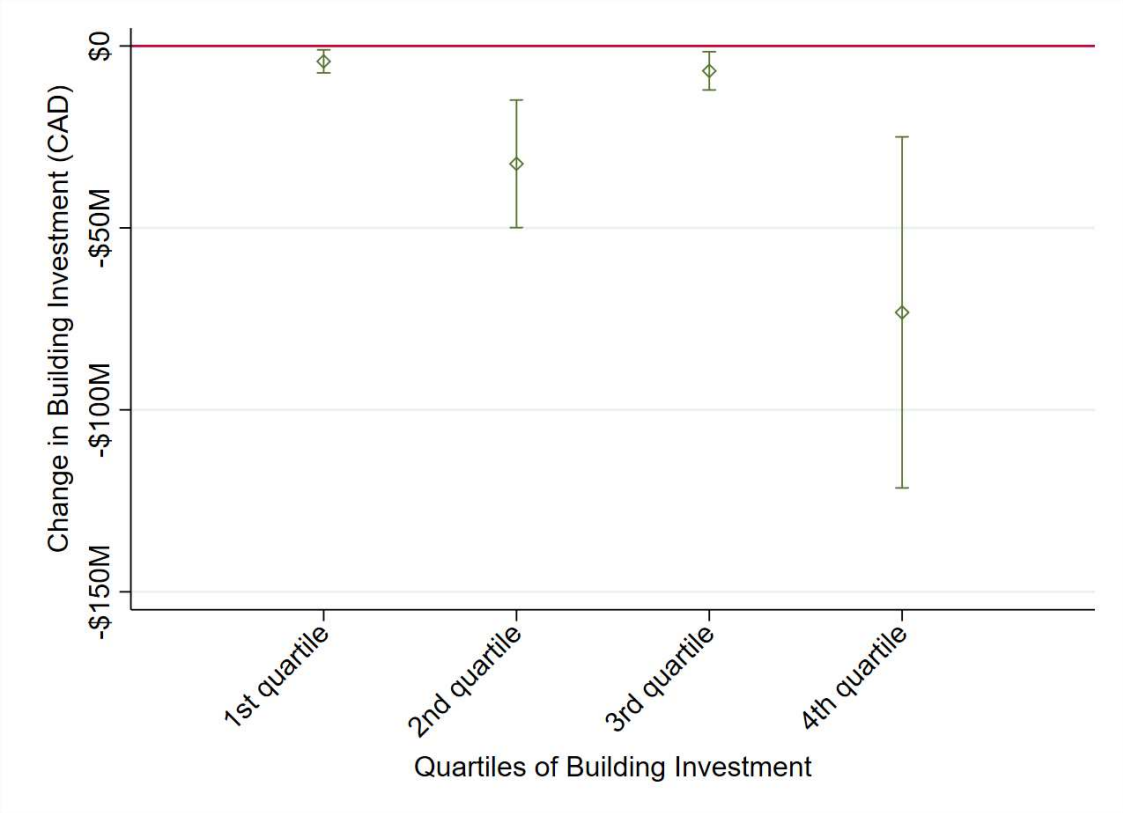

Figure C.3: Quantiles Regression Estimates of the Effect of Anti-SLAPP Laws on Total Building Investment

C.2.3 Results with City-level Fixed Effects

Table C.5 investigates total construction investment using city-level fixed effects and studying heterogeneity by city-size. These are finer grained fixed effects than in the main text. City and year fixed are included for three groups: all cities, cities with more than 250,000 residents and cities with fewer than 250,000 residents. Results illustrate that the main effect is driven by larger centers. Small cities have statistically insignificant, small and positive coefficient estimates. Table C.6 includes city fixed effects but uses single family housing and apartment starts as the dependent variables.

[29]The confidence intervals in Figure C.3 are bootstrapped.

	(1)	(2)	(3)
Restriction on cities:	All	Large	Small
	Dependent Variable in Levels		
Anti-SLAPP Law	-247.1	-441.0	10.9
	(86.9)	(137.2)	(3.1)
	Dependent Variable in Logs		
Anti-SLAPP Law	0.26	-0.17	0.17
	(0.09)	(0.07)	(0.05)
No. of Cities	35	19	16
Observations	4,111	1,983	2,128

All models include city and year fixed effects. Large cities are defined as having a population greater than 250,000 residents. One city changes categorization, moving from small to large, during the period of analysis. Standard errors are clustered on city-year.

Table C.5: Effect of Anti-SLAPP Laws on Total Construction Investment, City-level Fixed Effects

	(1)	(2)
	Single Family	Apartments
	Dependent Variable in Levels	
Anti-SLAPP Law	-27.84	253.76
	(12.37)	(47.18)
	Dependent Variable in Logs	
Anti-SLAPP Law	-0.44	0.60
	(0.10)	(0.17)
Observations	12,168	12,168

All models include interacted city-year fixed effects. Standard errors, in parentheses, are clustered on city-year.

Table C.6: Effect of Anti-SLAPP Laws on Starts, City-Year Fixed Effects

C.2.4 COVID Robustness: Dropping Observations from 2020 and 2021

Table C.7 shows the main construction investment and housing start estimates, excluding observations during the covid pandemic. Specifically, all observations after January 2020 are dropped from the sample.

	(1)	(2)
	Investment	Starts
Anti-SLAPP Law	-105.33	-122.61
	(20.01)	(15.30)
Observations	3,669	12,144

All models include interacted province-year fixed effects. Standard errors are clustered on city-year.

Table C.7: Effect of Anti-SLAPP Laws on Construction Investment and New Single Family Starts, Excluding Observations Affected by Covid

D Extensions using US Data

As described in section 3.3, US data are used to study two extensions to the main results. Section D.1 investigates city-level home prices against a counterfactual where those cities were not subject to anti-SLAPP rules. Section D.2 looks at the positivity of the emotional tone of public expression in states with anti-SLAPP laws compared to a counterfactual scenario where those laws were not introduced.

Data on US SLAPPs were obtained from anti slapp.org (2021). anti slapp.org (2021) is part of the Public Participation Project, a project that maintains a database on state-level SLAPP legislation.[30] There are 31 states with some form of anti-SLAPP provision. These rules were introduced at different points over three decades. anti slapp.org (2021) collects information on these laws as well as any revisions or repeals. anti slapp.org (2021) also grades the stringency of the state's legislation. Grades range from "A", for states such as Colorado to "F", when the state does not have any anti-SLAPP statues. The variable, "strong SLAPP", represents the subset of states who are deemed to have strong anti-SLAPP legislation according to anti slapp.org (2021). Strong SLAPPs are defined as having greater than a "B" grade in the database. Gardner's (2021) estimator is used to obtain coefficient values for all models.

D.1 SLAPPs and House Prices

Table D.8 shows results from four regression models. These models use Zillow monthly median house price data for 933 US cities from January 2000 to December 2021 (https://www.zillow.com/research/data/ Information is not available for all cities over the entire period, so the data are unbalanced.

Table D.8 shows that states that cities located in states with anti-SLAPP laws experienced larger median house price increases than a counterfactual scenario where they did not pass those laws. Further, the effect is more pronounce in states with "strong" anti-SLAPP legislation as judged by anti slapp.org (2021).

D.2 Sentiment of Public Expression

Anti-SLAPP laws chill real estate investment. A goal of these laws is to also fortify public expression. Expression is challenging to define and measure. Table D.9 explores the effect of anti-SLAPP laws on the emotional tone or sentiment of public expression.

Data on public expression and its emotional tone is collected from the Global Database of Events, Language, and Tone (GDELT). Relying on "hundreds of thousands" of broadcast, print and online news sources, the GDELT project has collected and categorized more than a quarter-billion event records, tagged by location and time. Its objective is "to construct a catalog of hu-

[30] For a small number of situations, when enactment dates for specific rules were unclear in anti slapp.org (2021), state's government websites were consulted.

	(1)	(2)	(3)	(4)
	Levels	Levels	Logs	Logs
Anti-SLAPP Law (USD)	24,462.6		0.02	
	(1,268.4)		(0.01)	
Strong Anti-SLAPP Law (USD)		109,347.1		0.40
		(4,625.1)		(0.02)
Observations	173,679	173,679	173,679	173,679

The dependent variable is median (or "typical") house prices in USD. All models include interacted state-year fixed effects. Standard errors, in parentheses, are clustered on city-year. Median home prices provided by Zillow (https://www.zillow.com/research/data/).

Table D.8: Effect of US Anti-SLAPP Laws on Median Home Prices

man societal-scale behavior and beliefs across all countries of the world, connecting every person, organization, location, count, theme, news source, and event across the planet into a single massive network that captures what's happening around the world, what its context is and who's involved, and how the world is feeling about it, every single day" (Leetaru and Schrodt, 2013; GDELT, 2022). GDELT is accessible through Google's BigQuery data warehouse.

An extremely wide array of variables are collected in GDELT. Extensive documentation on what these variables measure and how they are collated are found on GDELT (2022). I focus on US state-level event records. These data are collected every 15 minutes, information that is aggregated to the state-month level for the years 1997-2021. The analysis uses a dataset with 14,682 observations, with events assigned to states according to the text in the expression. GDELT measures the average emotional "tone" of each expression. The close connection between anti-SLAPP laws and defamation suggest that it is not merely whether a particular remark was expressed, but what precisely was said. Tone helps to capture this dimension. Tonal scores range from -100, indicating an extremely negative expression, to +100 for extremely positive. The average emotional tone equals 3.4 and has a narrow, bi-modal distribution. A clear positive tonal peak is evident as is a marked negative peak (a smaller positive peak near zero is also apparent). Given this distribution, tonality is coded as a binary variable. Positive average tonality is defined as a one. Negative tone (or neutrality) is coded as a zero.

Table D.9 shows results from two linear probability models. The dependent variable for both takes a value of one, if the average tone of an expression in a state, within a specific month, is recorded as positive. The point estimates, thus, show the marginal effect of passing anti-SLAPP laws on having a positive average emotional tone. The point estimate in column (1), as a start, shows that passing anti-SLAPP laws reduce the probability that the average expression is positive by 33%. This coefficient is precisely estimated and the model controls for state-year effects,

while clustering the standard errors by state-year. Column (2), using the same model as column (1), studies states with strong anti-SLAPP laws and shows a large reduction in the probability that expression has a positive emotional tone. Column (2) shows that states that implement strong anti-SLAPP laws have a 45% smaller probability of having a positive average expression. The effect is precisely estimated, with both a precise standard error and a randomization p-value equal to 0.00.

Table D.9: Effect of Anti-SLAPP Laws on the Tone of Expression

	(1)	(2)
Anti-SLAPP Law	-0.33	
	(0.02)	
Strong Anti-SLAPP Law		-0.45
		(0.06)
Randomization p-value	0.00	0.00
Observations	14,682	14,682

The dependent variable is an indicator taking the value of one when the average tone of expression is positive. Coefficients represent marginal effects from a linear probability model. All models include state-year fixed effects. Standard errors, in parentheses, are clustered on state-year. Randomization p-values are calculated based on 500 placebo regressions.